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UNITED STATES DISTRICT COURT

FOR THE SOUTHERN DISTRICT OF CALIFORNIA

JAMES MILLER, et al.,

Plaintiffs,

vs.

XAVIER BECERRA, in his official capacity as Attorney General of California, et al.,

Defendants.

Case No. 3:19-cv-01537-BEN-JLB

Hon. Roger T. Benitez
Magistrate Hon. Jill L. Burkhardt

NOTICE OF ERRATA RE: PLAINTIFFS MOTION FOR PRELIMINARY INJUNCTION

Complaint filed: August 15, 2019
Amended Complaint filed: September 27, 2019

Date: Thursday, January 16, 2020
Time: 10:00 a.m.
Department: Courtroom 5A (5th floor)

TO THE COURT, ALL PARTIES, AND THEIR ATTORNEYS OF RECORD:

Plaintiffs file this Notice of Errata to correct inadvertent errors in Plaintiffs' Notice of Motion and Motion for Preliminary Injunction, the Memorandum of Points and Authorities, and the Declarations of Plaintiffs James Miller, Wendy Hauffen, Ryan Peterson, John Phillips, Neil Rutherford, Adrian Sevilla, and the Declarations of John Lott, and Ashley Hlebinsky, all of which were filed on December 6, 2019.

In reviewing the motion and supporting papers, Plaintiffs noticed a few typographical errors, as follows:

1. On the caption page of the notice, the memorandum of points and authorities, and the declaration of Ashley Hlebinsky, the Judges assigned to this case were not properly identified. The caption pages have been corrected and properly identify "Hon. Roger T. Benitez" and "Magistrate Hon. Jill L. Burkhardt."
2. On page two of the notice of motion, Plaintiffs' incorrectly state, "This motion is also based on the *Second* Amended Complaint, pleadings, and records already on file…." This has been corrected to refer to the operative pleading, the "First Amended Complaint."
3. In reviewing Plaintiffs' memorandum of points and authorities, Plaintiffs noted the need to properly align the table of authorities and add four cases inadvertently omitted from the table, but cited in the brief. Plaintiffs have also included the full citation of *Jackson v. City of San Francisco* 746 F.3d 953 (9th Cir. 2014) on page 19 of the points and authorities.
4. In reviewing the Declarations of Plaintiffs James Miller, Wendy Hauffen, Ryan Peterson, John Phillips, Neil Rutherford, and Adrian Sevilla, the relief

requested did not encapsulate the entirety of the relief sought. Therefore, the last paragraph of each Plaintiff declaration was revised, and the declarations were re-signed.

5. In reviewing the Declaration of expert Dr. John Lott, Dr. Lott realized that his previous residence was inadvertently listed on page one of his declaration. His current residence has been corrected. Also, pin citations in paragraphs 20 and 21 of his declaration were corrected.

The corrected versions of the Notice of Motion and Motion for Preliminary Injunction, Plaintiffs' Memorandum of Points and Authorities in Support of Motion for Preliminary Injunction, and the Declarations of James Miller, Wendy Hauffen, Ryan Peterson, John Phillips, Neil Rutherford, Adrian Sevilla, Ashley Hlebinsky, and John Lott are filed simultaneously with this Notice of Errata.

December 13, 2019

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Defendants.

Case No. 3:19-cv-01537-BEN-JLB

Hon. Roger T. Benitez
Magistrate Hon. Jill L. Burkhardt

PLAINTIFFS' NOTICE OF MOTION AND MOTION FOR PRELIMINARY INJUNCTION

Complaint filed: August 15, 2019
Amended Complaint filed: September 27, 2019

Date: Thursday, January 16, 2020
Time: 10:00 a.m.
Department: Courtroom 5A (5th floor)

TO THE COURT, ALL PARTIES, AND THEIR ATTORNEYS OF RECORD: Notice is hereby given that on January 16, 2020, at 10:00 a.m. in Courtroom 5A (5th floor) of the above-captioned Court, located at 221 W. Broadway, San Diego, California 92101, Plaintiffs will move for preliminary injunction under Rule 65(a) of the Federal Rules of Civil Procedure. Specifically, Plaintiffs will seek an order enjoining Defendants Attorney General Xavier Becerra and his agent, servants, employees, and those working in active concert with him, from enforcing or giving effect to California Penal Code sections 30515 (a) and (b), 30600, 30605, 30800, 30910, 30915, 30925, 30945, 30950, 31000, and 31005, as well as Title 11, California Code of Regulations section 5460 and 5471 during the pendency of this action.

Plaintiffs bring this motion on the grounds that California Penal Code sections 30515 (a) and (b), 30600, 30605, 30800, 30910, 30915, 30925, 30945, 30950, 31000, and 31005, as well as Title 11, California Code of Regulations section 5460 and 5471 apply a categorical ban on common semiautomatic firearms with common characteristics in violation of the Second Amendment of the U.S. Constitution. The criminalization of the Plaintiffs and other law-abiding individuals from, inter alia, keeping, bearing, buying, selling, transferring, possessing, transporting, or passing down to heirs or other by bequest of so-called "assault weapons" infringes on the Second Amendment's core right of the individual right to keep and bear arms for self defense and other lawful purposes.

The motion is based on this notice of motion and motion, the memorandum of points and authorities field concurrently with this motion, and the supporting and concurrently filed Declarations of Jim Miller, Neil Rutherford, Ryan Peterson, Adrian Sevilla, John Phillips, Wendy Hauffen, Michael A. Schwartz, Gene

Hoffman, Alan Gottlieb, Brandon Combs, Adam Kraut, Allen Youngman, Ashley Hlebinsky, Emanuel Kapelsohn, George A. Mocsary, James Curcuruto, and Dr. John Lott. This motion is also based on the First Amended Complaint, pleadings, and records already on file, and on any further matters the Court deems appropriate at or before the time of the hearing.

December 6, 2019

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Case No. 3:19-cv-01537-BEN-JLB

Hon. Roger T. Benitez
Magistrate Hon. Jill L. Burkhardt

PLAINTIFFS' MEMORANDUM OF POINTS AND AUTHORITIES IN SUPPORT OF MOTION FOR PRELIMINARY INJUNCTION

Complaint filed: August 15, 2019
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TABLE OF CONTENTS

TABLE OF CONTENTS

TABLE OF AUTHORITIES

Cases

TABLE OF AUTHORITIES

Cases

TABLE OF AUTHORITIES

Cases

California Penal Code

TABLE OF AUTHORITIES

TABLE OF AUTHORITIES

California Code of Regulations

Code

Other Authorities

I. INTRODUCTION

Semiautomatic firearms with various popular characteristics are among the most common arms in the country. And among some of the most common characteristics of such firearms are ammunition magazines that can be removed and that can hold more than ten rounds, different types of ergonomic grips, adjustable stocks, and muzzle devices that reduce flash. The State of California is one of only six states to single out and ban some of the most popular firearms in the nation because they possess such common characteristics. The State disparagingly and arbitrarily calls the entire class of such firearms "assault weapons," and imposes severe penalties for their possession, transfer, and use for otherwise lawful purposes. But forty-four States impose no prohibitions based on such common characteristics. Indeed, law-abiding citizens throughout the country own tens of millions of such firearms and use them for lawful purposes, including self-defense, proficiency training, sport, and hunting. And these firearms—neither uniquely dangerous nor unusual—are rarely used in crime.

California's ban on this common class of firearms, and Defendants' enforcement of same, violates the Second Amendment. As the Supreme Court explained in *District of Columbia v. Heller*, 554 U.S. 570, 624-25 (2008), the Second Amendment protects the right to of individuals to keep and bear arms that are in common use for lawful purposes, such as self-defense, sport, hunting, and maintaining preparedness for service in the militia.

In *Duncan v. Becerra*, 366 F. Supp.3d 1131 (S.D. Cal. 2019), this Court recognized that the Second Amendment protects the right to keep and bear common arms and firearm magazines that are useful for self-defense or use in a militia, and declared unconstitutional and enjoined California's ban on so-called "large-capacity" magazines.

This case is a logical result of *Duncan's* analysis and seeks nothing more or less for the common arms that can use those magazines.

II. FACTUAL BACKGROUND

A. California's Assault Weapon Control Act

California's Roberti-Roos Assault Weapons Control Act of 1989 (AWCA), California Penal Code section 30500, *et seq.*,[1] established an arbitrary class of firearms pejoratively categorized as "assault weapons," and threatens severe criminal penalties for the acquisition, transportation, use, and transfer of those common firearms.

The AWCA's ban initially covered firearms as identified by a list of specific makes and models.[2] It was later expanded to include arms with common characteristics, such as so-called "large-capacity" magazines (LCMs),[3] and ultimately the broad category of common firearms and common characteristics at issue in this case.[4]

Under the AWCA, a rifle is an "assault weapon" if it is: (1) a semiautomatic, centerfire rifle that does not have a "fixed magazine" [5] but does have a pistol grip that

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[1] Further statutory citations are to the California Penal Code unless otherwise noted.

[2] Section 30510 (former § 12276); Senate Bill 263 (1991-92 Reg. Sess.); 11 California Code of Regulations (C.C.R.) § 5499.

[3] Section 30515(a); § 16740 (LCM defined as "any ammunition feeding device with the capacity to accept more than 10 rounds" unless specifically excepted).

[4] Section 30515, as amended by Senate Bill 880 and Assembly Bill 1135 (2015-16 Reg. Sess.).

[5] "Fixed magazine" means an ammunition feeding device contained in, or permanently attached to, a firearm in such a manner that the device cannot be removed without disassembly of the firearm action. § 30515(b); *see also* 11 C.C.R. §§ 5471(a-b), (f), (k), (m-n), and (p). Semiautomatic firearms that have "fixed magazines" with the otherwise-proscribed characteristics generally are not considered "assault weapons" unless identified as such by other provisions of the law, such as § 30515(a)(2).

protrudes conspicuously beneath the action of the rifle, a thumbhole stock,[6] a folding or telescoping stock, a grenade or flare launcher,[7] a flash suppressor,[8] and/or a forward pistol grip (section 30515(a)(1)(A)-(F)); or (2) a semiautomatic, centerfire rifle that has a fixed magazine with the capacity to accept more than 10 rounds (section 30515(a)(2)); or, (3) a semiautomatic, centerfire rifle that has an overall length of less than 30 inches (section 30515(a)(3)). Comparable provisions, also challenged and sought to be enjoined here, define common pistols and shotguns with various common characteristics as prohibited "assault weapons." §§ 30515(a)(4)-(8); *see also* 11 C.C.R. §§ 5459-60; 5469-71; 5472-78 (regulations implementing expanded definitions).

The State's complicated AWCA ban leads to various odd consequences and

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[6] A "thumbhole" stock simply allows the thumb of the user's trigger hand to be inserted into a hole in the stock, providing some users with a better grip, and hence better control over a firearm, than with another pistol-style grip.

[7] Grenades and grenade launchers are separately and heavily regulated by the federal government (as "destructive devices" pursuant to the National Firearms Act of 1934) and the State. Flare launchers, by contrast, are "used to launch signal flares," 11 C.C.R. § 5471(q), and can have a legitimate safety and rescue purpose. *See* Declaration of Emanuel Kapelsohn (Kapelsohn Dec.) ¶ 31, filed herewith. There is little evidence of any criminal use of flares.

[8] A "flash suppressor" is defined as "any device attached to the end of the barrel, that is designed, intended, or functions to perceptibly reduce or redirect muzzle flash from the shooter's field of vision. A hybrid device that has either advertised flash suppressing properties or functionally has flash suppressing properties would be deemed a flash suppressor." 11 C.C.R. § 5174(r). A "muzzle brake" is "[a] muzzle attachment or feature that uses the propellant combustion gas with the desired effect of redirecting the recoil," a "compensator" is "[a] muzzle attachment or feature to redirect propellant gases with a goal of reducing muzzle lift," and a "flash hider" (also known as a "flash suppressor") is "[a] muzzle attachment designed to reduce muzzle flash." https://saami.org/saami-glossary. "Flash suppressors" and other devices, like many "muzzle brakes" and "compensators" that "functionally" have secondary "flash

divisions among firearms. For example, an otherwise California-compliant semiautomatic "fixed magazine" firearm may lawfully possess one or more of the characteristics in section 30515(a)—but if a lawfully owned "large-capacity magazine" is merely inserted into that same firearm, it would immediately convert into an illegal "assault weapon," subjecting the user to multiple felony violations.

Tens of millions of common semiautomatic firearms with various combinations of common characteristics banned by California are possessed throughout the United States and are widely used for lawful purposes. The characteristics California uses to define "assault weapons," individually and collectively, are *neither* unusual nor dangerous. They instead provide material benefits to millions of law-abiding firearm users, including improved ergonomics, enhanced control and accuracy while firing, and safer operation. For example, a "[p]istol grip that protrudes conspicuously beneath the action of the weapon" assists in controlling common firearms such as AR-15s and is often a necessary design characteristic. Kapelsohn Dec. ¶ 28, **Ex. 12**. Similarly, a "folding" or "telescoping" adjustable stock, as defined in 11 C.C.R. sections 5471(nn) & (oo), is just a stock that is readily adjustable "to properly fit the user" and does not significantly affect the firearm's concealability. Kapelsohn Dec., ¶ 30, **Ex. 14**.[9] Firearms with adjustable stocks can be safer and more easily controllable by law abiding users—and thus safer for others—by allowing them to fit the firearm properly to their size, stature, and other factors. *Id*. A "flash suppressor" likewise improves

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suppressing properties," generally affix to common semiautomatic firearms used for lawful purposes by the use of a "threaded barrel." 11 C.C.R. § 5174(rr).

[9] Common semiautomatic firearms with traditional folding or telescoping stocks do not violate minimum length requirements, avoiding "short-barreled" categorization under 26 U.S.C. § 5801, *et seq.* and Penal Code §§ 17170, 17180.

safety by protecting the user's vision by mitigating muzzle flash directed at the firearm user, though others could still see the flash from other angles. "The use of a [firearm] without a flash suppressor under [low-light] circumstances is likely to temporarily blind the user, or at least seriously impair the user's vision, placing the law abiding user at a disadvantage to a criminal attacker." Kapelsohn Dec., ¶ 32, **Exs. 15, 16**. Such a characteristic would be important, for example, to a homeowner defending against a home invasion at night, when much violent crime occurs. See Declaration of Wendy Hauffen (Hauffen Dec.) ¶ 10, filed herewith.

Firearm control and safety are likewise improved by a "forward pistol grip"—"a grip that allows for a pistol style grasp forward of the trigger" (11 CCR section 5174(t)), and/or with regard to "assault weapon" pistols, a "second handgrip"—"a grip that allows the shooter to grip the pistol with their non-trigger hand" (11 C.C.R. section 5174(gg)). Having one's "non-trigger hand" help a user grip *any* type of firearm obviously will "assist the shooter in weapon control" before, during, and after firing it; and it is actually necessary for safe operation of many firearms, and thus improves safety for both the user and bystanders. Kapelsohn Dec. ¶ 33. Simply, the State is attempting to control where, or the angle that, a firearm owner decides to place their hands on their firearm.

Far from being the menacing hazards California implies when it categorizes firearms with such characteristics as "assault weapons," these firearms are instead a meaningfully safer and more controllable category of firearms in common use for lawful purposes. The AWCA nonetheless makes it a crime to possess such so-called "assault weapons," even by law-abiding private individuals for lawful purposes like self-defense in the home. § 30605(a); *Silveira v. Lockyer*, 312 F.3d 1052, 1059 (9th Cir. 2002). It generally imposes felony criminal penalties on the manufacture, distribution,

transportation, importation, keeping, offering, or exposing for sale, or giving or lending of any "assault weapon." § 30600(a). Further, violations of the AWCA subject firearms owners to other "civil" penalties of confiscation and destruction of their property, and severe fines. §§ 30800(a)-(d); 18005(c).

That some might use such safer firearms toward unlawful ends does not change the nature – or the Constitution's protection – of such firearms any more than the illegal use of any other arm changes the protected status of those tools. The common arms with common characteristics (and related conduct) California unconstitutionally bans are overwhelmingly possessed and used by law-abiding people for many lawful purposes. Plaintiffs' motion should be granted.

B. Plaintiffs' Injuries

The individual Plaintiffs are responsible adult California residents legally eligible to possess firearms. *See* Declarations of James Miller (Miller Dec.), Neil Rutherford (Rutherford Dec.), Ryan Peterson (Peterson Dec.), Adrian Sevilla (Sevilla Dec.), John Phillips (Phillips Dec.), and Hauffen Dec. filed herewith. Additionally, each of the individually named Plaintiffs are members of the organizational Plaintiffs. *Id.*; see also Declarations of Michael Schwartz (Schwartz Dec.), Gene Hoffman (Hoffman Dec.), Alan Gottlieb (Gottlieb Dec.), and Brandon Combs (Combs Dec.) filed herewith.

Plaintiffs Miller and Peterson lawfully own and possess semiautomatic firearms with characteristics such as pistol grips, collapsible stocks, flash hiders, and/or forward pistol grips that are not currently categorized as "assault weapons" because they have "fixed" magazines. Specifically, Plaintiff Miller owns a semiautomatic rifle and Plaintiff Peterson owns a semiautomatic pistol. Miller Dec. ¶¶ 4-10, Peterson Dec. ¶¶ 4-8. Plaintiffs Miller and Peterson also lawfully own and possess "large-capacity" magazines compatible with their firearms. *Id*. Plaintiffs Miller and Peterson wish to use

their magazines with their firearms while maintaining the common characteristics of their firearms such as a detachable magazine, pistol grip, collapsible stock, and/or flash suppressor, but do not because of the State's laws and fear of criminal prosecution. *Id*. Additionally, Plaintiffs Miller and Peterson would acquire additional semiautomatic firearms with said characteristics but for the State's laws and Defendants' policies, practices, and customs. Miller Dec. ¶¶8-9; Peterson Dec. ¶¶ 7-8.

Plaintiff Hauffen lawfully owns and possesses a semiautomatic, centerfire "featureless" rifle that does not have any of the other listed characteristics under section 30515(a) except a detachable magazine. Although functionally identical to many banned "assault weapons," it is not considered an "assault weapon." Hauffen Dec. ¶ 4. Plaintiff Hauffen purchased parts to convert her firearm into this configuration so it did not meet the definition of an "assault weapon" and thus allow her to possess, use, and eventually pass down her firearm to her heirs. *Id*. at ¶ 5. But for the AWCA and Defendants' enforcement of it, Plaintiff Hauffen would not have made this conversion. As a female firearms instructor, Plaintiff Hauffen prefers AR-15 style firearms for self-defense purposes and has selected this type of firearm specifically because of its characteristics. *Id*. at ¶¶ 8-9, **Ex. 1**. Plaintiff Hauffen would configure and use her firearm in a standard configuration with characteristics common throughout the country, but for California's laws, Defendants' policies, practices, and customs, and her fear of prosecution. *Id*. at ¶¶ 6-7.

Plaintiff Hauffen also owns a Sig Sauer P239 9mm semiautomatic pistol and wishes to be able to replace the firearm's standard barrel with a threaded barrel allowing her to readily attach either a flash suppressor or a muzzle brake. Hauffen Dec. ¶ 10. Plaintiff Hauffen would attach the muzzle brake to her pistol when using the gun for firearms instruction and recreational shooting. *Id*. She wants to readily change these

attachments and attach a flash suppressor to her pistol when carrying her pistol at night as she is a concealed weapons permit holder. *Id*. However, Plaintiff Hauffen is prevented from doing so because installing a threaded barrel on her semiautomatic pistol would render it an illegal assault weapon under the AWCA. § 30515(a)(4).

Plaintiffs Hauffen, Miller, Rutherford, Sevilla, and Peterson would acquire, possess, use, and transfer various models of pistols, rifles, and shotguns now covered by the AWCA due to their characteristics, but for the State's laws, Defendants' policies, practices, and customs, and their fear of prosecution. Hauffen Dec.¶ 10; Miller Dec. ¶¶7-9 ; Rutherford Dec. ¶¶ 4-5; Sevilla Dec. ¶¶ 4-5, and Peterson Dec. ¶ 6.

Plaintiff Gunfighter Tactical is owned and operated by Plaintiff Peterson. Plaintiff Gunfighter Tactical would acquire, sell, and otherwise lawfully transfer common firearms covered by the AWCA to ordinary lawful adults, but is prohibited by California's laws and Defendants' policies, practices, and customs, and a fear of loss of his licenses and prosecution. Peterson Dec. ¶¶ 9-11.

Plaintiff Poway Weapons and Gear (PWG) is owned and operated by Plaintiff Phillips. In addition to other state, federal, and local licenses and permits allowing operation as a legal firearms dealer and shooting range, Plaintiffs PWG and Phillips maintain a Dangerous Weapons Permit issued by the California Department of Justice and are permitted to sell "assault weapons" to exempt entities and individuals. Phillips Dec. ¶¶ 3-5. Plaintiffs PWG and Phillips would sell, or rent for use at the PWG range, common firearms covered by the AWCA to individual adults who are not prohibited from possessing or acquiring firearms but are prohibited by California's AWCA and Defendants' policies, practices, and customs, but for a fear of losing their licenses and prosecution. *Id*.

But for California's laws and Defendants' policies, practices, and customs

criminalizing the acquisition, possession, and use of common firearms covered by the AWCA due to their characteristics, Plaintiffs and other similarly situated adults in California would import, acquire, assemble/manufacture, transfer, use, transport, and pass down those common semiautomatic firearms. Hauffen Dec. ¶ 10; Miller Dec. ¶¶ 7-9; Rutherford Dec. ¶¶ 4-5; Peterson Dec. ¶ 6; and Sevilla Dec. ¶¶ 4-5. Thus, California's ban and Defendants' policies, practices, and customs criminalizing the acquisition, possession, and use of such firearms violates Plaintiffs' Second Amendment rights.

Organizational Plaintiffs San Diego County Gun Owners PAC (SDCGO), California Gun Rights Foundation (CGF), Second Amendment Foundation (SAF), and Firearms Policy Coalition (FPC) represent thousands of members and supporters with all of the indicia of membership, who are not prohibited from purchasing or possessing firearms, but who are similarly situated to the individually named Plaintiffs. *See* Schwartz Dec., Hoffman Dec., Gottlieb Dec., and Combs Dec. These members include but are not limited to adult individuals who currently have (i) firearms identified as assault weapons which cannot be transferred or passed down to their heirs or others by bequest; (ii) "fixed-magazine" semiautomatic, centerfire and rimfire firearms; (iii) "featureless" semiautomatic, centerfire firearms; (iv) lawfully owned and possessed "large-capacity" magazines; and (v) semiautomatic shotguns with non-detachable magazines (who wish to use standard, detachable magazines). Members of these organizations also include individuals who wish to acquire and use common semiautomatic firearms with common characteristics, train their children (minors under 18) on the safe handling and use of such firearms, and pass down their property to their heirs. Schwartz Dec. ¶¶ 3-8; Hoffman Dec. ¶¶ 3-8; Gottlieb Dec. ¶¶3-9; and Combs Dec. ¶¶ 4-6. The organizational Plaintiffs have expended and diverted time and

resources that could have been used on other programs due to the State's ban and Defendants' policies, practices, and customs. *Id*. The organizational Plaintiffs seek relief on behalf of themselves, their members and supporters, and similarly situated members of the public, because the Second Amendment rights of those individuals are violated, and continue to be violated, by California's AWCA ban and Defendants' polices, practices, and customs that enforce the ban.

III. LEGAL STANDARD

To obtain preliminary relief, a plaintiff "'must establish that he is likely to succeed on the merits, that he is likely to suffer irreparable harm in the absence of preliminary relief, that the balance of equities tips in his favor, and that an injunction is in the public interest.'" *Am. Trucking Ass'ns, Inc. v. City of Los Angeles*, 559 F.3d 1046, 1052 (9th Cir. 2009) (quoting *Winter v. Natural Res. Defense Council, Inc.*, 555 U.S. 7, 20 (2008)). Alternatively, injunctive relief "is appropriate when a plaintiff demonstrates that serious questions going to the merits [are] raised and the balance of hardships tips sharply in the plaintiff's favor." *Alliance for the Wild Rockies v. Cottrell*, 632 F.3d 1127, 1134-35 (9th Cir. 2011).

IV. ARGUMENT

A. Plaintiffs Are Likely to Succeed on the Merits.

The United States Constitution protects a fundamental, individual right to keep and bear arms. "A well regulated Militia, being necessary to the security of a free State, the right of the people to keep and bear Arms, shall not be infringed." U.S. CONST. amend. II. That right "extends, prima facie, to all instruments that constitute bearable arms, even those that were not in existence at the time of the founding," *Heller*, 554 U.S. at 582, and "is fully applicable to the States," *McDonald v. City of Chicago*, 561 U.S. 742, 750 (2010). The "central" holding in *Heller* was "that

the Second Amendment protects a personal right to keep and bear arms for lawful purposes, most notably for self-defense within the home." *McDonald*, 561 U.S. at 780.

While banning common semiautomatic firearms with common characteristics and magazines may be popular in California and a few other jurisdictions, such a prohibition "is no less unconstitutional by virtue of its popularity." *Silveira*, 312 at 1091. Whatever policy arguments the State may have for these kinds of proscriptive laws, "[t]he very enumeration of the right takes out of the hands of government—even the Third Branch of Government—the power to decide on a case-by-case basis whether the right is *really worth* insisting upon." *Heller*, 554 U.S. at 634.

1. The AWCA's Complete Ban on Commonly Owned Firearms Violates the Second Amendment Under *Heller*'s Categorical Analysis.

Precisely like the common magazines at issue in *Duncan*, Defendants can offer no historical support for their ban of the common firearms with common characteristics—including the same magazines—at issue here because such a ban *has* "no historical pedigree." *Duncan*, 366 F. Supp.3d at 1149. Such common firearms with common characteristics, like semiautomatic firearms in general, have been in existence for over a century and were unregulated in the State until 1989 or later—the polar opposite of a "longstanding" regulation. *See* Declaration of Ashley Hlebinsky (Hlebinsky Dec.), ¶¶ 10-28, **Exs. 5–35** filed herewith. Indeed, such common semiautomatic firearms with common characteristics were for decades "typically possessed by law-abiding citizens for lawful purposes." *Heller*, 554 U.S. at 624-25. They remain among the most popular firearms throughout most of the country to this day.

Given such straight-forward alignment with the baseline constitutional standard set forth in *Heller*, there is no need to analyze this case using varying "tiers of scrutiny."

Rather, a clear-cut categorical rejection of the challenged prohibitions is consistent with *Heller* itself and is a common approach in our nation's constitutional law. *See* David B. Kopel & Joseph G.S. Greenlee, *The Federal Circuits' Second Amendment Doctrines*, 61 ST. LOUIS U. L.J. 193, 303–04 (2017) (examples under the First, Fifth, Sixth, Eighth, Tenth, and Fourteenth Amendments); *cf. Wrenn v. D.C.*, 864 F.3d 650, 666 (D.C. Cir. 2017) ("*Heller I*'s categorical approach is appropriate here even though our previous cases have always applied tiers of scrutiny to gun laws."); *Heller v. District of Columbia*, 670 F.3d 1244, 1271 (2011) ("*Heller II*") (Kavanaugh, J., dissenting) (courts should "assess gun bans and regulations based on text, history, and tradition, not by a balancing test such as strict or intermediate scrutiny."

As this Court has recognized, *Heller*'s categorical analysis asks simply whether the arms being regulated or banned are in common use for lawful purposes. *Duncan v. Becerra*, 366 F. Supp.3d at 1142. The text, history, and tradition of the Second Amendment all point in the same direction: the firearms and conduct banned through operation and application of section 30515(a) have long been and continue to be commonly owned by law-abiding citizens for lawful purposes and are not uniquely dangerous *and* unusual in any manner that provides a historical basis for their prohibition. "A weapon may not be banned unless it is *both* dangerous *and* unusual,;" it "is a conjunctive test." *Caetano v. Massachusetts*, 136 S. Ct. 1027, 1031 (2016) (Alito, J., concurring). Because the arms California bans in the AWCA are not unusual, the Court need not consider if they are "dangerous" in a manner different from the inherent "danger" of firearms in general—such "danger" to those who pose a threat, of course, being the very reason arms are protected and useful in the first place. As Justice Alito explained, "the relative dangerousness of a weapon is irrelevant when the weapon belongs to a class of arms commonly used for lawful purposes." *Id.*

Accordingly, just like the District of Columbia's ban on handguns in *Heller*, the City of Chicago's ban on handguns in *McDonald*, and California's ban on "large-capacity" magazines in *Duncan*, California's sweeping ban on this ever-expanding category of firearms is categorically unconstitutional—full stop.

2. The AWCA Bans Arms in "Common Use."

As for the primary predicate of such categorical analysis, there is no genuine question that the semiautomatic firearms banned by California are common, not prohibited in the vast majority of States, and have been used for close to a century by millions of responsible, law-abiding people for various lawful purposes such as self-defense, hunting, recreation, competition, and collecting. Declaration of James Curcuruto (Curcuruto Dec.) ¶¶ 7-14, **Exs. 1-7**, filed herewith. The only rarity regarding such firearms is the very few States that seek to restrict them by recharacterizing them as "assault weapons." Kapelsohn Dec. ¶¶ 17-26, **Exs. 1-10**.

Firearms capable of holding and firing more than 10 rounds without reloading arrived well before 1900, and the first semiautomatic rifle was produced by Mannlicher in 1885. Hlebinsky Dec. ¶¶ 11-15, **Exs. 5-21**. Early semiautomatic pistols, rifles, and shotguns were developed in the first years of the 1900s and were configured with many of California's banned characteristics, such as detachable and large capacity magazines, pistol grips, and adjustable stocks. *Id.*

Today, semiautomatic firearms with such common characteristics are among the most popular firearms in the United States. Curcuruto Dec., ¶¶8-12 (discussing prevalence of relevant semiautomatic rifles and massive numbers of common semiautomatic shotguns and pistols with such characteristics). "We think it clear enough in the record that semi-automatic rifles and magazines holding more than ten rounds are indeed in 'common use,' as the plaintiffs contend." *Heller II*, 670 F.3d

at 1261.

Although categorical commonality is not exclusively based on number of any particular type of arm owned by individuals, the numbers are telling. Ownership of semiautomatic rifles configured in a manner banned by California has previously been conservatively estimated at least 5 million strong. Curcuruto Dec., ¶¶ 7-13, **Exs. 1-7**. Indeed, in 2016 alone, 2.2 million such rifles were either manufactured in or imported into the U.S. for sale. *Id*. at ¶ 8, **Ex. 3**. As of 2019, 96.5% of firearm retailers sell firearms that would be prohibited by the AWCA. *Id.* at ¶ 10, **Ex. 5.** However, most recently, reports show there are 17.7 million privately owned "modern sporting rifles" in the country alone—54% of the firearms manufactured and imported in the U.S. Curcuruto Dec. ¶15, **Ex. 8**.

One of the most common firearms meeting California's definition of "assault weapon" is the AR-15 platform firearm, which has been sold to the public since 1950 with standard characteristics like detachable magazines that hold more than 10 rounds, pistol grips, collapsible or otherwise adjustable stocks, flash suppressors, and/or forward vertical grips. Kapelsohn Dec. ¶ 18; Curcuruto Dec. ¶ 8. Such firearms are lawful under federal law and in most States. As this Court previously recognized:

> Over the last three decades, one of the most popular civilian rifles in America is the much maligned AR-15 style rifle. Manufactured with various characteristics by numerous companies, it is estimated that more than five million have been bought since the 1980s. These rifles are typically sold with 30-round magazines. *These commonly owned guns with commonly-sized magazines* are protected by the Second Amendment and *Heller's* simple test for responsible, law-abiding citizens to use for target practice, hunting, and defense.

Duncan, 366 F. Supp.3d at 1145 (emphasis added).

3. Firearms Covered by California's Ban Are Well-Suited for Self-Defense.

The common firearms banned under the AWCA are not only in common use, but ideal for self-defense. Kapelsohn Dec. at ¶¶18-26, **Exs. 1-10**. They are highly beneficial to lawful gun owners; especially, but not exclusively, in life or death self-defense situations. Hauffen Dec. ¶ 8, **Ex. 1**. As described previously, the regulated characteristics improve the control, accuracy, function, and safety of firearms. Kapelsohn Dec. ¶¶ 27-37. These characteristics also make them ideal for lawful purposes such as sport and hunting. Common sense dictates that standard characteristics that enhance accuracy, control, and safety should be encouraged, not banned. But rather than promoting safer firearm handling, the State's regulatory scheme actually prevents firearm users from maximizing their safe and controlled use of common semiautomatic firearms.

4. Firearms Covered by California's Ban Are Well-Suited for Militia Service.

In addition to meeting *Heller*'s common-use predicate, the firearms banned by the AWCA are especially fit for militia service should the need arise, as contemplated by the Second Amendment's prefatory clause and history. The common AR-15 platform firearm, for example, has standardized and interchangeable parts, magazines, and ammunition; is durable, reliable, relatively inexpensive, and lightweight; and readily fulfils the same purposes sought (and mandated) by the founding-era Militia Acts. *See* Declaration of Allen Youngman (Youngman Dec.), ¶ 14-19, filed herewith.

Such utility for militia service helps to understand the breadth of arms protected under the Second Amendment. In the pre-*Heller* decision in *United States v. Miller*, 307 U.S. 174, 178 (1939), the Supreme Court looked to "ordinary military equipment" that could "contribute to the common defense" in identifying weapons covered by the

Second Amendment. It further explained that the debates, history, legislation, and commentary preceding and surrounding the Bill of Rights "plainly" showed that:

> the Militia comprised all males physically capable of acting in concert for the common defense. "A body of citizens enrolled for military discipline." And further, that ordinarily when called for service these men were expected to appear bearing arms supplied by themselves and of the kind in common use at the time.

307 U.S. at 179.

When the Court in *Heller* later resolved the individual nature of Second Amendment rights, it clarified that *Miller* was establishing "that the sort of weapons protected were those 'in common use at the time.'" 554 U.S. at 627 (quoting *Miller*, 307 U.S. at 179). This Court itself has agreed that "*Miller* implie[d] that possession by a law-abiding citizen of a weapon that could be part of the ordinary military equipment for a militia member, or that would contribute to the common defense, is protected by the Second Amendment.". *Duncan*, 265 F. Supp.3d at 1116.

Firearms in common use and suitable for militia service were *expected*—indeed, often *required*—to be kept by ordinary citizens. [10] Today, such arms are semiautomatic firearms, such as the AR-15 rifle, with the common characteristics discussed above. Youngman Dec. ¶ 19.

5. California's "Assault Weapons" Ban Does Not Fall Within Any Historically Permissible Limit on the Right to Keep and Use Arms.

In contrast to the strong historical support for protecting the firearms at issue here, there is no historical support at all for prohibiting such firearms. As noted earlier, at page 13-14 semiautomatic rifles, pistols, and shotguns and detachable magazines have been in existence since the late 1800s and early 1900s. As early as 1779, firearms

[10] David B. Kopel and Joseph G. S. Greenlee, *The Second Amendment Rights of Young Adults*, 43 S. ILL. U. L.J. 495 (2019).

had capacities of up to 30 rounds. *Hlebinsky Dec.* ¶¶11-13, **Exs. 5-18**. During World War I, detachable magazines with capacities of 25-to-32 rounds were introduced and available in the commercial market. Kapelsohn Dec., ¶ 18. Other characteristics such as the ergonomic pistol-style grip and thumbhole stock, collapsible stock, flash suppressor, and forward vertical grips have been commercially available and offered on semiautomatic firearms for decades. Hlebinsky Dec. ¶¶ 10-28, **Exs. 5-35**.

Despite the long history of such firearms, and even longer prior history of militia-suitable firearms being available to the population in general, it was not until 1989 that California became the first State to implement any "assault weapon" ban with the first and narrower iteration of the AWCA based on specific makes and models. The only federal regulation on semiautomatic firearms having characteristics at issue here did not occur until 1994 in the Public Safety and Recreational Firearms Use Protection Act (the "Federal Assault Weapons Ban")(103rd Congress (1993-1994)), a subsection of the Violent Crime Control and Law Enforcement Act of 1994 (Pub. L. 103-322), which was allowed to sunset 10 years later due to its lack of effect on crime. *See* Declaration of John Lott (Lott Dec.) ¶¶ 8, filed herewith. The few subsequent state "assault weapon" bans have an even shorter "historical" pedigree. *See* Declaration of George A. Mocsary (Mocsary Dec.) ¶¶ 23-49, **Exs. 2-9**. Such late-adopted restrictions by a mere handful of jurisdictions do not remotely qualify as the historically permissible limits mentioned in *Heller*. *Cf. Heller II*, 670 F.3d at 1260 ("We are not aware of evidence that prohibitions on either semi-automatic rifles or large-capacity magazines are longstanding and thereby deserving of a presumption of validity"); *Staples v. United States*, 511 U.S. 600, 603 n.1, 612 (1994) (discussing the AR-15 and stating that weapons that fire "only one shot with each pull of the trigger" "traditionally have been widely accepted as lawful possessions"). Mocsary Dec. ¶¶10-22.

Under each aspect of *Heller*'s straightforward analysis, California's AWCA violates the Second Amendment. It criminalizes the lawful use and possession of common firearms with common characteristics, suitable for militia service and used for lawful purposes such as self-defense, proficiency training, hunting, recreation, and competition. Such prohibition has no longstanding historical predicate and broadly restricts the protected activities of virtually all law-abiding adults in California for effectively all purposes. And like the ban struck down in *Heller*, it threatens citizens with substantial criminal penalties. *Heller*, 554 U.S. at 634. Because the challenged law fails *Heller*'s categorical analysis, this Court need go no further to find that Plaintiffs have a high likelihood of success on the merits.

6. The AWCA Fails the Ninth Circuit's Two-Part Test.

The State's AWCA scheme also fails the Ninth Circuit's two-part test applying tiered scrutiny.[11] Assuming *arguendo* that an interest-balancing test is required, the challenged provisions still fail any level of "heightened scrutiny."

The Ninth Circuit applies a two-part test to some Second Amendment challenges. *United States v. Chovan*, 735 F.3d 1127 (9th Cir. 2013). This "inquiry '(1) asks whether the challenged law burdens conduct protected by the Second Amendment and (2) if so, directs courts to apply an appropriate level of scrutiny.'"

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[11] Plaintiffs preserve and maintain their position that such a test, and tiered scrutiny, are inappropriate for categorical bans, including the AWCA's at issue here. *Heller*, 554 U.S. at 634, 635 ("We know of no other enumerated constitutional right whose core protection has been subjected to a freestanding 'interest-balancing' approach"; "[t]he Second Amendment . . . is the very *product* of an interest balancing by the people"); *Ezell v. City of Chicago*, 651 F.3d 684, 703 (7th Cir. 2011) ("Both *Heller* and *McDonald* suggest that broadly prohibitory laws restricting the core Second Amendment right—like the handgun bans at issue in those cases, which prohibited handgun possession even in the home—are categorically unconstitutional.").

Bauer v. Becerra, 858 F.3d 1216, 1221 (9th Cir. 2017) (quoting *Jackson v. City of San Francisco*, 746 F.3d 953, 960 (9th Cir. 2014)). The level of scrutiny to be applied depends on the closeness to the core and "the severity of the law's burden," on the Second Amendment. *Chovan*, 735 F.3d at 1138.

a. Burden on the Second Amendment

As shown above, at page 12-14, semiautomatic firearms with common characteristics proscribed by the AWCA are in common use for lawful purposes and thus protected arms under the Second Amendment. The State's ban thus "amounts to a prohibition of an entire class of 'arms' that is overwhelmingly chosen by American society for lawful purposes, including for possession in the home, where the need for defense of self, family, and property is most acute." *Heller*, 554 U.S. at 628.[12] The AWCA and Defendants' policies, practices, and customs impose a *substantial* burden on Second Amendment rights, and thus deserves strict scrutiny "to afford the Second Amendment the respect due an enumerated constitutional right." *Silvester v. Becerra*, 138 S. Ct. 945, 945 (2018) (Thomas, J., dissenting from denial of certiorari); *Pena v. Lindley*, 898 F.3d 969, 977 (9th Cir. 2018) ("We strictly scrutinize a 'law that implicates the core of the Second Amendment right and severely burdens that right'") (citation omitted); *Mance v. Sessions*, 896 F.3d 699, 705-06 (5th Cir. 2018), *pet'n for*

[12] Any suggestion by Defendants that the AWCA bans only a small subset of firearms or that there are other classes of firearms available and thus the AWCA is not a categorical ban is foreclosed by *Heller*. "It is no answer to say […] that it is permissible to ban the possession of handguns so long as the possession of other firearms (i.e., long guns) is allowed." *Heller* 554 U.S. at 629; *see also Parker v. District of Columbia*, 478 F.3d 370, 400 (D.C. Cir. 2007) ("The District contends that since it only bans one type of firearm, 'residents still have access to hundreds more,' and thus its prohibition does not implicate the Second Amendment because it does not threaten total disarmament.

cert. filed (Nov. 19, 2018) (applying strict scrutiny in Second Amendment cases).

Even if the AWCA's broad ban somehow were deemed less severe, and only intermediate scrutiny applied, the prohibitions challenged here would still fail "constitutional muster." *Heller*, 554 U.S. at 628–29. Plaintiffs thus will discuss only intermediate scrutiny on the understanding that such discussion applies all the more acutely and fatally under strict scrutiny. They preserve their claims to *Heller*'s categorical analysis and, alternatively, for strict scrutiny should the need arise here or on appeal.

b. Heightened Scrutiny Imposes a High Bar for the State when Defending Infringements of Second Amendment Rights

Under any form of heightened scrutiny, the government bears the burden of justifying its restrictions. *See, e.g.*, *R.A.V. v. City of St. Paul*, 505 U.S. 377, 382 (1992) (content-based speech regulations are presumptively invalid); *United States v. Chester*, 628 F.3d 673, 680 (4th Cir. 2010) (unless conduct "not protected by the Second Amendment at all, the [g]overnment bears the burden of justifying the constitutional validity of the law."); *Tyler v. Hillsdale County Sheriff's Dept.*, 837 F. 3d 678, 694 (6th Cir. 2016) ("the burden of justification is demanding and it rests entirely on the State.") (citation omitted).

The intermediate scrutiny to be applied is the same as, and is drawn from, such scrutiny in the First Amendment context. *Jackson*, 746 F.3d at 961 (heightened scrutiny in Second Amendment cases is "guided by First Amendment principles"); *Silvester v. Harris*, 834 F.3d 816, 821 (9th Cir. 2016) (applying in Second Amendment case "the test for intermediate scrutiny from First Amendment cases"), *cert. denied*, *Silvester v.*

We think that argument frivolous. It could be similarly contended that all firearms may

Becerra, 138 S. Ct. 945 (2018). Various cases in the Ninth Circuit have described that test as whether "(1) the government's stated objective [is] significant, substantial, or important; and (2) there [is] a 'reasonable fit' between the challenged regulation and the asserted objective." *Silvester*, 843 F.3d at 821–22 (quoting *Chovan*, 735 F.3d at 1139). The Supreme Court has emphasized that "reasonable" tailoring demands a considerably closer fit than mere rational basis scrutiny, and requires evidence that the restriction directly and materially advances a *bona fide* state interest. The test under intermediate scrutiny is "whether the challenged regulation advances these interests in a direct and material way, and whether the extent of the restriction on protected speech is in reasonable proportion to the interests served." *Edenfield v. Fane*, 507 U.S. 761, 767 (1993). "'[T]he regulation may not be sustained if it provides only ineffective or remote support for the government's purpose.'" *Id*. at 770 (quoting *Central Hudson Gas & Electric Corp. v. Public Serv. Comm'n*, 447 U.S. 557, 564 (1980)).

Further, the government's burden of justifying its restriction on constitutional rights "is not satisfied by mere speculation or conjecture; rather, a governmental body seeking to sustain a restriction on commercial speech must demonstrate that the harms it recites are real and that its restriction will in fact alleviate them to a *material degree*." *Edenfield*, 507 U.S. at 770-71 (emphasis added). Restrictions on constitutional rights must be analyzed in their specific context, and "will depend upon the identity of the parties and the precise circumstances of the" protected activity. *Edenfield*, 507 U.S. at 774. Generalized risk does not warrant restrictions as to all persons, and "a preventative rule" aimed at such generic hazards is "justified only in situations 'inherently conducive to'" the specific dangers identified. *Id*. (quoting *Ohralik v. Ohio State Bar Ass'n*, 436

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be banned so long as sabers were permitted."), *aff'd sub nom. Heller*, 554 U.S. 570.

U.S. 447, 449 (1978)). Finally, even where the challenged restrictions materially address a genuine harm, the State must prove that its chosen means are "'closely drawn'" to achieve that end without "'unnecessary abridgment'" of constitutionally protected conduct. *McCutcheon v. FEC*, 134 S. Ct. 1434, 1456-57 (2014) (quoting *Buckley v. Valeo*, 424 U.S. 1, 25 (1976)).

Assuming *arguendo* the importance of the State's highly generalized claimed interests in public safety and reducing "gun violence," those interests must "rely on . . . hard facts and reasonable inferences drawn from convincing analysis amounting to substantial evidence based on relevant and accurate data sets." *Duncan*, 366 F. Supp.3d at 1161. Further, if the State's claimed interest is instead a more specific desire to prevent or mitigate so-called "mass shootings," *Rupp v. Becerra*, 401 F. Supp. 3d 978, 991 (C.D. Cal. 2019), then it is far from clear that an interest directed at such rare events is significant and/or important. Nevertheless, even assuming the importance of the interest at either level of generality, the AWCA's sweeping ban on common firearms with common characteristics is not a reasonable fit for achieving these interests.

c. There is No Reasonable Fit Between the Government's Interests and the AWCA

The AWCA's broad ban on common semiautomatic firearms is not a reasonable fit for a plethora of reasons. First, the ban does not "alleviate [the claimed harms] to a material degree." *Edenfield*, 507 U.S. at 770-71. All credible research on the effectiveness of "assault weapon" bans in reducing gun violence and/or mass shootings shows no demonstrable correlation between the two. Lott Dec. ¶¶ 6-65, **Exs. 2-19**. The experiment of these bans has been tried, and they have failed to demonstrate that they directly or materially advance any government interest relating to gun violence. At the

federal level, the 1994 Federal Assault Weapons Ban was allowed to expire due to its lack of effect. *Id*. California has had an ever-expanding ban on "assault weapons" since 1989, with no indication it has done anything to alleviate the problems cited by the State. Lott Dec. ¶ 6-17, **Exs. 2-5**. As this Court previously observed:

> No case has held that intermediate scrutiny would permit a state to impinge even slightly on the Second Amendment right by employing a known failed experiment. Congress tried for a decade the nationwide experiment of prohibiting large capacity magazines. It failed. California has continued the failed experiment for another decade and now suggests that it may continue to do so ad infinitum without demonstrating success. That makes no sense.

Duncan, 366 F. Supp.3d at 1169.

Second, any correlation between different crimes and the weapons used therein does not establish a reasonable fit for a ban on all such weapons. Thus, notwithstanding the District Court's findings in *Rupp*, that "such weapons are disproportionately used in mass shootings," *Rupp v. Becerra*, 401 F. Supp. 3d 978, 993 (C.D. Cal. 2019), such findings do not even suggest that a ban would do anything other than divert such criminals to alternative legal or illegal weapons, or would in any way mitigate the problems. Further, any alleged higher incidence of "assault weapons" being used in crimes, mass shootings, and/or police shootings cannot justify a sweeping ban on lawful ownership of protected arms. Lawful use of such arms overwhelmingly outweighs any criminal use. Government "may not regulate the secondary effects of speech by suppressing the speech itself." *City of Los Angeles v. Alameda Books, Inc.*, 535 U.S. 425, 445 (2002) (opinion of Kennedy, J.). Indeed, in *Heller* itself, it was accurately observed that handguns are involved in the majority of all firearm-related deaths and the government argued that such fact established the government's interest in banning handguns to prevent or mitigate firearm-related homicides. *Heller,* 554 U.S. at 695-696

(Breyer, J., dissenting). The Court rejected that argument, finding that a ban on possessing commonly owned firearms lacked any fit to further the government's interest under any level of scrutiny. *Heller*, 554 U.S. at 628-29.

Constitutionally protected activities cannot be banned because the activity could lead to criminal abuses. *See Se. Promotions Ltd. v. Conrad*, 420 U.S. 546, 559 (1975); *accord Vincenty v. Bloomberg*, 476 F.3d 74, 84-85 (2d Cir. 2007); *Robb v. Hungerbeeler*, 370 735, 743 (8th Cir. 2004); *Ashcroft v. Free Speech Coal.*, 535 U.S. 234, 245 (2002); *Stanley v. Georgia*, 394 U.S. 557, 567 (1969). Indeed, computing devices connected to the Internet are now the most common tool for engaging in lawful, protected First Amendment activities, but undoubtedly also the most common tool for engaging in many unprotected and sometimes illegal forms of speech (*e.g.*, defamation, true threats) and other illegal conduct (*e.g.*, child pornography, hacking, and identity theft) as well. The latter hardly can justify restricting *lawful* use of computers connected to the Internet by law-abiding people who wish to publish their protected content and viewpoints.

Third, the line drawn by California between permitted and proscribed weapons is arbitrary and based on speculation and conjecture. The characteristics that trigger prohibition in fact improve the safe and controlled use of firearms so equipped. Kapelsohn Dec. ¶ 27-37, **Exs. 11-18**. Thus, they improve public safety relating to the lawful use of such firearms. As for unlawful use, there is no indication that criminals are particularly concerned about avoiding collateral or unintended damage through greater accuracy or control and, in any event, there is no evidence criminals would be

any less destructive using California-compliant "featureless" firearms. *Id.*[13] The prohibited characteristics in the AWCA do not change the fundamental semiautomatic function of the firearms, nor do they affect the ballistics of their projectiles. The District court in *Rupp* accepted the State's claims that the various targeted characteristics enhance the accuracy, capacity, and hence danger of the prohibited firearms—"[a]s discussed throughout, that the rifles are more accurate and easier to control is precisely why California has chosen to ban them"—and thus, upheld the AWCA. *Rupp* 401 F. Supp.3d at 993 (C.D. Cal. 2019). The District Court's analysis was deeply flawed. Such a standard would justify a ban on nearly all modern firearms, as they are all more accurate and controllable than early firearms (*e.g.*, muskets). It would also curb most future innovation in firearms, as any improvements would justify a ban.

While the pistol grip assists in the safe control of a firearm, it *does not* significantly increase the speed or ability of reloading compared to "featureless" non-"assault weapons." *See* Video (comparing a common AR-15 platform semiautomatic firearm in a California-compliant "featureless" configuration with a standard configuration commonly available in the majority of other states), online at http://bit.ly/miller-kraut-video; *see also* Declaration of Adam Kraut (Kraut Dec.) ¶¶ 4-14; Kapelsohn Dec. ¶ 28, **Ex. 12.** Further, semiautomatic firearms with the regulated characteristics are not more deadly in the hands of a criminal than a firearm without those characteristics. *Id.*, **Exs**. **17, 18**. Indeed, many notable crimes have been committed by criminals with semiautomatic firearms that did not have the regulated

[13] Or that criminals would be deterred from illegally obtaining or creating prohibited firearms. It is absurd to suggest that a person intent on the grotesque crime of mass murder would pause for a second at the prospect of also violating the AWCA while he was at it. In for a pound, in for a penny. Lott Dec. ¶¶ 6-27, **Exs. 2-5**.

characteristics. Kapelsohn Dec., ¶ 34. In fact, some of the worst mass shootings used only handguns or bolt action rifles.

Fourth, the AWCA burdens far more protected activity than necessary by imposing a *complete ban* on an ordinary, law-abiding individual's acquisition, purchase, transfer, and use of a common class of arms. Even under intermediate scrutiny, "a reasonable fit requires tailoring, and a broad prophylactic ban on acquisition or possession of all" common semiautomatics with common characteristics "for all ordinary, law-biding, responsible citizens is not tailored at all." *Duncan*, 366 F. Supp.3d at 1180; *see also Turner Broadcasting System, Inc. v. FCC*, 512 U.S. 622, 682-83 (1994) (O'Connor, J., concurring in part and dissenting in part) ("A regulation is not 'narrowly tailored'—even under the more lenient [standard applicable to content-neutral restrictions]—where ... a substantial portion of the burden on speech does not serve to advance [the State's content-neutral] goals.... Broad prophylactic rules in the area of free expression are suspect. Precision of regulation must be the touchstone....") (brackets in original) (citations and quotations omitted). By prohibiting even fully background-checked and law-abiding citizens from possessing a common and effective class of firearms, the law imposes considerably more burden than is warranted by the rare instances of criminal violence using such firearms. "The right to self-defense is largely meaningless if it does not include the right to choose the most effective means of defending oneself." *Friedman v. City of Highland Park*, 784 F.3d 406, 418 (7th Cir. 2015) (Manion, J., dissenting).

California's regulatory scheme for common semiautomatic firearms and common characteristics undermines public safety and does not materially advance any legitimate public interest. The State's justification that the self-same characteristics that make the firearms here suitable for lawful self-defense may also make them effective

tools for crime if misused, thus necessitating a ban, misses the point and would gut the Second Amendment. After all, the very point of protecting arms, and firearms in particular, is that they allow law-abiding people to project force against unjust force at a distance, and thereby defend themselves and others against a violent threat as soon as possible with the least amount of damage to those being protected. Inevitably, all firearms that are at all suitable for self-defense or militia service are comparably dangerous in the hands of a violent criminal. The notion that improvements that make firearms better and safer for lawful use likewise make them comparably better for unlawful use simply leads to the absurdity that firearms may never be improved because the harms *ipso facto* outweigh the benefits and justify a ban. However, the Second Amendment itself has already balanced the need for and dangers from arms that can effectively project force against another. As "the very *product* of an interest balancing by the people," the Second Amendment "elevates above all other interests the right of law-abiding, responsible citizens to use arms in defense of hearth and home." *Heller*, 554 U.S. at 634-35. "Constitutional rights are enshrined with the scope they were understood to have when the people adopted them, whether or not future legislatures . . . think that scope too broad." *Id.*

The AWCA's ban on an entire class of common semiautomatic firearms having common characteristics imposed against law-abiding individuals has no constitutional fit, let alone a reasonable one. The challenged law fails the categorical analysis; it fails even intermediate scrutiny.

B. All Other Preliminary Injunction Factors Favor Enjoining The AWCA

For the reasons above, Plaintiffs are likely to succeed on the merits. And Plaintiffs also satisfy the other preliminary injunction factors.

1. Likelihood of irreparable harm absent preliminary relief

"It is well established that the deprivation of constitutional rights 'unquestionably constitutes irreparable injury." *Melendres v. Arpaio*, 695 F.3d 990, 1002 (9th Cir. 2012) (quoting *Elrod v. Burns*, 427 U.S. 347, 373 (1976)); 11A Charles Alan Wright et al., *Federal Practice and Procedure* § 2948.1 (2d ed. 1995) ("When an alleged deprivation of a constitutional right is involved, most courts hold that no further showing of irreparable injury is necessary."). Plaintiffs have been and continue to be deprived of their fundamental Second Amendment rights. See Miller Dec., Hauffen Dec., Rutherford Dec., Sevilla Dec., Phillips Dec., Peterson Dec., Gottlieb Dec., Hoffman Dec., and Combs Dec.

Further, the Ninth Circuit has applied the First Amendment's "irreparable-if-only-for-a-minute" rule to cases involving other rights and, in doing so, has held a deprivation of these rights represents irreparable harm per se. *Monterey Mech. Co. v. Wilson*, 125 F.3d 702, 715 (9th Cir. 1997). S*ee also Ezell v. Chicago*, 651 F.3d 684, 700 (7th Cir. 2011) (a deprivation of the right to arms is "irreparable," with "no adequate remedy at law"). Moreover, the AWCA's restrictions on otherwise lawful and innocuous conduct are enforced by severe criminal and "civil" penalties, which can result in incarceration and a lifetime prohibition on an individual's Second Amendment rights. Thus, the AWCA and Defendants' policies, practices, and customs have and will continue to cause irreparable harm absent injunctive relief.

2. The Balance of Equities Tips in Plaintiffs' Favor

The next factor considers the balance of equities, or "the balance of hardships between the parties." *Alliance for the Wild Rockies v. Cottrell*, 632 F.3d 1127, 1137 (9th Cir. 2011). The state "cannot suffer harm from an injunction that merely ends an unlawful practice or reads a statute as required to avoid constitutional concerns."

Rodriguez v. Robbins, 715 F.3d 1127, 1145 (9th Cir. 2013); *see also Valle del Sollnc. v. Whiting,* 732 F.3d 1006, 1029 (9th Cir. 2013) ("[I]t is clear that it would not be equitable ... to allow the state ... to violate the requirements of federal law.") (citations omitted). The likelihood of success on the merits thus largely drives the equitable balance as well. Additionally, because no credible evidence supports the effectiveness of California's "assault weapon" ban, *supra* at p. 21-22, there is no genuine harm from enjoining such a scheme. Conversely, Plaintiffs and other similarly situated law-abiding individuals would indeed benefit from the safety and control characteristics of the otherwise restricted firearms and thus are injured by the restrictions beyond the direct injury to their Second Amendment rights. The balance of equities tips sharply in Plaintiffs' favor.

3. An Injunction Is in The Public Interest

When challenging government action that affects the exercise of constitutional rights, "[t]he public interest … tip[s] *sharply* in favor of enjoining the" law. *Klein v. City of San Clemente*, 584 F.3d 1196, 1208 (9th Cir. 2009) (emphasis added). Again, the likelihood of success on the merits and the lack of effectiveness in advancing the State's claimed interests largely drive a comparable conclusion regarding the public interest. Furthermore, mass shootings are extremely rare. Since 1982, there were still 19 public mass shootings in California. These incidents involved: multiple firearms, banned firearms, illegally modified firearms, firearms with and without common characteristics, and firearms with and without "large-capacity" magazines—all while the AWCA was in effect. Any suggestion that the AWCA has any salutary effect on the harms of mass shootings thus is speculative at best, and demonstrably false at worst. Additionally, it is not only *Plaintiffs'* rights at stake, but the rights of all law-abiding

adults in California—and future adults—as well. Thus, the public interest tips even more sharply in Plaintiffs' favor. *Id.* at 1208.

Further, even with the AWCA and Defendants' policies, practices, customs, and regulations properly enjoined, all firearm purchases still must go through federal and state background checks. Purchasers and transferees also must still: (a) take and pass a firearms safety test; (b) present a valid firearm safety certificate for any transfer; (c) provide proofs of identity and residency; (d) complete a ten-day waiting period; (e) complete a safe handling demonstration of the firearm being purchased; (f) sign a gun safe affidavit, or purchase a firearm cable lock; and (g) complete a background check for ammunition purchases. This list is not exhaustive, but provides a summary of the vast array of firearms regulations already in place that, according to the State, ensure public safety.

V. CONCLUSION

For the foregoing reasons, Plaintiffs request that the Court grant their motion and preliminarily enjoin the AWCA and Defendants' policies, practices, customs, and regulations that enforce it.

Respectfully submitted,

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UNITED STATES DISTRICT COURT

FOR THE SOUTHERN DISTRICT OF CALIFORNIA

JAMES MILLER, et al.,

Plaintiffs,

vs.

XAVIER BECERRA, in his official capacity as Attorney General of California, et al.,

Defendants.

Case No. 3:19-cv-01537-BEN-JLB

Hon. Roger T. Benitez
Magistrate Hon. Jill L. Burkhardt

DECLARATION OF ASHLEY HLEBINSKY IN SUPPORT OF PLAINTIFFS' MOTION FOR PRELIMINARY INJUNCTION

Complaint filed: August 15, 2019
Amended Complaint filed: September 27, 2019

Date: Thursday, January 16, 2020
Time: 10:00 a.m.
Department: Courtroom 5A (5th floor)

DECLARATION OF ASHLEY HLEBINSKY

I, Ashley Hlebinsky, declare as follows:

1. I am the Robert W. Woodruff Curator of the Cody Firearms Museum as well as a firearms and ammunition related museum consultant, expert witness, freelance writer, guest lecturer, and founder of the newly formed Association of Firearms History and Museums. I have been retained by the plaintiffs in this matter to provide historical testimony regarding the lineages of several key technologies listed in the California Penal Code 30515 to highlight that many of these features were developed over a century ago and have seen "common use" and are "not dangerous or unusual." I have personal knowledge of the facts stated herein, and if called as a witness, I could competently testify to these facts.

2. This declaration is executed in support of plaintiffs' motion for the issuance of a preliminary injunction, made pursuant to FRCP 65.

QUALIFICATIONS AND EXPERIENCE

3. I am the Robert W. Woodruff Curator of the Cody Firearms Museum at the Buffalo Bill Center of the West. At the museum, I manage an encyclopedic collection of around 7,000 firearms. Prior to my work at the Buffalo Bill Center of the West, I researched in the Smithsonian Institution's National Firearms Collection for about three years. During this time, I studied firearms from the 1200s through modern day. I not only studied the evolution of firearms technology but completed work on the United States Patent Office Collection. I also worked as a liaison between the Smithsonian Institution and the Buffalo Bill Center of the West, helping to facilitate the loan of 64 firearms from the Smithsonian collection to the Center. A large portion of that loan and subsequent loans thereafter centered around the Patent Collection and early evolution of firearms technologies. In addition to my work with the National Firearms

Collection, I earned Bachelor's and Master's degrees in American History, with a certification in Museum Studies, focusing my research towards the latter half of my degree on a macro historical approach to studying how advancement of firearms technology affected industry, society, and culture as well as the perception of those firearms within a given culture. During my time in graduate school, I was awarded the Edward Ezell Firearms Fellowship from the University of Delaware, which allowed me to complete my research on the Smithsonian collection. Additionally, I was a teaching assistant in a military history survey course. During this survey, I taught the firearms portion of the class. I am an NRA Certified Firearms Instructor, in Basic Pistol and Personal Protection Inside the Home. I simultaneously earned my Well Armed Woman Instructor Certification. At the museum, I have been responsible for the education of hundreds of students from elementary through college levels, where we teach not only firearms safety and basics, but the historical and technical evolution of the firearm. Additionally, I served as the Project Director on a $12.9 million full scale renovation and reimagining of the Cody Firearms Museum, which reopened July 6, 2019. I was responsible for all aspects of the renovation from fundraising to content. As a museum consultant, under a single member LLC (The Gun Code), I conduct workshops on firearms collections, survey collections and curate exhibitions at institutions such as the Houston Museum of Natural Science, the Winchester Mystery House, CM Russell Museum & Complex, the Mob Museum, and the Adirondack Experience (November 2019.) I am also a freelance firearms writer, guest lecturer, on-camera firearms historian, and firearms related television producer.

4. I have also made contributions to the academic study of firearms. In 2017, I developed the first full scale symposium in the United States dedicated to the study of firearms as material culture. That symposium has grown and is carried out annually. In

October 2018, I also founded an academic association in the US for the study of firearms (Association of Firearms History and Museums) which is still in its early stages of development. A current copy of my Curriculum Vitae summarizing my education and experience is attached as **Exhibit 1**.

PRIOR EXPERT WITNESS TESTIMONY

5. Because my research covers centuries of firearms and ammunition development, I have a large breadth of topics related to the subject matter on which I can testify. I have served as an expert witness in the following matters:

Shannon Wayne Garrison, et al v Sturm, Ruger & Company, Inc.
Report written November 2017
Deposition Testimony, Chicago, IL November 27, 2017

Regina v Carvel Clayton
Halifax, Nova Scotia
Report written December 2017

SCOPE OF WORK

6. This declaration will provide some historical background on many of the firearms and firearms related technologies outlined in California Penal Code 30515 as attributed to the term "assault weapon." It should be noted that the term "assault weapon" in and of itself is a legislative term in which the definition changes depending on state and federal legislation and bills proposed. The Cody Firearms Museum typically defines assault weapon as, "a legislative catch-all term used in the 1994 assault weapons ban and since has had differing definitions in proposed legislation typically centered around largely cosmetic features of semi-automatic firearms." This declaration will look briefly at the origins or early appearances of these technologies throughout history not only for battlefield use but in the civilian sphere. The opinions expressed in this declaration are mine, and are not reflective of any position of the Cody Firearms Museum.

7. It is important to note from an overall historical perspective, early firearms technology was often driven by war. Once that technology was developed, inventors and designers pushed the boundaries of firearms technology. For example, the first handheld portable gun, or firearm, was known as a handcannon or handgonne, which appeared on the battlefield in the 1200s. The ignition system was basic, utilizing a touchhole and external fire source to ignite powder and fire the gun. While many examples were single shot, some handcannons were developed with multiple barrels to have a repeating function. An example of a handcannon, and a multiple barrel version, are attached hereto as **Exhibit 2**.[1]

8. Often the technology advanced too quickly and would go beyond common battlefield use, finding popularity in the civilian population. Military firearms in a general sense were limited by tactics and government bureaucracy while civilian arms until recently were predominantly limited by individual budget. Additionally, civilian arms could be applied in a far greater variety of uses (e.g., hunting, self-defense, sport). The first true ignition system, the matchlock, was developed around 1400. This firearm, which utilized a burning match cord, was a popular military arm for centuries around the world. By the turn of the 16th century, however, matchlocks and subsequent ignition systems began appearing in early target shooting competitions. (**Exhibit 3)**.

9. By circa 1509, a highly advanced handgun was developed – the wheel-lock. (**Exhibit 4**). This gun, developed for horseback use, operated by the turning of a spring loaded wheel. While it saw battlefield use, it was expensive and difficult to repair. As a result, it was used for specialized purpose on the battlefield and for civilian use, especially as a sporting arm. The matchlock continued to be used on the battlefield

[1]All further exhibits attached to this declaration are true and correct examples of the

despite this availability of superior technology. While it may seem trivial to discuss the earliest firearms history within the context of a case on "assault weapons," it is important 1. to identify a precedent set for why, how, and whom firearms technology has evolved over 800 years. Since the beginning of firearm invention, while firearms have been applied for use in war, the civilian market bore the fruits of innovation. As an additional layer of the common interplay between military and civilian firearms, weapons used *in* war were often sold on the civilian market both during and after wars' end. For example, after the American Civil War, post war weapons surplus firearms became available on the civilian market. Soldiers could buy their firearms for as inexpensive as six dollars and many dealers and distributors sold them in their catalogs. This continued in the 20th century, with firearms such as the Springfield Model 1903 bolt action rifle and even with semi-automatics such as the M1 Garand rifle. There has always been an eb and flow of civilian and military firearms for centuries. And 2. several features listed in Penal Code 30515 date back just about as long as some of these early firearms and firearms technology in some form or another, predating even semi-automatic technology.

HISTORICAL REVIEW OF FIREARM FEATURES:
CAL. PENAL CODE § 30515(a)

10. There are many terms used to qualify rifles, pistols, and shotguns regulated in California under this code. A few overarching categorical terms that appear across the type of firearm are the terms: repeater, magazine (fixed or detachable), centerfire, and semi-automatic. Please note the following history is not comprehensive, rather serves to provide a sampling of the early appearances of each individual technology to illustrate

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firearm/feature being referenced.

their long history of both military *and civilian* use and their commonality.

11. To reiterate, the concept of a repeating firearm dates to the earliest technology of firearms. The idea of repeating firearms was not initially popular on the battlefield due to cost and convenience, however, repeating firearms in the civilian market were popular for those who could afford them. Many double barrel firearms were developed to provide hunters with a second shot, but that repeating concept quickly moved far beyond the California penal code's definition of "high capacity" at ten rounds. In the mid-1600s in Italy, the Lorenzoni system of firearm was developed and then imitated by many designers in long gun and pistol form. (**Exhibit 5**). This gun was a flintlock, magazine-fed repeater that fired around seven shots before having to reload. A century later the, Girardoni/Girandoni (1779) air rifle (**Exhibit 6**) could fire about 20 rounds from a tubular magazine. By the mid-1800s, many firearms both obscure and common had magazine capacities at ten or greater rounds including the 1854 patented Volcanic repeating pistols (**Exhibit 7**) (.31 caliber 6in barrel: 10 rounds, .41 caliber 8in barrel: 10 rounds) and carbines (16in barrel: 20 rounds, 20in barrel: 25 rounds, 24in barrel: 30 rounds), the 1860 Henry rifle (**Exhibit 8**) (15+1 rounds), and the1853 (Belgium) and 1857 (US) patented Genhart Rifles (**Exhibit 9**) (10 rounds), as well as multiple models of Winchester starting in 1866. By the end of the 19th century, the earliest versions of semi-automatic pistols such as the Borchardt C-93 contained eight rounds from a detachable magazine (1893) and the Mauser C-96 had a 10-round magazine (1895). Even certain Luger semi-automatic pistols in the early 1900s had the option of 32-round snail drum magazines. (**Exhibit 10**).

12. The next major concept is the presence of a magazine, fixed or detachable. Magazine fed firearms dates to at least the 1600s with the Lorenzoni system. (**Exhibit 11**). The Girardoni air rifle as previously stated used a tubular magazine in the

late 1700s. The tubular magazine was first patented in the US in the 1840s, notably with the Hunt Volitional Rifle (**Exhibit 12**), the oldest direct ancestor to the Winchester rifle. Magazines came in many shapes and sizes and became prevalent in the mid-1800s. For example, the Spencer repeating rifle (**Exhibit 13**) utilized a detachable tubular magazine from the buttstock. In the 1850s, the Genhart turret rifle (**Exhibit 14)** had a detachable circular magazine with an externally visible shot/round counter. Between 1859 and 1862, the Jarre Harmonica Pistol and Rifle received several patents. This gun has a horizontally seated magazine that slides after each round is fired like a typewriter. It is also detachable. (**Exhibit 15**).

13. In terms of box magazines, early ones were patented by designers including Rollin White in 1855. A detachable version was patented in 1864 by Robert Wilson. (**Exhibit 16**). A vertically stacked box magazine was patented by James Paris Lee in 1879 which was applied to several rifles including the Mannlicher semi-automatic Model 1886. (**Exhibit 17**). In terms of other semi-automatics, the Mauser C-96 pistol had a fixed magazine and the Borchardt C-93 had a detachable one. Several semi-automatic models of Winchester utilized magazines, including the Winchester Model 1907, a centerfire rifle with various sizes of box magazine (5 and 10) and some Winchester Model 1903s had a lesser known Sabo 96-round detachable tubular magazine. (**Exhibit 18**).

14. The next major feature of this penal code is the term, centerfire. This term refers specifically to the type of ammunition the gun fires. Centerfire refers to the location of the priming compound. Self-contained cartridges typically consist of a case, primer, powder, and projectile. Centerfire has a separate primer in the center of the head of the cartridge case. This is to distinguish it from rimfire, which has an integral primer in the rim of the cartridge case. (**Exhibit 19**). Traditionally, people are most aware of

.22 caliber rimfires but there have been many larger calibers including the .44 Flat Henry Rimfire cartridge. Centerfire cartridges started in the early 1800s. In 1808, Jean Samuel Pauly invented an early form of centerfire cartridge and the true centerfire was developed in 1829 by French inventor Clement Pottet and perfected by the 1850s.

15. Finally, the term that this Penal Code addresses most of all is semi-automatic. Semi-automatic operation involves pressing a trigger to fire one round, eject a spent case, and load another to be fired on the next trigger pull. (**Exhibit 20**). Today, a majority of firearms are semi-automatic rifles, pistols, or shotguns. Semi-automatic technology was developed in the 1880s around the same time as automatic technology. The Mannlicher rifle is generally attributed to be the first semi-automatic rifle (**Exhibit 21**); handguns followed shortly after. The first mass produced semi-automatic pistol was the Hugo Borchardt designed C-93 with detachable 8-round magazine. The Mauser C-96 followed, as did the John Moses Browning's Model 1899/1900 pistol. Often in the marketing of these pistols in the late 19th and 20th centuries, the companies would refer to them as "Automatic" pistols. However, please note they are still semi-automatic in function. According to the definitions of the Gun Control Act of 1968, such firearms made before 1898 are not federally regulated firearms, they are antiques. By that definition and regulation, some semi-automatic pistols and rifles are so old, they are not legally firearms according to the federal government. In the 20th century, semi-automatic firearms used in conjunction with a variety of the features listed above have been and continue to be made into thousands of models by countless companies. They are commonly used in the civilian market as well as the military, incorporating many other features addressed in the Penal Code.

16. The following is a list of additional features addressed in Penal Code § 30515:

17. **Pistol Grip:** Pistol grips appear on long arms dating to at least the 1700s.

(**Exhibit 22**). Single shot flintlock and later percussion pistols sometimes would have the feature of a detachable stock. When assembled these long guns would use the grip from the pistol as a maneuverable device. This trend continued with repeating arms, including several models of Colt revolvers, in the civilian and military market. The Borchardt semi-automatic pistol of 1893 and the Mauser C96 also had a detachable stock option. If a user didn't have one of these models, universal holsters to convert a pistol to a rifle with a detachable stock existed. (**Exhibit 23**). On firearms without detachable stocks, pistol grips appear on all variances of firearms actions. Machine guns, including the Colt Model 1895, French Chauchat (1907) and several Maxim models had pistol grips. Submachine guns like the Thompson (1918) had them as well. Pistol Grips not only appear in machine guns but also other guns, such as shotguns –the Ithaca Auto & Burglar (1922), the Harrington & Richardson Handi Gun (1921), and the Marble Game Getter (1908) – as well as semi-automatic firearms including the M1A1Paratrooper Carbine designed with not only a pistol grip but folding stock. (**Exhibit 24**).

18. **Forward Grips:** One of the earliest forward pistol grips is found on the French Magot rifle from the 1860s. Possibly one of the only copies of this gun is in the Cody Firearms Museum as it was purchased by Winchester during their lawsuit with the company Bannerman. (**Exhibit 25**).

19. **Thumbhole Stocks:** While a traditional thumbhole stock is difficult to historically trace, their regulation has a deep impact on sporting and Olympic firearms in the modern era. The concept of a stabilizing entity to help with maneuverability and accuracy dates to the earliest civilian sporting arms firearms. For example, Schuetzenfest, dating from the 1600s through today, had elaborate sporting rifles created with molded cheek pieces and places for the hand including palm rests - while

not technically a thumbhole, these provided the same stability for which a thumbhole is used. German Frei pistol of the 19th and 20th centuries, used handguns that were made specifically as a stabilizing placement custom for the individual athlete. (**Exhibit 26).** Certain Olympic rifles feature thumbhole stocks, including several models of Winchester, dating to the 1950s. This type of concept or technology is a very prominent shooting sports feature. (**Exhibit 27**).

20. **Folding or Telescoping Stock:** The Cody Firearms Museum has a folding stock snaphaunce blunderbuss that dates to around 1650-1700. (**Exhibit 28**). With early firearms, folding or adjustable stocks are not necessarily seen because pieces in the civilian world were made by artisans prior to mass production. However, the appearance of detachable stocks – converting a pistol to a rifle/carbine – appear in the 1700s on flintlocks and continue to be incorporated on percussion, revolver, and semi-automatic guns. The Luger Model 1902 semi-automatic carbine has an added stock to convert the pistol to a carbine. (**Exhibit 29**). As guns begin to be mass produced on scale, various models are often made, such as a Junior or Ladies rifle that provide a different size option for the sport shooter. The flexibility of stock size is very strong in the civilian market where comfort and having firearms suited for the individual are preferable and feasible. In the early 1900s, and possibly earlier, Try Guns were carried by salesmen to allow the consumer to adjust the stock to fit them to see what size this person needed. Two examples in the Cody Firearms Museum collection are the Winchester Model 12 and LC Smith Try Guns. (**Exhibit 30)**. This lays the foundation for a consumer market interested in customizing and adjusting their stocks to fit them appropriately. Folding stocks do make appearances in the military sphere with the M1A1 Paratrooper Carbine model as well as several submachine guns. (**Exhibit 31)**.

21. **Grenade Launcher or Flare Launcher:** Grenade launchers, also known as

hand mortars, date to the 1600 and 1700s. Flare guns were in use by the 1800s.

22. **30 Inches or Less:** The idea behind a shorter rifle is known as a carbine. While the definition can vary, it typically refers to a barrel less than 20 inches. Additionally, many pistols with detachable stocks fall under this category. By adding a stock to a C-93, C-96 or Luger it converts a semi-automatic pistol into a semi-automatic rifle.

23. **Flash Suppressor:** Flash suppressors appear on machine guns from World War I and earlier including the Chauchat and Maxim but technically, any gun affixed with a Silencer, invented in 1902, could be considered to have a flash suppressor. Silencers were heavily marketed to the civilian population as target accessories, so this would have been available for numerous firearms models. The traditional flash hider on military arms, not classified as a machine gun, were used during WWII on guns such as the Lee-Enfield "jungle carbine" and have appeared on AR platform firearms, invented in the1950s. (**Exhibit 32**).

24. **Threaded Barrel:** An early idea of a quick attachment system in or on a barrel of a gun is the bayonet. Developed in the 16th century, the bayonet was commonly used for both military and civilian firearms. There have been a variety of muzzle devices that have attached to a barrel since (compensators, silencers, muzzle brakes, flash hiders etc). While some early semi-automatic rifles, pistols, and shotguns had threaded barrels, the military did not always use threaded barrels for their suppressed firearms, nor did the civilian market. This is because Hiram Percy Maxim, the inventor of the Silencer, sold his silencer often with an adapter that allowed a silencer to be affixed without a threaded barrel, making the need for a threaded barrel or the thought that no threaded barrel would prevent a silencer moot.

25. **Barrel Shroud:** According to the penal code, the concern for a barrel shroud is that it would prevent "burning the bearer's hand." While typically not thought about, by

that definition, any firearm with a full length stock fits the definition, like a Brown Bess or early single shot pistols. (**Exhibit 33**). To speak in more modern terms, target shooting pistols also tend to have a partial barrel shroud on examples such as the Remington XP100 from the 1960s and the Browning Buckmark Silhouette. (**Exhibit 34)**.

26. **Detachable Magazine:** Although already stated, the detachable magazine was already in use by the 1890s on semi-automatics. Many earlier firearms in the 1800s such as the Spencer, Genhart, Jarre, and Lee Metford also had detachable magazines. These firearms were popular and common both on the military but also the commercial market. For example, the standard infantry arm of the American Civil War was a single shot muzzleloading musket. The repeaters that were readily available at the same time were not openly embraced by military and therefore were a popular consumer product. In fact, the trend of the commercial market being decades ahead in innovation than the military adopted firearms is a trend that has continued into the modern era.

27. **Shotgun with a Revolving Cylinder:** The earliest revolving firearms had shotgun models. For example, the Collier (1814), a flintlock and later percussion revolver in which the user had to manually rotate the cylinder, had shotgun models. Samuel Colt, the creator of the modern revolver, sold revolving shotguns as early as 1839, just four years after his first US patent. (**Exhibit 35**).

CONCLUSION

28. To reiterate, this examination of the firearms features of the California Penal Code 30515 is not comprehensive but is meant to serve as a springboard of understanding that these technologies, in most respects, have been used for centuries far before the invention of Armalite's AR-15 in the 1950s or the Kalashnikov AK-47. By the 20th century, semi-automatic firearms with various combinations of features such as

pistol grips, flash hiders, folding/telescoping stocks, and detachable magazines had been modified and perfected to the point of replication in hundreds, possibly thousands, of models by countless manufacturers for both civilian and military markets. If called to testify, my report would go beyond the origins into the proliferation of these features on a modern consumer market of the 20th and 21st centuries. However, with the determination that these features were conceptualized and in use prior to the 1950s, it is fair to say that really one of the only major new features incorporated to the AR-15 were lightweight alloys to synthetic materials that became a popular experimentation across all firearms platforms after World War II as history provides many examples of these features being used in conjunction with semi-automatic firearms in a number of configurations for nearly a century.

I declare under penalty of perjury that the foregoing is true and correct. Executed on December 6, 2019.

Ashley Hlebinsky

EXHIBITS
TABLE OF CONTENTS

EXHIBITS
TABLE OF CONTENTS

EXHIBITS
TABLE OF CONTENTS

EXHIBIT "1"

Exhibit 1: Ashley Hlebinsky Curriculum Vitae

Ashley Hlebinsky

2313 Central Avenue Unit A

Cody, WY, 82414

Email: theguncode@gmail.com

Phone: 412-491-2493

Education:

Master of Arts, American History, University of Delaware, 2013

Bachelor of Arts, American History, University of Delaware, 2011

- President: Phi Alpha Theta History Honor's Society, 2010-2011

Recent Honors/Awards:

Wyoming Business Report's Top 40 Under 40, 2017

National Shooting Sports Foundation & Professional Outdoor Media Association's Grits Gresham Shooting Sports Communicator of the Year Award, 2017

Nominee – Wyoming's Non-Profit Woman of Influence, 2017

Grants:

National Endowment for the Humanities, 2017

Institute of Museum and Library Services, 2017

Gretchen Swanson Family Foundation, 2015, 2016, 2017, 2018, 2019

Kinnucan Arms Chair Grant, 2012

Fellowships:

Firearms Curatorial Resident, Buffalo Bill Center of the West, 2013

Edward Ezell Fellowship, University of Delaware, 2012

Buffalo Bill Resident Fellowship, Buffalo Bill Center of the West, 2011

Committees and Memberships:

Founding President – Association of Firearms History and Museums

- First academic association for the study of firearms history in United States

Founder – Arsenals of History Symposia Series

- First international symposia series on the academic study of firearms

Spokesperson – NSSF/AFSP Suicide Prevention and Project ChildSafe Programs

National Rifle Association's Gun Collecting Committee

American Alliance of Museums – Member

American Society of Arms Collectors – Member

Winchester Arms Collectors Association – Honorary

Remington Society of Arms Collectors – Member

Weatherby Collector's Association –Life Member

Selected Firearms-Related Professional Experience:

Consultant. Adirondack Experience. November 2019

Project Director, Cody Firearms Museum Renovation, Buffalo Bill Center of the West, Cody, WY, 2015-2019

Consultant. Winchester Mystery House, August 2019.

Consulting Scholar. National Park Service & Organization of American Historians, March 2019.

Robert W. Woodruff Curator, Cody Firearms Museum, Buffalo Bill Center of the West, Cody, WY, 2015-Present

Consulting Curator, Houston Museum of Natural Sciences, 2018 - Present

Producer. *Gun Stories with Joe Mantegna*, Outdoor Channel. 2017-Present

Consulting Producer. *Brothers in Arms*, History Channel. 2017

Consultant/Curator. Daniel Defense, Black Creek, Georgia. 2017

Consultant, National Museum of Law Enforcement and Organized Crime (Mob Museum), Las Vegas, NV, 2016

Associate & Acting Curator, Cody Firearms Museum, Buffalo Bill Center of the West, Cody, WY, 2015

Guest Curator. C.M. Russell Museums and Complex, 2015-2016

Guest Curator. Cody Firearms Experience, 2015

Assistant Curator, Cody Firearms Museum, Buffalo Bill Center of the West, Cody, WY, 2013-2014

Teaching Assistant, The Jewish Holocaust: 1933-1945, University of Delaware, 2013

Teaching Assistant, Introduction to Military History, University of Delaware, 2012

Teaching Assistant, History Education, University of Delaware, 2011

Researcher/Fellow, National Museum of American History, Smithsonian Institution, 2010-2013

Archival Assistant, University of Delaware Special Collection, 2010-2011

Firearm Intern, Soldiers and Sailors National Memorial Hall, 2008

Publicly Disclosed Expert Witness Testimony:

Regina (Nova Scotia) vs Clayton, January 2019

Garrison vs Sturm, Ruger & Company, Inc. 2018

Selected Media Appearances:

Co-Host. *Master of Arms*. Discovery Channel, 2018.

On Camera Expert. Gun Stories with Joe Mantegna, Outdoor Channel, 2015 - Present

Re-Occurring Expert. Mysteries at the Museum, Travel Channel, 2016-Present

Re-Occurring Guest. Sportsmen of Colorado, Radio The Source 560 AM KLZ, 2014 - Present

Guest. To the Best of Our Knowledge, National Public Radio, 2016

On Camera Expert. American Genius, National Geographic, 2015

Guest Host. Gunrunner Radio. Big Horn Radio, 2014

Also appears on: National Public Radio, Fox News, Media, Entertainment, Arts, WorldWide, Women's Outdoor News, Outdoor Life, Shooting USA, NRA News, Gun Talk Media, National Shooting Sports Foundation, Discovery Channel, Travel Channel, National Geographic

Has been profiled by: The Bourbon Review, Recoil Magazine, Outdoor Life Magazine, Guns.com, Blue Press Magazine, and many other national news outlets.

Selected Lectures/Panels:

Panelist. Firearms and Museums in the 21st Century. National Council for Public History. March 2019.

Scholars Roundtable. Coltvsille National Historic Site. Organization of American Historians & National Park Service, March 2019.

Forum Speaker. The Art of the Hunt: Embellished Sporting Arms in America. New Orleans Antique Forum, August 2018

Guest Lecturer. Unloading the Gun: Firearms, History, and Museums. Yakima Valley Museum, June 2018

Guest Lecturer. Perpetrators and Protectors: The Mob, The Law and Firearms, National Museum of Law Enforcement and Organized Crime (Mob Museum), September 2017

Organizer. Arsenals of History: Firearms and Museums in the 21st Century, Buffalo Bill Center of the West, July 2017

Lecturer. The Cody Firearms Museum, Arsenals of History Symposium, Buffalo Bill Center of the West, July 2017

Moderator. Addressing the Press: Firearms and the Media, Arsenals of History Symposium, Buffalo Bill Center of the West, July 2017

Moderator. Forming an Association: Legitimizing Firearms in Academic Study, Arsenals of History Symposium, Buffalo Bill Center of the West, July 2017

Guest Lecturer. Displaying the "Politically Incorrect," C.M. Russell Museums and Complex, May 2017

Guest Lecturer. Displaying the "Politically Incorrect," Blackhawk Museum, March 2017

Panelist. Curator Roundtable, Firearms and Common Law Symposium, Aspen Institute, September 2016

Guest Lecturer. Displaying the "Politically Incorrect," Canadian Guild of Antique Arms Historians, April 2016

Guest Lecturer. The Cody Firearms Museum Renovation, American Society of Arms Collectors, September 2016

Guest Lecturer. From Protector to Perpetrator: Demystifying Firearms in History, Art Institute of Chicago, November 2015

Guest Lecturer. Winchester ’73: The Illusion of Movie Making, Winchester Arms Collectors Association, July 2014

Guest Lecturer. Unloading the Six Shooter: Disassembling the Glamorization and Demonization of Firearms in the Arts, Buffalo Bill Center of the West, 2011

Selected Firearms Exhibitions:

Curator/Project Director. *Cody Firearms Museum Renovation*. Buffalo Bill Center of the West. Upcoming July 6, 2019

Co-Curator. *The Art of the Hunt: Embellished Sporting Arms from 1500-1800.* Houston Museum of Natural Sciences. March 2019

Curator. *Glock Makes History: The Birth of the Polymer Handgun Market*. Buffalo Bill Center of the West. June 2016

Guest Curator. *Designing the American West: The Artist and the Inventor*. C.M. Russell Museum & Complex. February 2016

Curator. *The Greatest Gun Designer in History: John Moses Browning.* Buffalo Bill Center of the West. December 2015

Curator. *Journeying West: Distinctive Firearms from the Smithsonian Institution.* Buffalo Bill Center of the West. December 2015

Curator. *The Forgotten Winchester: Great Basin National Park*. Buffalo Bill Center of the West. June 2015

Curator. Western Firearms Gallery, including *Shoot for the Stars: The Tradition of Cowboy Action Shooting*. Buffalo Bill Center of the West. April 2015.

Curator. *Steel Sculptures: Engraving Individuality from Mass Production*. Buffalo Bill Center of the West. Winter 2014.

Certifications:

NRA Certified Firearms Instructor, Basic Pistol, 2016

NRA Certified Firearms Instructor, Personal Protection Inside the Home, 2016

Well Armed Woman Instructor Certification, 2016

Museum Studies Certification, University of Delaware, 2013

Publication History

Books:

Co-Author. *Fifty Featured Firearms at the Buffalo Bill Center of the West*. Mowbray Publication: Rhode Island, 2017 (in process)

Contributor. *Buffalo Bill Center of the West.* Buffalo Bill Center of the West: Cody, WY, 2016.

Articles:

Author. "Burton Light Machine Rifle." *Recoil Magazine*. October, 2019

Founder/Editor/Author. Arsenals of History Journal, Annual Publication, 2018 - Present

Author. "It's Complicated: The Short Answer to Firearms, Museums and History. *Journal of the Early Republic – The Panorama*, September 2018.

Contributor. "Firearms Curator Roundtable" *Technology & Culture Journal*, August 2018

Author. "Displaying the 'Politically Incorrect.'" *CLOG X Guns*: Chicago, IL, September 2017

Author. "Does History Repeat Itself? The Smith & Wesson LadySmith." *CLOG X Guns:* Chicago, IL, September 2017

Author. "Renovating the Cody Firearms Museum." *International Committee of Museums and*

Collections of Arms and Military History Magazine. Issue 17, May 2017. Pg. 38 - 41

Author. "Renovating the Cody Firearms Museum." *American Society of Arms Collectors Journal*. Fall 2016.

Author. "Glock Exhibit Opening." *Glock Magazine.* Bang Media. Annual 2017

Author. "The 28 Most Notable Guns from Remington's 200-Year History." *Outdoor Life Magazine*. Bonnier Corporation, 2016

Author. "Cassie Waters: Businesswoman of the Old West." *Guns of the Old West.* Harris Publications, Spring 2016

Author. "Making History: GLOCK Pistols at the Cody Firearms Museum" *Glock Magazine.* Harris Publications. Annual 2016

Author. "Pocket Pistols: 10 Seminal Guns from the Past 300 Years." *Pocket Pistols.* Harris Publications. 2016

Author. "The Gun that Won the Western and the Unforeseen Stars of *Winchester '73*" *Guns of the Old West.* Harris Publications.

Author. "Frontier Profile: Jedediah Strong Smith" *American Frontiersman*. Harris Publications

Author. "Frontier Legend John Johnston." *American Frontiersman.* Harris Publications

Author. "The Guns of John Johnston." *American Frontiersman.* Harris Publications

Author. "Annie Oakley VS Lillian Smith: A Female Sharpshooter Rivarly." *Guns of the Old West.* Harris Publications, Spring 2015

Author. "Icons and Has-beens." *American Handgunner*. FMG Publications, 2014

Author. "Triggering Memory: American Identity in *Cowboys and Aliens*." *Points West.* Spring 2012

Author. "Unloading the Six-Shooter: Disassembling the Glamorization and Demonization of Firearms in the Arts." *Points West*, Fall 2011.

Columns:

Author/Brand Ambassador. *The Bourbon Review*.

Author. *American Association for State and Local History*. Summer 2019

Author. "Weird West: Fact or Fiction" *Guns of the Old West*. Athlon Outdoors (formerly Harris Publications)

1st Assault Rifle

Colt VS Winchester Revolver

Did Winchester Really Win the West?

Oliver Winchester's Lever Action Shotgun

Remington Cane Gun

Author. "Cowboy Action Round Up." SHOT Show New Products. *Guns of the Old West*. Athlon Outdoors (formerly Harris Publications). 2015, 2016, 2017

Reviews:

Reviewer: Edited by Jonathan Obert, Andrew Poe, and Austin Sarat. Oxford: Oxford UniversityPress, 2018. *Journal of Technology & Culture*, Fall 2019

Author. "Everybody Loves an Outlaw: Taylor's Outlaw Legacy Revolver Series." *Guns of the Old West.* Harris Publications

Reviewer: Richard Rattenbury. *A Legacy in Arms: American Firearms Manufacture, Design and Artistry, 1800-1900*. *Chronicle of Oklahoma*, Spring 2016

Selected Blogs & Vlogs:

Recoil Magazine

Weekly video series beginning October 2017 to Present

Dillon Precision

Historical Videos on Ammunition (Upcoming)

Outdoor Life

Top 10 Guns in American History

Guns of the Old West: 10 Iconic Firearms and the Legendary Men (and Women) Who Shot Them

13 of the Biggest Gun Fails in Recent Firearms History

Gun of the Week:

John Martz Luger

Apache Revolver

German Frei Pistol

King Louis XV Embellished Blunderbuss

Armalite AR-17 Shotgun

Getting the Christmas Goose with a Goose Rifle & Cutaway Suppressor

Mossberg Brownie

Wesson & Leavitt Belt Revolver

William Harnett and the Faithful Colt 1890

Winchester Model 1894 Lever Action Rifle

Ruger Semi-Automatic Pistol, 1 of 5,000

Herb Parson's Winchester Model 71 Lever Action Rifle

Lincoln Head Hammer Gun

American Trap Gun

Browning Brother's Single Shot Rifle Patent

Feltman Pneumatic Machine Gun

U.S. Springfield-Allin Conversion Model 1866 Trapdoor Rifle

Winchester Wetmore-Wood Revolver

Webley-Fosbery Automatic Revolver

Hopkins & Allen XL3 Double Action Revolver

DuBiel Modern Classic Rifle

Colt Model 1877 "Thunderer" Double Action Revolver

Tom Tobin's Colt Model 1878 Frontier Revolver

Walch 10-Shot Double Hammers Pocket Revolver

Winchester Model 1887, Serial No. 1

Deringer vs Derringer

The Forgotten Winchester 1873 of Great Basin National Park

Range 365

To the One Who Got Away

Gun Review: New Glock 19 Gen 5

Ain't She a Pistol? 10 Historic Gun Ads Featuring Women

National Shooting Sports Foundation

- The Gun Vault:
- Winchester 1873 Found in Great Basin National Park
- Col. Jeff Cooper's Colt MK IV Series 80
- 500+ Year Old Firearms, Matchlocks, Flintlocks
- U.S. Presidents Guns
- Cross Dominance Shotgun
- Herb Parson's Winchester Model 71 Rifle
- Audie Murphy's Colt Bisley Revolver
- 4 Gauge Winchester Wildfowler
- Pocket Pistols
- Henry Ford's Winchester Model 1887 Lever Action Shotgun
- Tom Knapp's First Gun
- Buffalo Bill Cody's Winchester 1873
- Colt Model 1861 Navy Serial No. 1
- Cassie Waters' Hopkins & Allen XL3 Revolver
- Glock 17

The Truth About Guns

- Presidential Presentation Rifles
- Factory Cut-Away M16A1
- 1854 Smith & Wesson Repeating Rifle (Serial Number 8)
- Winchester World's Fair Model 1866 Deluxe Sporting Rifle
- Raymond Wielgus Collection

Gastinne-Renette Muzzleloading Percussion Target Pistols

Oliver Winchester's Jennings Repeater

Henry Ford's Winchester Model 1887

Winchester Model 1866 Musket in .44 Rimfire

English Wheellock

Southern Belle American Longrifle

Annie Oakley's Model 1892 Smoothbore Rifle

Catherine the Great of Russia's Blunderbuss Gift to King Louis XV of France

Color Case-Hardened GLOCK 43: Merging the Old West with the New

Buffalo Bill Center of the West – Unloading the Myth

The Cody Firearms Museum – Yesterday, Today, and Tomorrow

Guns of the Week – Christmas List

Guns of the Week: December 15-19

Guns of the Week – The Cody Firearms Museum

Guns of the Week – German Firearms

Guns of the Week – Scheutzenfest

Guns of the Week – Air Guns

Guns of the Week – Early Firearms Law

Guns of the Week – October 13-17

Guns of the Week – Ingenious Engineering

Guns of the Week – Remington – Smoot

Guns of the Week – September 22-26

Guns of the Week – September 15-19

Guns of the Week – September 8 -12

CSI: Firearms Museum Edition

Confessions of a Gun Historian

Art Guns: Aesthetics Over Function?

What Good's a Gun Without a Firing Pin?

Gun Installations, Trials & Tribulations

A True Test of Marital Trust and Love

Remembering Tom Knapp

Cody Firearms Museum Goes Hollywood

When Will My Firearms Go On Display

What's Your Cody Firearms Museum

To Vlog or Not to Vlog

We Don't Just Have Old Guns in Our Museum: SHOT Show 2014

Taking a Staba at Displaying More Guns

"Hi Yo Silver" Cook Away! Lone Ranger Display

The Shooting Wire

Winchester's 150th Anniversary Website

Remington's 200th Anniversary Website

EXHIBIT "2"


1988. 8. 3208

EXHIBIT "3"

(obverse)

132

A GERMAN PARCEL-GILT SILVER TARGET SHOOTING PRIZE

DRESDEN, DATED 1537, UNMARKED

Formed as a large shaped circular pendant medallion suspending two smaller openwork medallions, the moulded border with applied foliage and inner laurel wreath centering on a cherub's mask with suspension ring above, the centre *repoussé* and chased with a huntsman with raised matchlock rifle amongst scrolling flowers and foliage, standing beside an escutcheon of arms of Dresden held by a putto, the backplate engraved with inscription within scrolling foliage incorporating two figures supporting a target above and a standing putto below, the smaller pendant shields each formed as a foliage and berried wreath enclosing an escutcheon, engraved with instruments emblematic of the building trade, one further engraved with initals BK, coat-of-arms and date 1541, the reverse with inscription and date 1540, the other with initials HR, coat-of-arms and date 1563, the reverse with date 1562, within engraved wreath

EXHIBIT "4"

EXHIBIT "5"

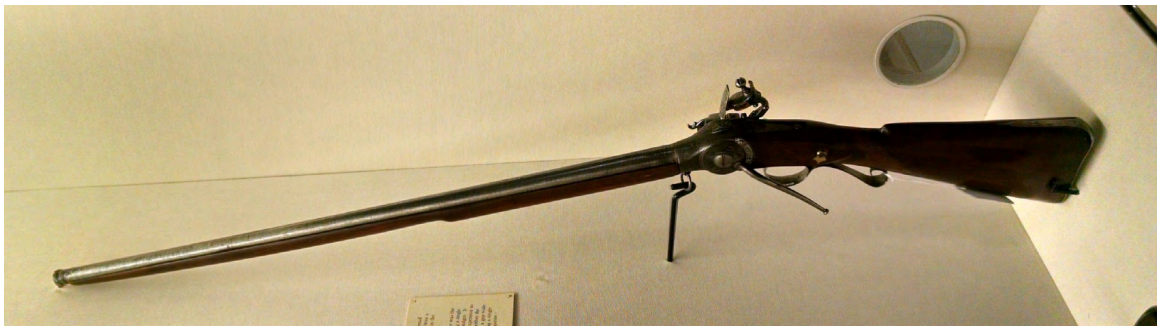

EXHIBIT "6"

Exhibit 6
0023

EXHIBIT "7"

EXHIBIT "8"

Exhibit 8
0027

EXHIBIT "9"


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EXHIBIT "10"


Passler & Seidl Semi-Automatic Pistol
Maker: Passler & Seidl, Vienna, Austria
Serial Number: 12
Date: about 1894
Caliber: 7.7 mm (probably 7.65 mm Borchardt centerfire cartridge or 8 mm Bergmann tapered rimless cartridge)
Catalogue Number: 1988.8.2263
Winchester Arms Collection; gift of Olin Corporation
Patented in 1887 by Franz Passler of Ottakring and Ferdinand Seidl of Budweis, Austria, this pistol was made only in limited numbers. The design was modified slightly by Gustav Bittner of Weipert, Bohemia, and produced limited
Although named "Bergmann," the pistol's system was actually the design of Louis Schmeisser. After various experiments between 1892 and 1894 with Schmeisser's designs, Theodor Bergmann introduced an 8 mm semi-automatic pistol with box magazine forward of the triggerguard. In 1896 a revised pistol was introduced for the commercial market in 6.5 mm bore.
Mannlicher "Model 1905" Semi-Automatic Pistol
Maker: Waffenfabrik Steyr, Steyr, Austria
Serial Number: 8,243
Date: about 1905
Caliber: 7.63 (7.65 mm Mannlicher" tapered centerfire cartridge)
Catalogue Number: 1988.8.513
Winchester Arms Collection; gift of Olin Corporation
The "Model 1905" Mannlicher was the final evolution of the Mannlicher "Model 1901" semi-automatic pistol. Although its caliber
Colt Model 1911 Semi-Automatic Pistol, Fitted with Shoulder Stock
Maker: Colt's Patent Firearms Manufacturing Company; Hartford, CT
Serial Number: C 87,011
Date: 1917
Caliber: .45 (.45 automatic Colt pistol (ACP) centerfire cartridge)
Catalogue Number: 1988.8.1164
Winchester Arms Collection, Gift of the Olin Corporation
This pistol has been fitted with a modified Mauser 1896 ("Broom handle") shoulder stock/holster. New oversized grips were added, and the stock cavity has been modified/enlarged to accept the pistol.
Borchardt "Model 1893" Semi-Automatic Pistol-Carbine
Maker: Waffenfabrik Loewe, Berlin, Germany
Serial Number: 949
Date: 1894

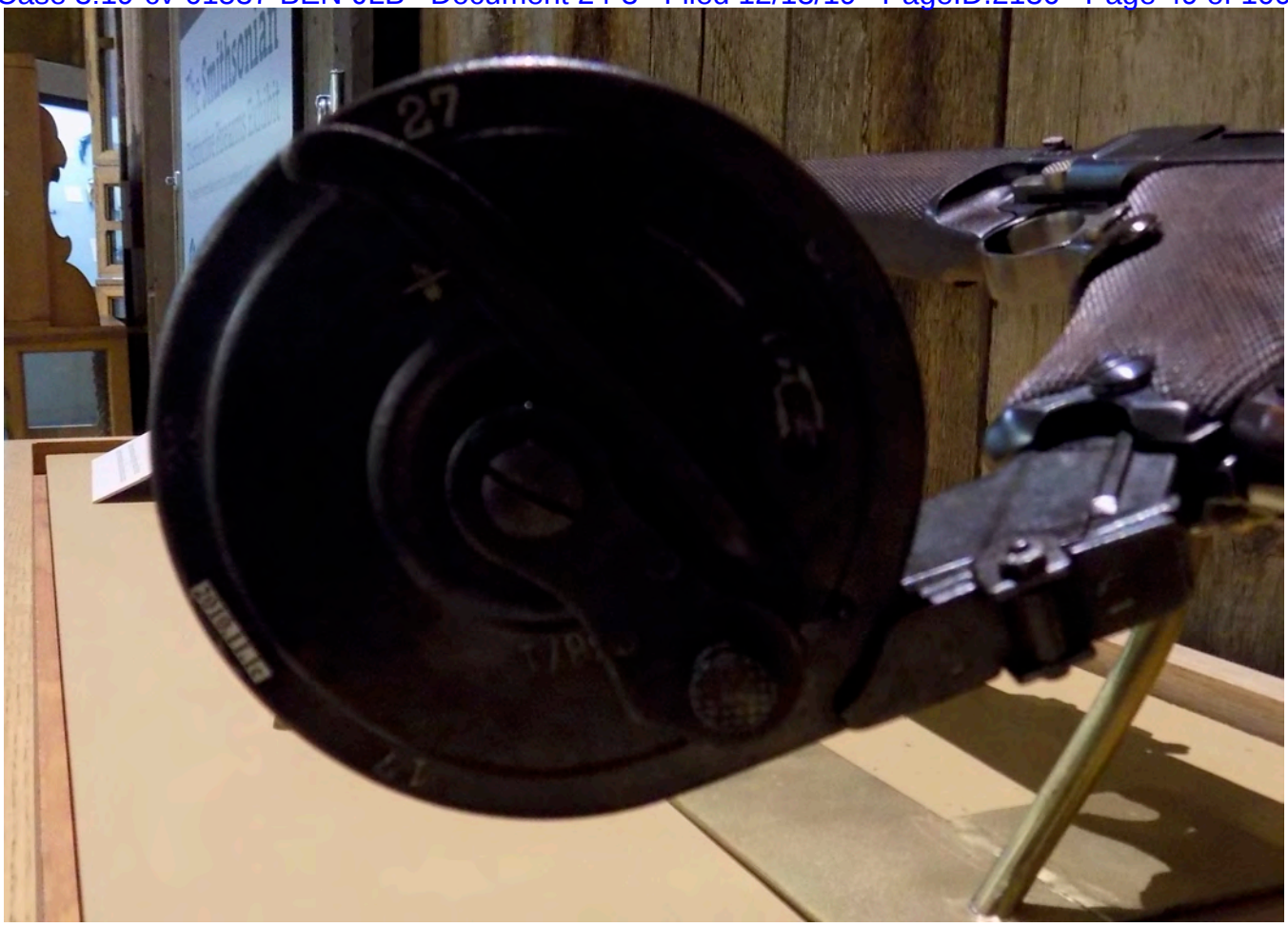

EXHIBIT "11"

EXHIBIT "12"

EXHIBIT "13"

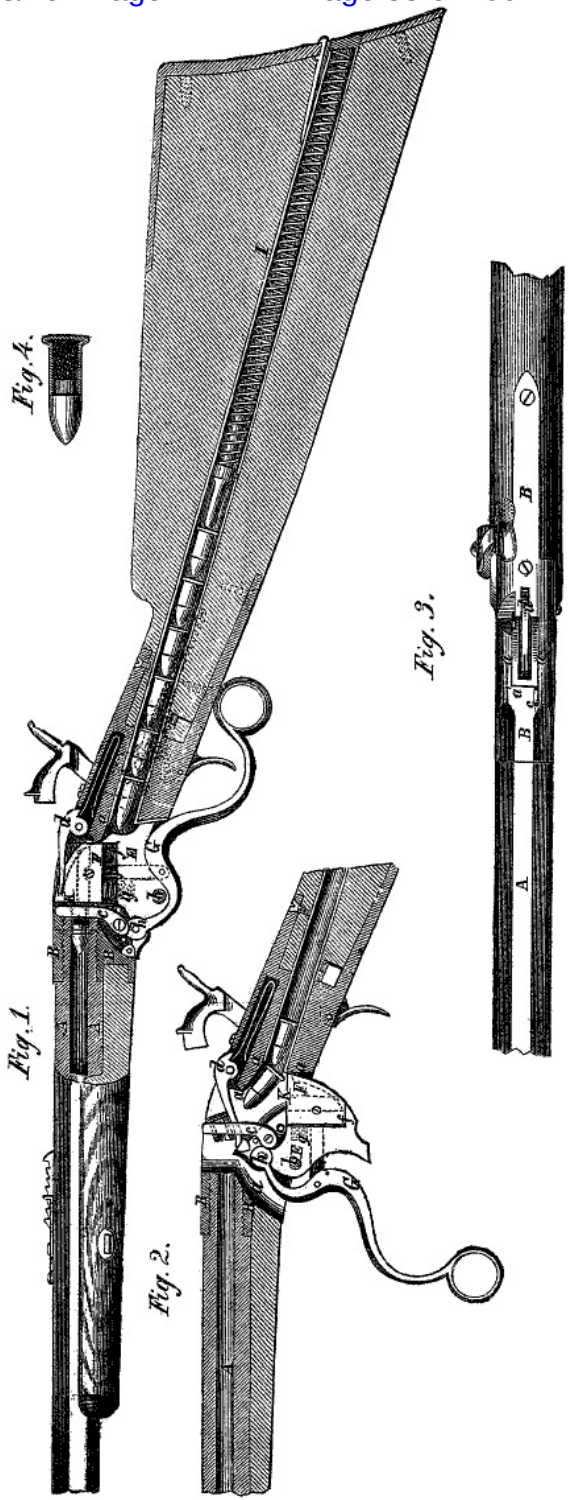
Fig. 4.
Fig. 3.
Fig. 1.
Fig. 2.

EXHIBIT "14"


10
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EXHIBIT "15"

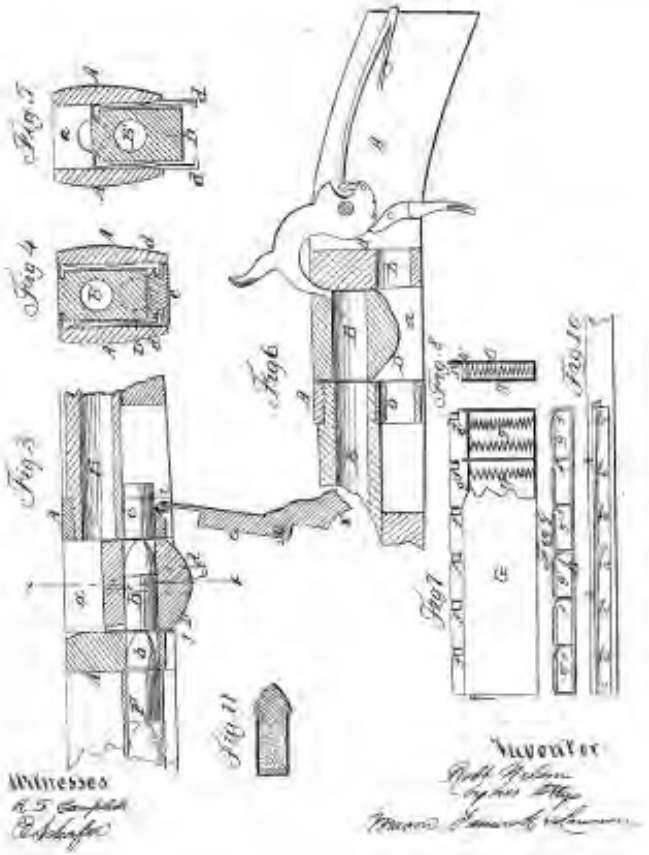
EXHIBIT "16"

2 Sheets—Sheet 2

R. WILSON.

Magazine Fire-Arm.

No. 45,106. Patented Nov. 15, 1864



Witnesses

Inventor

R. WHITE.

REPEATING FIREARM.

No. 12,648. Patented Apr. 3, 1855.

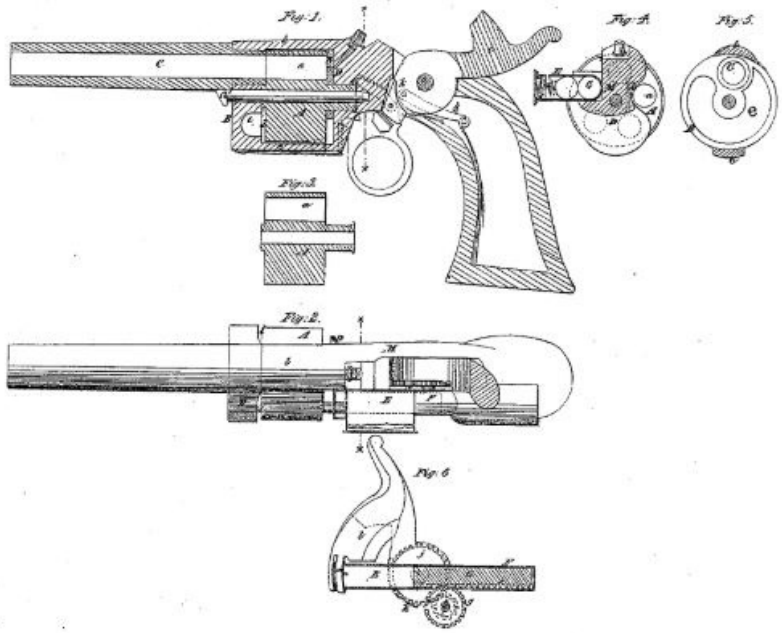
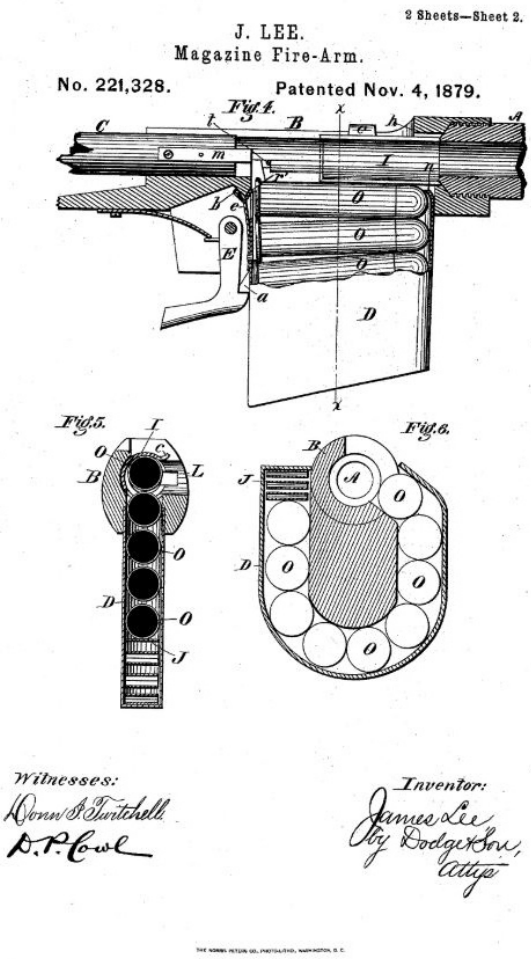


Exhibit 16
0044

EXHIBIT "17"



2 Sheets—Sheet 2.

J. LEE.

Magazine Fire-Arm.

No. 221,328. Patented Nov. 4, 1879.

Witnesses:

Donn B. Tuttchell

D. P. Lowe

Inventor:

James Lee

by Dodger Bros.

Attys

EXHIBIT "18"

Exhibit 18
0048

EXHIBIT "19"

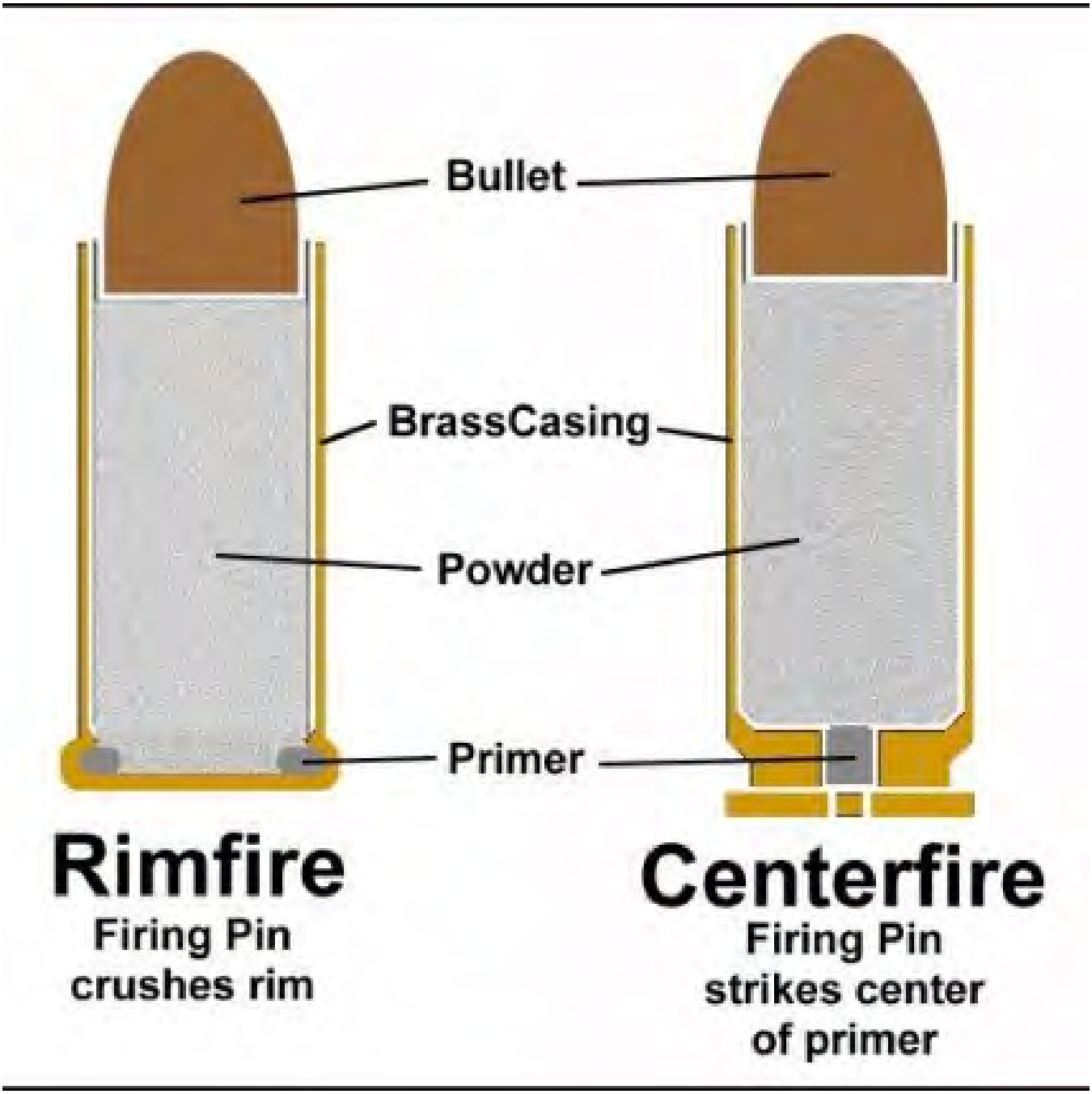

Bullet
BrassCasing
Powder
Primer
Rimfire
Firing Pin
crushes rim
Centerfire
Firing Pin
strikes center
of primer

EXHIBIT "20"

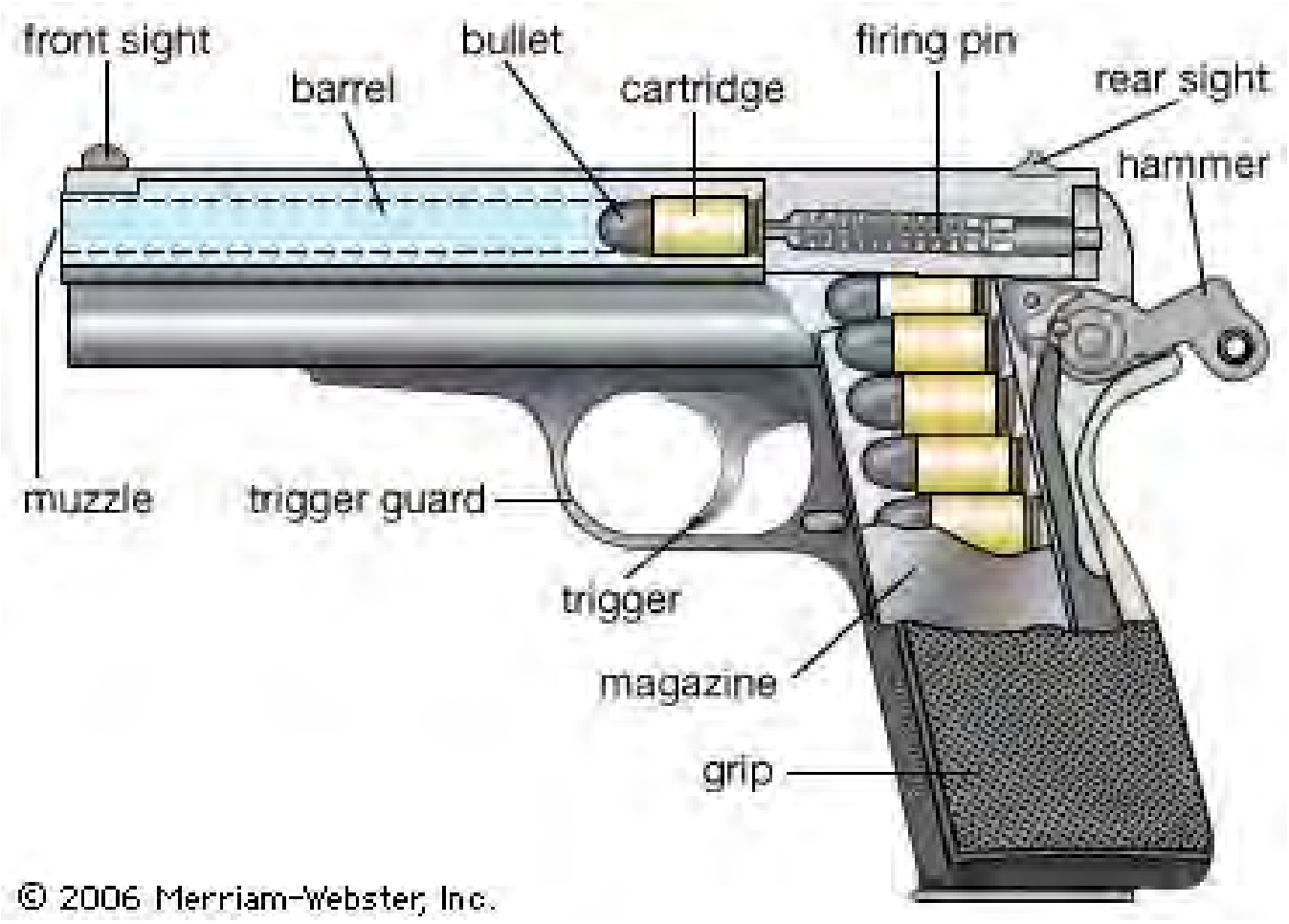

front sight
barrel
bullet
cartridge
firing pin
rear sight
hammer
muzzle
trigger guard
trigger
magazine
grip
© 2006 Merriam-Webster, Inc.

EXHIBIT "21"

Exhibit 21
0054

EXHIBIT "22"

Exhibit 22
0056

EXHIBIT "23"


Passler & Seidl Semi-Automatic Pistol
Maker: Passler & Seidl; Vienna, Austria
Serial Number: 12
Date: about 1894
Caliber: 7.7 mm (probably 7.65 mm Borchardt centerfire cartridge or 8 mm Bergmann tapered rimless cartridge)
Catalogue Number: 1988.8.2263
Winchester Arms Collection; gift of Olin Corporation
Mannlicher "Model 1905" Semi-Automatic Pistol
Maker: Waffenfabrik Steyr; Steyr, Austria
Serial Number: 8,243
Date: about 1905
Caliber: 7.63 (7.65 mm Mannlicher" tapered centerfire cartridge)
Catalogue Number: 1988.8.513
Winchester Arms Collection; gift of Olin Corporation
Colt Model 1911 Semi-Automatic Pistol, Fitted with Shoulder Stock
Maker: Colt's Patent Firearms Manufacturing Company; Hartford, CT
Serial Number: C 87,011
Date: 1917
Caliber: .45 (.45 automatic Colt pistol (ACP) centerfire cartridge)
Catalogue Number: 1988.8.1164
Winchester Arms Collection, Gift of the Olin Corporation

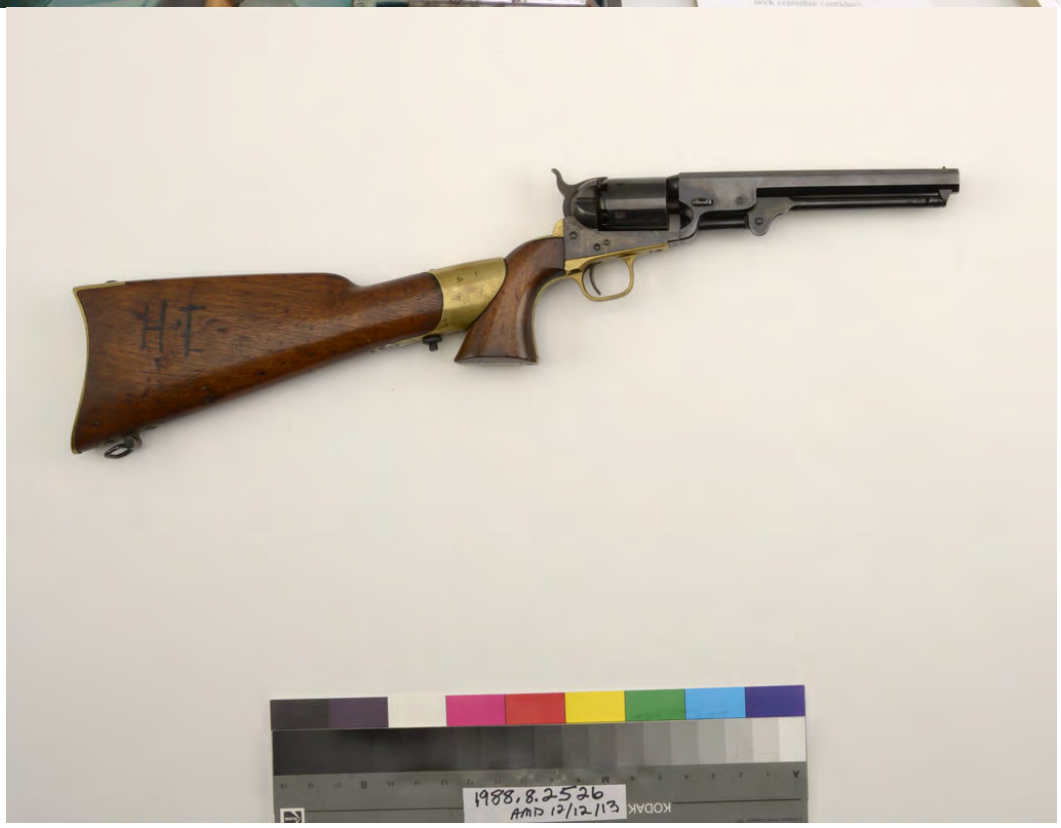

1988.8.2526
AMD 12/12/13

EXHIBIT "24"


Auto and Burglar Gun
MADE BY
ITHACA GUN COMPANY
ITHACA, NEW YORK


U.S.

EXHIBIT "25"

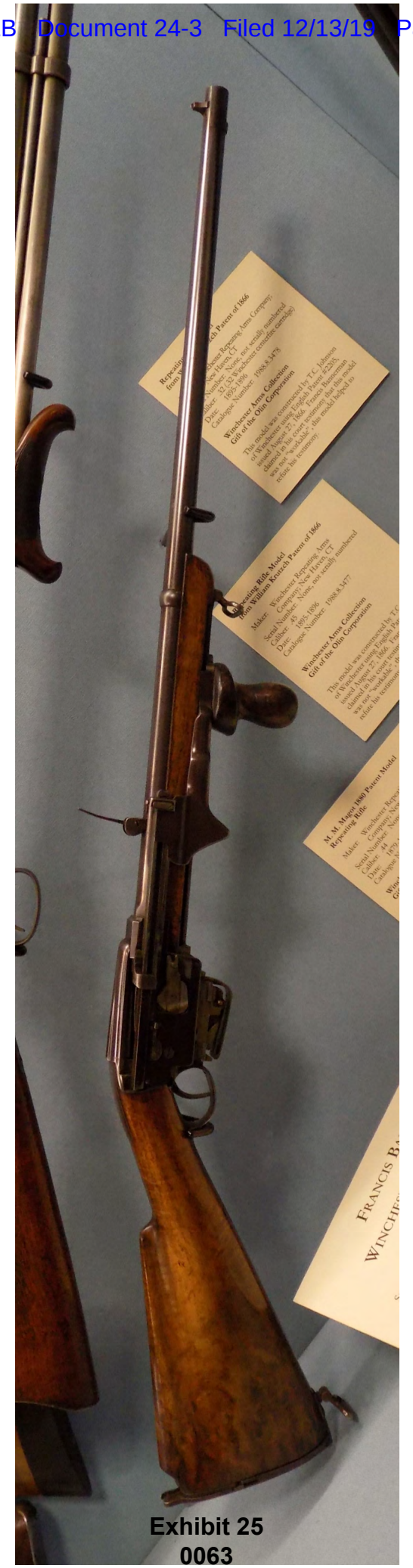


Exhibit 25
0063

EXHIBIT "26"


1989.6.27

EXHIBIT "27"

Exhibit 27
0067

EXHIBIT "28"

Exhibit 28
0069

EXHIBIT "29"

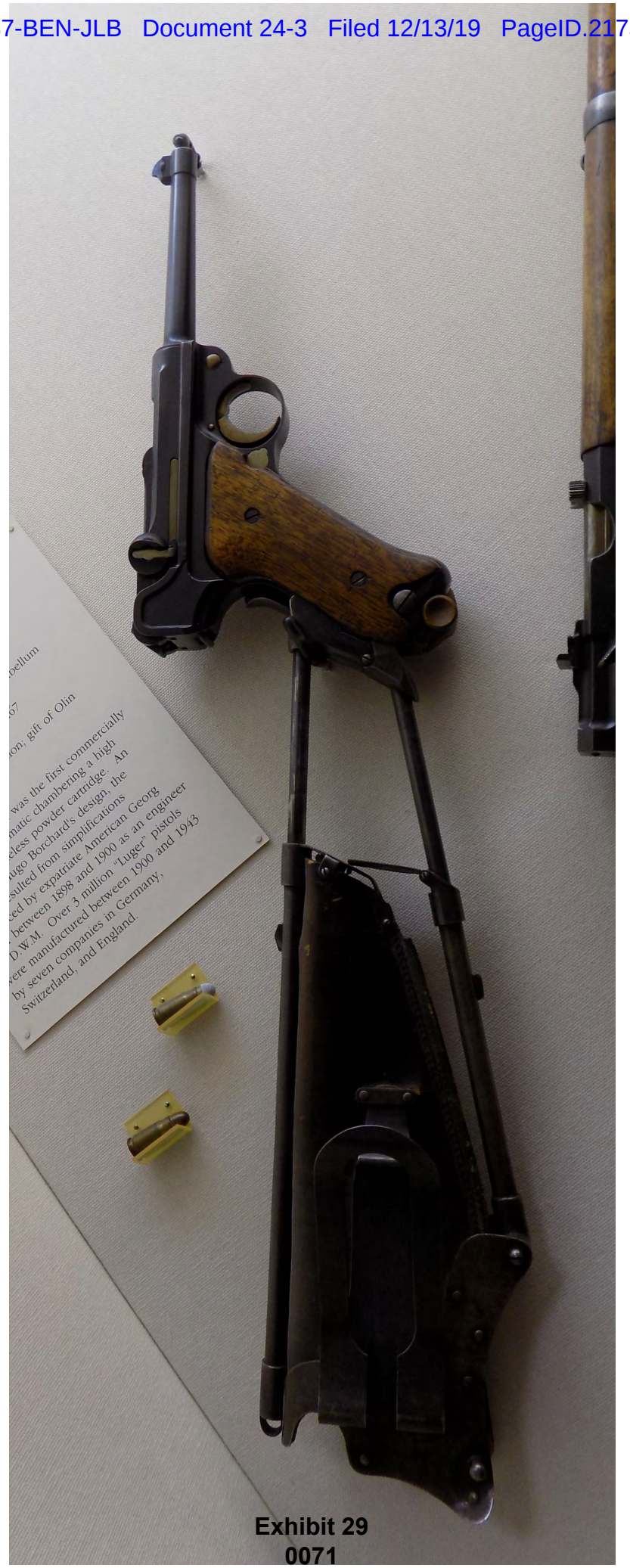


Exhibit 29
0071

EXHIBIT "30"

EXHIBIT "31"

Exhibit 31
0075

EXHIBIT "32"

Exhibit 32
0077

EXHIBIT "33"


KODAK Gray Scale
1988.8.1017
EMH 12/12/13

EXHIBIT "34"


2000.5.18
AMD 12/17/2013

EXHIBIT "35"

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Attorneys for Plaintiffs

UNITED STATES DISTRICT COURT

FOR THE SOUTHERN DISTRICT OF CALIFORNIA

JAMES MILLER, et al.,

Plaintiffs,

vs.

XAVIER BECERRA, in his official capacity as Attorney General of California, et al.,

Defendants.

Case No. 3:19-cv-01537-BEN-JLB

Hon. Roger T. Benitez
Magistrate Hon. Jill L. Burkhardt

DECLARATION OF PLAINTIFF JAMES MILLER IN SUPPORT OF PLAINTIFFS' MOTION FOR PRELIMINARY INJUNCTION

Complaint filed: August 15, 2019
Amended Complaint filed: September 27, 2019

Date: Thursday, January 16, 2020
Time: 10:00 a.m.
Courtroom: 5A, 5th floor

DECLARATION OF JAMES MILLER

I, James Miller, declare as follows:

1. I am an adult resident of the County of San Diego, California, and am a named plaintiff in the above matter. I have personal knowledge of the facts stated herein, and if called as a witness, I could competently testify to these facts.

2. This declaration is executed in support of plaintiffs' motion for a preliminary injunction.

3. I am not prohibited from owning firearms under federal or state law. For many years, in fact, I have held a license to carry a concealed weapon (CCW), issued by my local county sheriff, that requires a background check and good moral character. Under state law, this CCW must be renewed every two years.

4. I am the lawful owner of a semi-automatic, centerfire rifle that is specifically described as an AR-15 pattern rifle, which has one or more of the characteristics listed in Penal Code § 30515(a)(1), to wit: a pistol grip (§ 30515(a)(1)(A)), a telescoping stock (§ 30515(a)(1)(C)), and a flash suppressor (§ 30515(a)(1)(E)). However, this rifle is not considered to be an "assault weapon" under section 30515(a)(1) because it has a "fixed magazine," that is, it contains an ammunition feeding device that cannot be removed from the firearm without disassembly of the firearm action." *See*, Pen. Code § 30515(b) and 11 CCR § 5471(m).

5. I rendered this firearm with a fixed magazine in order to preserve the other salient features listed above, without having to destroy, deface or otherwise alter these characteristics of the firearm, and to avoid having to register the firearm as an "assault weapon" pursuant to Pen. Code § 30900(b). If registered as an "assault weapon," I would be effectively prohibited from transferring or passing along the firearm to my heirs, or selling it to anyone else.

6. I am also in lawful possession of a so-called "large capacity magazine" (as that term is defined in Pen. Code § 16740) that would be used in this firearm.

7. The only thing that prevents me from inserting or using the large capacity magazine, which would then allow it to be in ordinary configuration as an ordinary AR-15 rifle with a standard, 30-round magazine ("Standard Capacity Magazine"), is by operation of Pen. Code § 30515(a)(2), which further defines as an assault weapon "[a] semiautomatic, centerfire rifle that has a fixed magazine with the capacity to accept more than 10 rounds." However, it is my understanding that this Court has already found that the state's prohibition on large-capacity magazines was unconstitutional, and enjoined enforcement of those provisions of the Penal Code that would have prohibited their possession. It would be my understanding that along with the right to possess large-capacity magazines, for the reasons expressed in this Court's judgment, would be the right to use such magazines in an otherwise legally-owned firearm.

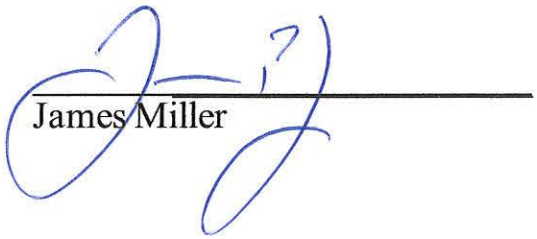
8. I wish to continue to possess my firearm, together with a Standard Capacity Magazine inserted therein, without being subject to arrest and/or prosecution under Pen. Code §§ 30600 (for manufacturing, transporting, or transferring an "assault weapon"), or 30605 (for possessing an "assault weapon").

9. It is also my desire to obtain and acquire additional AR-15 pattern firearms that either have some or all of the features listed in Pen. Code § 30515(a)(1) and which do not have a fixed magazine, or to obtain and acquire a semiautomatic, centerfire rifle that has a fixed magazine with the capacity to accept more than 10 rounds.

///

10. Accordingly, and for the reasons set forth in Plaintiffs' motion, I respectfully ask that the Court grant Plaintiffs' motion for preliminary injunction.

I declare under penalty of perjury that the foregoing is true and correct. Executed on December 12, 2019.



James Miller

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Attorneys for Plaintiffs

UNITED STATES DISTRICT COURT

FOR THE SOUTHERN DISTRICT OF CALIFORNIA

JAMES MILLER, et al.,

Plaintiffs,

vs.

XAVIER BECERRA, in his official capacity as Attorney General of California, et al.,

Defendants.

Case No. 3:19-cv-01537-BEN-JLB

Hon. Roger T. Benitez
Magistrate Hon. Jill L. Burkhardt

DECLARATION OF PLAINTIFF WENDY HAUFFEN IN SUPPORT OF PLAINTIFFS' MOTION FOR PRELIMINARY INJUNCTION

Complaint filed: August 15, 2019
Amended Complaint filed: September 27, 2019

Date: Thursday, January 16, 2020
Time: 10:00 a.m.
Courtroom: 5A, 5th floor

DECLARATION OF WENDY HAUFFEN

I, Wendy Hauffen, declare as follows:

1. I am an adult resident of the County of San Diego, California, and am a named plaintiff in the above matter. I have personal knowledge of the facts stated herein, and if called as a witness, I could competently testify to these facts.

2. This declaration is executed in support of plaintiffs' motion for a preliminary injunction.

3. I am not prohibited from owning firearms under federal or state law. In fact, I currently hold a license to carry a concealed weapon (CCW), issued by my local county sheriff, that requires a background check, good cause, and good moral character in order to obtain. Under state law, this CCW must be renewed every two years.

4. I am the lawful owner of a semi-automatic, centerfire rifle that is specifically described as an AR-15 pattern rifle. However, this firearm does not have any of the features listed in Penal Code § 30515(a)(1), (*e.g.*, a pistol grip (§ 30515(a)(1)(A)), a thumbhole stock (§ 30515(a)(1)(B)), a telescoping stock (§ 30515(a)(1)(C)), a grenade launcher/flare launcher (§ 30515(a)(1)(D)), a flash suppressor (§ 30515(a)(1)(E)), or a forward pistol grip (§ 30515(a)(1)(F))). Thus, because my rifle does not have any of the statutorily-described features, this rifle is not considered to be an "assault weapon" under section 30515(a)(1).

5. I rendered this firearm in this "featureless" configuration (see, e.g., 11 CCR § 5471(o)) in order to lawfully avoid having to register the firearm as an "assault weapon" pursuant to Pen. Code § 30900(b). I would not have otherwise purchased these "featureless" parts for my firearm and installed them on to my firearm if I was not required to do so, because I prefer my firearm to have a number of the listed features in penal code section 30515(a). However, to have these features, I would have had to

register my firearm as an "assault weapon." Registering would effectively prohibited me from transferring or passing along the firearm to my heirs or selling it to anyone else. Eventually, I do plan on either passing down my firearms to my heirs or selling my firearms if the need should ever arise.

6. I wish to continue to lawfully possess this firearm, and to reattach some or all of the § 30515(a)(1) features listed above, but fear that I would be subject to arrest and/or prosecution under Pen. Code §§ 30600 (for manufacturing, transporting, or transferring an "assault weapon"), or 30605 (for possessing an "assault weapon").

7. By reattaching some or all of the features described by 30515(a)(1) to my firearm, or acquiring additional firearms that bear some or all of these features, I would possess and therefore desire to possess ordinary and standardized semiautomatic, centerfire firearms with listed features, like the AR-15, that are commonly and lawfully held, and used lawful purposes, in many other parts of the country.

8. As a female firearms trainer who specializes in training other women in the proficiency of arms and self-defense, I find the many semiautomatic, centerfire firearms with listed features, like the AR-15 rifle, to be well-suited to women shooters, because of its relatively light weight and because it can easily be customized to accommodate smaller shooters. In particular, the collapsible/telescoping stock which is common on most AR-15 pattern rifles (and specifically prohibited by Pen. Code § 30515(a)(C)) makes it an ideal rifle with which to instruct and train women, and for women to own and use for self-defense and other purposes. Additionally, I prefer to have other ergonomic features on my firearm like a pistol grip or forward vertical grip to assist in controlling the firearm and ensuring accuracy while shooting. Also, the ability to use standardized 30-round magazines and low recoil ammunition are some other reasons why I, as well as many of my students, prefer semiautomatic, centerfire firearms with

listed features, like the AR-15 rifle. In the firearms and training communities, this is a widely-held and accepted understanding. As an example, attached hereto as **Exhibit 1** is a recent article entitled, "Female Gun Owners: We Prefer the AR-15" published at the Washington Free Beacon on November 10, 2019. As female a firearms instructor, I agree with the sentiments expressed in this article.

9. For these reasons, it is therefore and further my desire to obtain and acquire additional semiautomatic, centerfire firearms, like AR-15 pattern firearms, that either have some or all of the features listed in Pen. Code § 30515(a)(1). Such firearms would also include AR-15 pistols, which contain many of the same features listed above, and additional features described by § 30515(a)(4)(A)-(D).

10. I also own a standard Sig Sauer P239 9mm semiautomatic pistol. I use this firearm when I teach firearms classes and shoot recreationally at the range. I also carry this pistol in public as it is one of the listed firearms on my concealed weapons permit. I wish to be able to replace the standard barrel in my pistol with a threaded barrel that would allow me to attach either a flash suppressor or a muzzle brake to my firearm. The muzzle brake would assist my accuracy and control while shooting in my firearm's classes and recreational shooting. I would use a flash suppressor when carrying my pistol at night to help ensure that I would not be blinded by the muzzle flash of the gun if I were to ever have to use it in self-defense. However, regardless of what attachments I attach to the barrel, merely installing a threaded barrel would make my pistol an assault weapon and subject me to severe criminal penalties.

11. Due to California's assault weapons ban, I am prohibited from acquiring and using common, everyday semiautomatic firearms with listed features. This prohibition prevents me from exercising my Second Amendment right to acquire, own, and possess, common firearms for various lawful purposes like self-defense. But for California's

assault weapons ban, I would re-configure my currently possessed firearms and would also acquire additional firearms that would otherwise be classified as "assault weapons."

12. Accordingly, and for the reasons set forth in Plaintiffs' motion, I respectfully ask that the Court grant Plaintiffs' motion for preliminary injunction.

I declare under penalty of perjury that the foregoing is true and correct. Executed on December 12, 2019.

Wendy Hauffen

EXHIBITS
TABLE OF CONTENTS

EXHIBIT "1"

Female Gun Owners: We Prefer the AR-15

Courtney Manwaring looks over an AR-15 / Getty Images

Stephen Gutowski - NOVEMBER 10, 2019 5:00 AM

In the aftermath of a recent Florida self-defense shooting, female gun owners argued that the AR-15 provides specific advantages to women for home defense, vehemently rejecting the views of gun-control activists who insist the firearm is unnecessary.

Speaking with the *Washington Free Beacon* on Friday, five female firearm owners and advocates said the AR-15 platform offers several features that are ideal for women specifically. Robyn M. Sandoval, executive director of A Girl & A Gun Women's Shooting League, said the rifle is both more effective and safer for female shooters.

"ARs are an excellent choice for women for home defense," Sandoval told the *Free Beacon*. "The platform is relatively lightweight and easy to hold and customize so that the firearm fits her body correctly. Having a rifle that is the right size for the shooter makes it more comfortable to shoot and therefore more accurate and safer."

Many Democratic politicians, including 2020 frontrunner Joe Biden, have long decried the AR-15 as both dangerous and an impractical or unnecessary firearm for civilians, especially women. But the female firearm owners the *Free Beacon* spoke to rejected the logic of these pro-gun-control men.

"AR-15s are perfect for women," Mary Chastain, a writer and gun owner, said. "Despite the size, they are lightweight and have hardly any kickback. This allows us to aim well and shoot the target where we want to."

Dana Loesch, a nationally syndicated radio host and gun-rights activist who has faced threats to her safety throughout her career, said she picks an AR-15 when it comes to home defense.

"I was always taught in training that your pistol is what you use to get to your rifle, and the AR-15 is what I choose to use," Loesch told the *Free Beacon*.

The customizability of the rifle is a big selling point for women, competitive shooter and trainer Julie Golob said.

"The AR platform can be a useful and effective option for women when it comes to defending themselves and their property," she told the *Free Beacon*. "Starting with the fact that the length of pull can be adjusted easily, unlike rifles with fixed stocks, the AR can quickly become custom fit to its user. The pistol grip, combined with quick access to the safety and other controls, makes this platform one a woman can confidently control."

"I can choose my trigger, hand guard, barrel length, grip," Dianna Muller, a former police officer and head of the gun-rights group DC Project, added. "I can put a light, laser, etc. I call it the Mr. Potato Head for the gun connoisseur!"

The testimony of these women contradicts Biden, who has repeatedly claimed that AR-15s are hard to use and ineffective compared with shotguns. In 2013, he said he had advised his own wife to use a double-barrel shotgun instead of an AR-15.

"I said, 'Jill, if there's ever a problem, just walk out on the balcony here, walk out and put that double-barrel shotgun and fire two blasts outside the house,'" Biden said in an interview with *Parents Magazine*. "You don't need an AR-15—it's harder to aim.

It's harder to use, and in fact you don't need 30 rounds to protect yourself. Buy a shotgun! Buy a shotgun!"

Late last month, a heavily pregnant mother did exactly what Biden warned against to defend her family. She used an AR-15 to fend off two armed men who were attacking her husband and daughter in their Florida home.

The women who spoke with the *Free Beacon* disagreed with Biden's assertions that AR-15s are not necessary. Loesch said she was competent with shotguns, but has found the AR-15 is simply a better option.

"The 12 gauge is an excellent home defense gun, too, but the collateral consideration does affect my decision there (frangible ammo is an option)," Loesch told the *Free Beacon*. "AR-15s are easy to shoulder, lightweight, the low recoil makes it easier to maintain target acquisition, and the ergonomics are great. I can access everything without compromising a defensive stance. I also have more rounds with an AR-15."

Chastain also said that she finds the AR-15 easier than many other firearms to use.

"You can use it with one hand, which helps me," she said. "My entire left side is handicapped, caused by brain trauma at birth. There are many guns I cannot use. The AR is perfect because I can use the functions with only my right hand. The lightness of the gun makes it easy for my handicapped left arm and hand to hold it."

The women said the availability of magazines with more ammunition capacity than the double-barrel shotguns Biden highlighted—which hold only two rounds—is a significant advantage of the AR platform, as is the variety of ammunition types.

"Standard capacity magazines create a reduced chance to have to fumble to exchange mags under stress," Golob said.

"The ballistics of defensive ammunition prevent over-penetration, and standard-capacity magazines hold 30 rounds, which is more than a shotgun or pistol," Sandoval said.

The women who spoke to the *Free Beacon* stressed that, while they believe the AR-15 provides them certain advantages over other guns, women are more than able to become skilled with shotguns, handguns, or any other firearm.

"There are pros and cons to any self-defense tool," Golob said. "Practice on the range and training gun-handling skills, whether it's a rifle, pistol, or shotgun, is key. I feel that the best home defense option for a woman is the one she is most comfortable with and that she can produce the best results."

Sandoval encouraged women to "train extensively on any firearm they choose to use to protect their families" but also noted AR-15 classes are one of the most commonly available—one of its primary advantages in her opinion.

Some of the women also view the imposing nature and reputation of the AR-15 as a bonus feature.

"I also like the fact that they're scary looking," Chastain said. "A man breaks into my house, I don't mind using a scary looking weapon to defend myself."

"Ultimately, I want the meanest, most manageable thing I can get," Loesch said.

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Attorneys for Plaintiffs

UNITED STATES DISTRICT COURT

FOR THE SOUTHERN DISTRICT OF CALIFORNIA

JAMES MILLER, et al.,

Plaintiffs,

vs.

XAVIER BECERRA, in his official capacity as Attorney General of California, et al.,

Defendants.

Case No. 3:19-cv-01537-BEN-JLB

Hon. Roger T. Benitez
Magistrate Hon. Jill L. Burkhardt

DECLARATION OF PLAINTIFF RYAN PETERSON IN SUPPORT OF PLAINTIFFS' MOTION FOR PRELIMINARY INJUNCTION

Complaint filed: August 15, 2019
Amended Complaint filed: September 27, 2019

Date: Thursday, January 16, 2020
Time: 10:00 a.m.
Courtroom: 5A, 5th floor

DECLARATION OF RYAN PETERSON

I, Ryan Peterson, declare as follows:

1. I am an adult resident of the County of San Diego, California, and am a named plaintiff in the above matter. I have personal knowledge of the facts stated herein, and if called as a witness, I could competently testify to these facts.

2. This declaration is executed in support of plaintiffs' motion for a preliminary injunction. I am a member of the organizational plaintiffs, San Diego County Gun Owners PAC, California Gun Rights Foundation, Second Amendment Foundation and Firearms Policy Coalition, Inc. I make this declaration on my own behalf, and as the owner-manager of Plaintiff Gunfighter Tactical.

3. I am not prohibited from owning firearms under federal or state law. For many years, in fact, I have held a license to carry a concealed weapon (CCW), issued by my local county sheriff, that requires a background check and good moral character. Under state law, this CCW must be renewed every two years.

4. I am the lawful owner of a semi-automatic pistol that has a "fixed magazine," that is, it contains an ammunition feeding device that cannot be removed from the firearm without disassembly of the firearm action." *See*, Pen. Code § 30515(b) and 11 CCR § 5471(m).

5. I am also in lawful possession of a so-called "large capacity magazine" (as that term is defined in Pen. Code § 16740) that is compatible with, and could be used in my semiautomatic fixed magazine pistol referenced above.

6. However, California Penal Code section 30515(a)(5) prevents me from inserting or using my large capacity magazine in my fixed magazine pistol. Specifically, under Penal Code 30515(a)(5), an "assault weapon" is also defined as "[a] semiautomatic pistol with a fixed magazine with the capacity to accept more than 10

rounds." However, it is my understanding that this Court has already found that the state's prohibition on large-capacity magazines was unconstitutional, and enjoined enforcement of those provisions of the Penal Code that would have prohibited their possession. It is my understanding that along with the right to possess large-capacity magazines, for the reasons expressed in this Court's judgment, I would also have the right to *use* such magazines in an otherwise legally-owned firearm.

7. I wish to continue to lawfully possess my firearm and use my firearm with my lawfully possessed large capacity magazine inserted therein, without being subject to arrest and/or prosecution for possessing or transporting an "assault weapon."

8. It is also my desire to obtain and acquire additional semiautomatic, centerfire firearms, including AR-15 pattern firearms, that either have some or all of the features listed in Pen. Code § 30515(a)(1) and which do not have a fixed magazine, and/or to obtain and acquire a semiautomatic, centerfire rifle that has a fixed magazine with the capacity to accept more than 10 rounds.

9. I am also the owner/manager of Plaintiff Gunfighter Tactical, a Federal Firearms Licensee ("FFL") and firearms dealer in the California Department of Justice's Centralized List of Firearms Dealers. As a licensed firearms retailer, I would like to have the business purchase, sell, and transfer firearms in common use for lawful purposes—and which are commonly sold for lawful purposes in other parts of the country—hat contain some or all of the features described by Penal Code § 30515(a) to ordinary, non-prohibited adults through my FFL dealership. I would engage in this business but for the State's laws, and Defendants' policies, practices, customs, and enforcement of same which prevent me from doing so.

10. On information and belief, I cannot acquire a permit under Penal Code § 31005 in order to sell such firearms to ordinary, law-abiding people who are not otherwise

exempt (such as law enforcement officers, government agencies, military agencies, or governments) due to the State's laws and Defendants' policies, practices, and customs. Otherwise, I would, through my licensed dealership, acquire and sell firearms described under Penal Code § 30515 to ordinary, non-prohibited citizens. However, I am prevented from doing so due to Defendants' enforcement of the State's laws and Defendants' policies and practices which place me in fear of prosecution and loss of license and livelihood if I were to take part in such action.

11. Thus, California's assault weapon ban has violated my Second Amendment rights, both as an individual, and as the owner/operator of federally licensed firearms dealer, as I am prohibited from acquiring and using common semiautomatic, centerfire firearms with listed features for personal use and I am prohibited from acquiring and selling these firearms to ordinary lawful citizens in California as a part of my business.

12. Accordingly, and for the reasons set forth in Plaintiffs' motion, I respectfully ask that the Court grant Plaintiffs' motion for preliminary injunction.

I declare under penalty of perjury that the foregoing is true and correct. Executed on December 12, 2019.

Ryan Peterson

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Attorneys for Plaintiffs

UNITED STATES DISTRICT COURT

FOR THE SOUTHERN DISTRICT OF CALIFORNIA

JAMES MILLER, et al.,

Plaintiffs,

vs.

XAVIER BECERRA, in his official capacity as Attorney General of California, et al.,

Defendants.

Case No. 3:19-cv-01537-BEN-JLB
Hon. Roger T. Benitez
Magistrate Hon. Jill L. Burkhardt

DECLARATION OF PLAINTIFF JOHN PHILLIPS IN SUPPORT OF PLAINTIFFS' MOTION FOR PRELIMINARY INJUNCTION

Complaint filed: August 15, 2019
Amended Complaint filed: September 27, 2019

Date: Thursday, January 16, 2020
Time: 10:00 a.m.
Department: Courtroom 5A (5th floor)

DECLARATION OF JOHN PHILLIPS

I, John Phillips, declare as follows:

1. I am an adult resident of the County of San Diego, California, and am a named plaintiff in the above matter. I have personal knowledge of the facts stated herein, and if called as a witness, I could competently testify to these facts.

2. This declaration is executed in support of plaintiffs' motion for a preliminary injunction.

3. I am a Federal Firearms Licensee ("FFL") and firearms dealer in the Department of Justice's Centralized List of Firearms Dealers. I am the president of plaintiff PWGG, L.P., doing business as "Poway Weapons & Gear" and "PWG Range," which is both a licensed firearms retailer and a shooting range in Poway, California. I am also a member of the organizational plaintiffs, San Diego County Gun Owners PAC, California Gun Rights Foundation, Second Amendment Foundation and Firearms Policy Coalition, Inc. I make this declaration on my own behalf, and as President of PWGG, L.P.

4. As a licensed firearms retailer, I would like to have the business purchase, sell, and transfer firearms in common use for lawful purposes—commonly sold for lawful purposes in other parts of the country, which are defined as "assault weapons" and contain some or all of the features described by Penal Code § 30515(a)—to ordinary, non-prohibited adults through my FFL dealership, and would engage in this business but for the State's laws, and Defendants' policies, practices, customs, and enforcement of same.

5. PWG holds a "Dangerous Weapons License/Permit" issued and maintained by defendants through the California Department of Justice, Bureau of Firearms. This permit allows my dealership to acquire and sell so-called "assault weapons" to select

exempted recipients, such as law enforcement officers. However, the permit does not allow us to sell, transfer or rent so-called "assault weapons" to non-exempt agencies or individuals, ordinary, non-prohibited citizens such as the other individual plaintiffs named in the lawsuit, to the institutional and organizational plaintiffs or their law-abiding customers/members. In addition to potential criminal violations, violating these laws subjects our permit to revocation per 11 CCR § 4147.

6. I would, through my licensed dealership, acquire and sell the firearms described under Penal Code § 30515(a) to ordinary, non-exempt citizens such as, but not limited to, the other individual plaintiffs named in the lawsuit, but for the State's laws, and I would otherwise fear the loss of licenses, arrest, prosecution and loss of property and liberty under defendants' policies, practices and customs. In addition to violating my customers' Second Amendment rights, the State's laws and defendants thus further infringe upon my economic liberties, causing me financial damages because I cannot acquire and transfer to lawful persons such arms that are protected by the Second Amendment.

7. Accordingly, and for the reasons set forth in Plaintiffs' motion, I respectfully ask that the Court grant Plaintiffs' motion for preliminary injunction.

I declare under penalty of perjury that the foregoing is true and correct. Executed on December 12, 2019.

John Phillips

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UNITED STATES DISTRICT COURT

FOR THE SOUTHERN DISTRICT OF CALIFORNIA

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Plaintiffs,

vs.

XAVIER BECERRA, in his official capacity as Attorney General of California, et al.,

Defendants.

Case No. 3:19-cv-01537-BEN-JLB

Hon. Roger T. Benitez
Magistrate Hon. Jill L. Burkhardt

DECLARATION OF PLAINTIFF NEIL RUTHERFORD IN SUPPORT OF PLAINTIFFS' MOTION FOR PRELIMINARY INJUNCTION

Complaint filed: August 15, 2019
Amended Complaint filed: September 27, 2019

Date: Thursday, January 16, 2020
Time: 10:00 a.m.
Courtroom: 5A, 5th floor

DECLARATION OF NEIL RUTHERFORD

I, Neil Rutherford, declare as follows:

1. I am an adult resident of the County of San Diego, California, and am a named plaintiff in the above matter. I have personal knowledge of the facts stated herein, and if called as a witness, I could competently testify to these facts.

2. This declaration is executed in support of plaintiffs' motion for a preliminary injunction.

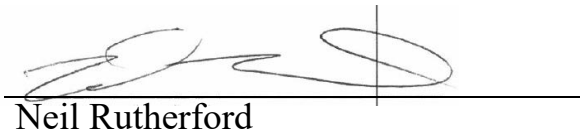
3. I am not prohibited from owning firearms under federal or state law.

4. I desire to obtain and acquire AR-15 pattern firearms that are commonly sold in many other jurisdictions in the country. Most of these off-the-shelf AR-15 rifles have some or all of the features listed in Pen. Code § 30515(a)(1) and have the capacity to accept detachable magazines. These commonly-sold firearms include centerfire rifles that are between 26-30 inches in overall length.

5. I also desire to obtain and acquire: AR pistols, which contain some or all of the features described in Pen. Code § 30515(a)(4)(A)-(D); and semi-automatic shotguns which contain some or all of the features described in Pen. Code § 30515(a)(6) and (a)(7), but am prevented by these definitions which characterize such firearms as "assault weapons" under California law.

6. Accordingly, and for the reasons set forth in Plaintiffs' motion, I respectfully ask that the Court grant Plaintiffs' motion for preliminary injunction.

I declare under penalty of perjury that the foregoing is true and correct. Executed on December 12, 2019.



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Attorneys for Plaintiffs

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FOR THE SOUTHERN DISTRICT OF CALIFORNIA

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XAVIER BECERRA, in his official capacity as Attorney General of California, et al.,

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Case No. 3:19-cv-01537-BEN-JLB

Hon. Roger T. Benitez
Magistrate Hon. Jill L. Burkhardt

DECLARATION OF ADRIAN SEVILLA IN SUPPORT OF PLAINTIFFS' MOTION FOR PRELIMINARY INJUNCTION

Complaint filed: August 15, 2019
Amended Complaint filed: September 27, 2019

Date: Thursday, January 16, 2020
Time: 10:00 a.m.
Courtroom: 5A, 5th floor

DECLARATION OF ADRIAN SEVILLA

I, Adrian Sevilla, declare as follows:

1. I am an adult resident of the County of San Diego, California, and am a named plaintiff in the above matter. I have personal knowledge of the facts stated herein, and if called as a witness, I could competently testify to these facts.

2. This declaration is executed in support of plaintiffs' motion for a preliminary injunction.

3. I am not prohibited from owning firearms under federal or state law.

4. I desire to obtain and acquire AR-15 pattern firearms that are commonly sold in many other jurisdictions in the country. Most of these off-the-shelf AR-15 rifles have some or all of the features listed in Pen. Code § 30515(a)(1) and have the capacity to accept detachable magazines. These commonly-sold firearms include centerfire rifles that are between 26-30 inches in overall length.

5. I also desire to obtain and acquire: AR pistols, which contain some or all of the features described in Pen. Code § 30515(a)(4)(A)-(D); and semi-automatic shotguns which contain some or all of the features described in Pen. Code § 30515(a)(6) and (a)(7), but am prevented by these definitions which characterize such firearms as "assault weapons" under California law.

6. Accordingly, and for the reasons set forth in Plaintiffs' motion, I respectfully ask that the Court grant Plaintiffs' motion for preliminary injunction.

I declare under penalty of perjury that the foregoing is true and correct. Executed on December 12, 2019.

Adrian Sevilla

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UNITED STATES DISTRICT COURT

SOUTHERN DISTRICT OF CALIFORNIA

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XAVIER BECERRA, in his official capacity as Attorney General of California, et al.,

Defendants.

Case No. 3:19-cv-01537-BEN-JLB

Hon. Roger T. Benitez
Magistrate Hon. Jill L. Burkhardt

DECLARATION OF JOHN LOTT IN SUPPORT OF PLAINTIFFS' MOTION FOR PRELIMINARY INJUNCTION

Complaint filed: August 15, 2019
Amended Complaint filed: September 27, 2019

Date: Thursday, January 16, 2020
Time: 10:00 a.m.
Courtroom: 5A, 5th floor

DECLARATION OF JOHN LOTT, JR.

I, John R. Lott, Jr., declare as follows:

I am not a party to the captioned action, am over the age of 18, have personal knowledge of the facts stated herein, and am competent to testify as to the matters stated and the opinions rendered below.

Background/Qualifications

1. I reside in Las Vegas, Nevada, and am an economist. I graduated with a bachelor's degree in economics from the University of California Los Angeles (UCLA) in 1980. I obtained my master's degree in economics from UCLA in 1982; and my PhD in economics from UCLA in 1984. I have held research and/or teaching positions at various higher education academic institutions, including the University of Chicago, Yale University, the Wharton School of the University of Pennsylvania, Stanford University, and Rice University; and was the chief economist at the United States Sentencing Commission during 1988-1989.

2. I have authored numerous academic and popular publications. For example, I have authored (a) nine books, including *More Guns, Less Crime*, *The Bias Against Guns*, and *Freedomnomics;* and (b) more than 100 articles in peer-reviewed academic journals.

3. I am also the founder and president of the Crime Prevention Research Center (CPRC). CPRC is a research and education organization dedicated to conducting academic quality research on the relationship between laws regulating the ownership or use of guns, crime, and public safety; educating the public on the results of such research; and supporting other organizations, projects, and initiatives that are organized and operated for similar purposes. CPRC has section 501(c)(3) status and does not accept donations from gun or ammunition makers or organizations such as the National

Rifle Association (NRA) or any other organizations involved in the gun control debate on either side of the issue.

4. CPRC's goal is to provide an objective and accurate scientific evaluation of both the costs and benefits of gun ownership as well as policing activities. CPRC's core activities include:

(a) Conducting and publishing academic quality research on the relationship between laws regulating the ownership or use of guns, crime, and public safety.

(b) Supporting affiliated academics in conducting and publishing similar research by means such as providing direct financial support, sharing data, and providing technical assistance.

(c) Educating the public, journalists, and policy makers on the results of research on these issues through books, public lectures, newspaper columns, academic seminars, information briefings, and other means.

(d) Making research and data available to researchers, the public, policy makers, and journalists by maintaining a comprehensive website.

(e) Engaging in other related activities consistent with the mission and goals of CPRC.

5. Attached hereto as **Exhibit 1** is a true and correct copy of my Curriculum Vitae. It describes my education, awards, fellowships, work experience, research, books and publications, presentations, and legislative and court testimony.

6. Based on my education, work experience, research background, publications, and review of the research of others, in my opinion, there is no credible evidence that so-called "assault weapons" bans have any meaningful effect of reducing gun homicides and no discernable crime-reduction impact.

7. I have conducted an extensive literature review and have summarized the results of my research of pertinent publications. I would consider other pertinent publications if presented with them.

8. The impact of state assault weapon bans is difficult to determine because very few states have implemented such restrictions and all restrictions are relatively new. However, based on my and others' research on assault weapon bans and their effects, I have no reason to believe that implementing state assault weapon bans has any more effect than the 1994 Federal Assault Weapons Ban. All credible research shows that the 1994 Federal Assault Weapons Ban had no discernable effect on gun violence or crime in general. This is why the federal ban was permitted to sunset due to its lack of effect.

9. More generally, based on my research, as well as my review of the research of others, gun bans have little to no effect on violent crime or the ability for criminal to obtain firearms illegally.

Criminals Buy Guns from Other Sources

10. First, the criminals do not buy their firearms legally. In determining where criminals obtain their firearms, the U.S. Bureau of Justice Statistics primarily relies on surveys of state and federal prisoners who possessed a firearm during the offense for which they are serving time. The surveys provide remarkably consistent results over time, with *very few* guns obtained through retail sources (i.e., a gun shop, pawn gun, flea market, or gun show). The latest survey in 2016 (published in 2019) showed that among the prisoners who had a gun during their offense, approximately 90% did not obtain it from retail sources, with just 0.4% from flea markets, 0.8% through gun

shows, and slightly more, 1.6%, from pawn shops and 7.5% from gun shops/stores.[1] Attached hereto as **Exhibit 2** is a true and correct copy of Alper, et al. (2019).

11. Interestingly, among the prisoners that obtained a firearm during their offense, more than half (56%), had either stolen it (6%), found it at the scene of a crime (7%), or obtained it off the street or from the underground market (43%). The remainder includes 1.6% obtained in theft from a family member or a friend, 1.5% from burglaries, 0.2% in theft from retail sources, and 3% in other unspecified thefts. (See **Exhibit 2**, Alper, et al. 2019 [Table 5].) Considering the number of firearms widely available throughout the U.S. that would fall under the various definitions of what constitutes an "assault weapon," it is unlikely a state ban would have any beneficial effect.

12. Also, since the California assault weapon ban is largely based on the features that are attached to the firearm, nothing prevents an individual from purchasing a complaint rifle, purchasing one or more of these banned features, and then unlawfully converting the firearm into an illegal configuration. To my knowledge, this is precisely what was done in 2014 in the San Bernardino terrorist attack. See attached hereto as **Exhibit 3** is a true and correct copy of the digital article from The Wall Street Journal, "Rifles Used In San Bernardino Shooting Illegal Under State Law."

[1] See Mariel Alper and Lauren Glaze, Special Report, "Source and Use of Firearms Involved in Crimes: Survey of Prison Inmates, 2016," U.S. Department of Justice, Office of Justice Programs, Jan. 2019 [Alper, et al. 2016]; and for numbers in 2001, see Caroline Wolf Harlow, Special Report, "Firearm Use by Offenders," U.S. Department of Justice, Office of Justice Programs, November 2001 [https://www.bjs.gov/content/pub/pdf/fuo.pdf], last accessed Aug. 2019.)

Firearm Bans Have Little Effect on Preventing Criminals From Obtaining Guns

13. Even if all sources for obtaining semiautomatic firearms with various features such as a pistol grip, telescoping stock, and/or high capacity magazines were closed off within the state of California, it is unlikely that such laws would stop the vast majority of criminals from acquiring these guns. Take Mexico where there has been only one-gun store in the country since 1972; and where only about 1% of Mexican adults have licenses to legally own guns, with the most powerful legally owned firearms are .22-caliber rifles, hardly the type of weapon used by criminals. Despite that, in 2019, Mexico has a murder rate that is more than five (5) times the U.S. rate.[2] The point is simple — criminals have guns and they get them illegally, primarily from drug dealers; and it is just as difficult to stop criminals from obtaining guns as it is to stop drug dealers from obtaining illegal drugs. It is also becoming increasingly common for criminal to make their own guns, so-called ghost guns. Arbitrary bans of firearm features will do little to stop this. Attached hereto as **Exhibit 4** are a true and correct copies of the article from the Associated Press (2019) and the FBI's crime report (2017) [see footnote 2, below].).

14. In fact, based on my research, every place that has banned guns (either all guns or all handguns) has seen murder rates go up. Examples include Chicago, Illinois, Washington D.C., and island nations such as England, Jamaica, Ireland, Venezuela, and obscure places like the Solomon Islands. The original research is available in Lott, *More Guns, Less Crime*, University of Chicago Press, 2010, Third edition. Support for

[2] Associated Press, "Mexico sets 1st half murder record, up 5.3%," July 22, 2019 (https://www.apnews.com/c197a3ee34834ea69f745975fa632ea2). Compare these numbers to the FBI Uniform Crime Report for 2017 (https://ucr.fbi.gov/crime-in-the-u.s/2017/crime-in-the-u.s.-2017/topic-pages/tables/table-1).

my opinion is found at https://crimeresearch.org/2016/04/murder-and-homicide-rates-before-and-after-gun-bans/.

15. According to the 2018 FBI Crime in the United States, murders involving all types of rifles and shotguns totaled 541 of 10,265 in the United States. See FBI Criminal Justice Information Services Division, "2018 Crime in the United States," Table 20 (referenced in paragraph 16 below). Assault weapon bans largely prohibit various rifles and shotguns (although certain handguns can be included in the definition of assault weapon). Thus, a country-wide ban may only have a chance of effecting less than .0527% of firearm murders. This percentage is even lower when you consider California's assault weapon law would only apply within the state of California. In California, murders involving all types of rifles and shotguns totaled 51 of 1,117 murders committed with firearms. *Id.* In other words, rifle and shotgun murders accounted for .0433% of murders involving firearms in California. *Id.*

16. By comparison, there were 87 murders with "hands, fists, feet etc." and 252 murders with "knives or cutting instruments in California. *Id.* Moreover, there were 615 total murder by "hands, fists, feet, etc" across the United States – 74 more than murders with rifles and shotguns combined. No significant or even discernable decrease in crime or gun violence could be attributable to a firearm ban that only effects such an insignificant percentage of the actual gun crime committed.

17. Attached hereto as **Exhibit 5** is a true and correct copy of the FBI Criminal Justice Information Services Division, "2018 Crime in the United States," Table 20 [Murder by State, Types of Weapons, 2018].

All Credible Studies Conclude That Assault Weapon Bans Have No Meaningful Effect on Gun Crime

18. In addition to my own peer-reviewed research, I have reviewed the pertinent studies and surveys regarding the effects of assault weapons bans on gun violence. Below is a list of the various research that has been conducted. With each publication, I have identified the title and authors, summarized the key findings of the article, and then presented an analysis of those conclusions, along with my professional opinion on the methods used in each publication.

19. **Christopher S. Koper, Daniel Woods, and Jeffrey A. Roth, "Impact Evaluation of the Public Safety and Recreational Firearms Use Protection Act of 1994," National Institute of Justice, United States Department of Justice, March 1997.** This paper was later republished in the Journal of Quantitative Criminology, September 2002. (Christopher S. Koper, Daniel Woods, and Jeffrey A. Roth, "Updated Assessment of the Federal Assault Weapons Ban: Impacts on Gun Markets and Gun Violence, 1994-2003," National Institute of Justice, United States Department of Justice, June 2004).

20. These two papers use panel data for the United States to study the impact of the Federal Assault Weapon Ban over two different periods of time to study homicide rates and mass shootings. Criminology professors Chris Koper and Jeff Roth concluded, in a 1997 report for the National Institute of Justice, that the gun homicide rates were reduced since the ban, however, "[t]he evidence is not strong enough for us to conclude that there was any meaningful effect (i.e., that the effect was different from zero)." It also found "no statistical evidence of post-ban decreases in either the number of victims per gun homicide incident, the number of gunshot wounds per victim, or the proportion of gunshot victims with multiple wounds." **Ex. 6** at 172.

Messrs. Koper and Roth suggested that it might be possible to find a benefit after the ban had been in effect for more years.

21. In 2004, they published the follow-up NIJ study with fellow criminologist Dan Woods. In this study, they found: "[w]e cannot clearly credit the ban with any of the nation's recent drop in gun violence" and "indeed, there has been no discernible reduction in the lethality and injuriousness of gun violence, based on indicators like the percentage of gun crimes resulting in death or the share of gunfire incidents resulting in injury[.]" The study concluded that should the federal ban be reauthorized (and it was not), "the evidence on these matters is too limited (both in volume and quality) to make firm projections of the [federal] ban's impact[.]" **Ex. 7** at 380-381.

22. Attached hereto as **Exhibit 6** is a true and correct copy of Christopher S. Koper, Daniel Woods, and Jeffrey A. Roth, "Impact Evaluation of the Public Safety and Recreational Firearms Use Protection Act of 1994," National Institute of Justice, United States Department of Justice, March 1997.

23. Attached hereto as **Exhibit 7** is a true and correct copies of (Christopher S. Koper, Daniel Woods, and Jeffrey A. Roth, "Updated Assessment of the Federal Assault Weapons Ban: Impacts on Gun Markets and Gun Violence, 1994-2003," National Institute of Justice, United States Department of Justice, June 2004).

24. **Kleck, "Large-Capacity Magazines and the Casualty Counts in Mass Shootings: The Plausibility of Linkages," Justice Research and Policy, Vol 17, 2016: 28-47.** This study looks at whether large-capacity magazines that are used in mass public shootings allow the murderers to fire their guns at faster rates than they could have otherwise fired their guns. But the report finds: "the data indicate that mass shooters maintain such slow rates of fire that the time needed to reload would not

increase the time between shots and thus the time available for prospective victims to escape." **Ex. 8** at 393.

25. Attached hereto as **Exhibit 8** is a true and correct copy of Kleck, "Large-Capacity Magazines and the Casualty Counts in Mass Shootings: The Plausibility of Linkages," Justice Research and Policy, Vol 17, 2016: 28-47.

26. **Gius, "An examination of the effects of concealed weapons laws and assault weapons bans on state-level murder rates," Applied Economics Letters, 2014.** This study follows different states over time from 1980 to 2009 to see whether they are covered by either the federal or state assault weapon bans through the use of panel data. The study uses variables that pick up the pre-existing average differences for different states and years. The study finds no impact from assault weapon bans.

27. This study only accounts for one other type of gun control. Problems with the study include that it does not account for any types of law enforcement. Only one specification is provided so no evidence is provided on the sensitivity of the estimate. No explanation is offered for the years studied.

28. Attached hereto as **Exhibit 9** is a true and correct copy of Gius, "An examination of the effects of concealed weapons laws and assault weapons bans on state-level murder rates," Applied Economics Letters, 2014.

29. **Lott, More Guns, Less Crime, University of Chicago Press, 2010, third edition**. This study follows different states over time from 1977 to 2005 to see whether they are covered by either the federal or state assault weapon bans through the use of panel data. The study finds no impact from assault weapon bans. The study uses variables that pick up the pre-existing average differences for different states and years. The study accounts for twelve other types of gun control and hundreds of other factors

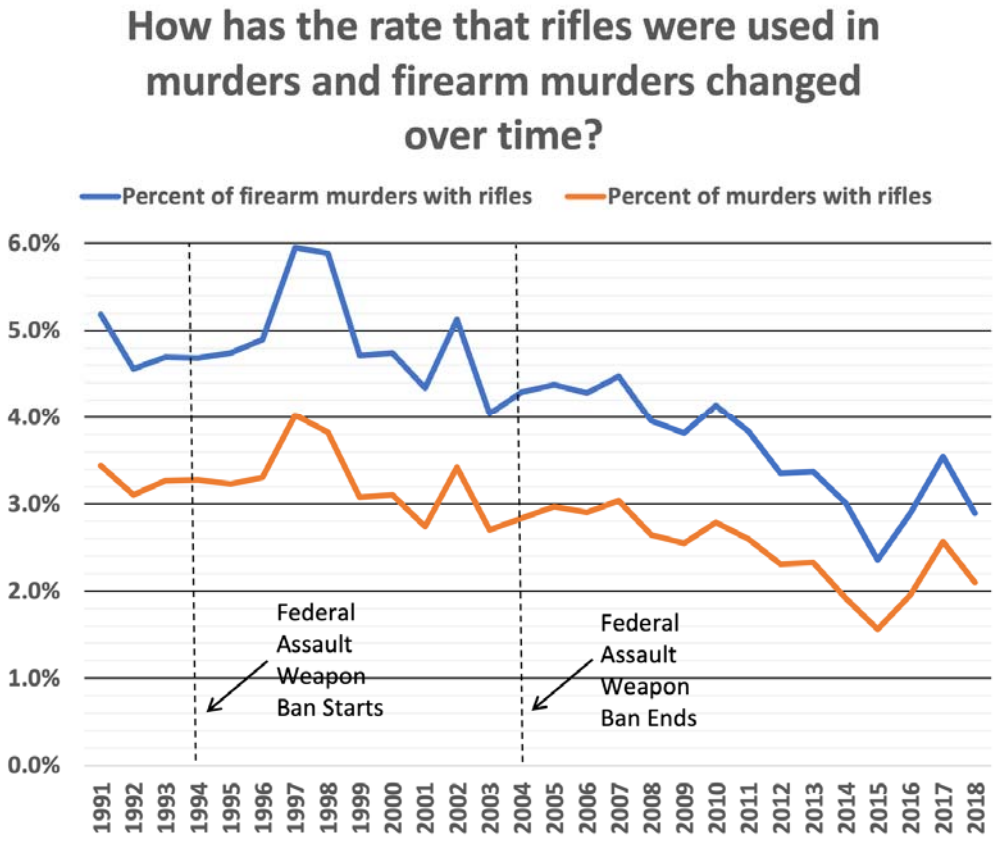
including detailed demographics, arrest rates, percent of the population in prison, poverty rates, and income.

30. Attached hereto as **Exhibit 10** are true and correct copies of relevant excerpts from Lott, More Guns, Less Crime, University of Chicago Press, 2010, third edition.

31. **Koper, Johnson, Nichols, Ayers, and Mullins, "Criminal Use of Assault Weapons and High-Capacity Semiautomatic Firearms: an Updated Examination of Local and National Sources," Journal of Urban Health (2018).** The purpose of this study was to investigate "current levels of criminal activity with assault weapons and other high-capacity semiautomatics in the USA…." This study provides no evidence that murders or mass public shootings were reduced by the assault weapon ban. Specifically, it provides city level data for ten cities showing that the percentage of guns recovered by the police from criminals that are assault weapons, and their numbers range from 2.4% in Baltimore to 8.5% in Syracuse. **Ex. 11**, at 879. They also provide some national data from the ATF that 5% of all guns recovered by police and reported to the ATF for tracing are assault weapons. Notably, the study states that "[p]atterns and trends in these particular cities may not be indicative of those elsewhere; further, some… are covered by state AW [assault weapon] and LCM [large capacity magazines] restrictions that were in effect during all or portions of the study period (this study does not attempt to evaluate the implementation and effects of these laws or variations therein." *Id*. at 877. The study also provides additional caveats throughout regarding its data collection. With regard to the study's data on firearms recovered by law enforcement agencies throughout the nation reported by the ATF, the study states that the information "do[es] not constitute a statistically representative sample for the nation given that gun tracing is voluntary… and varies between agencies and over time." *Id*. at 878. In other words, since police don't trace all guns, there is the concern of

self-selection. Also, ten cities provide only the roughest picture of what is happening nationally and cannot be relied on as credible evidence regarding this study's conclusions.

32. However, if you look at murders, the FBI Uniform Crime Reports show that only 2.9% of firearm murders and 2.1% of all murders are committed with any type of rifle. Not only is this small, but the share fell after the federal assault weapon ban fell ended. The percentage of firearm murders with rifles was 4.8% prior to the ban starting in September 1994, 4.9% from 1995 to 2004 when the ban was in effect, and just 3.6% after that (3.9% if you look at just the first ten years after the ban ended). There are similar drops over time if one looks at the share of total murders committed with rifles.



33. Attached hereto as **Exhibit 11** is a true and correct copy of Koper, Johnson, Nichols, Ayers, and Mullins, "Criminal Use of Assault Weapons and High-Capacity

Semiautomatic Firearms: An Updated Examination of Local and National Sources," Journal of Urban Health (2018).

34. **DiMaggio, Avraham, Berry, Bukur, Feldman, Klein, Shah, Tandon, and Frangos, "Changes in US mass shooting deaths associated with the 1994-2004 federal assault weapons ban: Analysis of open-source data," Trauma Acute Care Surgery 2018.** This study tested the hypothesis that the Federal Assault Weapons Ban was associated with a decrease in mass shooting fatalities in the United States. Unlike the other studies described above that use panel data, this study used purely time series United States data from 1981 to 2017. The difficulty with purely time-series evidence is that you only have two experiments: (i) the implementation of the Federal Assault Weapons Ban in 1994, and (ii) the elimination of the ban in 2004. Having only two experiments makes it difficult to account for any other factors, and this study accounts for no other factors. Although the study states that "mass-shooting related homicides in the United States were reduced during the years of the federal assault weapons ban of 1994 to 2004," it also acknowledges that it made no attempt to differentiate states with and without their own assault weapon bans. **Ex. 12**, at 887.

35. To the extent that mass public shootings are occurring in states that already have their own assault weapon bans in either the pre- or post-federal ban periods, their estimates will be misleading. There is another significant problem with these estimates. They make no attempt to see whether the *rate of shootings* with assault weapons have changed over these periods. These estimates lump together attacks with and without assault weapons. If their claims are correct, we should see a drop in the percent of attacks *with assault weapons* during the federal ban period and then an increase in the post-ban period, but as the next section below on Klarevas's work shows, that is not the case.

36. Importantly, the study states that its analysis does not "indicate that an assault weapon ban will result in fewer overall firearm-related homicides," but then claims these results "support a re-institution of the 1994 federal assault weapons ban as a way to prevent and control mass shooting fatalities in the United States." **Ex. 12**, at 897. Considering the methods used, it is difficult to come to the same conclusion.

37. Attached hereto as **Exhibit 12** is a true and correct copy of DiMaggio, Avraham, Berry, Bukur, Feldman, Klein, Shah, Tandon, and Frangos, "Changes in US mass shooting deaths associated with the 1994–2004 federal assault weapons ban: Analysis of open-source data," Trauma Acute Care Surgery 2018

38. **Gius, "The impact of state and federal assault weapons bans on public mass shootings," Applied Economics Letters, 2015: 281-284.** The purpose of this study was to determine the effects of federal and state assault weapons bans on public mass shootings. Unlike the DiMaggio et al., study above, this paper provides a panel level analysis. The author claims to find "fatalities due to mass shootings were lower during both the federal and state assault weapons ban periods." **Ex. 13**, at 910. However, like the DiMaggio paper, this conclusion is misleading. If Gius' claim is correct, we should see a drop in the percent of attacks *with assault weapons* during the federal ban period and then an increase in the post-ban period, but the Gius paper doesn't examine this. Thus, Guis' conclusions are flawed as they do not take into account the proper considerations. Moreover, as the next section shows, that is not the case.

39. There are a couple other important points to make. As in Gius' 2014 paper, no attempt is made to try to account for any impact from law enforcement or other gun control measures. The only gun control law accounted for is the assault weapon ban.

Thus, there is no way to differentiate between the assault weapons ban and other gun control measures that may have been in place during the period studied.

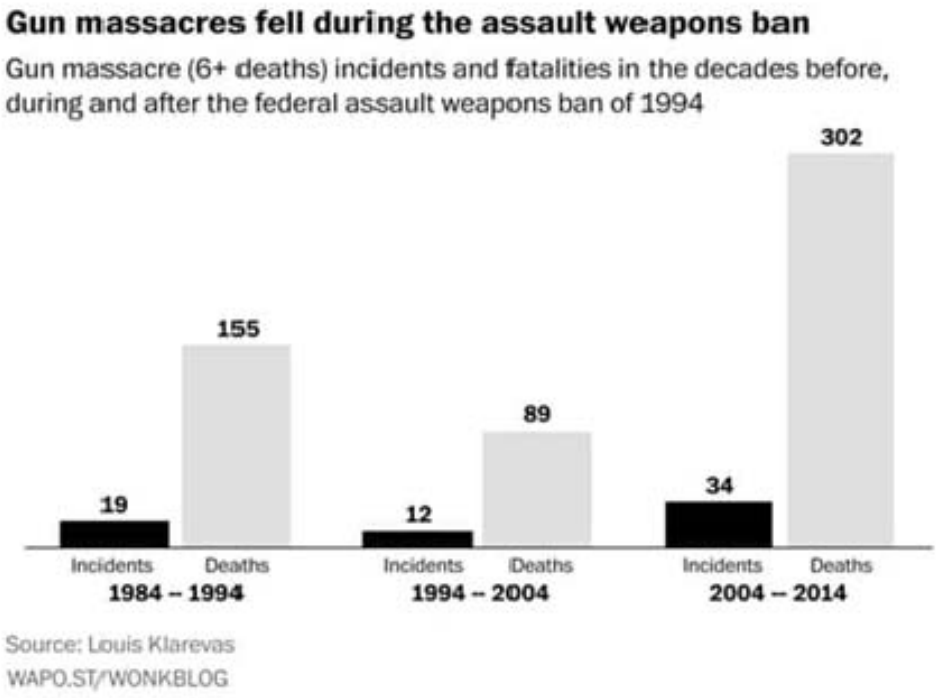
40. In addition, the Guis study used Mother Jones data as a basis for its analysis. The Mother Jones data is problematic. As criminologist James Alan Fox noted: "Not only is Mother Jones' decision to disqualify cases based on certain criteria hard to defend, the criteria themselves were not necessarily applied consistently. . . . Notwithstanding the questionable motive-based selectivity built into the Mother Jones analysis, it seems odd to ignore shootings with large death tolls just because there was more than one shooter or because the shooter was related to his or her victims. These incidents are no less devastating to the families and communities impacted by the crimes." James Alan Fox, "Mass shootings not trending," The Boston Globe, January 23, 2013 (http://archive.boston.com/community/blogs/crime_punishment/2013/01/mass_shootings_not_trending.html). Thus, the Mother Jones data was arbitrarily selective in its data collection.

41. Finally, the study acknowledges that "many of the perpetrators in these mass shootings used multiple types of firearms. Contrary to popular belief, however, assault rifles were not the predominant type of weapon used in these types of crimes. In fact, according to a recent study, handguns were to most commonly used type of firearm in mass shootings (32.99% of mass shootings); rifles were used in only 8.25% of mass shootings (Huff-Corzine et al., 2014)." **Ex. 13**, at 908. Thus, a conclusion that assault weapon bans (largely focused on rifles) have any significant effect on reducing mass shooting fatalities is problematic.

42. Attached hereto as **Exhibit 13** is a true and correct copy of Gius, "The impact of state and federal assault weapons bans on public mass shootings," Applied Economics Letters, 2015: 281-284.

43. ***Rampage Nation* by Louis Klarevas.** The non-refereed book titled, *Rampage Nation* by Louis Klarevas has been cited most frequently by gun control advocates and politicians, including Senator Feinstein. Jon Stokes, "The assault weapons ban didn't work. A new version won't, either," Los Angeles Times, March 1, 2018 (https://www.latimes.com/opinion/op-ed/la-oe-stokes-assault-weapon-ban-20180301-story.html). A very similar claim has been made in the New York Times by two other academics, John Donohue at Stanford Law School and Theodora Boulouta a student at Stanford. John Donohue and Theodora Boulouta, "That Assault Weapon Ban? It Really Did Work," New York Times, September 4, 2019 (https://www.nytimes.com/2019/09/04/opinion/assault-weapon-ban.html). In forming his analysis and conclusions, Klarevas limits his research to shootings with 6 or more fatalities. I don't know of any other study that does this, and Klarevas doesn't provide an explanation. Nor does he explain why he lumps in public shootings with gang shootings, failing to draw any distinction. These factors single out Klarevas' analysis as no other studies use these limitations or fail to make such distinctions. In Klarevas' book, he merely uses the alternative phraseology of "high-fatality" mass shootings. These issues call into question Klarevas' conclusions.

44. To further address the various problems with Klarevas' analysis and conclusions, I have provided a Washington Post graph that makes use of Klarevas' numbers below. Senator Dianne Feinstein (D-CA) showed President Trump this diagram when she met with him shortly after the Parkland school shooting in Florida in February 2018. Brian Doherty, "Dianne Feinstein Touts Research Claiming the Assault Weapon Ban Reduced Mass Shootings," Reason, March 2, 2018 (https://reason.com/2018/03/02/research-relied-on-to-defend-a-new-assau/).



45. Few academics would make the types of comparisons that Klarevas makes. They would instead observe how death rates changed in states where the federal ban actually affected the ability of citizens to own assault weapons. Then they would compare these states with other states where the law effectively remained unchanged because state-level bans were already in place. Klarevas did not do this. However, Koper and Roth did it in their studies, and as I have done in my own research. These studies did not find any impact from assault weapon bans.

46. Over time, the rate of mass shootings or mass public shootings may rise or fall for many reasons. But regardless of any other factors, if assault weapons bans reduced these attacks, the share of attacks committed with "assault weapons" should have decreased. For the sake of argument, let's follow Klarevas in looking at the total number of attacks before, during, and after the assault weapons ban. We will use the cases that Klarevas identifies in his book as mass shootings (pp. 72 and 73), as well as mass public shootings collected by Mother Jones and the CPRC. Klarevas doesn't provide any breakdown of shootings committed with assault weapons, even though the

1994 assault weapons ban is the subject of his research. In other words, as assault weapon bans would not affect firearms that are *not* defined as "assault weapons," any analysis on the effect of assault weapon bans needs to take into account mass shootings that did *not* involve an assault weapon.

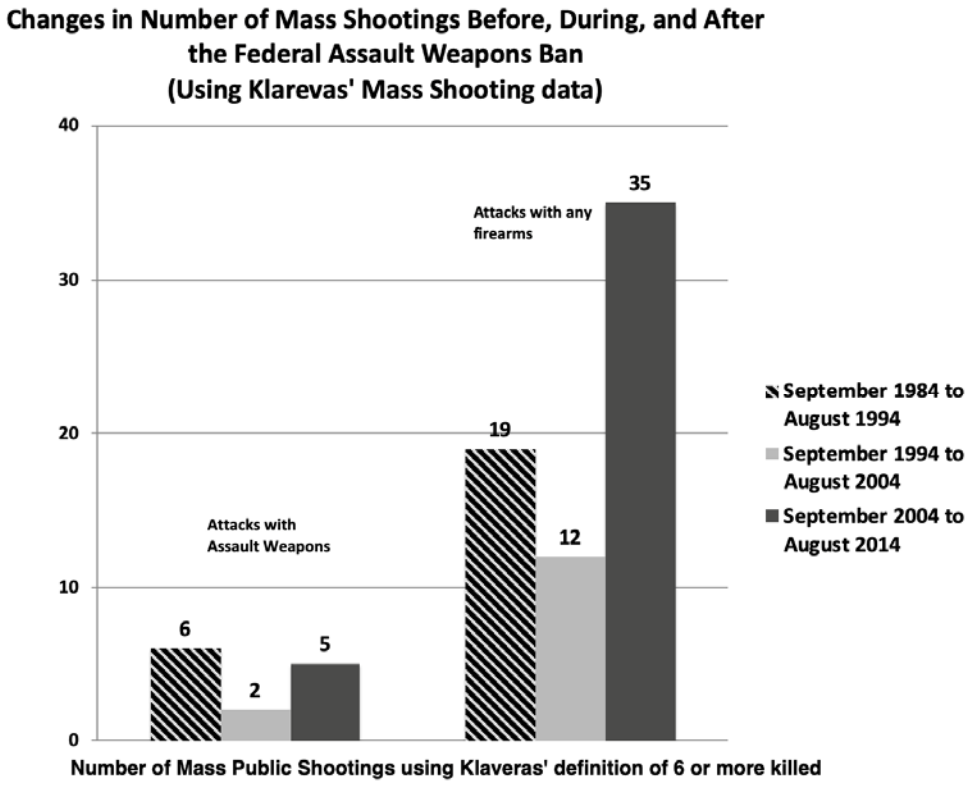
47. In an email to writer Jon Stokes, Klarevas identified seven mass shootings with assault weapons over the ten years from 1984 through 1993 (the earliest being the San Ysidro, California attack on a McDonald's in July 1983). Jon Stokes, "The assault weapons ban didn't work. A new version won't, either," Los Angeles Times, March 1, 2018 (https://www.latimes.com/opinion/op-ed/la-oe-stokes-assault-weapon-ban-20180301-story.html). Using Klarevas' definition, we identified only two cases involving assault weapons in the ten years during which the Federal Assault Weapons Ban was in effect – September 1994 to September 2004. The two attacks were the Columbine shooting in 1999 and a case in Wakefield, Massachusetts in 2000 that involved an AK-47. Our numbers will differ slightly from Klarevas' simply because we look at the 10-year periods from September 1984 to August 1994, September 1994 to August 2004, and September 2004 to September 2014. We only include part of 1984 and 2014 in our time range, whereas Klaveras includes both years in their entirety.

48. We utilize *Mother Jones* magazine's mass shootings dataset, even though it includes cases that don't meet the FBI's definition of mass public shootings. But since it is a widely cited source of cases, we have used it for our comparisons.

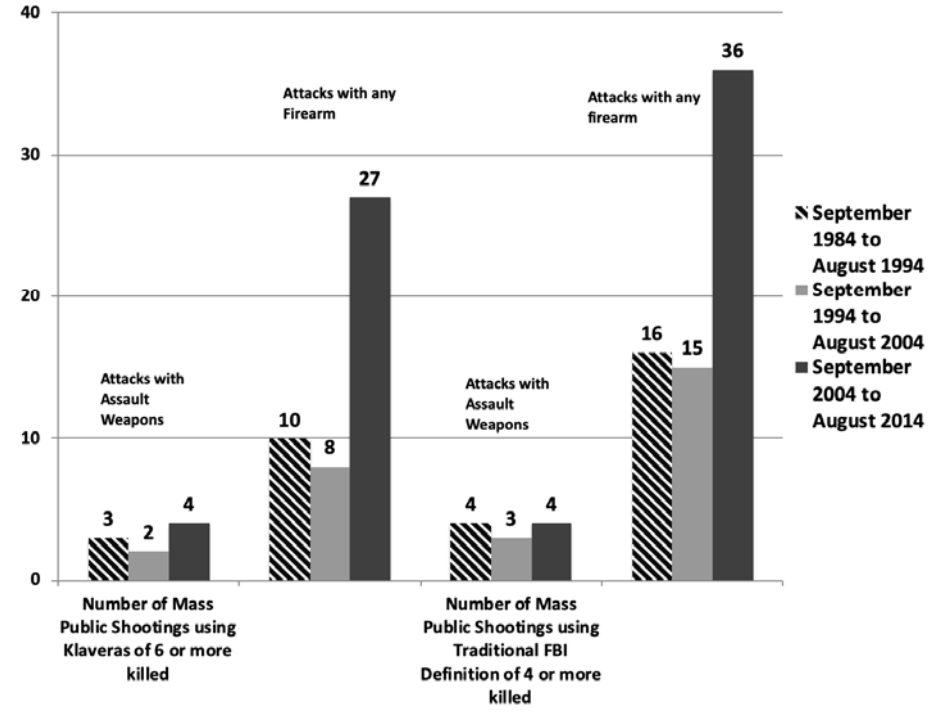
49. No matter which dataset we use, the number of mass shootings committed *with assault weapons* is very small compared to the total number of mass shootings. Looking at the number of attacks with assault weapons, the Mother Jones list shows a difference of only one or two between each of the three, ten-year periods. This holds true whether we use the traditional FBI definition of 4-or-more killed or Klarevas'

definition of 6-or-more killed. The Crime Prevention Research Center (CPRC) data are similar, showing differences of either zero, two, or three attacks between the ten-year periods. None of these changes are large enough to prove that the ban had any impact on the frequency of attacks.

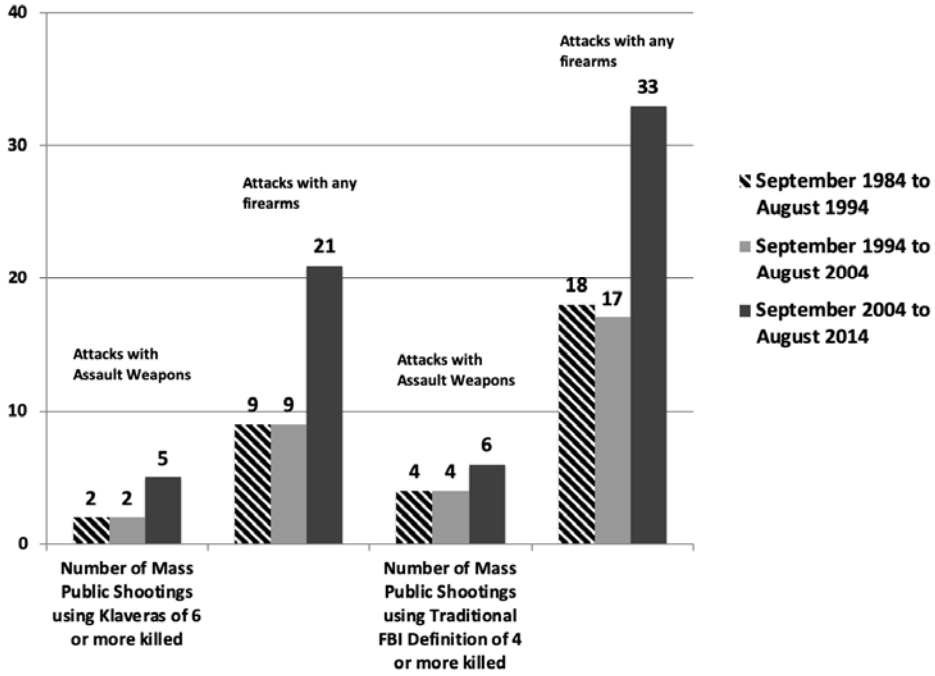
50. Looking at attacks committed with any type of firearm, the disparities that Klarevas finds between the pre-ban and ban periods either completely disappear or become differences of just one or two attacks.



Changes in Number of Mass Public Shootings Before, During, and After the Federal Assault Weapons Ban (Using Mother Jones' Mass Public Shooting data)

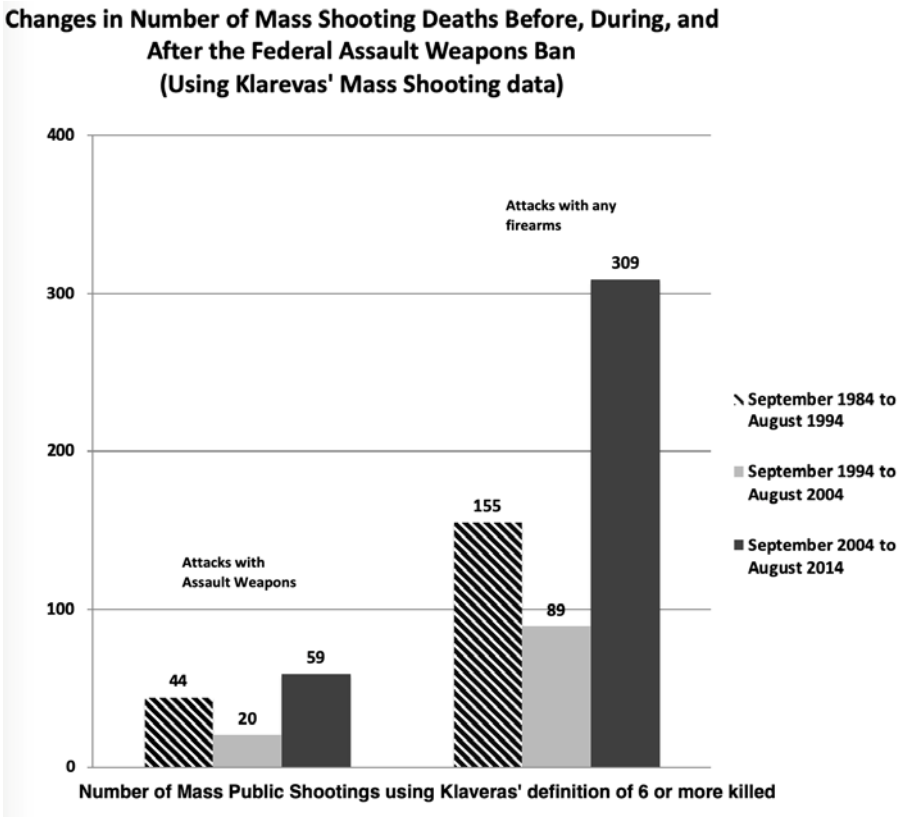


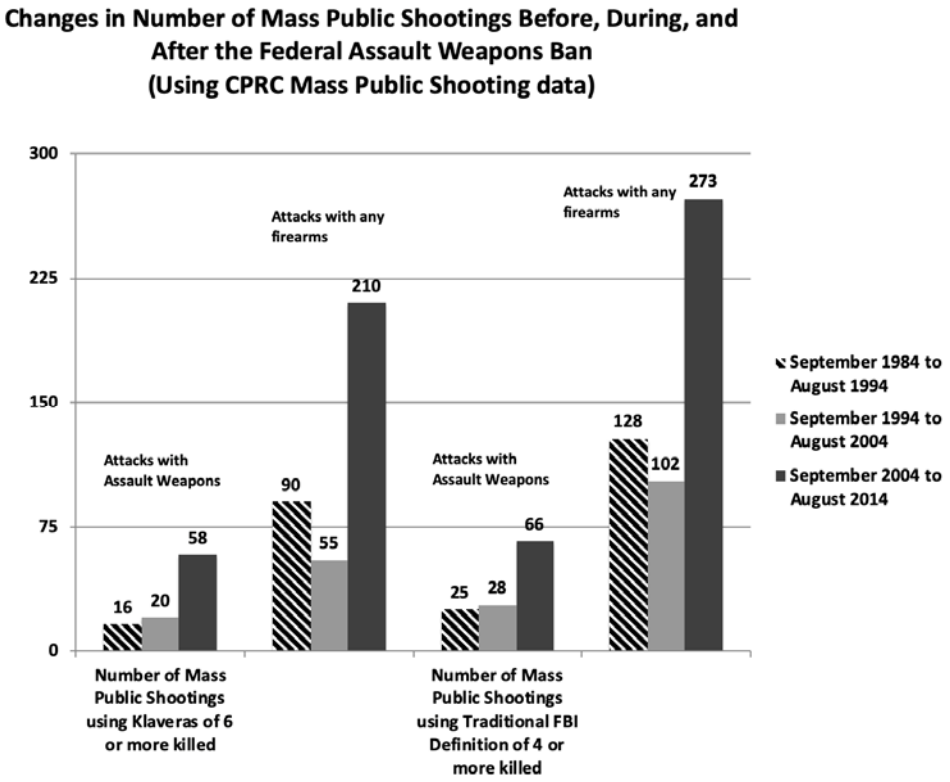
Changes in Number of Mass Public Shootings Before, During, and After the Federal Assault Weapons Ban (Using CPRC Mass Public Shooting data)



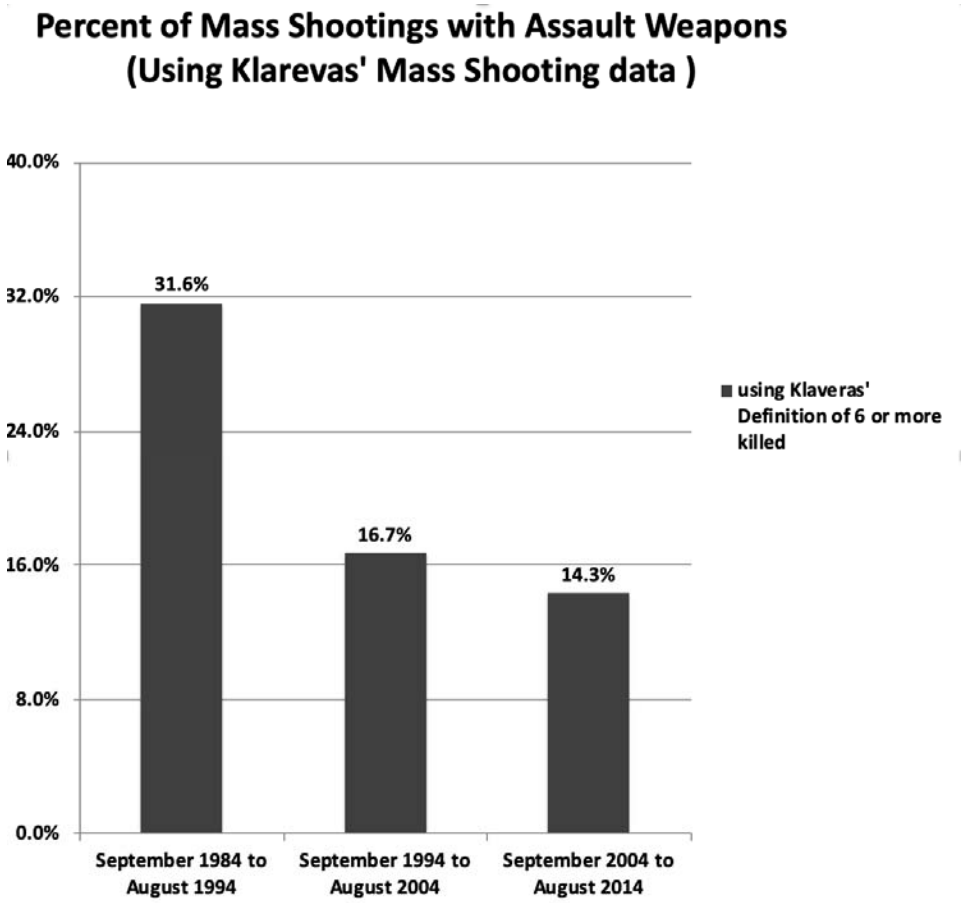
51. When we instead look at the number of fatalities instead of the number of mass public shootings, we see again that there is very little evidence of any benefit from the assault weapons ban. Using the Mother Jones list of cases, we find that compared to the preceding ten years, there was a drop of only four deaths in the decade of the assault weapons ban. That comes to 0.4 fewer deaths per year. The CPRC data actually shows that there was a slight increase in deaths when the ban was in effect. This pattern holds true for mass shootings with 6 or more fatalities.

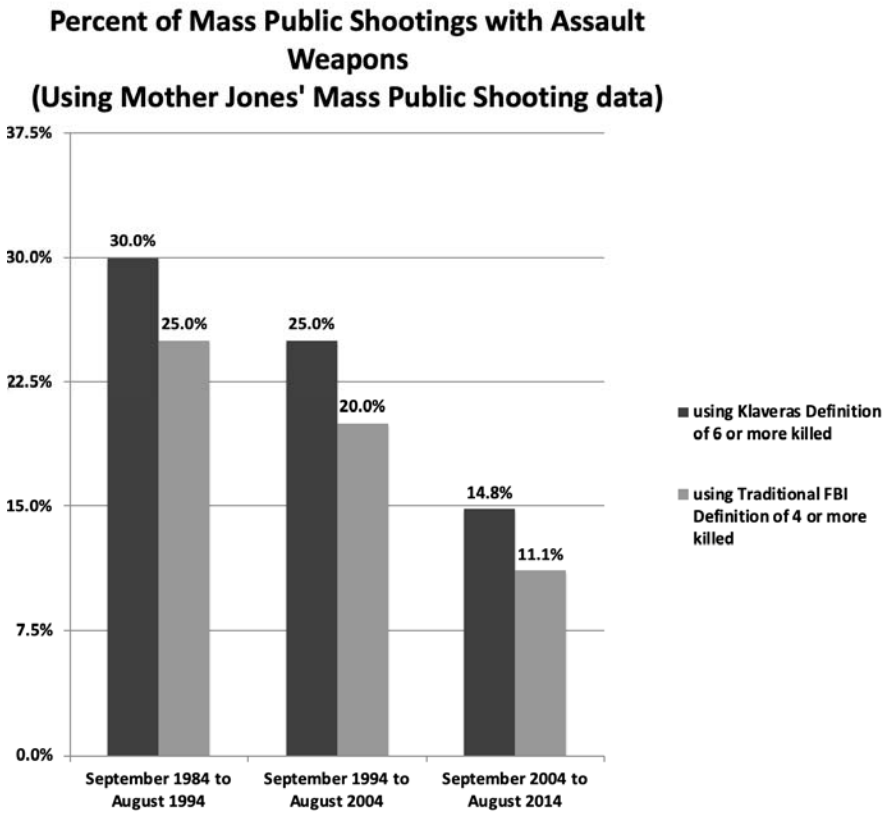
52. In fact, Klarevas' own data shows a decline over time that continued after the assault weapons ban expired. The Mother Jones and CPRC data show even steeper post-ban declines. Instead of the decade with the assault weapons ban showing the lowest share of deaths from assault weapons, the pattern is contrary to what gun control advocates would predict. This is true whether one uses the traditional FBI definition of 4-or-more killed or Klarevas' definition of 6-or-more killed. See graphs below.

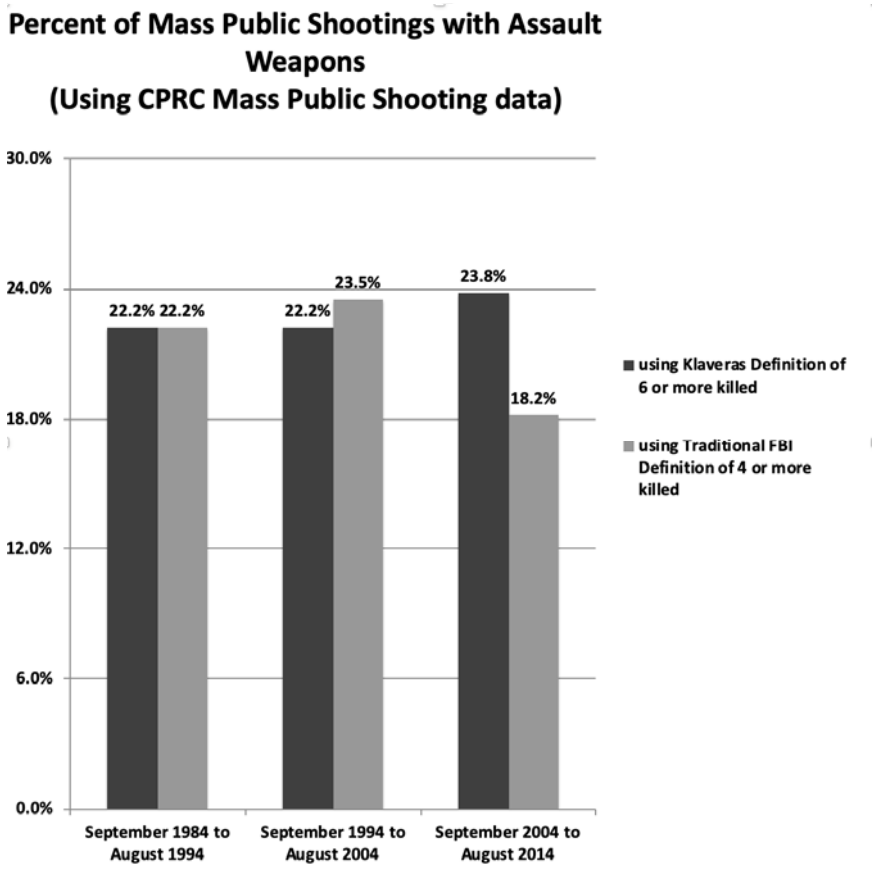




53. Again, the Federal Assault Weapons Ban should have caused the number of attacks with assault weapons to have declined relative to attacks committed using other types of firearms. Testing for this seems to be the best way of evaluating Klarevas' and other gun control advocates' claims. But whether we use Klarevas' list of cases and his definition of mass shootings, or the Mother Jones or CPRC lists and definitions, none of the results are consistent with what gun control advocates would predict. The share of attacks involving assault weapons did not reach its lowest point during the Federal Assault Weapons Ban. For both Klarevas and Mother Jones, the share of attacks committed with assault weapons continued to drop even after the assault weapons ban expired. The ten years after the end of the assault weapons ban (September 2004 to August 2014) saw the lowest share of shootings that involved assault weapons.


Percent of Mass Shootings with Assault Weapons
(Using Klarevas' Mass Shooting data)
40.0%
32.0%
24.0%
16.0%
8.0%
0.0%
31.6%
16.7%
14.3%
September 1984 to August 1994
September 1994 to August 2004
September 2004 to August 2014
using Klaveras' Definition of 6 or more killed


Percent of Mass Public Shootings with Assault Weapons
(Using Mother Jones' Mass Public Shooting data)
37.5%
30.0%
22.5%
15.0%
7.5%
0.0%
30.0%
25.0%
25.0%
20.0%
14.8%
11.1%
September 1984 to August 1994
September 1994 to August 2004
September 2004 to August 2014
using Klaveras Definition of 6 or more killed
using Traditional FBI Definition of 4 or more killed


Percent of Mass Public Shootings with Assault Weapons
(Using CPRC Mass Public Shooting data)
30.0%
24.0%
18.0%
12.0%
6.0%
0.0%
22.2% 22.2%
22.2%
23.5%
23.8%
18.2%
September 1984 to August 1994
September 1994 to August 2004
September 2004 to August 2014
using Klaveras Definition of 6 or more killed
using Traditional FBI Definition of 4 or more killed

54. **Chicoine, "Homicides in Mexico and the expiration of the U.S. federal assault weapons ban: a difference-in-discontinuities approach," Journal of Economic Geography, 2017.** This study finds a "statistically significant increase in the firearm homicide rate following the [2004] expiration of the Assault Weapon Ban" in Mexico. **Ex. 14**, at 913. Mexico's homicide and firearm homicide rates fell until 2007, three years after the federal assault weapons ban ended, and then had almost tripled by 2010. While the paper mentions that the Mexican government started using the military to fight the drug cartels in 2006 and that estimated cocaine sales to the United States fell after 2006, the author ignores the exact reason that the Mexican government war on the cartels was the reason for the increased the murder rate. During this time, the Mexican government succeeded in removing some of the top leadership in the cartels. This caused the cartels to splinter and wars broke out between the different factions to try control parts of the drug trade – which significantly increased the homicide rates. As this critical factor was not considered in this study, little credibility can be given to its conclusions.

55. Attached hereto as **Exhibit 14** is a true and correct copy of Chicoine, "Homicides in Mexico and the expiration of the U.S. federal assault weapons ban: a difference-in-discontinuities approach," Journal of Economic Geography, 2017.

Other Public Health Studies

56. **Fleegler, Lee, Monuteaux, Hemenway, and Mannix, "Firearm Legislation and Firearm-Related Fatalities in the United States," JAMA Internal Medicine, May 13, 2013.** This study attempts to determine whether more firearm laws in a state are associated with fewer firearm fatalities. Although the study claims to find "an association between the legislative strength of a state's firearm laws — as measured by a higher number of laws — and a lower rate of firearm fatalities," the study uses purely

cross-sectional data so it can't account for pre-existing differences across states. **Ex. 15**, at 979. Also, it does not separate out the impact of assault weapons laws. The study sums all the different laws together so that it is impossible to tell the impact of any one law on crime rates. Thus, it cannot claim that assault weapon laws have any effect on homicides or suicides. This study concludes by stating it "could not determine a cause-and-effect relationship, further studies are necessary to define the nature of this association." *Id.*

57. Attached hereto as **Exhibit 15** is a true and correct copy of Fleegler, Lee, Monuteaux, Hemenway, and Mannix, "Firearm Legislation and Firearm-Related Fatalities in the United States," JAMA Internal Medicine, May 13, 2013.

58. **Siegel, Pahn, Xuan, Fleegler, and Hemenway, "The Impact of State Firearm Laws on Homicide and Suicide Deaths in the USA, 1991–2016: A Panel Study," Journal of General Internal Medicine, 2019.** This study analyzed the relationship between state firearm laws and overall homicide and suicide rates at the state level across all 50 states over a 26-year period. Of the 10 laws analyzed, the paper found no effect of the assault weapon bans on either the homicide or suicide. These authors use different sets of control variables and specifications and different periods of time in different papers without any explanation. This raises concerns about data mining.

59. Attached hereto as **Exhibit 16** is a true and correct copy of Siegel, Pahn, Xuan, Fleegler, and Hemenway, "The Impact of State Firearm Laws on Homicide and Suicide Deaths in the USA, 1991–2016: A Panel Study," Journal of General Internal Medicine, 2019.

Survey Analysis Also Concludes Assault Weapon Bans Have No Discernable Effect

60. Several surveys have been conducted relating to whether assault weapon bans have any effect on reducing gun crime. These surveys form their conclusions based on the consensus of the various studies they have reviewed. It should be noted that these surveys make rules on what studies to include or exclude in their survey. Thus, they are not as reliable as the actual studies conducted to form conclusions. However, I have reviewed two surveys that discuss the effectiveness of assault weapons bans. All the studies that each survey considered are included in our above discussion. These surveys reached the same conclusion shown here – that there was no significant evidence that assault weapon bans reduced violent crime or suicide.

61. Attached hereto as **Exhibit 17** is a true and correct copy of **Hahn, Bilukha, Crosby, Fullilove, Liberman, Moscicki, Snyder, Tuma, and Briss, "Firearms Laws and the Reduction of Violence A Systematic Review," American Journal of Preventative Medicine, 2005**

62. Attached hereto as **Exhibit 18** is a true and correct copy of **Rand Corporation, "Effects of Bans on the Sale of Assault Weapons and High-Capacity Magazines on Mass Shootings," Gun Policy in America, March 2, 2018.**

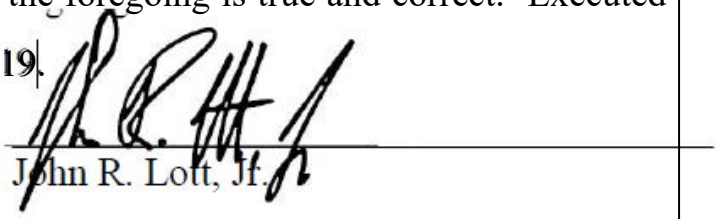
CONCLUSIONS

63. Based on my education, work experience, research background, publications, and review of the research of others, in my opinion, there is no credible evidence that so-called "assault weapons" bans have any meaningful effect on reducing gun homicides and no discernable crime-reduction impact. Several studies have shown no discernable crime-reduction impact. The studies or articles that do show some crime-reduction impact suffer from various flaws in their methods and data collection. These

factors call into question their conclusions and, therefore, the studies should not be relied on as credible.

64. Thus, all credible evidence shows that assault weapon bans have little to no effect in reducing mass shootings, homicides, or violent crime in general.

I declare under penalty of perjury that the foregoing is true and correct. Executed within the United States on December 6, 2019.



John R. Lott, Jr.

EXHIBITS
TABLE OF CONTENTS

EXHIBITS
TABLE OF CONTENTS

EXHIBIT "1"

November 20, 2016

Curriculum Vitae

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DEGREES: Ph.D.: UCLA, September 1984, Economics
MA: UCLA, 1982, Economics
BA: UCLA, 1980, Economics, Magna cum laude

DISSERTATION: "Alternative Explanations for Public Provision of Education"
CHAIRMAN: Harold Demsetz

RANKINGS: Number 27 among Economics, Law, and Business researchers in terms of lifetime downloads of papers at the Social Science Research Network
Worldwide Rankings of Economists and Economics Departments: 1969-2000 by Tom Coupe listed me 26th worldwide in terms of quality adjusted total academic journal output, 4th in terms of total research output, and 86th in terms of citations.
Listed in various editions of "Who's Who in Economics" by Mark Blaug and Howard Vane.

AWARDS AND FELLOWSHIPS:

Senior Research Scholar, School of Law, Yale University — Sept. 1999 to August 2001.

The John M. Olin Law and Economics Fellow, School of Law, University of Chicago — September 1995 to August 1999.

The John M. Olin Visiting Assistant Professor, The George J. Stigler Center for the Study of the Economy and the State, Graduate School of Business, University of Chicago — July 1994 to August 1995.

The John M. Olin Visiting Fellow, Cornell University Law School, March 1994.

Winner of the Duncan Black Award presented by the Public Choice Society for the best Public Choice paper of the year for 1992.

The John M. Olin National Fellow, Hoover Institution, Stanford University — September 1986 to August 1987.

Honorable Mention, Outstanding Doctoral Dissertation Contest in Government Finance and Taxation sponsored by the National Tax Association and the Tax Institute of America, 1984.

AWARDS AND FELLOWSHIPS (CONTINUED):

Weaver Fellowship, Intercollegiate Studies Institute, 1980-1981.

WORK EXPERIENCE:

President, Crime Prevention Research Center -- August 5, 2013 to Present.
Columnist, Fox News -- March 3, 2008 to Present.
Contributor, Fox News -- March 3, 2008 to November 11, 2013.
Senior Editorial Writer for Economics, Washington Times -- February 2009 to December 1, 2010.
Senior Research Scientist, University of Maryland Foundation, University of Maryland at College Park -- July 2007 to June 2009.
The Dean's Visting Professor, State University of New York at Binghamton – August 2006 to July 2007.

Resident Scholar, American Enterprise Institute — September 2001 to July 2006.

Senior Research Scholar, School of Law, Yale University — September 1999 to August 2001.

The John M. Olin Law and Economics Fellow, School of Law, University of Chicago — September 1995 to August 1999.
The John M. Olin Visiting Assistant Professor, The George J. Stigler Center for the Study of the Economy and the State, Graduate School of Business, University of Chicago — July 1994 to August 1995.
The John M. Olin Visiting Fellow, Cornell University Law School, March 1994.
The Carl D. Covitz Term Assistant Professor, The Wharton School, University of Pennsylvania — July 1991 to June 1995.
Visiting Assistant Professor, John E. Anderson Graduate School of Management, University of California at Los Angeles — July 1989 to June 1991.
Chief Economist (GS-15, Step 6), United States Sentencing Commission, Washington, D.C. — February 1988 to August 1989.

Visiting Assistant Professor, Department of Economics, Rice University — July 1987 to June 1988.

The John M. Olin National Fellow, Hoover Institution, Stanford University — September 1986 to August 1987.

Visiting Assistant Professor, Department of Economics, Texas A&M University — August 1984 to June 1986.

Lecturer, Department of Economics, California State University, Northridge — August 1983 to June 1984.

FIELDS OF INTEREST FOR RESEARCH:

Law and Economics, Public Choice, Industrial Organization, Labor, Public Finance, Microeconomic Theory, Environmental Regulation

COURSES TAUGHT (PARTIAL LIST):

Managerial Economics (MBA), Legal Environment of Business (MBA), Environmental Regulation (MBA), White Collar Crime and Corporate Criminal Penalties (MBA), Public Choice (Graduate), Microeconomics (Principles, Intermediate, and MBA), Macroeconomics (Principles, Intermediate, and MBA), Money and Banking (Undergraduate), Issues in Deterrence (Law), Empirical Law and Economics (Law), Cost-Benefit Analysis (Undergraduate, MBA, Graduate), Political Economy of the Public Sector (MBA), Economics of the Nonprofit Sector (MBA), Research Seminar for Law Students

OTHER AFFILIATIONS:

Adjunct Scholar, American Enterprise Institute, January 1995 to August 2001.
Member, The Mont Pelerin Society, September 1990 to present.
Associate, Political Economy Research Center, March 1987 to present.
Member, National Policy Forum, Economic Growth and Workplace Opportunity, January 1994 to 1996.

EDITORIAL ACTIVITY AND OTHER PROFESSIONAL POSITIONS:

Coeditor, ***Economic Inquiry***, November 1996 to August 1998.
Editorial Board, ***Regulation***, July 1989 to 2006.
Editorial Board, ***Public Choice***, March 1994 to December 2003.
Editorial Board, ***Managerial and Decision Economics***, January 1994 to July 1998.
Co-editor, Special Issue of ***Journal of Law and Economics*** on Penalties: Public and Private, 1999.
Co-editor, Special Issue of ***Economic Inquiry*** in Honor of Armen Alchian's 80th Birthday, July 1996.
Special Editor, ***Managerial and Decision Economics***, special issue on "The Economics of Corporate Crime," July-August 1996.
Nominating Committee for Presidency and Board of Directors of Western Economic Association, Western Economic Association, 1996.

OTHER ACTIVITIES (UNPAID):

Board of Advisers, Business & Media Institute, May, 2008 to present.
Wrote the Statistical Report for the Minority members of the U.S. Commission on Civil Rights on the "Probe of Election Practices in Florida During the 2000 Presidential Election."
Served as a Statistical expert for ***USA Today*** in evaluating the precinct level data that they had put together after the Florida Presidential Election in 2000.
Advisor to the Allied Pilots Association and the Airline Pilots Security Alliance on the issue of arming pilots in the cockpit: January 2002 to present.
Served as the statistical expert for the challenge by Senator Mitch McConnell against McCain-Feingold campaign finance regulations.

OTHER ACTIVITIES (PAID):

Consultant, Federal Trade Commission, April 17, 2002 to July 1, 2003.

BOOKS:

Uncertainty and Economic Evolution: Essays in Honor of Armen Alchian, edited volume, Routledge Press: New York (1997).

More Guns, Less Crime: Analyzing Crime and Gun Control Laws, University of Chicago Press: Chicago, Illinois (1998), translated into Portuguese (1999), Russian (2004), and German (2017). Second edition published 2000, third edition published May, 2010.

Are Predatory Commitments Credible?: Who Should the Courts Believe?, University of Chicago Press: Chicago, Illinois (1999).

The Bias Against Guns: Why Almost Everything You've Heard About Gun Control Is Wrong, Regnery Press, Washington, DC (2003) translated into Portuguese (Brazil and Portugal).

Freedomnomics: Why the Free Market Works and Other Half-Baked Theories Don't, Regnery Press, Washington, DC (2007), translated into Portuguese (Brazil and Portugal), Chinese, and Korean.

Straight Shooting: Firearms, Economics and Public Policy, Merril Press: Seattle, Washington, (2010).

Debacle: Obama's War on Jobs and Growth and What We Can Do Now to Regain Our Future, co-authored with Grover Norquist, John Wiley & Sons: New York, NY (March, 2012).

At the Brink, Regnery Press, Washington, DC (February 9, 2013)

Dumbing Down the Courts: How Politics Keeps the Smartest Judges Off the Bench, Hillcrest Media: Minneapolis, MN (September 8, 2013).

The War on Guns, Regnery Press, Washington, DC (2016)

PUBLICATIONS:

LAW AND ECONOMICS:

(1) "Licensing and Nontransferable Rents," ***American Economic Review***, Vol. 77, no. 3, June 1987: 453-455; "Licensing and Nontransferable Rents: Reply," ***American Economic Review***, Vol. 79, no. 4, September 1989: 910-912.

LAW AND ECONOMICS (CONTINUED):

(2) "Juvenile Delinquency and Education: A Comparison of Public and Private Provision," ***International Review of Law and Economics***, Vol.7, no. 2, December 1987: 163-175.

(3) "Should the Wealthy Be Able to 'Buy Justice'?" ***Journal of Political Economy***, Vol. 95, no. 6, December 1987: 1307-1316.

(4) "Why Comply: The One-Sided Enforcement of Price Controls and Victimless Crime Laws," co-authored with Russell Roberts, ***Journal of Legal Studies***, Vol. 18, no. 2, June 1989: 403-414, reprinted in ***The Economics of Corruption and Illegal Markets***, edited by Gianluca Fiorentini and Stefano Zamagni, Cheltenham, U.K.: Edward Elgar Publishing Limited, forthcoming.

(5) "A Transaction Costs Explanation For Why the Poor are More Likely to Commit Crime," ***Journal of Legal Studies***, Vol. 19, no. 1, January 1990: 243-245.

(6) "Optimal Penalties Versus Minimizing the Level of Crime: Does it Matter Who is Correct?" ***Boston University Law Review***, invited conference volume on the United States Sentencing Commission's proposed Organizational Sanctions, March 1991: 439-446.

(7) "An Attempt at Measuring the Total Monetary Penalty from Drug Convictions: The Importance of an Individual's Reputation," ***Journal of Legal Studies***, Vol. 21, no. 1, January 1992: 159-187, reprinted in ***The Economics of Corruption and Illegal Markets***, edited by Gianluca Fiorentini and Stefano Zamagni, Cheltenham, U.K.: Edward Elgar Publishing Limited, forthcoming.

(8) "Low-Probability-High-Penalty Enforcement Strategies and the Efficient Operation of the Plea Bargaining System," co-authored with Bruce H. Kobayashi, ***International Review of Law and Economics***, Vol. 12, no. 1, March 1992: 69-77.

(9) "Do We Punish High Income Criminals too Heavily?" ***Economic Inquiry***, Vol. 30, no. 4, October 1992: 583-608.

(10) "The Reputational Penalty Firms Bear for Committing Fraud," co-authored with Jonathan M. Karpoff, ***Journal of Law and Economics***, Vol. 36, no. 2, October 1993: 757-803, closely related version reprinted in ***The Economics of Organized Crime***, edited by Gianluca Fiorentini and Sam Peltzman, London: Cambridge University Press, 1995: 199-246.

(11) "The Expected Penalty for Committing a Crime: An Analysis of Minimum Wage Violations," co-authored with Russell Roberts, ***Journal of Human Resources***, Vol. 30, no. 2, Spring 1995: 397-408.

(12) "Should Criminal Penalties Include Third-Party Avoidance Costs?" co-authored with Kermit Daniel, ***Journal of Legal Studies***, Vol. 24, no. 2, June 1995: 523-534.

LAW AND ECONOMICS (CONTINUED):

(13) "The Optimal Level of Criminal Fines in the Presence of Reputation," ***Managerial and Decision Economics***, invited conference volume, Vol. 17, no. 4, July-August, 1996: 363-380.

(14) "In Defense of Criminal Defense Expenditures and Plea Bargaining," co-authored with Bruce Kobayashi, ***International Review of Law and Economics***, Vol. 16, no. 4, December 1996: 397-416.

(15) "Crime, Deterrence, and Right-to-Carry Concealed Handguns," co-authored with David Mustard, ***Journal of Legal Studies***, Vol. 26, no. 1, January 1997: 1-68; portion reprinted in ***The Gun Control Debate: A Documentary History***, edited by Marjolin Bijlefeld, Westport, CT.: Greenwood Publishing (1997): 88-91; single authored summary reprinted in the ***Valparasio University Law Review***, Vol. 31, no. 2 Spring 1997: 355-364; Reprinted in ***Guns in America: a reader***. 1999. edited by Jan E. Dizard, Robert M. Muth, Stephen P. Andrews, NYU Press. Reprinted in ***The Economics of Crime***. 2005. edited by Isaac Ehrlich and Zhiqiang Liu, The International Library of Critical Writings in Economics, Edward Elgar Pub.. Reprinted in ***Economics, Law and Individual Rights***, 2008, edited by Hugo M. Mialon, Paul H. Rubin, Routledge.

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(20) "Have changing Liability Rules Compensated Workers Twice for Occupational Hazards?: Earnings Premiums and Cancer Risks," co-authored with Richard Manning, ***Journal of Legal Studies***, Vol. 29, no. 1 (January 2000): 99-130.

(21) "Does a Helping hand Put Others At Risk?: Affirmative Action, Police Departments, and Crime," ***Economic Inquiry***, Vol. 38, no. 2 (April 2000): 239-277; republished in *The Economics of Affirmative Action*, edited by Harry J. Holzer, Edward Elgar Publishing Ltd.: Surrey, UK (2004).

(22) "The American Bar Association, Judicial Ratings, and Political Bias," ***Journal of Law & Politics***, (Winter 2001): 41-61.

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(23) "Safe Storage Gun Laws: Accidental Deaths, Suicides, and Crime," co-authored with John Whitley, ***Journal of Law and Economics***, Vol. 44, no. 2, part 2, (October 2001): 659-689.

(24) "Non-Voted Ballots and Discrimination in Florida." ***Journal of Legal Studies***, Vol. 32, no. 1 (January 2003): 181-220.

(25) "Measurement Error in County-Level UCR Data," with John Whitley, ***Journal of Quantitative Criminology***, Vol. 19, No. 2 (June 2003): 185-198.

(26) "The Judicial Confirmation Process: The Difficulty in Being Smart," ***Journal of Empirical Law and Economics***, Vol. 2, no. 3, 2005: 407-447 (Lead article).

(27)"The Reputational Penalties for Environmental Violations: Empirical Evidence," with Jonathan Karpoff and Eric Wehrly, ***Journal of Law and Economics***, Vol., no. 2 (October 2005): 653-675.

(28) "Abortion and Crime: Unwanted Children and Out-of-Wedlock Births***,"*** co-authored with John Whitley, ***Economic Inquiry***, Vol. 45, no. 2, (April 2007): 304-324.

(29) "Peer Effects in Affirmative Action: Evidence from Law Student Performance," co-authored with Mark Ramseyer and Jeffrey Standen, ***International Review of Law and Economics***, Vol. 31, no. 1 (March 2011): 1-15 (Lead article).

(30) "What a balancing test will show for Right-to-Carry Laws," ***University of Maryland Law Review***, Vol. 71, no. 4 (2012): 1205-1218.

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(1) "Brand Names and Barriers to Entry in Political Markets," ***Public Choice***, Vol. 51, no. 1, 1986: 87-92.

(2) "Political Cheating," ***Public Choice***, Vol. 52, no. 2, 1987: 169-186.

(3) "The Effect of Nontransferable Property Rights on the Efficiency of Political Markets: Some Evidence," ***Journal of Public Economics***, Vol. 32, no. 2, March 1987: 231-246.

(4) "The Institutional Arrangement of Public Education: The Puzzle of Exclusive Territories," ***Public Choice***, Vol. 54, no. 1, 1987: 89-96.

(5) "Why is Education Publicly Provided?: A Critical Survey," ***Cato Journal***, Vol. 7, no. 2, Fall 1987: 475-501, reprinted in ***The Economic Value of Education***, edited by Mark Blaug, Cheltenham, U.K.: Edward Elgar Publishing Limited, 1992, Chapter 27.

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(6) "Explaining Challengers' Campaign Expenditures: The Importance of Sunk Nontransferable Brand Name," ***Public Finance Quarterly***, Vol. 17, no. 1, January 1989: 108-118.

(7) "Deadweight Losses and the Saving Response to a Deficit," co-authored with Gertrud Fremling, ***Economic Inquiry***, Vol. 27, no. 1, January 1989: 117-129.

(8) "Shirking and Sorting in a Political Market with Finite-Lived Politicians," co-authored with W. Robert Reed, ***Public Choice***, Vol. 61, no. 1, April 1989: 75-96.

(9) "Time Dependent Information Costs, Price Controls, and Successive Government Intervention," co-authored with Gertrud Fremling, ***Journal of Law, Economics, and Organization***, Vol. 5, no. 2, Fall 1989: 293-306.

(10) "Attendance Rates, Political Shirking, and the Effect of Post-Elective Office Employment," ***Economic Inquiry***, Vol. 28, no. 1, January 1990: 133-150.

(11) "An Explanation for Public Provision of Schooling: The Importance of Indoctrination," ***Journal of Law and Economics***, Vol. 33, no.1, April 1990: 199-231.

(12) "Predation by Public Enterprises," ***Journal of Public Economics***, Vol. 43, no. 2, November 1990: 237-251.

(13) "Does Additional Campaign Spending Really Hurt Incumbents?: The Theoretical Importance of Past Investments in Political Brand Name," ***Public Choice***, Vol. 72, October 1991: 87-92.

(14) "A Critical Review and An Extension of the Political Shirking Literature," co-authored with Michael L. Davis, ***Public Choice***, Vol. 74, no. 4, December 1992: 461-484, winner of the Duncan Black Award presented by the Public Choice Society for the best Public Choice paper of the year.

(15) "Reconciling Voters' Behavior with Legislative Term Limits," co-authored with Andrew R. Dick, ***Journal of Public Economics***, Vol. 50, no. 1, January 1993: 1-14, reprinted in ***Term Limits: A Public Choice Perspective***, edited by Bernard Grofman, Dordrecht, Netherlands: Kluwer Academic Publishers, forthcoming 1996.

(16) "Time Series Evidence on Shirking by Members of the U.S. House of Representatives," coauthored with Stephen G. Bronars, ***Public Choice***, invited conference volume, Vol. 76, no. 1-2, June 1993: 125-149, reprinted in ***Foundations of Regulatory Economics***, edited by Robert B. Ekelund, Jr., London: Edward Elgar Publishing Limited, 2000.

(17) "An Explanation for Why Senators from the Same State Vote Differently So Frequently," coauthored with Gi-Ryong Jung and Lawrence W. Kenny, ***Journal of Public Economics***, Vol. 54, no. 1, May 1994: 65-96.

PUBLIC CHOICE AND PUBLIC FINANCE (CONTINUED):

(18) "Do Deficits Affect the Level of Insurance?" co-authored with Gertrud M. Fremling, ***Journal of Money, Credit, and Banking***, Vol. 26, no. 4, November 1994: 934-940.

(19) "Are Government or Private Enterprises More Likely to Engage in Dumping?: Some International Evidence," ***Managerial and Decision Economics***, Vol. 16, no. 3, May-June 1995: 185-204.

(20) "Legislator Voting and Shirking: A Critical Review of the Literature," co-authored with Bruce Bender, ***Public Choice***, Vol. 87, nos. 1 and 2, April 1996: 67-100.

(21) "Term Limits and Electoral Competitiveness: Evidence from California's State Legislative Races," co-authored with Kermit Daniel, ***Public Choice***, Vol. 90, nos. 1-4, March 1997: 165-184, reprinted in ***Constitutional Political Economy in a Public Choice Perspective***, edited by Charles K. Rowley, Kluwer Academic Publishers: Boston, 1997, Chapter 7, pp. 165-184.

(22) "Does Political Reform Increase Wealth?: Or, Why the Difference Between the Chicago and Virginia Schools is Really an Elasticity Question," ***Public Choice***, Vol. 91, nos. 3-4, June 1997: 219-227.

(23) "A Review Article on Donald Wittman's The Myth of Democratic Failure," ***Public Choice***, Vol. 92, no. 1-2, July 1997: 1-13 (Lead article).

(24) "How Term Limits Enhance the Expression of Democratic Preferences," coauthored with Einer Elhauge and Richard Manning, ***Supreme Court Economic Review***, Vol. 5, 1997: 59-81.

(25) "Do Campaign Donations Alter How a Politician Votes?," coauthored with Steve Bronars, ***Journal of Law and Economics***, Vol. 40, no. 2, October 1997: 317-350.

(26) "Did Women's Suffrage Change the Size and Scope of Government?," co-authored with Larry Kenny, ***Journal of Political Economy***, Vol. 107, no. 6, part 1, December 1999: 1163-1198.

(27) "Public Schooling, Indoctrination, and Totalitarianism," ***Journal of Political Economy***, Vol. 107, no. 6, part 2, December 1999: S127-S157.

(28) "A Simple Explanation for Why Campaign Donations are Increasing: The Government is Getting Bigger," ***Journal of Law and Economics***., Vol. 42, no. 2, October 2000: 359-393.

(29) "Documenting Unusual Declines in Republican Voting Rates in Florida's Western Panhandle Counties in 2000," ***Public Choice***, Vol. 123, June 2005: 349-361.

(30) "Campaign Finance Reform and Electoral Competition," ***Public Choice***: Vol. 129 (3-4), 2006: 263-300.

PUBLIC CHOICE AND PUBLIC FINANCE (CONTINUED):

(31) "Non-voted Ballots, The Cost of Voting, and Race," ***Public Choice***, Vol. 138, no. 1, (January 2009): 171-197.

(32) "What Does the American Bar Association Judicial Rating Really Measure?" ***Public Choice***, 2013.

(33) "Is Newspaper Coverage of Economic Events Politically Biased?" co-authored with Kevin Hassett, ***Public Choice***, (July 2014): 65-108.

INDUSTRIAL ORGANIZATION:

(1) "Brand Names, Ignorance, and Quality Guaranteeing Premiums," ***Applied Economics***, Vol. 20, no. 2, February 1988: 165-176.

(2) "Qualitative Information, Reputation, and Monopolistic Competition," co-authored with Michael Darby, ***International Review of Law and Economics***, Vol. 9, no. 1, June 1989: 87-103.

(3) "A Guide to the Pitfalls of Identifying Price Discrimination," co-authored with Russell D. Roberts, ***Economic Inquiry***, Vol. 29, no. 1, January 1991: 14-23, reprinted in ***Who Sets Prices?***, Pittsburgh, PA.: Enterprise & Education Foundation, 1991.

(4) "Do Some Firms Rely on Preferences Instead of Sunk Investments to Guarantee Performance?" coauthored with Andrew R. Dick, ***Managerial and Decision Economics***, invited conference volume, Vol. 14, no. 2, March-April 1993: 109-118.

(5) "Profiting from Induced Changes in Competitors' Market Values: The Case of Entry and Entry Deterrence," co-authored with Robert G. Hansen, ***Journal of Industrial Economics***, Vol. 43, no. 3, September 1995: 261-276.

(6) "Externalities and Corporate Objectives in a World with Diversified Shareholder/Consumers," co-authored with Robert G. Hansen, ***Journal of Financial and Quantitative Analysis***, Vol. 31, no. 1, March 1996: 43-68.

(7) "Testing Whether Predatory Commitments are Credible," co-authored with Tim Opler, ***Journal of Business***, Vol. 69, no. 3, July 1996: 339-382.

OTHER AREAS:

(1) "Why Do Workers Join Unions?: The Importance of Rent-Seeking," co-authored with Stephen G. Bronars, ***Economic Inquiry***, Vol. 27, no. 4, April 1989: 305-324.

(2) "The Winner's Curse and Public Information in Common Value Auctions: Comment," co-authored with Robert G. Hansen, ***American Economic Review,*** Vol. 81, no. 1, March 1991: 347-361, reprinted in ***Recent Developments in Experimental Economics***, Vol. II, edited by John D. Hey and Graham Loomes, Cheltenham, U.K.: Edward Elgar Publishing Limited, 1993, Chapter 9, pp. 154-168.

(3) "The Bias Towards Zero in Aggregate Perceptions: An Explanation Based on Rationally Calculating Individuals," co-authored with Gertrud Fremling, ***Economic Inquiry***, Vol. 34, no. 2, April 1996: 276-295; "The Bias Towards Zero in Identifying Relationships: Reply to Kennedy," co-authored with Gertrud Fremling, ***Economic Inquiry***, Vol. 37, no. 2, April 1999: 385-386.

(4) "The Effect of Macroeconomic News on Stock Returns: New Evidence from Newspaper Coverage," co-authored with Gene Birz, ***Journal of Banking and Finance***, Vol**.** 35, November 2011: 2791-2800 (Semifinalist for Best Paper in Investments Award at 2009 FMA Annual Meetings).

SHORTER PAPERS, BOOK CHAPTERS, AND BOOK REVIEWS:

(1) "A Note on Law, Property Rights, and Air Pollution," ***Cato Journal***, Vol. 3, no. 3, Winter 1983/1984: 875-878.

(2) "Education," Economics Research Directory, New York: Manhattan Institute, 1984: Chp. 7.

(3) "Rates of Return Promised by Social Security to Today's Young Workers," co-authored with Peter Ferrara, in Social Security: The Prospects for Real Reform, Peter Ferrara ed., Washington: Cato Institute, 1985: Chp. 1.

(4) Review of Unnatural Monopolies, edited by Robert Poole, ***Southern Economic Journal***, Vol. 53, no. 1, July 1986: 287-288.

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(8) "Some Thoughts on Tullock's New Definition of Rent-Seeking," ***Contemporary Policy Issues***, Vol. 6, no. 4, October 1988: 48-49.

(9) "Racial Employment and Earnings Differentials: The Impact of the Reagan Administration: Comment," ***The Review of Black Political Economy***, Vol. 17, no. 4, Spring 1989: 83-84.

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(11) Review of Televised Legislatures: Political Information, Technology, and Public Choice by W. Mark Crain and Brian Goff, ***American Political Science Review***, Vol. 83, December 1989: 1377-1378.

(12) "Getting Tough on White-Collar Crime," ***Regulation***, Vol. 13, no. 1, Winter 1990: 18-19.

(13) "A Comment on 'The Role of Potential Competition in Industrial Organization,'" co-authored with Andrew Dick, ***Journal of Economic Perspectives***, Vol. 4, no. 2, Spring 1990: 213-215.

(14) "Why is Education Publicly Provided?: Some Further Thoughts," ***Cato Journal***, Vol. 10, no. 1, Summer 1990: 293-297.

(15) "Nontransferable Rents and an Unrecognized Social Cost of Minimum Wage Laws," ***Journal of Labor Research***, Vol. 11, no. 4, Fall 1990: 453-460.

(16) "The Effect of Conviction on the Legitimate Income of Criminals," ***Economics Letters***, Vol. 34, no. 12, December 1990: 381-385.

(17) "Why the Commission's Corporate Guidelines May Create Disparity," ***Federal Sentencing Reporter***, co-authored with Jonathan Karpoff, November/December 1990: 140-141.

(18) Review of Institutions, Institutional Change, and Economic Performance, by Douglas C. North, ***Journal of Policy Analysis and Management***, Vol. 11, no. 1, 1992: 156-159.

(19) "Goring the U.S. Economy," Review of Earth in the Balance: Ecology and the Human Spirit, by Senator Albert Gore, Jr., ***Regulation***, Vol. 15, no. 3, Summer 1992: 76-80.

(20) Review of Reforming Products Liability, by W. Kip Viscusi, ***Journal of Policy Analysis and Management***, Vol. 11, no. 4, 1992: 726-728.

(21) Review of The Future of Economics, by John D. Hey (ed.), ***Public Choice***, Vol. 75, no. 4 (April 1993): 389-394.

SHORTER PAPERS, BOOK CHAPTERS, AND BOOK REVIEWS (CONTINUED):

(22) "Regulatory Common Sense vs. Environmental Nonsense," Reviews of Environmental Overkill: Whatever Happened to Common Sense? by Dixy Lee Ray with Lou Guzo and Science Under Siege: Balancing Technology and the Environment by Michael Fumento, ***Regulation***, Vol. 16, no. 1, Fall 1993: 80-82.

(23) "Regulating Indoor Air Quality: The Economist's View," coauthored with Robert G. Hansen, ***The EPA Journal***, Vol. 19, no. 4 (October-December, 1993): 30-31.

(24) "Environmental Economics: Fallacies and Market Incentives," Chapter 3 in Balancing the Earth's Economy and Ecology: Analysis and Constructive Alternatives to Earth in the Balance, John Baden (ed.), San Francisco: Pacific Research Institute, 1994: 77-89.

(25) "The Regulatory Quest for Safety at Any Cost," Review of Collision Course: The Truth About Airline Safety by Ralph Nader and Wesley J. Smith, ***Regulation***, Vol. 17, no. 1, Winter 1994: 80-81.

(26) "Armen A. Alchian's Influence on Economics," ***Economic Inquiry*** , Vol. 34, no. 3, July, 1996: 409-411, reprinted in Uncertainty and Economic Evolution: Essays in Honor of Armen Alchian, John R. Lott, Jr. (ed.), Routledge Press: New York (1997): 1-3.

(27) Moderated and Participated in "Roundtable discussion in Celebration of Armen Alchian's 80th Birthday," ***Economic Inquiry*** Vol. 34, no. 3 (July 1996): 412-426.

(28) "Corporate Criminal Penalties," ***Managerial and Decision Economics*** Vol. 17, no. 4 (July-August 1996): 349-350.

(29) "In Praise of Lost Mail and $900 Toilet Seats?" Review of The Myth of Democratic Failure: Why Political Institutions are Efficient, by Donald Wittman, ***Regulation***, no. 1, 1996: 85-89.

(30) "Concealed Handguns Can Save Lives," ***Agenda***, Vol. 3, no. 4, 1996: 499-502.

(31) "Freedom, Wealth, and Coercion," co-authored with Gertrud Fremling, in Uncertainty and Economic Evolution: Essays in Honor of Armen Alchian, John R. Lott, Jr. (ed.), Routledge Press: New York (1997): 151-164.

(32) "Survey of the Economics of Corporate Crime," ***Encyclopedia of Law and Economics***, Boudewijn Bouckaert and Gerrit De Geest, editors, forthcoming.

(33) "The Reputational Penalty Imposed on Criminals," ***The New Palgrave Dictionary of Economics and Law***, Peter Newman, editor, 1998.

(34) "Do Concealed Handgun Laws Save Lives?," ***Spectrum: The Journal of State Government***, Vol. 70 (Spring 1997): 28 and 29.

SHORTER PAPERS, BOOK CHAPTERS, AND BOOK REVIEWS (CONTINUED):

(35) "Who is Really Hurt By Affirmative Action?," ***Police Executive Research Forum***, Vol. 12, No. 5, May 1998: 1 and 3.

(36) "How to Stop Mass Shootings," ***The American Enterprise***, Vol. 9, no.4, July/August 1998: 66-67.

(37) "More Guns, Less Crime," Letter to the Editor, ***The New England Journal of Medicine***, May 20, 1999.

(38) "Refusing to Let Facts Get In The Way of A Good Story," ***The American Enterprise***, Vol. 10, No. 3 (May/June 1999): p. 68.

(39) "Public and Private Penalties: Introduction," ***Journal of Law and Economics***, Vol. 42, June 1999: 239-243.

(40) "Does Allowing Law-Abiding Citizens to Carry Concealed Handguns Save Lives?" in Guns in America: A Reader, edited by Jan Dizard, Robert Muth, and Stephen Andrews, New York University Press: New York (1999): 322-330.

(41) "Violence Prevention and Concealed Weapons Laws," Letter to the Editor, ***Journal of the American Medical Association***, Vol. 283 No. 9, March 1, 2000.

(42) "When Gun Control Costs Lives," ***Phi Kappa Phi Journal***, Vol. 80, Fall 2000: 29-32.

(43) "Another Media Murder of the Truth," ***The American Enterprise***, Vol. 11, no.8, December 2000: p. 10.

(1) "Impact of the Brady Act on Homicide and Suicide Rates," Letters to the Editor, ***Journal of the American Medical Association***, Vol. 284 No. 21, December 6, 2000, p. 2718.

(2) "Carrying Concealed Weapons Prevents Crime," Chapter 3, in Crime and Criminals, edited by Tamara L. Roeff, University of Michigan Press, 2000.

(3) "Guns, Crime, and Safety: Introduction," ***Journal of Law and Economics***, Vol. 44, no. 2, part 2, (October 2001): 605-614.

(47) "The Surprising Finding that 'Cultural Worldviews' Don't Explain People's Views on Gun Control," co-authored with Gertrud M. Fremling, ***University of Pennsylvania Law Review***, Vol. 151, no. 4 (April 2003): 1341-1348.

(48) "Correcting 'The March of Science' Editorial," ***Archives of Pediatrics & Adolescent Medicine***, June 2008: 589.

(49) "Civil Rights: Racial Preferences in Higher Education," ***Chapman Law Review***, Winter 2009: 344-350.

SHORTER PAPERS, BOOK CHAPTERS, AND BOOK REVIEWS (CONTINUED):

(50) "Reforms that ignore the black victims of crime," ***Cato Unbound***, March 13, 2009.

(51) "An Unsatisfying Change?: Canadians' satisfaction with their health care isn't much different from uninsured Americans," ***Regulation***, Summer 2009: 38-44.

(52) Forward to ***From Luby's to the Legislature: One Woman's Fight Against Gun Control***, by Suzanna Gratia Hupp, Privateer Publications (December 1, 2009).

(53) "Making Guns Less Available Does Not Reduce Gun Violence," in ***Opposing Viewpoints: Gun Violence***, Louise Gerdes, editor, Gale Publishing: Dallas, Texas (2011).

(54) Forward to ***Power Divided is Power Checked***, Jason Lewis, Bascom Hill Publishing (January 2011).

(56) "How do Multiple Victim Public Shooters Decide Where to Attack?" ***ACJS (Academy of Criminal Justice Sciences) Today***, September, 2012: pp. 14-17.

(55) "The Cost-Benefit Analysis of Crime," in ***Developing Standards for Benefit-Cost Analysis***, Richard Zerbe, editor, the University of Washington Benefit-Cost Center (2011).

(56) "How do Multiple Victim Public Shooters Decide Where to Attack?" ***Academy of Criminal Justice Sciences Today***, September, 2012: 14-17.

(57) "Did John Lott Provide Bad Data to the NRC?: A Note on Aneja, Donohue, and Zhang," co-authored with Carlisle E. Moody and Thomas Marvell, ***Econ Journal Watch***, January 2013.

REFEREED FOR:

American Economic Review; American Economic Journal: Applied Economics; American Journal of Political Science; Contemporary Economic Policy; Criminology; Criminology & Public Policy; Eastern Economic Journal; Economic Inquiry; Economica; Economics of Education Review; Economics of Governance; International Economic Review; International Journal of Industrial Organization; International Review of Law and Economics; Journal of the American Medical Association; Journal of Corporate Finance; Journal of Economic Behavior and Organization; Journal of Economic Education; Journal of Human Resources; Journal of Interpersonal Violence; Journal of Law and Economics; Journal of Law, Economics, and Organization; Journal of Legal Studies; Journal of Financial and Quantitative Analysis; Journal of Policy Analysis & Management; Journal of Political Economy; Journal of Politics; Journal of Public Economics; Legislative Studies Quarterly; Managerial and Decision Economics; National Science Foundation; Oxford University Press; Policy Studies Journal; Public Choice; Public Finance Quarterly; Quarterly Journal of Economics; Quarterly Review of Economics and Finance; RAND Journal of Economics; Rationality and Society; Research in Law and Economics; Review of Economics and Statistics; Review of Economics of the Household; Social Choice and Welfare; Southern Economic Journal; and Regulation.

SELECTED PRESENTATIONS:

PRESENTATIONS AT GOVERNMENT AGENCIES AND OTHER NON-UNIVERSITY INSTITUTIONS:

Ad Hoc Working Group on the Economics of the Pharmaceutical Industry: 1995; American Legislative Exchange Council (National Meeting): 1998; Cato Institute: 1996, 2000, 2007, 2010; City of Philadelphia Continuing Legal Education for Municipal Attorneys: 1999; Commodity Futures Trading Commission: 1991; Commonwealth Club of California (San Jose): 2008; Comstock Club (Stockton, California): 1997; Congressional Hispanic Caucus Institute: 2001; Contemporary Club (Charlottesville, Virginia): 2008; Doctors for Disaster Preparedness (San Francisco, Ca.): 2000; Eagle Council: 1999, 2000; Federal Trade Commission: 1990, 1992, 1996; Fortune Society debate on "Should Convicted Felons Have a Vote?": 2004; Frontiers of Freedom: 2000; Goldwater Institute (Phoenix, Arizona): 1998; Heartland Institute: 1999, 2007, 2010; Heritage Foundation: 1997; Illinois Police Association Annual State Convention (Luncheon Keynote Speaker): 1997; Innocenzo Gasparini Institute for Economic Research (Milan, Italy): 1993; Intelligence Squared: 2008; Koch Crime Commission (Topeka, Kansas): 1998; Lone Star Foundation: 1999; National Association of Treasury Agents Annual Convention: 1997; New Jersey Conference of Mayors: 2000; New Zealand Department of Justice: 2006; Orange County Federalist Society: 2007, 2009, 2013, 2016; "Bully Pulpit Speaker" series Sponsored by Wisconsin State House Speaker before legislative staff: 2000; Rand Corporation: 1991, 1999; Reason Weekend: 2000; Republican National Lawyers Association, National Summer Election Law Seminar & School: 2006; Seattle Economic Council: 2007; Souix Falls (South Dakota) City club: 2000; St. Louis Police Officers Association: 1999; Sunday Morning Breakfast Club (Philadelphia): 2000; U.S. Department of Education: 1988; U.S. Department of Justice: 1987, 1988, 1989, 1991, 1993; U.S. Office of Management and Budget: 1991; U.S. Securities and Exchange Commission: 1988, 1989; World Affairs Council of Philadelphia: 1993.

Conferences (Excluding Multiple Presentations at a conference):

American Bar Association's conference on Gun & Media Violence - Issues for the Litigator: 2001; American Criminology Society: 1996, 1998, 2003; American Economics Association: 1993, 1995, 1996, 1997, 1998, 1999, 2001, 2011; American Enterprise Institute's Panel to Discuss my book entitled More Guns, Less Crime: 1998; American Enterprise Institute's Panel to Discuss my paper on multiple victim shootings: 1999; American Enterprise Institute Conference on "Guns, Crime, and Safety": 1999; American Law and Economics Association Annual Meeting: 1993, 1994, 1995, 1996, 1997, 2000, 2001; American Statistical Association: 2001; Association of American Law Schools Meetings: 1999; Association of Managerial Economists Meetings: 1993; Association of Private Enterprise Economists: 2001 (Luncheon Speaker); Atlantic Economic Association Meetings: 1993; Cato Institute's Conference on The U.S. Sentencing Commission's Corporate Penalty Guidelines: 1991; Centre for Economic Policy Research Conference on the Economics of Organized Crime (Bologna, Italy): 1993; Constitutional Rights Foundation Youth Summit (Chicago), 2006; Cornell Political Forum: 2000; Economic Science Association Meetings: 1989; Federalist Society Faculty Division Conference: 1999; Federalist Society Southern Leadership: 1999; Firearm Safety Seminar (Sponsored by the New Zealand Police): 2006; Guns, Crime, and Punishment in America (University of Arizona): 2001; Handgun Control, Inc. Sponsored Debate on my Concealed Handgun Research: 1996; Harvard Law School Conference on the Economics of Law Enforcement: 1998; Heritage Foundation, Legal Strategy Form: 1999; International Symposium on Forecasting: 2007; Law and Economics Center's Conference to Discuss the fourth Edition of Economic Analysis of Law by Richard Posner: 1993; Law and Economics Center's National Conference on Sentencing of the Corporation: 1990; Missouri Farmer's Association: 2005; Neoliberal Policies for Development: Analysis and Criticism, sponsored by the Program for Studies in Capitalism, Yale University and Faculdade de Direeito da Universidade de Sao Paulo (Sao Paulo, Brazil): 2000; Penalties: Public and Private (held at the University of Chicago): 1997; National Lawyers Convention: 1999; Public Choice: 1985, 1986, 1987, 1988, 1989, 1990, 1991, 1992, 1994, 1995, 1996, 1997, 1999, 2001, 2005, 2011; Southern Economic Association: 1991, 1997; Strategy and Politics sponsored by the University of Maryland Collective Choice Center: 1996; Symposium on the Economic Analysis of Social Behavior to honor Gary Becker's 65th Birthday: 1995; Symposium on Election Law, Federalist Society National Conference: 2000; Symposium on Guns and Liability in America at the University of Connecticut School of Law: 2000; Western Economic Association: 1983, 1984, 1985, 1986, 1987, 1988, 1989, 1990, 1991, 1992, 1993, 1994, 1995, 1996, 1997, 2007; Wharton Health Care Conference: 1992; Young Republican Conference, 2006.

PRESENTATIONS AT UNIVERSITIES (REFERS TO BUSINESS SCHOOLS OR ECONOMICS DEPARTMENTS UNLESS NOTED OTHERWISE AND EXCLUDES PRESENTATIONS AT HOME INSTITUTION):

Arizona State University: 1988, 1989, 1991, 1998; Arizona State University Law School: 2016; Auburn University: 1999; Australian National University: 1996; Baylor University: 2007; Baylor University Law School: 2004, 2016; Boston University School of Law: 1998; Boston University Law School: 2000; Brigham Young University: 1992; Brigham Young University Law School: 2002; California State University, East Bay: 2008; Campbell University Law School: 2008; Carnegie Mellon University: 1994; Case Western Reserve University Law School: 2002, 2012, 2015; Catholic University School of Law: 2003; Chicago-Kent School of Law: 2000; Claremont Graduate School: 1989, 1998, 2012; Clemson University: 1988, 2008, 2010; Columbia University School of Law: 1999, 2003; Cornell University: 1989, 1994; Cornell University School of Law: 1994, 1998, 2005; California State University at Hayward: 1987, 1993, 2004, 2010; California State University at Northridge: 1984; Capitol University: 2000; Cardozo School of Law: 1999, 2000, 2012; Catholic University Law School: 2003; Chapman University School of Law: 2005; Dartmouth College: 1985, 2000; Detroit Mercy Law School: 2007; Duke University School of Law: 1998, 2003, 2008; Duquesne Law School: 2012, 2015; Elon University Law School: 2008; Emory University: 1993, 1996; Florida Coastal University School of Law: 2006, 2009; Florida International University School of Law: 2006; Florida State University School of Law: 2005, 2009, 2016; Florida State University: 2005, 2009; Fordham University School of Law: 1996; Furman University: 2009; George Mason University: 1988, 1989, 1991, 1994, 1995, 2001; George Mason University School of Law: 1988, 1990, 1992, 1993, 1996, 1999, 2001, 2005, 2008; George Washington University Law School: 2000; Georgetown University School of Law: 1997, 2000; Golden Gate Law School: 2012; Gonzaga University: 2003, 2010; Hamline University School of Law: 2007; Harvard University School of Law: 1996, 1999; University of Hawaii Law School: 2011, 2011; Hillsdale College: 2004, 2007; Hitotsubashi University: 2007; Hoover Institution, Stanford University: 2001; Indiana University: 1994; Indiana University Law School: 2016; Indiana University-Purdue University at Indianapolis: 1997; Johns Hopkins University: 2001; Lewis & Clark Law School: 2004, 2007; Louisville University: 1999; Loyola College (Maryland): 2000; McGeorge Law School: 2012; McKendree College: 2001; Michigan State University: 1997; University of Minnesota School of Law: 2007; Montana State University: 1986; University of Nevada at Las Vegas: 2010; New York University: 1988; New York University School of Law: 1998, 2001, 2008; North Carolina State University: 1990; Northwestern University: 1994; Northeastern University School of Law: 2016; Northwestern University School of Law: 1993, 1996, 2008; Northwestern University Medical School: 2003; Notre Dame University: 1995; Notre Dame University Law School: 2008; Nova Southeastern Law School: 2005; Ohio State University: 1989, 2000; Ohio State University Law School: 2000, 2003; Ohio University: 2003, 2004; Oklahoma City University School of Law: 1999; Pennsylvania State University: 1999; Pepperdine University: 1989; Pepperdine University Law School: 2012; Rice University: 1998; Roger Williams Law School: 2016; Rutgers University: 1991; Rutgers University Law School: 2012; Samford University Law School: 2005; St. Mary's University Law School: 2008, 2010; Santa Clara University: 1991, 2001; Santa Clara University Law School: 2012; Savanah University Law School: 2016; Seattle University School of Law: 2003, 2007; Simon Fraser University: 1991, 2001, 2007; South Dakota University: 2000; Southern Illinois University: 2000; Southern Methodist University: 1985, 1992;

PRESENTATIONS AT UNIVERSITIES (CONTINUED):

Southern Methodist University School of Law: 1999, 2004; Stanford School of Law: 1987, 1989, 1996, 2010; SUNY - Binghamtom: 1997; Texas A&M University: 1992; Texas Tech University: 2004; Trinity College: 2000; Tulane University: 1989; Touro Law School: 2012; University of Akron Law School: 2012; University of Alabama Law School: 2005; University of Alberta: 1991; University of Arizona: 1991; University of Arizona School of Law: 1991; University of Baltimore Law School: 2004; University of British Columbia: 2007; University of Canterbury (New Zealand): 2006; University of Chicago: 1990, 1992, 1993; University of Chicago Law School: 2004; UC Berkeley Law School: 1998, 2001, 2001, 2010; UC Davis: 1987, 1993, 1998; UC Irvine: 1989, 1991; UCLA: 1989, 1993, 1995, 1997; UCLA School of Law: 1991, 2005; UC Santa Barbara: 1987, 1990; University of Colorado at Bolder: 2016; University of Florida: 1988, 2000, 2009; University of Florida Law School: 2009; University of Georgia: 1992, 1993; University of Houston: 1984, 1985, 1986, 1987, 1992, 1999, 2010; University of Houston Law School: 1999; University of Idaho: 2003; University of Idaho Law School: 2004, 2010, 2012; University of Illinois (Champaign-Urbana): 1994; University of Illinois School of Law (Champaign-Urbana): 1998; University of Illinois (Chicago): 1996; University of Iowa School of Law: 2003; University of Kansas: 1999; University of Kansas School of Law: 1999; University of Kentucky: 1995, 2008; University of Maine Law School: 2012; University of Maryland Computer Science Department: 2004; University of Miami: 1989, 1990, 1998, 2005, 2006, 2009; University of Miami School of Law: 2005; University of Michigan: 1995, 1997, 2001; University of Michigan School of Law: 2000, 2001; University of Missouri at St. Louis: 2001; University of Memphis: 2005; University of Montana School of Law: 2003; University of Nebraska: 2016; University of New Hampshire Law School: 2016; University of New Mexico: 1984; University of Oklahoma: 1999; University of Oklahoma School of Law: 1999, 2012; University of Oregon School of Law: 2003, 2007; University of Pennsylvania: 1991; University of Pennsylvania School of Law: 1999; University of San Diego School of Law: 2000, 2003, 2005, 2008; University of South Carolina: 2005, 2010; University of Southern California: 1990, 1999; University of Southern California School of Law: 1999, 2012; University of Utah School of Law: 2002; Saint Cloud State University: 2002; St. Louis University: 2001; St. Louis University (general University talk): 2002; St. Mary's University Law School: 2015; St. Thomas University Law School: 2015; Stetson University School of Law: 2006, 2009; Thomas Cooley Law School (Grand Rapids: 2007) (Lansing: 2007) (Oakland: 2007); University of Tennessee School of Law: 2005; University of Texas (Austin): 1985, 2004, 2007; University of Texas School of Law (Austin): 1999, 2004, 2010, 2016; University of Texas (Dallas): 1992, 1998, 2007; Tiffin University: 1999; University of Tokyo School of Law: 2007; University of Toronto: 1991, 1995; University of Tulsa: 2012; University of Virginia: 1988; University of Virginia School of Law: 2000; University of Washington: 1990, 1997 (business school), 1997, 2007 (economics);, 2004 (Law); University of Western Ontario: 1993, 2006; Vanderbilt University School of Law: 2005; West Virginia University School of Law: 2003; Willamette University Law School: 2003, 2007; Williams College: 1999; University of Wisconsin (Madison): 1995; University of Wisconsin Law School (Madison): 1999; University of Wisconsin (Milwaukee): 1992; Virginia Polytechnic Institute: 1988; Wake Forest University: 1998; Washington State University: 1990;Washington University in St. Louis: 2000;

PRESENTATIONS AT UNIVERSITIES (CONTINUED):

Washington University in St. Louis School of Law: 2003; Wayne State University: 2000; Widener University Law School: 2016; College of William and Mary: 1999, 2006; William and Mary School of Law: 1999, 2007; William Mitchell Law School: 2003; Xavier University: 2004; Yale University School of Law: 1985, 1996.

LEGISLATIVE TESTIMONY:

U.S. House of Representatives, Subcommittee on Health and the Environment, Public Hearing on Prescription Drugs in the Health Security Act, Tuesday, February 8, 1994.

U.S. House of Representatives, Subcommittee on Health, Committee on Ways and Means, Public Hearing on Alternative Health Reform Proposals, Thursday, February 10, 1994.

Nebraska State Senate, Judiciary Committee, Public Hearings on Concealed Handgun Permits, Thursday, February 6, 1997 (lead witness).

Kansas State Senate, State and Federal Affairs Committee, Public Hearings on Concealed Handgun Permits, Monday, February 10, 1997 (lead witness).

Kansas State House, State and Federal Affairs Committee, Public Hearings on Concealed Handgun Permits, Monday, February 10, 1997 (lead witness).

Illinois State House, Transportation Committee, Public Hearings on Concealed Handgun Permits, Tuesday, March 18, 1997 (lead witness).

California State Assembly, Committee on Public Safety, Public Hearings on Concealed Handgun Permits, Tuesday, November 18, 1997.

City of Toledo (Ohio), City Council; Public Hearings on Ordinances to require handgun registration, require gun locks, and ban assault weapons; Monday, Dec. 13, 1998 (lead witness).

Minnesota Joint State Assembly and Senate Hearing, Committee on Public Safety, Public Hearings on Concealed Handgun Permits, February 19, 1999 (lead witness).

Ohio State House, Judiciary Committee, Public Hearings on Concealed Handgun Permits, Tuesday, March 2, 1999 (lead witness).

Maryland State House, Judiciary Committee, Public Hearings on Concealed Handgun Permits, Wednesday, March 17, 1999.

Maryland State Senate, Judicial Procedures Committee, Public Hearings on Concealed Handgun Permits, Wednesday, March 17, 1999 (lead witness).

LEGISLATIVE TESTIMONY (CONTINUED):

U.S. Senate, Rules and Administration Committee, Public Hearings on Campaign Finance Reform, Wednesday, March 24, 1999.

Michigan State House, Conservation and Outdoor Recreation Committee, Public Hearings on Concealed Handgun Permits, Wednesday, April 22, 1999 (lead witness).

U.S. House of Representatives, Judiciary Committee, Subcommittee on Crime, Public Hearing on Gun Control Legislation, Thursday, May 27, 1999.

Utah Joint State Assembly and Senate Hearing, Committees on the Judiciary and Law Enforcement, Wednesday, July 21, 1999 (lead witness).

Hawaii State Senate Joint Committee Hearing, Committee on the Judiciary and Committee on Transportation and Intergovernmental Affairs, Tuesday, February 15, 2000.

Maryland State House, Budget Committee, Public Hearings on Tax Credit for Gun Locks, Wednesday, February 16, 2000

Wisconsin State House, Judiciary and Privacy Committee, Public Hearings on Concealed Handgun Law, Tuesday, February 29, 2000.

Maryland State Senate, Judicial Procedures Committee, Public Hearings on Smart Gun Locks, Wednesday, March 15, 2000.

New Jersey State Senate, Law and Public Safety Committee, Public Hearings on Smart Gun Locks and raising the age at which a gun can be purchased to 21, Thursday, May 15, 2000.

Ohio State House, Civil and Commercial Law Committee, Public Hearings on Concealed Handgun Law, Wednesday, June 13, 2001 (lead witness).

U.S. Senate, Rules and Administration Committee, Public Hearing to examine a report from the U.S. Commission on Civil Rights regarding the November 2000 election and election reform in general, Wednesday, June 27, 2001.

U.S. House of Representatives, Judiciary Committee, Public Hearing on the "Help America Vote Act of 2001," Wednesday, December 5, 2001.

Wisconsin State Senate, Judiciary Committee, Public Hearings on Concealed Handgun Law, Saturday, March 9, 2002.

Ohio State Senate, Judiciary – Civil Justice Committee, Public Hearing on Concealed Handguns – License to Carry, Wednesday, May 22, 2002.

LEGISLATIVE TESTIMONY (CONTINUED):

Maryland State House, Judiciary Committee, Public Hearings on Concealed Handgun Law, Wednesday, March 16, 2004.

U.S. Election Assistance Commission, Public Meeting on Voting Systems, Wednesday, February 23, 2005.

U.S. House of Representatives, House Government Reform Committee, Public Hearing on the "District of Columbia Gun Ban," Tuesday, June 28, 2005.

U.S. Election Assistance Commission, Public Meeting on Voting Systems, Wednesday, August 23, 2005.

Illinois State House, Special Committee on Concealed Carry, Monday, April 10, 2012 (lead witness).

Governor's School Safety Task Force, Virginia, Hearing to examining Delegate Bob Marshall's bill to allow teachers to carry guns on school property, January 27, 2013, 5:30 to 6:00 PM.

U.S. Senate Judiciary Committee's Subcommittee on the Constitution, Civil Rights, and Human Rights, Hearing on "'Stand Your Ground' Laws: Civil Rights and Public Safety Implications of the Expanded Use of Deadly Force," October 29, 2013.

Joint Mexican Senate and House Constitution Committee Hearing on rewriting Article 10 of the Mexican Constitution that deals with gun ownership, November 16, 2016.

COURT TESTIMONY:

California Pro-life Council Political Action Committee v. Jan Scully, et al., United States District Court, Eastern District of California, NO. CIV. S-96-1965 LKK/DAD.

Colorado Right to Life Committee v. Buckley, United States District Court, District of Colorado, Case No. 96-S-2844.

Florida Right to Life, Inc., et al. v. Sandra Mortham, etc. et al., United States District Court, Central District of Florida , No. 98-770-CIV-ORL-19A.

Montana Right to Life Association et. al. v. Robert Eddelman et. al., United States District Court, District of Montana, NO. CIV. 96-165-BLG-JDS.

Daggett v. Webster, 74 F.Supp.2d 53(D.Maine 1999) and 81 F.Supp.2d 128 (D.Maine 2000).

Marcella Landell, et al. v. William Sorell, et al., United States District Court, District of Vermont , 118 F.Supp.2d 459 (D.Vt 2000).

Stewart, et. al. v. J. Kenneth Blackwell, et al., United States District Court, Northern District of Ohio, Eastern Division (February 20, 2004 and September 30, 2004), Case No. 5:02 CV 2028.

DEPOSITIONS ONLY:

Expert for the Virginia State Attorneys General, State Legislative Redistricting, September 2001.

Expert for *National Middle School Association v. Lloyds London,* Franklin County Court of Common Pleas, August, 2012.

CONFERENCES ORGANIZED AND RAISED MONEY:

Corporate Sentencing: The Guidelines Take Hold, held at the Four Seasons Hotel under the auspices of the Cato Institute, October 31, 1991.

"Penalties: Public and Private," held at the University of Chicago, December 1997 and published in the ***Journal of Law and Economics***, June 1999.

"Guns, Crime, and Safety," held at the American Enterprise Institute and co-sponsored with the Yale Law School, December 1999 and published in the ***Journal of Law and Economics***, October 2001.

SELECTED PUBLIC APPEARANCES (all times are EST, except where otherwise noted, primarily limited to National appearances):

C-SPAN, Press conference presenting letter that I authored which was signed by 565 economists on President Clinton's Health-care Plan, 8:00 AM, Friday, January 14, 1994 and 2:00 PM, Sunday, January 16, 1994.

CNN and **CNN** Headline News, Thursday, January 13, 1994.

All Things Considered, National Public Radio, Thursday, January 13, 1994.

Market Place, Public Radio International, Thursday, January 13, 1994.

The Nightly Business Report, National Public Television, Thursday, January 13, 1994.

At least 50 local television stations around the nation covered the letter, from Thursday, January 13, 1994 to Friday, January 14, 1994.

Appeared on 61 radio talk shows around the nation to discuss the economists' letter on President Clinton's Health-care Plan from Friday, January 14, 1994 to Friday, January 28, 1994.

CNN, Inside Business, Sunday, January 23, 1994.

New Jersey Network and National Empowerment Television, National Policy Forum "Taxpayers, Speak Out!: A National Tax Day Policy Forum," 1 to 3 PM, Friday, April 15, 1994.

ABC National Evening News, Friday, August 2, 1996.

SELECTED PUBLIC APPEARANCES (CONTINUED):

British Broadcasting Corporation Radio, Sunday, August 4, 1996.

Monitor Radio, Thursday, August 8, 1996.

MSNBC, 10:05 AM and 3:05 PM, Thursday, August 8, 1996.

NBC National Evening News, Thursday, August 8, 1996.

CNN and **CNN Headline News**, Thursday, August 8, 1996.

NBC - Nightside, Friday, August 9, 1996.

NBC News at Sunrise, Friday, August 9, 1996.

NBC Today Show, twice, Friday, August 9, 1996.

C-SPAN, Presentation of Concealed Handgun Study, 11:30 AM, Friday, August 9, 1996.

At least 41 local television stations around the nation (along with radio stations in at least New York, Los Angeles, Chicago, Hartford, and Miami) covered the findings of my concealed handgun study, from Thursday, August 8, 1996 to Saturday, August 10, 1996.

Appeared on approximately 100 radio talk shows around the nation to discuss the concealed handgun study from Thursday, August 8, 1996 to December 31, 1996.

Australian Broadcasting Corporation, The Breakfast with Peter Thompson, National Morning Radio Broadcast, 8:05 AM, Thursday, August 15, 1996, Sydney time.

As It Happens, Canadian Broadcasting Corporation, Tuesday, August 20, 1996.

Morning Edition, "Study Says Concealed Weapons Law Decreases Crime Rate," National Public Radio, Monday, September 23, 1996, 6:40, 8:40, and 10:40 AM.

CNN Early Prime, "Handguns are Becoming More Powerful, But More Compact," Monday, October 14, 1996, 4:38 PM.

British Broadcasting Corporation Radio, Wednesday, October 16, 1996.

C-SPAN, Presentation of Concealed Handgun Study, 9:00 AM, 2:30 PM, and 9:30 PM, Monday, December 9, 1996; 2:30 AM Tuesday, December 10, 1996; and 5:36 PM Saturday, December 14, 1996.

Morning Edition, National Public Radio, "Concealed Weapons Laws," Tuesday, December 10, 1996, 10:00 AM.

MSNBC, 4:08 to 4:20 PM, Monday, February 24, 1997.

SELECTED PUBLIC APPEARANCES (CONTINUED):

The O'Reilly Report, ***Fox News*** Channel, "Concealed Handguns and Crime," 6:10 to 6:17 PM, Thursday, April 10, 1997.

The Impact of Term Limits, C-SPAN, 11:00 AM, Wednesday, August 27, 1997; 7:00 AM Thursday, August 28, 1997.

The Hannity and Colmes Show, ***Fox News*** Channel, 9:30 to 10:00 PM, Tuesday, October 7, 1997.

Appeared on approximately 160 radio talk shows and 13 local television shows around the nation to discuss my book entitled More Guns, Less Crime, from Monday, March 30, 1998 to July 1, 1998.

The O'Reilly Factor, ***Fox News*** Channel, 8:40 to 8:45 PM, Monday, March 30, 1998.

The John Robbie Morning Radio Show (Johannesburg, Pretoria, and Cape Town), 5:44 to 5:55 AM (South Africa), Wednesday, April 15, 1998.

The John Mason Morning Radio Show (Cape Town), 6:38 to 6:49 AM (South Africa), Friday, April 17, 1998.

America's Voice Tonight, America's Voice Channel, 8:35 to 8:45 PM, Tuesday, April 28, 1998.

MSNBC, 4:10 to 5:30 PM, Thursday, May 21, 1998.

SkyNews Television, 4:51 to 4:55 PM (London), Friday, May 22, 1998.

Fox News Channel, 2:40 to 2:50 PM, Friday, May 22, 1998.

Anne Petrie's TalkTV, Canadian Broadcasting Corporation Newsworld, 6:15 to 6:50 PM, Monday, May 25, 1998.

American Family, America's Voice Channel, 9:07 to 9:30 AM, Thursday, June 11, 1998.

About Books, Discussion of More Guns, Less Crime at the American Enterprise Institute, ***C-SPAN2***, 9:00 PM to 10:30 PM, Saturday, June 20, 1998; 12:00 AM to 11:30 AM and 10:00 AM to 11:30 AM, Sunday, June 21, 1998.

The Today Show, ***NBC***, 7:40 to 7:45 AM, July 21, 1998. Replayed several times on ***MSNBC*** on July 21, 1998.

Interview with Philipa Thomas, The World Today, British Broadcasting Corporation, August 12, 1998.

SELECTED PUBLIC APPEARANCES (CONTINUED):

Interview with Fred Graham, Washington Watch, ***Court TV***, Friday, August, 28, 1998, 7:30 PM; Saturday, August, 29, 1998, 5:30 PM; and Sunday, August, 30, 1998, 5:30 PM.

Politics, Canadian Broadcasting Corporation Newsworld, Tuesday, September 22, 1998, 5:10 to 5:20 PM.

Newsworld Reports, Canadian Broadcasting Corporation Newsworld, Tuesday, September 22, 1998, 7:05 to 7:15 PM.

National Magazine, Canadian Broadcasting Corporation, Tuesday, September 22, 1998, 9:10 to 9:20 PM.

The ***CBS Morning News***, "Cities to go after gun makers to convince them to change manufacturing and distribution policies," Friday, December 11, 1998, 7:00 AM.

About Books, More Guns, Less Crime, ***C-SPAN2***, 10:50 AM to 12:17 PM and 3:12 PM to 4:39 PM, Saturday, February 20, 1999; 7:00 AM to 8:27 AM, Sunday, February 21, 1998.

Armstrong Williams Show, America's Voice Channel, 6:40 to 7:00 PM, Monday, March 15, 1999.

Howard Stern Radio Show, 8:10 to 8:35 AM, Tuesday, March 30, 1999.

Michael Medved Radio Show, 5:07 to 5:45 PM, Wednesday, April 6, 1999.

"Should We Have More Gun Control?" Debates/Debates, National Public Television, Week of April 21, 1999.

Interview, Canadian Television News, 8:05 to 8:12 PM, Thursday, April 22, 1999.

Rush Limbaugh Radio Show, 2:30 to 2:45 PM, Friday, April 23, 1999.

Sunday Morning Live, Canadian Broadcasting Corporation, 10:00 to 10:10 AM, Sunday, April 24, 1999.

Fox News Channel, 10:05 to 10:10 PM, Tuesday, April 27, 1999.

MSNBC, 6:08 to 7:00 PM, Tuesday, April 27, 1999.

CNN's "Talkback Live," 3:00 to 4:00 PM, Wednesday, April 28, 1999.

Fox News Channel, 10:15 to 10:30 AM, Thursday, April 27, 1999.

NBC National Evening News, Friday, April 30, 1999.

SELECTED PUBLIC APPEARANCES (CONTINUED):

Special on Columbine School Shooting, selective segments, ***Fox News*** Channel, 11:00 to 11:30 PM, Friday, April 30, 1999.

MSNBC, 11:45 to 11:55 AM, Saturday, May 1, 1999.

Fox News Channel, 12:35 to 12:45 PM, Monday, May 3, 1999.

The Fox News Report, ***Fox News*** Channel, 7:35 to 7:45 PM, Monday, May 3, 1999.

MSNBC, 11:45 to 11:55 AM, Sunday, May 9, 1999.

CNN & Company, 11:30 AM to 12:00 PM, Thursday, May 20, 1999.

Interview with Tony Snow, ***Fox News*** Channel, 10:20 to 10:30 AM, Friday, May 21, 1999.

Fox News Channel, 3:10 to 3:16 PM, Friday, May 21, 1999.

Internight with John Gibson, ***MSNBC***, 7:00 to 7:30 PM, Friday, May 21, 1999.

Fox News Channel, 4:10 to 4:15 PM, Sunday, May 23, 1999.

Hardball with Chris Matthews, ***CNBC***, Wednesday, May 26, 1999, 8:00 PM.

C-SPAN, Testimony Before House Judiciary Committee's Subcommittee on Crime, 2:00 PM, Thursday, May 27, 1999; 12:16 AM Friday, May 28, 1999; 2:00 PM Saturday, May 29, 1999.

Fox News Channel, 9:50 to 9:55 AM, Tuesday, June 1, 1999.

Fox News Channel, 1:40 to 1:45 PM, Wednesday, June 16, 1999.

"Gun Control Debate: Good Legislation or Bad Legislation?" CNN Today, ***CNN***, 1:29 TO 1:38 PM, Friday, June 18, 1999.

Hardball with Chris Matthews, ***CNBC***, Tuesday, June 22, 1999, 8:00 PM.

"Airlines and Government," ***C-SPAN3***, 11:30 AM to 1:30 PM, Monday, June 28, 1999; ***C-SPAN***, 9:20pm to 11:19 PM, Monday, June 28, 1999; ***C-SPAN2***, 1:20 to 3:20 AM, Tuesday, June 29, 1999.

"Gun Life," ***A&E*** series on "Guns in America," 9:00 to 10:00 PM, Monday, June 28, 1999.

The Hannity and Colmes Show, ***Fox News*** Channel, 9:20 to 9:40 PM, Wednesday, August 11, 1999.

SELECTED PUBLIC APPEARANCES (CONTINUED):

Hardball with Chris Matthews, ***CNBC***, Wednesday, August 18, 1999, 8:40 to 9:00 PM.

"*Buckley V. Valeo* Revisited: Are Existing Campaign Contribution Limits Consistent with the First Amendment?" ***C-SPAN1***, 2:30 to 3:40 PM, Friday, September 10, 1999; ***C-SPAN1***, 3:48 to 5:00 AM, Tuesday, September 14, 1999.

"Gunmakers Coming Under Increasing Pressure From All Areas to Make Guns Safer," ***National Public Radio***, Thursday, December 9, 1999.

"Watch It" with Laura Ingraham, ***MSNBC***, 6:15 to 6:30 PM, Thursday, December 9, 1999.

"Guns, Crime, and Safety," Conference at the American Enterprise Institute, ***C-SPAN1***, 3:30 PM to 5:40 PM, Friday, December 10, 1999; C-SPAN2, 8:00 AM to 10:40 AM, Monday, December 13, 1999. Also carried on C-SPAN radio.

"A Society of Violence," segment on The NewsHour with Jim Lehrer, 6:32 to 6:50 PM, Thursday, December 16, 1999.

"The spotlight is back on US gun laws," Australian Broadcasting Corporation's AM program, 8:20 to 8:25 AM AEDT, March 2, 2000.

"The Diane Rehm Show," National Public Radio, 11:00 to 11:50 AM, Thursday, May 11, 2000.

"The Michael Reagan Show," 7:30 to 8:00 PM, Friday, May 12, 2000.
"The News with Brian Williams," ***MSNBC***, 9:15 to 9:22 PM, Friday, May 12, 2000.

"Armed Informed Mothers March" ***C-SPAN1***, 4:00 to 6:00 PM, Sunday, May 14, 2000; ***C-SPAN1***, 2:00 to 4:00 AM, Monday, May 15, 2000; ***C-SPAN2***, and 10:09 PM to 12:44 AM, Monday, May 15, 2000.

Australian Broadcasting Corporation's "Late Line," 10:45 to 11:15 AEDT, Monday, May 15, 2000.

"Special Report with Brit Hume," ***Fox News*** Channel, 6:18 to 6:23 PM, Monday, May 15, 2000.

Gun Buyback Programs," Fox Report, ***Fox News*** Channel, 7:15 to 7:17 PM, Thursday, June 1, 2000.

The Hannity and Colmes Show, ***Fox News*** Channel, 9:00 to 9:15 PM, Friday, June 3, 2000.

Think Tank with Ben Wattenberg, ***National Public Television***, Week starting June 8, 2000.

SELECTED PUBLIC APPEARANCES (CONTINUED):

About Books, Discussion of More Guns, Less Crime at the Cato Institute, ***C-SPAN2***, 1:20 PM to 3:10 PM, Sunday, July 10, 2000.

Fox Report, ***Fox New*** Channel, 7:50 to 7:55 PM, Thursday, October 19, 2000.

"The Election and Guns," ***CNN NewsStand***, CNN, 10:32 to 10:55 PM, Tuesday, October 24, 2000 and 1:32 to 1:55 AM, Wednesday, October 25, 2000.

Politically Incorrect, ***ABC***, 12:05 to 12:35 PM, Monday, February 26, 2001.

"Reaction to Bancroft Winner Arming America," Columbia College Conservative Club, ***C-SPAN2***, 9:45 to 11:05 PM, Sunday, April 29, 2001; ***C-SPAN2***, 5:30 to 6:50 PM, Monday, April 30, 2001.

Special Report with Brit Hume, ***Fox News*** Channel, 6:25 to 6:27 PM, Tuesday, May 1, 2001.

"Zero Tolerance Policy," The News with John Gibson, ***Fox News*** Channel, 5:35 to 5:38 PM, Tuesday, June19, 2001.

"Shifting Gun Control Policy," To the Point, Public Radio International, 2:30 to 2:50 PM, Thursday, August 9, 2001.

"Should Guns be Allowed Near Schools?" interviewed by Rick Sanchez, ***MSNBC***, 1:33 to 1:38 PM, Monday, August 21, 2001.

The Sean Hannity Radio Show, 3:35 to 3:50 PM, Friday, September 28, 2001.

"Can Guns Stop Terrorists?" interviewed by Neil Cavuto, ***Fox News*** Channel, 4:43 to 4:46 PM, Monday, October 1, 2001.

"The Terrorist Threat," ***MSNBC***, Thursday, October 11, 2001, 4:32 to 5:00 PM.

"Mischaracterization of History?" ***Fox News*** Channel, Wednesday, January 9, 2002, 7:50 to 7:53 PM.

"Study: Guns No Safer When Locked Up," Special Report with Bret Hume, Fox News Channel, Friday, July 5, 2002, 6:35 to 6:38 PM and July 6, 2002, 12:35 to 12:38 AM; also presented on Fox Fair & Balanced, Fox News Channel, 3:55 to 3:58 PM.

"The Abrams Report," MSNBC, Monday, October 7, 2002, 6:25 to 6:28 PM.

Fox News, Friday, October 11, 2000, 1:50 to 1:56 PM.

"Ballistic Fingerprinting," Connie Chung Tonight, CNN, Wednesday, October 16, 2002, 8:33 to 8:40 PM.

SELECTED PUBLIC APPEARANCES (CONTINUED):

Laura Ingraham Radio Show, 8:41 to 8:57 PM, Wednesday, October 16, 2002.

"Advocates of Gun Control Use Killing Spree to Take Aim at Gun Lobby," The Big Story with John Gibson, Fox News Channel, Friday, October 18, 2002, 5:13 to 5:16 PM; also replayed on The Fox Report with Shepard Smith and other segments that day.

Dennis Praeger Radio Show, 12:05 to 12:55 PM, Tuesday, October 22, 2002.

"Interview with John Lott," Special Report with Bret Hume, Fox News Channel, Tuesday, October 22, 2002, 6:19 to 6:29 PM.

CNNfn, Thursday, October 24, 2002, 11:50 to 11:59 AM.

"Political Bias in Publishing," Fox Report, Saturday, January 19, 2003, 8:24 to 8:26 PM.

Buchanan & Press, MSNBC, Monday, May 26, 2003, 3:30 to 3:35 PM.

Scarborough Country, MSNBC, Tuesday, June 17, 2003, 10:03 to 10:11 PM and Wednesday, June 18, 2003, 1:03 to 1:11 AM.

Hardball with Chris Matthews, MSNBC, Monday, June 30, 2003, 7:30 to 7:38 PM and 11:30 to 11:38 PM.

News Conference, Armed Pilots Program, Airline Pilots Security Alliance, C-SPAN2, 5:12 AM to 5:52 AM, 8:48 AM to 9:28 AM, 1:20 PM to 2:00 PM, 8:48 PM to 9:28 PM, Wednesday, August 27, 2003 and 12:28 AM to 1:08 AM, Thursday, August 28, 2003.

"Are the Skies Safe?" Lou Dobbs Moneyline, CNN and CNNfn, Tuesday, September 2, 2003, 6:27 to 6:30 PM.

"Will Democrat candidates opt out of public funding?" NPR's Marketplace, Friday, October 17, 2003.

"A 'Jobless' Recovery?" CNBC, Friday, January 23, 2004, 5:35 to 5:41 PM.

"Granny Get Your Gun," Fox Report with Shepard Smith, Fox News, Monday, February 2, 2004; also replayed on Sunday Best, Fox News Channel, February 8, 2004, 9:35 to 9:37 PM.

"Big Story: Getting A Bead On The New Gun Control Law," CNNfn, Friday, February 27, 2004, 11:05 to 11:25 AM.

"The Lars Larson Show," nationally syndicated radio show, Monday, March 1, 2004, 7:45 to 7:58 PM.

SELECTED PUBLIC APPEARANCES (CONTINUED):

"Voting Rights for Felons," National Public Radio's The Connection, Monday, April 4, 2004, 10:00 to 11:00 AM.

"Ohio's New Concealed Handgun Law," National Public Radio's Day to Day, Friday, April 8, 2004, 9:20 to 9:25 AM.

"Five years after Columbine," Tony Snow's Radio Show, Tuesday, April 20, 2004, 10:50 to 10:55 AM.

"Guns: Self Defense or Public Health Crisis," Duquesne University Debate, Pennsylvania Cable Network TV, Tuesday, April 27, 2004, 10:30 AM to 12:10 PM; Wednesday, April 28, 2004, 9:00 AM to 10:40 AM.

"The Bias Against Guns: Why Almost Everything You've Heard About Gun Control Is Wrong," Book TV, C-SPAN2, Saturday, May 15, 2004, 1:00 to 2:39 PM; Sunday, May 16, 1:00 to 2:39 AM; and Monday, May 31, 3:15 to 5:00 AM; C-SPAN Radio, Sunday, May 16, 5:00 to 6:39 PM; C-SPAN2, Saturday, August 28, 10:30 AM to 12:09 PM.

The Michael Dresser Show, Friday, May 29, 2004, 9:05 to 9:30 PM.

"Electronic Voting Machines and Fraud," State Circle, Maryland Public Television, Friday, June 4, 2004.

Geoff Metcalf, nationally syndicated radio show, Friday June 18, 2004, 8:30 to 9:00 PM.

"Preventing Another Florida?: Will the Changes Make Things Better?" C-SPAN1, Monday, June 21, 2004, 10:15 AM to 12:15 PM and 1:00 to 2:00 PM; C-SPAN3, Tuesday, June 22, 2004, 6:52 to 8:57 PM; C-SPAN2, Tuesday, June 29, 2004, 2:28 to 4:30 PM; C-SPAN3, Wednesday, June 30, 2004, 5:50 to 7:45 PM; C-SPAN3, Tuesday, August 17, 2004, 4:12 to 6:03 PM.

Linda Chavez's nationally syndicated radio show, Friday, July 2, 2004, 11:15 to 11:30 AM.

Alan Colmes' nationally syndicated radio show, Friday, July 2, 2004, 10:15 to 10:30 PM.

"Electronic Voting Machines in Florida," Nightly Business Report, National Public Television, Thursday, July 22, 2004.

"Assault Weapons Ban," On the Point, National Public Radio, Friday, August 13, 2004, 7:29 to 7:33 PM.

"The Quiet Death of the Assault Gun Ban," The Connection, National Public Radio, Friday, September 10, 2004, 10 to 11 AM.

SELECTED PUBLIC APPEARANCES (CONTINUED):

"Media Bias," MSNBC with Lester Holt, Monday, September 10, 2004, 5:45 to 5:52 PM.

"Assault Weapons Ban," KNX Radio, Monday, September 10, 2004, 6:30 to 6:38 PM.

"Assault Weapons Ban," Larry Elder's Radio Talk Show, Monday, September 10, 2004, 7:10 to 7:30 PM.

"Media Bias" CNBC, Thursday, September 16, 2004, 5:45 to 5:50 PM.

"Gun Bias and the Media," C-SPAN3, Monday, September 27, 2004, 2:00 to 3:30 PM.

"Are the Elections any more Secure than 2000?" Lou Dobbs Tonight, Thursday, October 21, 2004, 6:30 to 6:37 PM and 11:30 to 11:37 PM and Friday, October 22, 2004, 4:30 to 4:37 AM.

"Judicial Confirmation Process," C-SPAN3, Monday, February 14, 2005, 9:00 to 10:30 AM; C-SPAN2, Monday, February 14, 2005, 9:55 to 11:25 PM; C-SPAN2, Tuesday, February 15, 2005, 1:30 to 3:00 AM; C-SPAN3, Tuesday, February 15, 2005, 12:30 to 2:00 PM; C-SPAN3, Tuesday, February 15, 2005, 7:00 to 8:30 PM; and C-SPAN3, Wednesday, February 16, 2005, 7:00 to 8:30 PM.

"Michael Reagan Radio Show," Friday, February 18, 2005, 7:45 to 8:30 PM.

Joe Scaraborough's national syndicated radio show, Tuesday, March 1, 2005, 10:15 to 10:40 AM.

Linda Chavez's nationally syndicated radio show, Thursday, March 3, 2005, 8:15 to 8:30 AM.

Laura Ingraham's national syndicated radio show, Thursday, March 3, 2005, 9:35 to 9:48 AM.

Connected: Coast to Coast, MSNBC, Tuesday, March 22, 2005, 12:03 to 12:20 PM.

To the Point, Public Radio International, Tuesday, March 22, 2005, 2:03 to 12:30 PM.

Scarborough Country, MSNBC, Tuesday, March 22, 2005, 10:50 to 10:58 PM.

Interview With John Lott, The Big Story with John Gibson, Fox News Channel, Thursday, April 21, 2005, 7:50 to 7:53 PM.

"Affirmative Action Factor in Atlanta Shooting?"*Fox Report with Shepard Smith*, Fox News Channel, Tuesday, April 26, 2005, 5:44 to 5:54 PM.

"Guns," Penn & Teller: BULLSHIT!: Gun Control," Showtime, June 27, 2005, 10:00 to 10:30 PM and 11:00 to 11:30 PM.

SELECTED PUBLIC APPEARANCES (CONTINUED):

"The Truth of Statistics: Freakonomics," Culture Time, German Television 3sat-Suchmaschine, July 14th.

"Campaign Spending Limits," C-SPAN 1, Wednesday, August 3, 2005, 2:05 to 4:05 PM and C-SPAN 1, Wednesday, August 3, 2005, 4:21 to 6:21 AM.

"Price Gouging," Dateline, Radio America, September 21, 2005, 6:10 to 6:20 PM.

"Brazilian Referendum Gun Ban Won't Make Country Safer, says American," British Broadcasting Corporation, October 18, 2005.

"Guns and Road Rage," The Lars Larson Show, February 2, 2006, 7:05 to 7:25 PM.

"Convenor say gun safety conference not forum for pro-gun lobby," Radio New Zealand, February 22, 2006, 8:15 AM (New Zealand time).

Tom Gresham's Gun Talk, March 26, 2006, 2:05 to 3:00 PM (on more than 100 radio stations).

Alan Colmes' Radio Show, April 18, 2006, 11:05 to 11:30 PM.

Mayor Bloomberg's Summit on Guns, Regional News Network (Cable News Channel in New York, New Jersey, and Connecticut), April 26, 2006, 5:05 to 5:15 PM.

Concealed Handgun Laws, G. Gordon Liddy Radio Show, Talk American Radio Network, Thursday, April 27, 2006, 11:30 AM to 12:00 PM.

"Special Report with Brit Hume," Fox News Channel, 6:15 to 6:18 PM, Monday, May 20, 2006.

Gunlocks, G. Gordon Liddy Radio Show, Talk American Radio Network, Thursday, April 27, 2006, 11:20 AM to 11:30 AM.

Abortion and Crime, Janet Parshall's America, Salem Radio Network, Tuesday, September 5, 2006, 3:30 to 4:00 PM.

Abortion and Crime, Lars Larson, nationally syndicated radio show, Friday, September 22, 2006, 6:35 to 6:49 PM.

Multiple Victim Public Shootings, Dennis Prager, nationally syndicated radio show, Tuesday, October 3, 2006, 1:35 to 1:42 PM.

Electronic Voting Fraud Claims, The Greg Knapp Experience, nationally syndicated radio show, October 24, 2006, 3:35 to 3:49 PM.

SELECTED PUBLIC APPEARANCES (CONTINUED):

"Should Felons have the Right to Vote?" National Public Radio's Justice Talking, play at various times nationally during the week of October 23, 2006, debate lasted 35 minutes.

"Guns in the Workplace," the Jerry Doyle Show, February 13, 2007, 5:35 to 6:00 PM.

The Lars Larson Show, March 30, 2007, 7:35 to 8:00 PM.

The G. Gordon Libby Radio Show, April 11, 2007, 12:05 PM to 1:00 PM.

Virginia Tech Shooting, Larry Elder, nationally syndicated radio show, Monday, April 16, 2007, 4:05 to 4:42 PM.

Virginia Tech Shooting, Lars Larson, nationally syndicated radio show, Monday, April 16, 2007, 6:05 to 6:30 PM.

Virginia Tech Shooting, The Mark Levine Show, nationally syndicated radio show, Tuesday, April 17, 2007, 6:05 to 6:24 PM.

Guns and Crime, Al-Jazeera main news broadcast, Tuesday, April 17, 2007, 5:20 to 5:24 PM.

Virginia Tech Shooting, The Alan Colmes Show, nationally syndicated radio show, Tuesday, April 17, 2007, 11:05 to 11:30 PM.

Virginia Tech Shooting, Wisconsin Public Radio, statewide syndicated radio show, Wednesday, April 18, 2007, 8:05 to 8:30 AM.

Virginia Tech Shooting, Laura Ingraham, nationally syndicated radio show, Wednesday, April 18, 2007, 10:40 to 10:55 AM.

Virginia Tech Shooting, Dennis Miller, nationally syndicated radio show, Wednesday, April 18, 2007, 11:05 to 11:30 AM.

Virginia Tech Shooting, Sean Hannity, nationally syndicated radio show, Wednesday, April 18, 2007, 5:05 to 5:17 PM.

Virginia Tech Shooting, Larry Elder, nationally syndicated radio show, Wednesday, April 18, 2007, 6:40 to 6:48 PM.

Gun Control, The John Gibson Show, nationally syndicated radio show, Thursday, April 19, 2007, 7:35 to 7:47 PM.

Virginia Tech Shooting, The Jerry Doyle Show, nationally syndicated radio show, Friday, April 20, 2007, 3:05 to 3:25 PM.

SELECTED PUBLIC APPEARANCES (CONTINUED):

Gun Control, The Bill Bennett Show, nationally syndicated radio show, Monday, April 23, 2007, 7:35 to 7:47 AM.

Virginia Tech Shooting, Mancow in the Morning, nationally syndicated radio show, Wednesday, April 25, 2007, 7:07 to 7:15 AM.

The Jerry Doyle Show, May 31, 2007, 4:05 to 4:30 PM.

The G. Gordon Liddy Show, Tuesday, June 5, 2007, 11:35 AM to 12:30 PM.

Michael Medved Show, Wednesday, June 6, 2007, 5:05 to 6:00 PM.

The G. Gordon Liddy Show, Friday, June 9, 2007, 12:05 to 1:00 PM.

The Source with Paul Anderson, Sunday, June 10, 2007, 9:06 to 10:00 PM.

Mike McConnell Show, Monday, June 11, 2007, 10:06 to 10:30 AM.

Radio Rusy Humphries Show, Tuesday, June 12, 2007, 9:36 to 10:00 PM.

The Jerry Doyle Show, Wednesday, June 13, 2007, 4:35 to 5:00 PM.

The Christian Broadcasting Network, Friday, June 15, 2007, 2 minutes.

Dennis Miller, Monday, June 18, 2007, 11:15 to 11:45 AM.

Lars Larson, nationally syndicated radio show, Monday, June 16, 2007, 6:20 to 6:50 PM.

Thom Hartman, Air America, Monday, June 16, 2007, 1:06 to 1:30 PM.

"New Union Rules," Kudlow & Company, CNBC, Monday, June 21, 2007, 4:42 to 4:47 PM.

Michael Medved Show, Friday, June 29, 2007, 4:05 to 5:00 PM.

The Dennis Prager Show, July 2, 2007, 2:35 to 3:00 PM.

The Laura Ingraham Show, Monday, July 16, 2007, 11:23 to 11:30 AM.

Book TV, Discussion of Freedomnomics: Why the Free Market Works and Other Half-Baked Theories Don't at the Heritage Foundation, C-SPAN2, 11:00 AM to Noon, Sunday, August 12, 2007; Midnight to 1 AM, Sunday, August 12, 2007; and 7:00 to 8:00 PM, Saturday, August 18, 2007; and 5:00 PM to 6:0 PM, Monday, September 3, 2007.

Lars Larson, nationally syndicated radio show, Monday, August 13, 2007, 6:20 to 6:30 PM.

The Dennis Prager Show, August 13, 2007, 12:35 to 12:45 PM.

SELECTED PUBLIC APPEARANCES (CONTINUED):

The G. Gordon Liddy Show, Tuesday, August 14, 2007, 10:05 to 10:45 AM.

Washington Post Radio, Tuesday, August 14, 2007, 4:08 to 4:15 PM.

The Glenn Beck Show, CNN Headline News, Wednesday, August 29, 2007, 7:07 to 7:11 PM and 9:07 to 9:11 PM.

The Glenn Beck Radio Show, Thursday, August 30, 2007, 10:35 to 10:53 AM.

Lars Larson Radio Show, Tuesday, September 18, 2007, 6:20 to 6:30 AM.

Freedomnomics: Why the Free Market Works and Other Half-Baked Theories Don't at the Eagle Forum, C-SPAN2, 46 minutes, Friday, September 12, 2007; 7:00 PM to 7:46 PM, Sunday, November 25, 2007.

The G. Gordon Liddy Show, Monday, September 24, 2007, 10:05 to 10:45 AM.

Hugh Hewitt, Monday, October 9, 2007, 8:34 to 8:50 PM.

The Glenn Beck Show, CNN Headline News, Tuesday, October 10, 2007, 7:21 to 7:25 PM and 9:21 to 9:25 PM, Monday, October 11, 2007, 12:21 to 12:25 AM Saturday, October 13, 2007, 7:21 to 7:25 PM and 9:21 to 9:25 PM; and Sunday, October 14, 2007, 12:21 to 12:25 AM.

Should teachers carry guns in US School, World Have Your Say, BBC World Service radio, Tuesday, October 24, 2007, 1:50 to 2:00 PM.

The G. Gordon Liddy Show, Monday, November 12, 2007, 11:05 to 11:30 AM.

Lars Larson Radio Show, Monday, November 12, 2007, 6:35 to 6:50 PM.

Interview on carrying concealed handguns on university campuses, CNN Radio News, Wednesday, November 22, 2007.

Women Voting and the Growth of Government, C-SPAN 1, Thursday, November 29, 2007, 10:40 to 11:00 AM.

Women Voting and the Growth of Government, The Thom Hartmann Show, Air America, November 29, 2007, 12:05 to 12:15 PM.

The G. Gordon Liddy Show, Friday, November 30, 2007, 12:15 to 1:00 PM.

The Dennis Prager Show, Thursday, December 6, 2007, 12:15 to 12:30 PM.

Andrew Wilkow, The Wilkow Majority, Sirius Satellite Radio Patriot 144, Thursday, December 6, 2007, 1:40 to 2:00 PM.

SELECTED PUBLIC APPEARANCES (CONTINUED):

Lars Larson Radio Show, Thursday, December 6, 2007, 6:35 to 7:00 PM.

Bill Cunningham Radio Show, Premiere Radio Network, Sunday, December 9, 2007.

The Greg Knapp Experience, Thursday, December 20, 2007, 5:07 to 5:20 PM.

Lars Larson Radio Show, Monday, January 14, 2008, 6:20 to 6:30 PM.

The G. Gordon Liddy Show, Tuesday, January 15, 2008, 11:35 AM to Noon.

Dennis Miller Show, Wednesday, February 13, 2008, 10:35 AM to 10:50 AM.

The G. Gordon Liddy Show, Wednesday, February 13, 2008, 12:35 PM to 12:48 PM.

Dennis Miller Show, Monday, March 10, 2008, 11:35 AM to 11:42 AM.

Martha Zoller Show, Wednesday, March 12, 2008, 1:15 PM to 1:50 PM.

Rusty Humphries Show, Wednesday, March 12, 2008, 9:48 PM to 10:00 PM.

The G. Gordon Liddy Show, Monday, March 17, 2008, 10:06 AM to 10:30 AM.

Lars Larson Radio Show, Monday, March 17, 2008, 6:15 PM to 6:30 PM.

Mancow in the Morning, nationally syndicated radio show, Tuesday, March 18, 2008, 7:45 to 8:00 AM.

Brian and the Judge, Fox News Radio, Tuesday, March 18, 2008, 9:22 to 9:30 AM.

The Michael Reagan Show, Tuesday, March 18, 2008, 7:20 to 7:30 PM.

Debate with Paul Helmke, the president of the Brady Campaign, Bloomberg Television, Tuesday, March 18, 2008, 9:35 to 9:41 PM.

Greg Garrison Show, Wednesday, March 19, 2008, 10:35 to 11:00 AM.

The Glenn Beck Show, Wednesday, March 19, 2008, 11:06 to 11:20 AM.

Discussion on the Economy, Dennis Miller Show, Wednesday, April 2, 2008, 10:35 AM to 10:42 AM.

Discussion on the Economy, Fox & Friends, Fox News, Thursday, April 3, 2008, 6:22 AM to 6:27 AM.

"Charlton Heston and Guns," Weekend Breakfast, BBC Radio 5 Live, Sunday, April 6, 2008, 2:06 AM to 2:15 AM EDT.

The G. Gordon Liddy Show, Tuesday, April 15, 2008, 12:22 PM to 1:00 PM.

SELECTED PUBLIC APPEARANCES (CONTINUED):

The G. Gordon Liddy Show, Tuesday, April 29, 2008, 10:35 AM to 10:50 AM.

Lars Larson Radio Show, Tuesday, April 29, 2008, 7:35 PM to 7:45 PM.

The G. Gordon Liddy Show, Tuesday, May 13, 2008, 12:07 PM to 12:19 PM.

Lars Larson Radio Show, Friday, May 30, 2008, 6:23 PM to 6:30 PM.

The G. Gordon Liddy Show, Monday, June 16, 2008, 10:06 AM to 10:30 AM.

"The Cost of the Iraq War," Lars Larson Radio Show, Monday, June 16, 2008, 6:20 PM to 6:50 PM.

Supreme Court Decision on Heller, Michael Gallagher Radio Show, June 26, 2008, 10:45 AM to 10:55 AM.

Supreme Court Decision on Heller, Tom Sullivan Radio Show, June 26, 2008, 2:05 PM to 2:15 PM.

Supreme Court Decision on Heller, Kresta in the Afternoon, June 26, 2008, 4:07 PM to 4:20 PM.

Supreme Court Decision on Heller, The Jason Lewis Radio Show, June 26, 2008, 5:30 PM to PM.

Supreme Court Decision on Heller, Jerry Johnson Live, June 26, 2008, 6:15 PM to 6:30 PM.

Supreme Court Decision on Heller, The Rusty Humphries Show, June 26, 2008, 9:15 PM to 9:30 PM.

Debate with Brady Campaign President Paul Helmke over Supreme Court Decision on Heller, The Alan Colmes Show, June 26, 2008, 9:15 PM to 9:30 PM.

The Steve Malzberg Radio Show, Monday, June 30, 2008, 4:35 PM to 4:50 PM.

Discussion about profits in health care, The Mark Levin Show, July 10, 2008, 7:35 to 7:43 PM.

Discussion of Oil Company Profits, The Thom Hartmann Show, Air America, July 18, 2008, 1:06 to 1:16 PM.

Tom Gresham Radio Show, Sunday, July 27, 2008, 2:06 PM to 3:00 PM.

Lars Larson Radio Show, Thursday, July 31, 2008, 6:23 PM to 6:46 PM.

SELECTED PUBLIC APPEARANCES (CONTINUED):

"Handguns on College Campuses," a debate with Paul Helmke, president of the Brady Campaign, C-SPAN1, Friday, August 1, 2008, 1:10 to 2:15 PM; reboadcast on C-SPAN1, Friday, August 1, 2008, 9:02 to 10:07 PM.

"The Second Amendment, post Heller decision," C-SPAN2, Monday, August 4, 2008, 4:10 to 5:06 PM; rebroadcast 1:06 to 1:58 AM, Tuesday, August 19, 2008, C-SPAN2; 1:43 to 2:35 PM, Thursday, August, 21, 2008, C-SPAN2.

The Mark Levin Show, Thursday, August 28, 2008, 7:35 to 7:50 PM.

Obama on Gun Control, The Steve Malzberg Radio Show, Monday, September 1, 2008, 5:35 PM to 5:50 PM.

"The Cost of the Iraq War," Lars Larson Radio Show, Wednesday, September 17, 2008, 6:24 PM to 6:50 PM.

"The Bailout Bill," Lars Larson Radio Show, Thursday, October 2, 2008, 7:07 PM to 7:20 PM.

The Michael Medved Show, Tuesday, October 14, 2008, 5:05 PM to 6:00 PM.

The Michael Medved Show, Friday, October 24, 2008, 5:05 PM to 6:00 PM.

Jason Lewis Show, Friday, October 31, 2008, 6:05 PM to 7:00 PM.

"Minnesota Ripe for Election Fraud," Lars Larson Radio Show, Monday, November 10, 2008, 8:20 PM to 8:30 PM.

"Minnesota Ripe for Election Fraud," Glenn Beck Show, Tuesday, November 11, 2008, 10:45 to 10:52 AM.

Thom Hartmann's Show, Air America, Tuesday, November 11, 2008, 2:09 to 2:30 AM.

"Minnesota Ripe for Election Fraud," interviewed by Neil Cavuto, Fox News Network, Tuesday, November 11, 2008, 4:37 to 4:41 PM.

"Minnesota Ripe for Election Fraud," Steve Malzberg Show, Wednesday, November 12, 2008, 4:47 to 5:00 PM.

"Minnesota Ripe for Election Fraud," Bill Bennett's Morning In America, Thursday, November 13, 2008, 8:05 to 8:15 AM.

"Minnesota Ripe for Election Fraud," Mike Gallagher Show, Thursday, November 13, 2008, 9:47 to 9:55 AM.

"Minnesota Ripe for Election Fraud," Dennis Prager Show, Thursday, November 13, 2008, 2:06 to 2:16 PM.

SELECTED PUBLIC APPEARANCES (CONTINUED):

"Minnesota Ripe for Election Fraud," Fox News Live with Jamie Colby, Fox News, Sunday, November 16, 2008, 11:41 to 11: 45 AM.

Terrorist Attack in India, The Steve Malzberg Show, Wednesday, December 3, 2008, 3:35 to 3: 45 PM.

Terrorist Attack in India, The Greg Knapp Experience, nationally syndicated radio show, Friday, December 6, 2008, 4:35 to 4:43 PM.

Terrorist Attack in India, Lars Larson, nationally syndicated radio show, Friday, December 6, 2008, 7:35 to 7:45 PM.

The G. Gordon Liddy Show, Monday, December 15, 2008, 12:35 PM to 1:00 PM.

The Auto Bailout, The Steve Malzberg Show, Wednesday, December 16, 2008, 4:35 to 4: 45 PM.

Tim Farley, The Morning Briefing, POTUS, XM Radio, December 18, 2008, 6:20 AM to 6:30 AM.

The G. Gordon Liddy Show, Monday, December 22, 2008, 12:35 PM to 1:00 PM.

Minnesota Recount and Gun Control, Dennis Miller Show, Tuesday, December 23, 2008, 11:35 AM to 11:47 AM.

Minnesota Recount, the Jerry Doyle Show, Tuesday, December 23, 2008, 4:05 to 4:30 PM.

The G. Gordon Liddy Show, Tuesday, January 13, 2009, 12:22 PM to 12:30 PM.

Lars Larson Radio Show, Thursday, January 28, 2009, 6:38 PM to 6:53 PM.

Gun Control, Coast to Coast AM, Thursday, February 5, 2009, 1:17 to 2:00 AM.

Gun Control and Gangs in Canada, The Roy Green Show, Corus Radio Network (National Canadian Network), Saturday, February 7, 2009, 3:35 to 4:00 PM.

A. Gordon Liddy, Tuesday, February 10, 2009, 12:35 PM to 12:55 PM.

The Dennis Miller Show, Thursday, February 12, 2009, 10 minutes.

"The Cost of the Stimulus Bill," ***Fox News***, Monday, February 16, 2009, 11:10 to 11:12 AM; and other times during the day.

"Nationalizing Banks," Glenn Beck Show, ***Fox News***, Monday, February 16, 2009, 5:04 to 5:11 PM.

SELECTED PUBLIC APPEARANCES (CONTINUED):

The G. Gordon Liddy Show, Wednesday, February 25, 2008, 11:35 to 11:45 AM.

Mancow in the Morning, Friday, February 27, 2009, 7:55 to 8:03 AM.

The Jason Lewis Show, Thursday, March 12, 2009, 8:05 to 9:00 PM.

G. Gordon Liddy, Wednesday, April 1, 2009, 11:35 AM to 12:00 PM.

Gun Control, Coast to Coast AM, Saturday, April 4, 2009, 1:03 to 1:08 AM.

The Jason Lewis Show, Monday, April 13, 2009, 8:05 to 9:00 PM.

Washington Journal, ***C-SPAN***, Tuesday, April 14, 2009, 8:02 to 8:30 AM.

Freedomnomics and Gun Control, Coast to Coast AM, Monday, May 4, 2009, 1:15 to 4:00 AM.

Health care debate, Street Signs, ***CNBC***, Friday, May 8, 2009, 2:20 to 2:30 PM.

The Jason Lewis Show, Wednesday, May 13, 2009, 8:05 to 9:00 PM.

The Dennis Miller Show, Tuesday, May 19, 2009, 11:34 to 11:46 AM.

Gun Control, Coast to Coast AM, Thursday, May 21, 2009, 1:10 to 1:55 AM.

Lars Larson Radio Show, Friday, May 29, 2009, 6:21 PM to 6:30 PM.

"Discussing the Obama Administration's spending policy when it comes to issues such as health care, U.S. automakers, & the financial system," Washington Journal, ***C-SPAN***, Sunday, June 14, 2009, 7:30 to 8:30 AM.

G. Gordon Liddy, Wednesday, June 25, 2009, 11:35 AM to 12:00 PM.

Greg Garrison Show, Wednesday, July 1, 2009, 10:35 to 11:00 AM.

Steve Malzberg Show, Wednesday, July 1, 2009, 5:35 to 6:00 PM.

The Jason Lewis Show, Tuesday, July 14, 2009, 8:05 to 9:00 PM.

The G. Gordon Liddy Show, Monday, July 20, 2009, 11:35 AM to 12:00 PM.

Gun Control, Coast to Coast AM, Wednesday, July 22, 2009, 1:10 to 1:15 AM.

The Thom Hartmann Show, Air America, Monday, August 3, 2009, 12:06 to 12:17 PM.

The G. Gordon Liddy Show, Tuesday, August 4, 2009, 11:05 AM to 12:00 PM.

SELECTED PUBLIC APPEARANCES (CONTINUED):

The John Gibson Show, Tuesday, August 4, 2009, 1:10 to 1:20 PM.

Cash for Clunkers and Health care, ***FOX Business***, Thursday, August 6, 2009, 7:05 to 7:12 PM.

The Jason Lewis Show, Friday, August 14, 2009, 8:05 to 9:00 PM.

Greg Garrison Show, Thursday, August 20, 2009, 10:35 to 11:00 AM.

Discussion of Health Care Debate, The Thom Hartmann Show, Air America, Thursday, August 20, 2008, 12:06 to 12:16 PM.

The Thom Hartmann Show, Air America, Tuesday, September 1, 2009, 1:15 to 1:30 PM.

Greg Garrison Show, Monday, September 7, 2009, 10:05 to 11:00 AM.

The G. Gordon Liddy Show, Monday, September 7, 2009, 11:05 AM to 12:00 PM.

The Jason Lewis Show, Monday, September 7, 2009, 8:05 to 9:00 PM.

The G. Gordon Liddy Show, Thursday, September 17, 2009, 11:05 AM to 12:00 PM.

Lars Larson Radio Show, Thursday, September 17, 2009, 6:35 PM to 6:45 PM.

The Thom Hartmann Show, Air America, Monday, September 21, 2009, 1:05 to 1:15 PM.

The Dennis Miller Show, Wednesday, September 23, 2009, 11:08 to 11:18 AM.

Mancow & Cassidy, WLS, September 30, 2009, 11:33 to 11:43 AM.

The Jason Lewis Show, Monday, October 5, 2009, 8:05 to 9:00 PM.

The Thom Hartmann Show, Air America, Tuesday, October 20, 2009, 12:05 to 12:15 PM.

The Thom Hartmann Show, Air America, Monday, October 27, 2009, 12:05 to 12:15 PM.

The Thom Hartmann Show, Air America, Monday, November 9, 2009, 2:05 to 2:15 PM.

Gun Control, Coast to Coast AM, Wednesday, November 18, 2009, 1:10 to 1:15 AM.

"White House Aims to Cut Deficit With TARP Cash," ***FOX Business***, Friday, November 13, 2009, 7:02 to 7:15 PM.

The G. Gordon Liddy Show, Tuesday, November 24, 2009, 12:15 to 1:00 PM.

Climate-gate, The Ryan Doyle Show, simulcast in Montreal and Toronto, Friday, November 27, 2009, 8:05 to 8:22 PM.

SELECTED PUBLIC APPEARANCES (CONTINUED):

The Jason Lewis Show, Wednesday, December 2, 2009, 8:05 to 9:00 PM.

The Steve Malzberg Show, Wednesday, December 16, 2009, 4:34 to 4:49 PM.

The Mark Levine Show, nationally syndicated radio show, Wednesday, December 16, 2009, 8:35 to 8:45 PM.

The Thom Hartmann Show, Tuesday, January 12, 2010, 1:05 to 1:15 PM.

The Jason Lewis Show, Tuesday, January 19, 2010, 8:05 to 9:00 PM.

The Thom Hartmann Show, Wednesday, January 27, 2010, 2:05 to 2:15 PM.

Lars Larson Radio Show, Tuesday, March 2, 2010, 8:20 PM to 8:30 PM.

Supreme Court Gun Control, Coast to Coast AM, Tuesday, March 2, 2010, 1:10 to 1:55 AM.

Supreme Court Gun Control, Freedom Watch, Judge Napolitano, Tuesday, March 2, 2010, 1:15 to 1:30 PM.

Supreme Court Gun Control, Lou Dobbs Radio Show, Tuesday, March 2, 2010, 4:20 to 4:30 PM.

Supreme Court Gun Control, Lars Larson Radio Show, Tuesday, March 2, 2010, 8:20 to 8:30 PM.

Supreme Court Gun Control, Jason Lewis Radio Show, Tuesday, March 2, 2010, 8:30 to 9:00 PM.

Supreme Court Gun Control, Ave Maria Radio Show, Wednesday, March 3, 2010, 7:30 to 8:00 PM.

Knowing Barack Obama at Chicago, Mark Levin Radio Show, Wednesday, March 3, 2010, 7:30 to 8:00 PM.

Steve Malzberg Show, Monday, April 5, 2010, 5:20 to 5:30 PM.

Gun Control, Coast to Coast AM, Friday, April 9, 2010, 1:03 to 1:08 AM.

The Lars Larson Radio Show, Friday, April 16, 2010, 7:35 PM to 7:46 PM.

The Jason Lewis Show, Friday, April 16, 2010, 8:05 to 9:00 PM.

The Thom Hartmann Show, Wednesday, April 28, 2010, 12:05 to 12:15 PM.

The Jim Bohannon Show, Thursday, April 29, 2010, 10:05 to 11:00 PM.

SELECTED PUBLIC APPEARANCES (CONTINUED):

The Jason Lewis Show, Thursday, May 13, 2010, 8:05 to 9:00 PM.

The Lars Larson Show, Monday, May 24, 2010, 7:35 to 7:45 PM.

The Steve Malzberg Show, Tuesday, May 25, 2010, 4:35 to 5:00 PM.

The Jim Bohannon Show, Tuesday, May 25, 2010, 11:05 PM to 11:58 PM.

Coast to Coast AM, Wednesday, May 26, 2010, 1:10 to 1:15 AM.

The Greg Garrison Radio Show, May 26, 2010, 10:05 to 10:30 AM.

The Lou Dobbs Radio Show, Wednesday, May 26, 2010, 2:35 to 2:55 PM.

The Ed Morrissey Hot Air Radio, Wednesday, May 26, 2010, 4:05 to 4:30 PM.

The Dennis Miller Show, Friday, May 28, 2010, 11:35 to 11:57 AM.

The G. Gordon Liddy Show, Wednesday, June 2, 2010, 12:05 AM to 1:00 PM.

The Michael Savage Show, Thursday, June 3, 2010, during third hour.

The Jason Lewis Show, Monday, June 21, 2010, 8:05 to 9:00 PM.

The Steve Malzberg Show, Monday, June 28, 2010, 4:22 to 4:30 PM.

American Family Radio, Monday, June 28, 2010, 6:15 to 6:30 PM.

The Alan Colmes Show, Monday, June 28, 2010, 11:06 to PM.

Coast to Coast AM, Tuesday, June 29, 2010, 1:10 to 2:00 AM.

The Mancow Show, Tuesday, June 29, 2010, 7:05 to 7: AM.

The John McCaslin Show, Tuesday, June 29, 2010, 8:15 to 8:30 AM.

The Michael Savage Show, Tuesday, June 29, 2010, during third hour.

Discussing Supreme Court Ruling on Gun Rights, Washington Journal, C-SPAN, Wednesday, June 30, 2010, 9:15 to 10:10 AM.

Kagan's Confirmation Hearing, CBS's Unplugged, Wednesday, June 30, 2010, 1:15 to 1:50 PM.

The Jason Lewis Show, Monday, July 5, 2010, 8:05 to 9:00 PM.

Fox News' "Strategy Room," Tuesday, September 7, 2010, 9:30 to 9:42 AM.

SELECTED PUBLIC APPEARANCES (CONTINUED):

The Lou Dobbs Radio Show, Tuesday, September 7, 2010, 3:35 to 3:45 PM.

Coast to Coast AM, Wednesday, September 8, 2010, 1:10 to 3:00 AM.

Fox News' "Strategy Room," Thursday, September 23, 2010, 10:30 to 10:40 AM.

The Thom Hartmann Program, Free Speech TV Network and the RT America Channel, Friday, October 22, 2010.

The Thom Hartmann Program, Free Speech TV Network and the RT America Channel, Monday, November 1, 2010, 8:05 to 8:09 PM.

The Jason Lewis Show, Friday, November 5, 2010, 8:05 to 9:00 PM.

The Thom Hartmann Program, Free Speech TV Network and the RT America Channel, Wednesday, December 8, 2010, 7:32 to 7:42 PM.

The Jason Lewis Show, Monday, December 13, 2010, 8:05 to 9:00 PM.

The Lars Larson Show, Tuesday, December 14, 2010, 6:35 to 6:45 PM.

The Thom Hartmann Program, Free Speech TV Network and the RT America Channel, Wednesday, December 22, 2010, 7:32 to 7:42 PM.

The Thom Hartmann Program, Free Speech TV Network and the RT America Channel, Wednesday, December 29, 2010, 7:32 to 7:42 PM.

"Should the debt limit be raised?" Fox News Live with Kimberly Guilfoyle, Fox News, Friday, January 7, 2011, 1:40 to 11: 47 PM.

Discussion of Representative Gabrielle Giffords' shooting, Fox News, Sunday, January 9, 2011, 5:23 to 5:29 PM.

Armed American Radio, Sunday, January 9, 2011, 8:30 to 9:00 PM.

Arizona Shooting, Coast to Coast AM, Tuesday, January 11, 2011, 1:10 to 2:00 AM.

The Jason Lewis Show, Tuesday, January 11, 2011, 8:05 to 9:00 PM.

Gun Control Legislation after the Arizona Shooting, Bloomberg TV, Wednesday, January 12, 2011, 5:00 to 5:30 PM.

State and Federal Gun Laws, Washington Journal, C-SPAN, Thursday, January 22, 2011, 7:51 to 8:36 AM; replayed C-SPAN, Thursday, January 13, 2011, 12:17 to 1:00 PM.

The Jim Bohannon Show, Thursday, January 13, 2011, 10:05 to 11:00 PM.

SELECTED PUBLIC APPEARANCES (CONTINUED):

Fred Thompson Radio Show, Friday, January 14, 2011, 12:34 to 12:45 PM.

Rusty Humphries Radio Show, Tuesday, January 18, 2011, 4:35 to 4:45 PM.

Dennis Miller Radio Show, Wednesday, January 19, 2011, 11:07 to 11:39 PM.

The G. Gordon Liddy Show, Friday, January 21, 2009, 10:35 to 11:00 AM.

The Thom Hartmann Show, Thursday, February 3, 2011, 12:05 to 12:15 PM.

The Steve Malzberg Show, Thursday, February 3, 2011, 4:05 to 4:15 PM.

"Why Obama Can't Do the Math On Jobs?" Fox News Live, Tuesday, February 8, 2011, 11:23 to 11:30 AM.

"University of Guns," Fox Business, Thursday, February 24, 2011, 8:42 to 8:47 PM.

"Is our government seeing double?" Fox News Live, Wednesday, March 2, 2011, 12:24 to 12:40 PM.

"The Truth About Obama and Budget Cuts," Fox News Live, Wednesday, March 9, 2011, 12:50 to 12:57 PM.

The Jason Lewis Show, Friday, March 25, 2011, 8:05 to 9:00 PM.

S&P Warning on Downgrading US Debt, Fox News Live, Thursday, April 15, 2011, 10:30 to 10:45 AM.

Budget Deficit Debate, Fox News Live, Thursday, April 21, 2011, 9:35 to 9:45 AM.

Gun Ownership, Coast to Coast AM, Tuesday, May 3, 2011, 1:10 to 3:00 AM.

"Texas State Senate passes Concealed Carry on Campus," Coast to Coast AM, Tuesday, May 10, 2011, 1:08 to 1:10 AM.

"Gun Control," Freedom Watch with Judge Andrew Napolitano, Fox Business Channel, Friday, May 20, 2011, 8:40 to 8:45 PM.

The Jason Lewis Show, Wednesday, June 8, 2011, 8:05 to 9:00 PM.

Coast-to-Coast AM, Thursday, June 16, 2011, 1:15 to 2 AM.

"Winnipeg carried a handgun for protection," The Caldwell Account, Sun News Network, June 28, 2011.

"Gun Myths," The Source with Ezra Levant, Sun News Network, July 5, 2011.

SELECTED PUBLIC APPEARANCES (CONTINUED):

Discussion of Operation Fast and Furious, Fox News Live, Friday, July 8, 2011, 11:35 to 11:45 AM.

"Myths about Debt Ceiling 'Disaster'," Fox News Live, Monday, July 18, 2011, 10:30 to 10:41 AM.

Discussion of the Norway Bombing and Shooting, Al Jazeera Television, Saturday, July 23, 2011, 2:30 to 2:45 AM.

"Gang of Six Not Ready for Prime Time?" ***Fox News Live***, Monday, July 25, 2011, 10:30 to 10:41 AM.

"Winners & Losers from the Debt Limit Deal," ***Fox News Live***, Wednesday, August 3, 2011, 10:16 to 10:23 AM.

Lars Larson, Wednesday, August 3, 2011, 7:35 to 7:55 PM.z

"Guns on College Campuses," a debate between myself and the Brady Campaign's Colin Goddard took about 50 minutes of this conference, C-SPAN3, Monday, August 8, noon to 3:30 PM; replayed C-SPAN3, Tuesday, August 9, 2011, 6:44 to 10:03 AM; C-SPAN2, Tuesday, August 9, 2011, 9:57 to 11:07 AM; C-SPAN2, Wednesday, August 10, 2011, 3:45 to 5:43 PM.

Lars Larson, Monday, September 19, 2011, 6:35 to 6:45 PM.

The Jason Lewis Show, Monday, October 24, 2011, 7:05 to 8:00 PM.

The Jason Lewis Show, Friday, November 11, 2011, 8:05 to 9:00 PM.

Coast-to-Coast AM, Thursday, November 17, 2011, 1:07 to 1:10 AM.

Lars Larson, Friday, December 3, 2011, 8:20 to 8:30 PM.

The Jason Lewis Show, Friday, December 16, 2011, 8:05 to 9:00 PM.

Discussing the January Unemployment Report, The Mark Levine Show, Friday, February 3, 2012, 7:20 to 7:45 PM.

Jason Lewis Show, Friday, February 10, 2012, 8:05 to 9:00 PM.

Larry Elder Show, Tuesday, March 6, 2012, 8:35 to 8:45 PM.

Crime after DC and Chicago Supreme Court Decisions, Fox News, Thursday, March 8, 2012, 12:10 to 12:12 PM.

Jason Lewis Show, Friday, March 11, 2012, 8:05 to 9:00 PM.

SELECTED PUBLIC APPEARANCES (CONTINUED):

The Kudlow Report, ***CNBC***, Tuesday, March 13, 2012, 8:50 to 8:54 PM.

The Dennis Miller Show, Wednesday, March 14, 2012, 12:07 to 1:00 PM.

The G. Gordon Liddy Show, Thursday, March 15, 2012, 12:05 to 12:30 PM.

Michael Medved Radio Show, Monday, March 19, 2012, 5:07 to 5:55 PM.

Jason Lewis Show, Thursday, March 22, 2012, 8:05 to 9:00 PM.

The Drive Home with Steve Ray and Rachel Crowson, Tuesday, March 27, 2012, 8:15 to 8:25 PM.

The Phil Valentine Show, Thursday, March 29, 2012, 4:35 to 4:50 PM.

The Rusty Humphries Show, Friday, March 30, 2012, 6:10 to 6:20 PM.

Discussing "Debacle: Obama's War on Jobs and Growth and What We Can Do Now to Regain Our Future," Washington Journal, C-SPAN 2 Book TV, Saturday, March 31, 2012, 2:00 to 3:04 PM; Sunday, April 15th at 12:15 AM and at 10:45 AM.

Dateline Washington, Greg Comoros, national radio show, Wednesday, April 4, 2012, 3:05 to 3:15 PM.

The Dennis Miller Show, Friday, April 6, 2012, 11:35 to 11:45 AM.

America's Morning News with John McCaslin, Monday, April 9, 2012, 7:30 to 7:40 AM.

Fox News Live, Monday, April 9, 2012, 10:40 to 10:45 AM.

American Now with Andy Dean, Tuesday, April 10, 2012, 8:35 to 8:55 PM.

Point of View with Kerby Anderson, Wednesday, April 11, 2012, 2:05 to 3:00 PM.

Dateline Washington, Thursday, Wednesday, April 12, 2012, 2:05 to 2:20 PM.

The Jason Lewis Show, Friday, April 20, 2012, 7:05 to 8:00 PM.

Debate on Stand Your Ground Laws, The Sean Hannity Show, Monday, April 23, 2012, 5:07 to 5:20 PM.

Clothing being made for concealed handgun permit holders, ***BBC Radio Scotland***, Wednesday, April 25, 2012, 12:45 to 12:55 PM.

Discussion on Stand Your Ground Laws, The Phil Valentine Show, Friday, April 27, 2012, 3:35 to 3:50 PM.

SELECTED PUBLIC APPEARANCES (CONTINUED):

Debate on Stand Your Ground Laws, The Daily Rundown, MSNBC, Tuesday, May 1, 2012, 9:35 to 9:45 AM.

The Jason Lewis Show, Wednesday, May 23, 2012, 8:20 to 9 PM.

Coast-to-Coast AM, Tuesday, June 19, 2012, 1:10 to 1:12 AM.

The Jason Lewis Show, Wednesday, June 27, 2012, 7:35 to 7:50 PM.

The Lars Larson Show, Friday, July 6, 2012, 6:20 to 6:30 PM.

The Jason Lewis Show, Friday, July 6, 2012, 8:05 to 9:00 PM.

UN Arms Trade Treaty, Coast-to-Coast AM, Thursday, July 12, 2012, 1:15 AM to 2:00 AM.

UN Arms Trade Treaty, The Sean Hannity Show, Thursday, July 12, 2012, 5:05 to 5:20 PM.

Colorado Shooting, BBC World Radio Service, Friday, July 20, 2012, 12:15 to 12:50 PM.

Colorado Shooting, Rusty Humphries Show, July 20, 2012, 3:07 to 3:20 PM.

Colorado Shooting, ***BBC World Television News***, Saturday, July 21, 2012, 8:10 to 8:15 PM.

Mark Levin Radio Show, Monday, July 23, 2012, 6:35 to 6:49 PM.

Bret Baier's Special Report, ***Fox News***, Monday, July 23, 2012.

Piers Morgan Tonight, ***CNN***, Monday, July 23, 2012, 9:30 to 9:46 PM.

Mike Huckabee Radio Show, Tuesday, July 24, 2012, 12:35 to 12:45 PM.

Sean Hannity Radio Show, Tuesday, July 24, 2012, 5:05 to 5:21 PM.

Dennis Miller Radio Show, Wednesday, July 25, 2012, 11:05 to noon.

"More or Less," ***BBC Radio 4***, Wednesday, July 25, 2012.

Laura Ingraham Show, Friday, July 27, 2012, 10:35 to 10:42 AM.

Mike Huckabee Radio Show, Friday, July 27, 2012, 2:35 to 2:45 PM.

Lou Dobbs Tonight, ***Fox Business***, Friday, July 27, 2012, 7:07 to 7:15 PM.

USA Radio Network, Thursday, August 2, 2012, 1:15 to 1:40 PM.

SELECTED PUBLIC APPEARANCES (CONTINUED):

Sean Hannity Radio Show, Thursday, August 2, 2012, 5:36 to 5:52 PM.

Outfront with Erin Burnett, ***CNN***, Monday, August 6, 2012, 7:51 to 7:57 PM.

Jason Lewis Radio Show, Monday, August 6, 2012, 8:05 to 9:00 PM.

Coast to Coast AM, Tuesday, August 7, 2012, 1:05 to 1:08 AM.

Wall Street Shuffle, Tuesday, August 22, 2012, 6:35 to 6:45 PM.

Jason Lewis Radio Show, Tuesday, August 22, 2012, 8:05 to 9:00 PM.

Coast to Coast AM, Tuesday, September 4, 2012, 1:09 to 1:13 AM.

Hugh Hewitt Radio Show, Friday, September 8, 2012, 7:05 to 7:12 PM.

Coast to Coast AM, Saturday, December 1, 2012, 1:05 to 1:08 AM.

Mark Levin Radio Show, Monday, December 3, 2012, 6:05 to 6:14 PM.

Jason Lewis Radio Show, Wednesday, December 12, 2012, 8:05 to 9:00 PM.

Mark Levin Radio Show, Friday, December 14, 2012, 7:35 to 7:55 PM.

Piers Morgan Tonight, ***CNN***, Friday, December 14, 2012, 9:30 to 9:46 PM.

Coast to Coast AM, Saturday, December 15, 2012, 1:12 to 2:00 AM.

CTV (Canadian Television), Saturday, December 15, 2012, 7:15 to 7:19 PM.

State of the Union, ***CNN***, Sunday, December 16, 2012, 10:07 to 10:12 AM.

BBC World Service Radio, Sunday, December 16, 2012, 3:35 to 3:40 PM.

BBC Newsday, ***BBC***, Sunday, December 16, 2012, 9:00 to 9:05 PM.

Starting Point with Soledad O'Brien, ***CNN***, Monday, December 17, 2012, 7:33 to 7:43 AM.

"Sandy Hook Massacre Changes Gun Control Conversations," Talk of the Nation, NPR, Monday, December 17, 2012, 1:03 to 1:12 PM.

"Would more guns make America safer?" Washington Post, Monday, December 17, 2012.

Dennis Prager Show, Monday, December 17, 2012, 1:05 to 1:30 PM.

SELECTED PUBLIC APPEARANCES (CONTINUED):

Heated Gun Debate Begins, ***CNN*** International, Monday, December 17, 2012, 3:07 to 3:17 PM.

BBC Newsnight, television, ***BBC***, Monday, December 17, 2012, 5:30 to 5:35 PM.

Coast to Coast AM, Tuesday, December 18, 2012, 1:05 to 1:08 AM.

Mark Levin Radio Show, Tuesday, December 18, 2012, 6:35 to 6:55 PM.

"Kudlow Report," ***CNBC***, Wednesday, December 19, 2012, 7:00 to 7:05 PM.

"Chat: Chat: Gun control, nation after Newtown," Video conference call, ***USA Today***, Wednesday, December 19, 2012, 2:00 to 2:45 PM.

"Examining the Efficacy and Limitations of Gun Control Laws to Stop Violence," ***PBS Newshour***, Wednesday, December 19, 2012, 7:12 to 7:20 PM.

Piers Morgan Tonight, ***CNN***, Wednesday, December 19, 2012, various points from 9:20 to 9:50 PM.

World Have Your Say, ***BBC World Service***, Friday, December 21, 2012, 1:50 to 1:58 PM.

Megan Kelly, ***Fox News***, Friday, December 21, 2012, 2:10 to 2:20 PM.

America's Gun Debate, CBC, Friday, December 21, 2012, 5:12 to 5:18 PM.

Today Show, NBC, Saturday, December 22, 2012, 7:03 to 7:07 AM.

"Control de armas," Noticiero, Telemundo, Sunday, December 23, 2012, 12:05 to 12:10 PM.

Coast to Coast AM, Wednesday, December 26, 2012, 1:05 to 1:08 AM.

Gun Control, The Bill Bennett Show, nationally syndicated radio show, Thursday, December 27, 2012, 7:05 to 7:12 AM.

Byline, Sun News, Monday, December 31, 2012, 11:35 to 11:45 AM.

Coast to Coast AM, Friday, January 4, 2013, 1:05 to 1:08 AM.

Gun Control, The Bill Bennett Show, nationally syndicated radio show, Thursday, December 27, 2012, 8:05 to 8:15 AM.

Kilmeade & Friends, nationally syndicated radio show, Friday, January 11, 2013, 9:20 to 9:30 AM.

Laura Ingraham, Friday, January 11, 2013, 9:35 to 9:45 AM.

SELECTED PUBLIC APPEARANCES (CONTINUED):

Geraldo Rivera, Friday, January 11, 2013, 10:12 to 10:20 AM.

CNN, Saturday, January 12, 2013, 8:10 to 8:18 AM.

CBS Radio, January 14, 2013, five minutes taped.

Dennis Prager Show, Wednesday, January 16, 2013, 1:05 to 2:00 PM.

Washington Journal, C-SPAN, Saturday, January 19, 2013, 8:30 to 9:15 AM.

Fox News, Saturday, January 19, 2013, 4:30 to 4:40 PM.

Coast to Coast AM, Friday, January 25, 2013, 1:05 to 1:08 AM.

Geraldo Rivera, Thursday, January 31, 2013, 10:08 to 10:30 AM.

Sean Hannity Radio Show, Friday, February 1, 2013, 5:07 to 5:20 PM.

WSJ Live, Monday, February 11, 2013, 1:25 to 1:29 PM.

WSJ Live, "Gun Control: Smart or Illegal," Monday, February 11, 2013, 3:57 to 4:01 PM.

The Dennis Miller Show, April 2, 2013, 12:05 to 12:30 PM.

Steve Malzberg's Show on News Max, April 2, 2013, 3:35 to 3:45 PM.

Geraldo Rivera Radio Show, April 4, 2013, 10:35 to 10:47 AM.

The Dennis Miller Show, April 10, 2013, 11:05 to 11:30 AM.

Mark Levin Show, Wednesday, April 10, 2013, 6:35 to 6:47 PM.

Geraldo Rivera Radio Show, April 11, 2013, 11:35 to 11:45 AM.

Senate Debate over Gun Control, Fox News Live, April 11, 2013, 12:10 to 12:17 PM.

The Scott Hennen Show, April 11, 2013, 1:35 to 1:50 PM.

Mike Huckabee's Radio Show, Thursday, April 11, 2013, 2:35 to 2:47 PM.

Jason Lewis Show, Thursday, April 11, 2013, 8:05 to 9:00 PM.

BBC World TV, Thursday, April 11, 2013, 10:05 to 10:10 PM.

The Rusty Humphries Show, Friday, April 12, 2013, 5:35 to 5:45 PM.

SELECTED NATIONAL PUBLIC APPEARANCES (CONTINUED):

Bill Cunningham's "The Great Show," Sunday, April 14, 2013, 10:35 to 11:00 PM.

"Am Agenda," Sun News Network, Thursday, April 18, 2013, 9:20 to 9:28 AM.

Senate Vote on Gun Control, Fox News Live, Thursday, April 18, 2013, 12:20 to 12:28 PM.

NY1's The Call Tonight: Senate votes against expanding background checks, Thursday, April 18, 2013, 9:15 to 9:20 PM.

Jim Bohannon Show, Thursday, April 18, 2013, 10:35 to 11:00 PM.

Coast to Coast AM, Wednesday, May 8, 2013, 1:15 to 2:00 AM.

Lars Larson Show, Tuesday, June 11, 2013, 7:10 to 8:00 PM.

Substitute host for Jason Lewis Show, Thursday, June 13, 2013, 6:05 to 9:00 PM.

Substitute host for Jason Lewis Show, Friday, June 14, 2013, 6:05 to 9:00 PM.

Mike Huckabee's Radio Show, Tuesday, July 15, 2013, 12:35 to 12:47 PM.

Wilkow!, The Blaze TV, Friday, July 19, 2013, 4:50 to 4:57 PM.

Stand Your Ground Laws, C-SPAN Washington Journal, Saturday, July 20, 2013, 7:45 to 9:15 AM.

Steve Malzberg Show, News Max Radio, Tuesday, July 23, 2013, 3:45 to 3:55 PM.

Coast to Coast AM, Wednesday, July 25, 2013, 1:07 to 1:10 AM.

Steve Malzberg Show, News Max Radio, Wednesday, July 31, 2013, 5:32 to 5:45 PM.

Dana Loesch Show, Thursday, August 1, 2013, 1:35 to 1:45 PM.

"Opinion: Cory Booker: Reality v. Rhetoric," WSJ Live, Thursday, August 15, 2013, 1:10 to 1:15 PM.

Byline, Sun News, Monday, August 29, 2013, 9:06 to 9:11 PM.

Sandy Rios, American Family Radio, September 3, 2013, 8:45 to 8:55 AM.

Steve Malzberg Show, News Max Radio, Tuesday, September 3, 2013, 5:32 to 5:45 PM.

Coast to Coast AM, Friday, September 13, 2013, 1:07 to 1:10 AM.

SELECTED PUBLIC APPEARANCES (CONTINUED):

Shooting in Washington, DC, Sky News UK, Monday, September 16, 2013, 1:14 to 1:19 PM.

Shooting in Washington, DC, Sean Hannity Radio Show, Monday, September 16, 2013, 5:32 to 5:50 PM.

Shooting in Washington, DC, Piers Morgan Tonight, CNN, Monday, September 16, 2013, 9:25 to 9:41 PM.

Sandy Rios, American Family Radio, Tuesday, September 17, 2013, 8:20 to 8:45 AM.

Dennis Miller Radio Show, Wednesday, September 18, 2013, 11:15 to 11:45 AM.

Steve Malzberg Show, News Max Radio, Thursday, September 19, 2013, 4:20 to 4:30 PM.

Lou Dobbs, Fox Business, Thursday, September 19, 2013, 7:35 to 7:40 PM.

Lars Larson Radio Show, Thursday, September 19, 2013, 8:08 to 8:30 PM.

Bill Martinez Live!, Friday, September 20, 2013, 9:16 to 9:50 AM.

Fox News, Friday, September 20, 2013, 1:40 to 1:45 PM.

Dana Loesch Show, Friday, September 20, 2013, 2:35 to 2:50 PM.

The Source, Sun New, Friday, September 20, 2013, 5 PM hour, 7 minutes.

The Alan Colmes Radio Show, Friday, September 20, 2013, 7:06 to 7:30 PM.

Jason Lewis Radio Show, Friday, September 20, 2013, 8:05 to 9:00 PM.

Fox & Friends, Saturday, September 21, 2013, 6:45 to 6:49 AM.

"Dumbing Down the Courts," Federalist Society Teleforum, Wednesday, September 25, 2013, 12:00 to 1:00 PM.

"Obama's call for continued gun control," Voice of Russia, Moscow, Wednesday, September 25, 2013, 1:05 to 1:10 PM.

Sun New, Wednesday, September 25, 2013, 3:17 to 3:23 PM.

Glenn Beck, The Blaze TV, October 1, 2013, 3:20 to 3:25 PM.

"Is the push for gun control over?" Fox News Live, Wednesday, October 2, 2013, 12:33 to 12:40 PM.

SELECTED PUBLIC APPEARANCES (CONTINUED):

"Book Discussion on 'Dumbing Down the Courts,'" C-SPAN2, Saturday, October 20, 2013, 11:00 PM to 12:06 AM; C-SPAN2, Sunday, October 21, 2013, 3:45 to 4:51 PM; and C-SPAN2, Saturday, October 26, 2013, 9:45 to 10:51 AM.

The Dennis Miller Show, Monday, October 22, 2013, 10:46 to 11:00 PM.

"Gun sales a snap on Instagram," New Day, CNN, Saturday, October 26, 2013, 9:15 to 9:20 AM.

"Stand Your Ground Laws," Senate Judiciary Subcommittee on the Constitution, Civil Rights, and Human Rights, C-SPAN2, Thursday, October 31, 2013, 8:00 to 10:00 AM; C-SPAN2, Thursday, October 31, 2013, 5:26 to 7:26 PM; C-SPAN3, Friday, November 1, 2013, 8:00 to 10:00 PM; and C-SPAN3, Saturday, November 2, 2013, 2:00 to 4:00 AM.

Steve Malzberg Show, News Max Radio, Thursday, November 21, 2013, 4:35 to 4:45 PM.

Mike Huckabee's Radio Show, Friday, November 22, 2013, 11:35 to 1:45 PM.

"Kudlow Report," ***CNBC***, Friday, November 22, 2012, 7:50 to 7:57 PM.

Dana Loesch Show, Friday, December 6, 2013, 1:35 to 1:50 PM.

Lars Larson Radio Show, Wednesday, December 11, 2013, 8:35 to 8:55 PM.

Glenn Beck TV Show, Friday, December 13, 2013, 5:50 to 6:00 PM.

The Jim Bohannon Show, Monday, December 16, 2013, 10:05 to 11:00 PM.

Geraldo Rivera, Tuesday, December 17, 2013, 10:06 to 10:15 AM.

Coast-to-Coast AM, Wednesday, December 18, 2013, 1:05 to 1:07 AM.

The Jerry Agar Show, Sun News (Canada), Tuesday, December 24, 2013, 4:35 to 4:41 PM.

Lars Larson Radio Show, Friday, January 3, 2014, 6:22 to 6:30 PM.

"Will More Guns Keep You Safe?" Outfront with Erin Burnett, CNN, Friday, January 3, 2014, 7:33 to 7:40 PM.

Jason Lewis Radio Show, Friday, January 3, 2014, 8:05 to 9:00 PM.

"Gun Myths Shot Down," Byline with Brian Lilley, Sun News (Canada), Tuesday, January 7, 2014, 6:03 to 6:10 PM.

SELECTED PUBLIC APPEARANCES (CONTINUED):

Coast-to-Coast AM, Wednesday, January 8, 2014, 1:06 to 1:09 AM.

The Mancow Experience, Wednesday, January 29, 2014, 11:37 to 11:50 AM.

Sean Hannity Show, Tuesday, March 4, 2014, 5:06 to 5:21 PM.

Lars Larson Radio Show, Friday, March 7, 2014, 5:35 to 5:50 PM.

Jason Lewis Radio Show, Friday, March 7, 2014, 7:35 to 8:00 PM.

"Putting America Back to Work," Byline with Brian Lilley, Sun News (Canada), Friday, March 7, 2014, 10:23 to 10:30 PM.

The Source with Paul Anderson, Sunday, March 9, 2014, 10:05 to 10:40 PM.

Dennis Miller Show, Monday, March 10, 2014, 12:35 to 12:50 PM.

The Gary Nolan Radio Show, Thursday, March 13, 2014, 11:35 to noon.

The Dana Show, Thursday, March 20, 2014, 1:36 to 1:47 PM.

Sandy Rios' Radio Show, Friday, March 21, 2014, 8:20 to 8:35 AM.

The Steve Malzberg Show, Friday, March 21, 2014, 5:20 to 5:30 PM.

Blaze TV, Friday, March 21, 2014, 5:21 to 5:26 PM.

Coastal Daybreak, Tuesday, March 25, 2014, 8:45 to 9 AM.

Bill Bennett's Morning in America, Friday, March 28, 2014, 7:06 to 7:15 AM.

The Lars Larson Show, Wednesday, April 2, 2014, 7:45 to 7:53 PM.

The Jim Bohannon Show, Wednesday, April 2, 2014, 10:35 to 10:42 PM.

WMAL, Thursday, April 3, 2014, 7:05 to 7:14 AM.

KTSA Radio, Thursday, April 3, 2014, 7:36 to 7:45 AM.

The Sandy Rios, Thursday, April 3, 2014, 8:36 to 8:51 AM.

The Dana Show, Thursday, April 3, 2014, 1:35 to 1:48 PM.

The Sean Hannity Show, Thursday, April 3, 2014, 3:35 to 3:51 PM.

The Janet Mefferd Show, Thursday, April 3, 2014, 4:10 to 4:20 PM.

SELECTED PUBLIC APPEARANCES (CONTINUED):

Phil Valentine Show, Thursday, April 3, 2014, 4:35 to 4:48 PM.

The Mark Levin Show, Thursday, April 3, 2014, 6:07 to 6:30 PM.

The David Webb Show, Thursday, April 3, 2014, 10:05 to 10:15 PM.

Big John and Amy Show, Friday, April 4, 2014, 7:35 to 7:45 AM.

Michigan Talk Radio, Friday, April 4, 2014, 8:35 to 8:45 AM.

"Fort Hood Shooting," Jansing & Co with Chris Jansing, MSNBC, Friday, April 4, 2014, 10:05 to 10:14 AM.

The Jason Lewis Radio Show, Friday, April 4, 2014, 8:05 to 9:00 PM.

Tom Gresham's Gun Talk, Sunday, April 6, 2014, 3:35 to 3:45 PM.

The Dennis Miller Show, Tuesday, April 8, 2014, 1:05 to 1:30 PM.

Coast to Coast AM, Thursday, April 10, 2014, 1:08 to 1:11 AM.

The Ed Morrissey Show, Tuesday, April 22, 2014, 4:48 to 5:00 PM.

Jason Lewis Radio Show, Tuesday, April 22, 2014, 8:05 to 9:00 PM.

Peter Schiff Radio Show, Wednesday, April 23, 2014, 10:35 to 11:00 AM.

Phil Valentine Radio Show, Wednesday, April 23, 2014, 3:35 to 3:45 PM.

Conservative News/Talk KNUS, Wednesday, April 23, 2014, 4:35 to 4:45 PM.

Fox News, Thursday, April 24, 2014, 3:33 to 3:37 PM.

The Michigan Talk Network, Thursday, April 24, 2014, 8:35 to 8:41 AM.

Coast to Coast AM, Thursday, April 24, 2014, 1:08 to 1:10 AM.

Greg Garrison Radio Show WIBC in Indianapolis, Thursday, April 24, 2014, 9:35 to 10:00 AM.

"New Georgia Law Expands Rights of Gun Owners," The Real Story with Gretchen Carlson, Fox News, Thursday, April 24, 2014, 3:33 to 3:37 PM.

Nancy Grace, CNN Headline News, Thursday, April 24, 2014, 10:40 to 10:45 PM.

Justice with Judge Jeanine, Fox News, Saturday, April 26, 2014, 9:33 to 9:37 PM.

SELECTED PUBLIC APPEARANCES (CONTINUED):

Southern California Public Radio, KPCC, Monday, April 28, 2014.

The Peter Boyle Show, Wednesday, April 30, 2014, 7:10 to 7:30 AM.

The Source with Ezra Levant, Canada's Sun News, Monday, May 5, 2014.

Dennis Miller Radio Show, Wednesday, May 7, 2014, 2:06 to 2:30 PM.

Dennis Miller Radio Show, Wednesday, May 14, 2014, 2:06 to 2:30 PM.

Dennis Miller Radio Show, Friday, May 16, 2014, 12:33 to 1:00 PM.

WMAL, Tuesday, May 20, 2014, 5:37 to 5:45 PM.

Larry Elder Show, Tuesday, May 27, 2014, 7:14 to 7:21 PM.

Lars Larson Show, Tuesday, May 27, 2014, 7:35 to 8:00 PM.

The Real Side, Tuesday, May 27, 2014, 6:08 to 6:30 PM.

The Marc Bernier Show, Tuesday, May 27, 2014, 5:20 to 5:30 PM.

Dr. Gina Loundon Show, Tuesday, May 27, 2014, 4:20 to 4:30 PM.

Istook Live!, Tuesday, May 27, 2014, 1:05 to 1:20 PM.

John Gibson Radio Show, Tuesday, May 27, 2014, 12:35 to 12:50 PM.

The C4 Show, WBAL, Tuesday, May 27, 2014, 11:05 to 11:20 AM.

Doug McIntyre KABC Los Angeles, Tuesday, May 27, 2014, 9:05 to 9:14 AM.

The Tom Woods Radio Show, Thursday, May 29, 2014.

Byline with Brian Lilley, Canada's Sun News Network, "Moncton Shooting," Thursday, June 5, 2014, 6 to 7 PM.

The Jason Lewis Radio Show, Thursday, June 5, 2014, 8:17 to 9 PM.

The Sandy Rios Show, Monday, June 9, 2014, 8:40 to 9:00 AM.

News Max, Midpoint, Monday, June 9, 2014, 12:15 to 12:26 PM.

KTTH Radio in Seattle, Tuesday, June 10, 2014.

The David Boze Show, Tuesday, June 10, 2014, 10:05 to 10:30 AM.

SELECTED PUBLIC APPEARANCES (CONTINUED):

The Arena with Michael Coren, Canada's Sun News, "More Control, More Violence," Wednesday, June 11, 2014.

The Jim Bohannon Show, Monday, June 16, 2014, 10:35 to 11 PM.

"Gun Rights" a presentation to a dinner for Gun Rights North Carolina, C-SPAN3, Saturday, July 5, 2014, 4:36 to 6:11 PM and C-SPAN2, Thursday, September 4, 2014, 10:35 AM to 12:10 PM.

The Jason Lewis Radio Show, Sunday, July 20, 2014.

"Mayor Emanuel to blame for Chicago violence," Fox TV Chicago, Monday, July 28, 2014.

WMAL, Tuesday, July 29, 2014, 5:07 to 5:15 PM.

An interview with Cam & Company and Katie Pavlich, Monday, August 4, 2014, 2:40 to 2:51 PM.

Jim Bohannon Radio Show, Monday, August 4, 2014, 10:35 to 11:00 PM.

Michigan Talk Network, Wednesday, August 13, 2014, 8:30 to 8:40 AM.

KFI, John and Ken Show, Wednesday, August 13, 2014, 8:35 to 8:45 PM.

Coast to Coast AM, Thursday, August 14, 2014, 1:07 to 1:09 AM.

Sportsman Channel, Thursday, August 14, 2014, 5:15 to 5.25 PM.

The Dana Loesch Show (radio), Tuesday, August 19, 2014, 2:06 to 2:20 PM.

The Glenn Beck TV Show, Tuesday, August 19, 2014, 4:30 to 4:50 PM.

The Lars Larson National Radio Show, Monday, August 25, 2014, 6:20 to 6:30 PM.

Coast to Coast AM, Tuesday, August 26, 2014, 1:07 to 1:10 AM.

Women stalking victims and guns used for self protection, The Blaze TV with Dana Loesch, Tuesday, August 26th, 2014, 5:20 to 5:40 PM.

Washington, DC's WMAL, Friday, August 29, 2014, 5:35 to 5:40 PM.

The Michigan Talk Network, Friday, August 29, 2014, 8:35 to 8:55 AM.

The Larry Elder Show, Wednesday, September 3, 2014, 8:20 to 8:30 PM.

WVON, Thursday, September 4, 2014, 8:05 to 9:15 PM.

SELECTED PUBLIC APPEARANCES (CONTINUED):

"Russian versus US murder rates," Voice of Russia, Saturday, September 20th, 2014, 9:07 to 9:16 AM.

Mistakes by police and civilians using guns, The Blaze TV with Dana Loesch, Tuesday, September 30, 2014, 6:09 to 6:19 PM.

"It's a Mean, Mean World?" The John Stossel Show, Fox News, Saturday, October 4, 2014, 5 PM and 10 PM; Sunday, October 5, 2014, 10 PM; Monday, October 6, 2014, 1 AM. Also shown on Fox Business, Thursday, October 2, 2014, 9 PM; Friday, October 3, 2014, 1 AM; Saturday, October 4, 2014, 9 PM and Midnight; Sunday, October 5, 2014, 9 PM.

Michael Smerconish Show, CNN, Saturday, October 11, 2014, 9:30 to 9:35 AM and 6:30 to 6:35 PM.

The Lars Larson National Radio Show, Tuesday, October 14, 2014, 6:35 to 6:43 PM.

The Dennis Miller National Radio Show, Tuesday, October 14, 2014, 1:15 to 1:30 PM.

The Tom Gresham's National Radio Show, Sunday, October 19, 2014, 2:07 to 2:19 PM.

The Sand Rios National Radio Show, Monday, October 20, 2014, 8:20 to 8:37 AM.

Errors in FBI report on public shootings, The Blaze TV with Dana Loesch, Monday, October 20, 2014, 5:20 to 5:30 PM.

Coast-to-Coast AM Radio, Thursday, October 14, 2014, 1:07 to 1:11 AM.

The Lars Larson National Radio Show, Thursday, October 23, 2014 from 6:37 to 6:45 PM.

"Does gun control save lives or does it tilt the battlefield in favour of terrorists?" The Source with Ezra Levant, Sun News (Canada), Tuesday, November 3, 2014, 8:48 PM.

The Bill Martinez Live Radio Show, Tuesday, December 2, 2014, 9:20 to 9:45 AM.

Coast-to-Coast AM Radio, Wednesday, December 3, 2014, 1:06 to 1:09 AM.

The Lars Larson National Radio Show, Monday, December 8th, 2014, 6:35 to 6:44 PM.

The Dennis Miller National Radio Show, Wednesday, December 10th, 2014, 2:05 to 2:30 PM.

The Lars Larson National Radio Show, Thursday, December 12th, 2014, 7:35 to 7:40 PM.

SELECTED PUBLIC APPEARANCES (CONTINUED):

The Sean Hannity Radio Show, Friday, December 19th, 2014, 5:09 to 5:16 PM.

The Steve Malzberg Show, Monday December 22nd, 2014, 5:20 to 5:30 PM.

Michigan Talk Network, Monday, December 22nd, 2014, 7:45 to 7:52 AM.

The Flipside, Tuesday, December 23, 2014.

"Shoot an intruder in Idaho & the intruder goes to jail. In Canada the shooter might," Byline with Brian Lilley, Sun News (Canada), Monday, January 19, 2015.

The Dana Loesch Radio Show, Thursday, January 22nd, 2015, 2:23 to 2:30 PM and 3:05 to 3:16 PM.

The Lars Larson National Radio Show, Thursday, January 22nd, 2015, 6:05 to 6:15 PM.

Michigan Talk Network, Thursday, January 22, 2015, 8:35 to 8:41 AM.

The Frank Beckmann Show on WJR, Friday, January 30th, 2015.

The Lars Larson National Radio Show, Thursday, February 12th, 2015, 6:21 to 6:30 PM.

The Jim Bohannon National Radio Show, Wednesday, February 18th, 2015, 10:05 to 11:00 PM.

Jim Bohannon's America in the Morning, Friday, February 20th, 2015, 5 AM hour.

John Gambling on WOR, Friday, February 27, 2015, 11:20 to 11:30 AM.

Coast-to-Coast AM, Friday, February 27, 2015 from 1:07 to 1:09 AM.

Wyoming Public Television, Friday, February 27th, 2015 at 8 PM.

Mark Levin Radio Show, Tuesday, March 10th, 2015 from 7:35 to 7:46 PM.

Sean Hannity Radio Show, Monday, March 16th, 2015, 5:06 to 5:20 PM.

Steve Malzberg NewsMax TV Show, March 20, 2015.

John Stossel's Show on **Fox News** and **Fox Business**, March 22, 23, and 24, 2015.

Lars Larson's National Radio Show, Wednesday, March 25, 2015, 6:20 to 6:30 PM.

The Jim Bohannon National Radio Show, Friday, March 13, 2015, 10:35 to 11:00 PM.

The Jim Bohannon National Radio Show, Wednesday, March 18, 2015, 10:35 to 11:00 PM.

SELECTED PUBLIC APPEARANCES (CONTINUED):

The Phil Valentine Show, Monday, March 30, 2015, 2:35 to 3:00 PM.

Larry O'Connor's Show, WMAL, Thursday, April 16th, 2015 from 5:35 to 5:50 PM.

Michigan Talk Network, Tuesday, April 21, 2015 from 6:40 to 6:47 AM.

Washington Journal, C-SPAN, April 25, 2015, 7:51 to 8:20 AM.

Michigan Talk Network, Tuesday, April 28, 2015 from 7:35 to 7:43 AM.

Coast-to-Coast AM, Tuesday, April 28th, 2015, 1:15 to 3:00 AM.

Michigan Talk Network, Tuesday, May 5, 2015 from 6:40 to 6:47 AM.

Lars Larson's National Radio Show, Monday, May 4, 2015, 6:20 to 6:30 PM.

"What National Crime Wave?" Wall Street Journal's Opinion Journal, June 9, 2015.

The Jim Bohannon National Radio Show, Wednesday, June 4, 2015, 11:05 to 12:00 PM

Larry O'Connor's Show, WMAL, Thursday, June 11th, 2015 from 5:07 to 5:14 PM.

Lars Larson's National Radio Show, Thursday, June 11th, 8:20 to 8:42 PM.

Coast-to-Coast AM, Friday, June 12th, 1:07 to 1:10 AM.

Coast-to-Coast AM, Friday, June 19, 2015 from 1:07 to 1:10 AM.

Bill Bennett's Morning in America, Friday, June 19, 2015 from 7:05 to 7:16 AM.

Lars Larson's National Radio Show, Friday, June 19, 2015 from 6:22 to 6:30 PM.

Dennis Prager National Radio Show, Monday, June 22, 2015 form 2:05 to 3:00 PM.

Sean Hannity's Television Show, Fox News, Monday, June 22, 2015 from 10:18 to 10:20 PM.

The Daily Wrap's co-hosted by Joe Concha and Rick Ungar, NewsMax TV, Tuesday, June 23, 2015 from 7:10 to 7:17 PM.

Larry O'Connor's Show, WMAL, Thursday, July 2nd, 2015 from 8:40 to 8:45 AM.

Lars Larson Show on Thursday, July 16th, 2015 from 7:20 to 7:30 PM.

Larry Elder Show on Friday, July 17th, 2015 from 3:20 to 3:30 PM.

Laura Ingraham Radio Show, Friday, July 24, 2015 from 10:07 to 10:16 AM.

SELECTED PUBLIC APPEARANCES (CONTINUED):

Coast-to-Coast AM, Tuesday, July 28, 2015 from 1:08 to 1:11 AM.

The John Stossel Show on Fox News and Fox Business on Friday, July 31, 2015.

Aljazeera America, Monday, August 10, 2015.

Lars Larson's National Radio Show, Thursday, August 13, 2015 from 6:22 to 6:30 PM.

UN Arms Trade Treaty, Radio America, Thursday, August 27, 2015.

Steve Malzberg's Show, NewsMax TV, Thursday, August 27, 2015 from 7:34 to 7:40 PM.

CBS Evening News, Thursday, August 27th, 2015, 6:45 to 6:47 PM.

Coast-to-Coast AM, Friday, August 28th, 2015 from 1:15 to 2 AM.

Jim Bohannon's America in the Morning, Tuesday, August 18, 2015, 5 AM hour.

The Daily Wrap's co-hosted by Joe Concha and Rick Ungar, NewsMax TV, Friday, August 28, 2015, 6:05 to 6:09 PM.

Sandy Rios National Radio Show, Tuesday, September 1, 2015, 8:15 to 8:50 AM.

Dana TV, The Blaze, October 1, 2015.

Aljazeera International, October 2, 2015 from 11:10 to 11:15 AM.

CNN Newsroom with Carol Costello, ***CNN***, October 2, 2015.

Coast-to-Coast AM, Friday, October 2, 2015 from 1:07 to 1:10 AM.

Mark Levin National Radio Show, Friday, October 2, 2015 from 6:35 to 6:53 PM.

Laura Ingraham Radio Show, Friday, October 2, 2015 from 1:15 to 1:23 PM.

John Gibson National Radio Show, Friday, October 2nd, 2012 from 1:05 to 1:17 PM.

Sean Hannity Radio Show, Monday, October 6, 2015, 5:06 to 5:21.

"The Danger of Gun-free Zones," ***Wall Street Journal's*** Opinion Journal, October 5, 2015.

NewsMax TV, Friday, October 5, 2015 from 8:24 to 8:29 PM.

"Would New Guns Laws Make You Safer?" ***Fox & Friends***, October 10, 2015.

Not yet updated after this date.

SELECTED NONACADEMIC WRITINGS ON ECONOMICS:

"Stop Subsidizing the Future Rich," ***USA Today***, February 19, 1985, p. 8A.

"Social Security's a Bad Deal That's Also Difficult to Justify," ***Houston Post***, Sunday, October 27, 1985, p. B3, also appeared in ***The Orange County Register*** and ***Peoria (Illinois) Star Journal***, Sunday, November 3, 1985.

"Competition Would Benefit Schools," ***Bozeman (MT) Daily Chronicle***, Thursday, July 31, 1986, p. 4.

"Teachers: They Could Stand Some Competition," ***Detroit News***, Thursday, July 17, 1986, also appeared in the ***St. Louis Post-Dispatch***, Friday, July 18, 1986; ***Pittsburgh Post-Gazette***, Saturday, July 12, 1986; and the ***Washington Times***, Monday, July 14, 1986.

"Advantage, Incumbents," coauthored with Gertrud Fremling, ***The Orange County Register***, April 18, 1987, p. A28.

"Incumbents Benefit if Spending Caps are Equal," ***The Wall Street Journal***, Wednesday, June 10, 1987, p. A10.

"Academic Freedom at a Public University," ***The Freeman***, March 1990, pp. 112-115.

"Blaming the Bad News Bearer," ***Los Angeles Times***, Monday, August 13, 1990, p. B5; also appeared in the ***Courier-Journal*** (Louisville, Kentucky), Tuesday, August 14, 1990, p. 9A; the ***Sacramento Bee***, Wednesday, August, 15, 1990, p. B7; and carried on ***Los Angeles Times*** newswire service.

"Raising Commodity Prices Justifiable?" debate with U.S. Senator Joseph Lieberman, ***PM Editorial Services***, August 1990.

"What Should We Do About the Deficit?" ***United Press International Newswire***, October 11, 1990.

"Strong Arm of the Law Left Milken Defenseless," ***The Orange County Register***, Sunday, March 31, 1991, p. G1 and G2.

"Is Curbing Crime Worth the Cost?" ***New York Times***, coauthored with Michael Block, Sunday, May 5, 1991, p. F 13.

"Our Economy is Still the World's Leader," ***New York Newsday***, Tuesday, April 7, 1992, p. 85; also appeared in the ***Indianapolis Star***, Wednesday, April 22, 1992, p. A13; carried on ***Los Angeles Times*** newswire service.

"Clinton Economic Plan Won't Work," ***Philadelphia Inquirer***, Sunday, April 19, 1992, p. D13, also appeared in the ***Richmond Times-Dispatch***, Sunday, April 26, and carried on Knight-Ridder newswire service.

SELECTED NONACADEMIC WRITINGS ON ECONOMICS (CONTINUED):

"Czechoslovak Mutual Funds Fuel Reform," ***The Wall Street Journal Europe***, coauthored with Robert G. Hansen, Friday, July 17, 1992, p. 6.

"Al Gore on the Environment: Warped and Extremist Thinking," ***Philadelphia Inquirer***, Friday, August 27, 1992, p. A23, also appeared in the ***Detroit News***, Monday September 28, 1992, p.7A.

"If Al Gore's a Conservative, Clinton Must Be Waaay Out," ***New York Newsday***, Tuesday, October 6, 1992, p. 75, carried on ***Los Angeles Times-Washington Post*** newswire service.

"But Look at the Price Tag," ***Philadelphia Inquirer***, Thursday, October 15, 1992, p. A23, also appeared in the ***St. Louis Post-Dispatch***, Sunday, October 11, 1992, p. 7E, ***The Seattle Times***, Friday, October 23, 1992, and carried on Knight-Ridder newswire service.

"Clinton's 'Big Lie' on Economy," ***Philadelphia Inquirer***, Monday, November 2, 1992, p. A15.

"Give Reagan-Bush Their Due: They Did Build Up the Economy," ***Philadelphia Inquirer***, Wednesday, January 20, 1993, p. A9; also appeared in the ***Orange County Register***, Sunday, January 24, 1993; ***Long Beach (Ca.) Press-Telegram***, Friday, January 22, 1993; ***Lexington (KY) Herald-Leader***, Monday, January 25, 1993; ***The Wichita Eagle***, Sunday January 24, 1993; ***Corpus Christi (TX) Caller Times***, Saturday, January 23, 1993; ***News Sentinel*** (Fort Wayne, Indiana), Saturday, January 23, 1993; and carried on Knight-Ridder newswire service."Energy Tax Hits the Middle Class," ***Philadelphia Inquirer***, Monday, February 8, 1993, p. A13; also appeared in the ***Orange County Register***, Sunday, February 21, 1993, pp. J1 and J2; and carried on Knight-Ridder newswire service.

"Drug Research: Pay Now or Later," ***Philadelphia Inquirer***, Saturday, March 20, 1993, p. A11, carried on Knight-Ridder newswire service.

"Clinton: Proteger a EE UU," ***Cinco Dias*** (Second largest business newspaper in Spain), Friday, April 16, 1993, pp. 2 and 3.

"U.S. Taxpayers Will Pay Dearly for that Biodiversity Treaty," ***Philadelphia Inquirer***, Wednesday, May 19, 1993, p. A11; also appeared in the ***Washington Times***, Tuesday, June 1, 1993, pp. E1 and E4; the ***Phoenix Gazette***, Saturday, May 22, 1993, p. A13; and the ***Montgomery County (PA.) Observer***, Wednesday, May 26, 1993, p. 11.

"Who Gets Socked — Salmon or Energy Consumers?: The True Cost of Clinton's Energy Bill," ***Philadelphia Inquirer***, Thursday, June 10, 1993, p. A23.

"Clinton Plan Isn't Campaign Reform," ***Philadelphia Inquirer***, Saturday, August 21, 1993, p. A7.

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EXHIBIT "2"

U.S. Department of Justice
Office of Justice Programs
Bureau of Justice Statistics

SPECIAL REPORT

JANUARY 2019 NCJ 251776

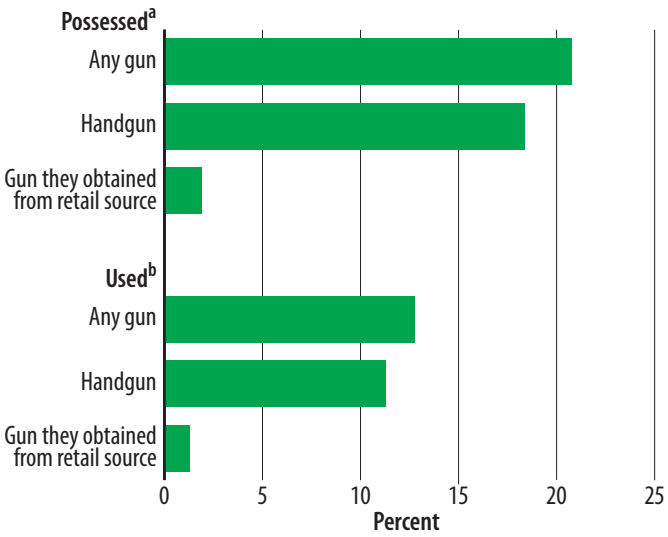
Source and Use of Firearms Involved in Crimes: Survey of Prison Inmates, 2016

Mariel Alper, Ph.D., and Lauren Glaze, *BJS Statisticians*

Based on the 2016 Survey of Prison Inmates (SPI), about 1 in 5 (21%) of all state and federal prisoners reported that they had possessed or carried a firearm when they committed the offense for which they were serving time in prison **(figure 1)**. More than 1 in 8 (13%) of all prisoners had used a firearm by showing, pointing, or discharging it during the offense for which they were imprisoned. Fewer than 1 in 50 (less than 2%) of all prisoners had obtained a firearm from a retail source and possessed, carried, or used it during the offense for which they were imprisoned.

An estimated 287,400 prisoners had possessed a firearm during their offense. Among these, more than half (56%) had either stolen it (6%), found it at the scene of the crime (7%), or obtained it off the street or from the underground market (43%). Most of the remainder (25%) had obtained it from a family member or friend, or as a gift. Seven percent had purchased it under their own name from a licensed firearm dealer.

FIGURE 1
Percent of all state and federal prisoners who had possessed or used a firearm during their offense, 2016



Note: See appendix table 1 for standard errors.
[a]Includes prisoners who carried or possessed a firearm during the offense.
[b]Includes prisoners who showed, pointed, or discharged a firearm during the offense.
Source: Bureau of Justice Statistics, Survey of Prison Inmates, 2016.

HIGHLIGHTS

- About 21% of state and 20% of federal prisoners said they possessed a gun during their offense, while 79% of state and 80% of federal prisoners did not.
- About 29% of state and 36% of federal prisoners serving time for a violent offense possessed a gun during the offense.
- About 1.3% of prisoners obtained a gun from a retail source and used it during their offense.
- Handguns were the most common type of firearm possessed by state and federal prisoners (18% each); 11% of all prisoners used a handgun.
- Among prisoners who possessed a gun during their offense, 90% did not obtain it from a retail source.
- Among prisoners who possessed a firearm during their offense, 0.8% obtained it at a gun show.
- About 1 in 5 state and federal prisoners who possessed a firearm during their offense obtained it with the intent to use it during the crime.
- Among state prisoners who possessed a gun during their offense, 27% killed someone with it, another 12% injured someone, 7% fired the gun but did not injure anyone, and 54% did not fire it.
- State prisoners with no military service were more likely to possess a gun during their offense (21%) than prisoners who had served in the military (16%).

Statistics in this report are based on self-reported data collected through face-to-face interviews with a national sample of state and federal prisoners in the 2016 SPI. (See *Methodology*.)

The 2016 SPI data collection was conducted from January through October 2016. The SPI was formerly known as the Survey of Inmates in State and Federal Correctional Facilities (SISFCF). The Bureau of Justice Statistics (BJS) has periodically conducted the survey since the 1970s, with the most recent iteration fielded in 2004. The survey collects information from prisoners on a variety of topics, including firearm possession during the crime for which a prisoner was serving time and how the firearm was used during the crime. It also collects information on the method, source, and process that prisoners used to obtain the firearm. (See appendix 1, *Questions related to firearms in the Survey of Prison Inmates, 2016*.)

Terms and definitions

- **Firearm** – a weapon that uses gunpowder to shoot a bullet. Primary types are handguns, rifles, and shotguns:[1]
  - ***Handgun*** – a firearm which has a short stock and is designed to be held and fired by the use of a single hand.
  - ***Rifle*** – a firearm intended to be fired from the shoulder and designed to use the energy of an explosive to fire only a single projectile through a rifled bore for each single pull of the trigger.
  - ***Shotgun*** – a firearm intended to be fired from the shoulder and designed to use the energy of an explosive to fire through a smooth bore either a number of ball shot or a single projectile for each pull of the trigger.
- **Firearm possession** – carrying or possessing at least one firearm when the offense for which prisoners were serving a sentence was committed.
- **Firearm use** – showing a firearm to or pointing a firearm at anyone or discharging a firearm during the offense for which a prisoner was serving time.
- **Source of the firearm** – from where and how prisoners reported obtaining the firearm they possessed during the crime for which they were imprisoned—
  - ***Purchased or traded from a retail source*** – includes a gun shop or store, pawn shop, flea market, or gun show.
    - **Gun shop or store** – a business establishment that sells firearms in an open shopping format.
    - **Pawn shop** – a business that offers secured loans to customers, with personal property used as collateral. This personal property is sold to the public if the loan is not repaid.
    - **Flea market** – a market that rents space to individuals to sell or barter merchandise.
    - **Gun show** – a temporary market where licensed dealers and unlicensed sellers can rent tables or booths to sell firearms.
  - ***Obtained from an individual*** – includes purchasing, trading, renting, or borrowing from a family or friend. Also includes when the firearm was gifted to or purchased for the person.
  - ***Off the street or underground market*** – illegal sources of firearms that include markets for stolen goods, middlemen for stolen goods, criminals or criminal enterprises, or individuals or groups involved in sales of illegal drugs.
  - ***Theft*** – includes stealing the firearm during a burglary or from a retail source, family member, friend, or another source.
  - ***Other sources*** – includes a firearm that a prisoner obtained or found at the location of the crime, including one that belonged to a victim or that someone else brought to the location of the crime. This category also includes sources for which there were few responses, such as for guns bought online, and other sources that did not fit into one of the existing categories. This also includes instances where there was not enough information to categorize the source, such as when a firearm was purchased from an unknown source or obtained from another person by an unknown method.

[1]The definitions of types of firearms in this section were taken from 18 U.S.C. § 921 (2009). They have been edited for length.

Controlling-offense characteristics

About 29% of state and 36% of federal prisoners serving a sentence for a violent offense in 2016 possessed a firearm during the crime **(table 1)**. About a quarter of state (23%) and federal (25%) prisoners serving time for a violent offense used a firearm during the crime. "Firearm use" is defined in this report as showing, pointing, or discharging a firearm during the offense for which a prisoner was serving a sentence.

Among prisoners serving time for homicide, more than 2 in 5 (44%) state prisoners and more than 1 in 3 (36%) federal prisoners had possessed a firearm during the crime. About 37% of state and 28% of federal prisoners serving time for homicide used a firearm during the homicide.

Among those serving time for robbery, more than 2 in 5 state prisoners (43%) and federal prisoners (46%) possessed a firearm during the offense, and nearly a third of state (31%) and federal (32%) prisoners used a firearm during the robbery. Firearm possession was less common among state prisoners serving a sentence for rape or sexual assault (2%). Less than 1% of state prisoners serving time for rape or sexual assault used a firearm in the commission of their crime.

TABLE 1

Firearm possession and use among state and federal prisoners during the offense for which they were serving time, by type of controlling offense, 2016

Controlling offense[a]	Estimated number of state prisoners[b]	Percent of state prisoners who— Possessed a firearm[b]	Used a firearm[c]	Estimated number of federal prisoners[b]	Percent of federal prisoners who— Possessed a firearm[b]	Used a firearm[c]
Total	1,211,200	20.9%	13.9%	170,400	20.0%	5.0%
Violent*	667,300	29.1%	23.0%	20,900	36.2%	25.3%
Homicide[d]	191,400	43.6	37.2	3,800	35.9	28.4
Rape/sexual assault	144,800	2.0	0.8	2,400	:	:
Robbery	149,600	43.3	31.5	10,700	46.3	32.1
Assault	149,400	25.0	20.6	2,900	29.0	18.1
Other violent[e]	32,200	17.0	12.6	1,200	34.1	:
Property	186,100	4.9% †	2.0% †	12,000	2.6% †	:
Burglary	88,100	6.7	3.2	300	:	:
Other property[f]	98,000	3.3	1.0	11,800	2.4	:
Drug	180,800	8.4% †	0.8% †	80,500	12.3% †	0.6% †
Trafficking[g]	130,500	9.4	0.9	72,300	12.9	0.7
Possession	45,900	6.1	:	3,500	:	:
Other/unspecified drug	4,300	:	:	4,700	:	:
Public order	158,300	21.5% †	5.6% †	52,900	30.2%	5.3% †
Weapons[h]	43,800	67.2	15.7	22,200	66.9	11.3
Other public order[i]	114,400	4.0	1.7	30,700	3.6	:
Other	3,900	:	:	1,800	:	:
Unknown	14,900	4.3% †	:	2,200	:	:

Note: See appendix table 2 for standard errors.

*Comparison group.

†Difference with comparison group is significant at the 95% confidence level across main categories, and no testing was done on subcategories (e.g., homicide).

: Not calculated. Too few cases to provide a reliable estimate, or coefficient of variation is greater than 50%.

[a]See *Methodology* for information on how controlling offense was measured.

[b]Excludes 3.0% of state prisoners and 1.7% of federal prisoners who were missing responses on firearm possession. Includes prisoners who were missing responses on firearm use.

[c]Excludes 3.0% of state prisoners and 1.7% of federal prisoners who were missing responses on firearm possession, and an additional 0.6% of state prisoners and 0.7% of federal prisoners who were missing responses on firearm use.

[d]Includes murder and both negligent and non-negligent manslaughter.

[e]Includes kidnapping, blackmail, extortion, hit-and-run driving with bodily injury, child abuse, and criminal endangerment.

[f]Includes larceny, theft, motor vehicle theft, arson, fraud, stolen property, destruction of property, vandalism, hit-and-run driving with no bodily injury, criminal tampering, trespassing, entering without breaking, and possession of burglary tools.

[g]Includes possession with intent to distribute.

[h]Includes being armed while commiting a crime; possession of ammunition, concealed weapons, firearms and explosive devices; selling or trafficking weapons; and other weapons offenses. Among federal prisoners, weapons offense include violations of federal firearms and explosives.

[i]Includes commercialized vice, immigration crimes, DUI, violations of probation/parole, and other public-order offenses.

Source: Bureau of Justice Statistics, Survey of Prison Inmates, 2016.

State and federal prisoners serving time for a violent offense were much more likely to have possessed a firearm during the offense (29% state, 36% federal) than prisoners serving time for a property (5% state, 3% federal) or drug (8% state, 12% federal) offense. Among prisoners serving time for a public-order offense, about 1 in 5 (21%) state prisoners and nearly 1 in 3 (30%) federal prisoners reported that they possessed a firearm during the crime, and about 1 in 20 reported they had used it. About two-thirds of state and federal prisoners sentenced for a weapons offense said they possessed a firearm during the crime.[2]

[2]In addition to prisoners serving a sentence in state or federal prison in 2016 who possessed a firearm during the offense, weapons offenses include prisoners who were convicted of trafficking firearms but did not possess them at the time of the offense and prisoners who were convicted of a weapons offense that did not involve a firearm.

Extent of firearm use among prisoners during the crime

State and federal prisoners in 2016 who had possessed a firearm during their offense were about equally likely to report that they had obtained the firearm with the intent to use it during the offense (19% state, 20% federal) **(table 2)**. However, state prisoners (68%) who possessed a firearm were more than 2.5 times as likely as federal prisoners (26%) who possessed a firearm to have used it during the crime.

Nearly half of state prisoners (46%) serving a sentence for a crime during which they possessed a firearm discharged the firearm when they committed the crime, compared to 12% of federal prisoners. Among state prisoners who possessed a firearm during their offense, 27% killed a victim with the firearm and another 12% injured or shot a victim but did not kill him or her. Federal prisoners who possessed a firearm when they committed their offense were much less likely to have killed (4%) or injured (2%) a victim with the firearm than state prisoners.

TABLE 2

Among state and federal prisoners who possessed a firearm during the offense for which they were serving time, extent of firearm use, 2016

			State prisoners		Federal prisoners	
Firearm use	State prisoners*	Federal prisoners	Violent offense*	Non-violent offense[a]	Violent offense*	Non-violent offense[a]
Total	100%	100%	100%	100%	100%	100%
Obtained firearm because planned to use in controlling offense[b]						
Yes	19.3%	19.7%	17.7%	24.6% †	26.4%	18.0%
No	80.7	80.3	82.3	75.4 †	73.6	82.1
Used firearm[c]	68.0%	25.9% †	81.0%	24.8% †	72.5%	12.9% †
Discharged	46.5%	11.9% †	55.9%	15.4% †	27.3%	7.5% †
Killed victim	27.1	4.1 †	35.0	:	16.5	:
Injured/shot victim but did not kill victim	12.4	2.2 †	14.5	5.3 †	:	:
Discharged firearm but did not shoot anyone	7.0	5.6	6.4	9.0	5.7	5.4
Did not discharge[d]	21.5%	14.0% †	25.2%	9.4% †	45.3%	5.4% †
Did not use firearm	32.0%	74.1% †	19.0%	75.2% †	27.5%	87.1% †
Estimated number of prisoners who possessed a firearm (with valid data)[e]	245,400	32,900	187,800	57,000	7,200	25,600

Note: Percentages are based on data reported on firearm possession, use, and controlling offense. Excludes 3.1% of state prisoners and 3.5% of federal prisoners who possessed a firearm during the offense and were missing responses on firearm use and 0.3% of state prisoners and 0.7% of federal prisoners who possessed a firearm and were missing a controlling offense. The sum of violent offense and non-violent offense does not equal the total number of state and federal prisoners who possessed a firearm in this table due to an estimated 600 state and 100 federal prisoners whose offense type was unknown. See appendix table 3 for standard errors.

*Comparison group.

†Difference with comparison group is significant at the 95% confidence level.

: Not calculated. Too few cases to provide a reliable estimate, or coefficient of variation is greater than 50%.

[a]Includes property, drug, public order, and other non-violent offenses.

[b]Percentages are based on the 246,200 state and 32,600 federal prisoners who reported they carried or possessed a firearm and whether they obtained a firearm to use during the offense.

[c]Includes prisoners who showed a firearm to anyone, pointed a firearm at anyone, or discharged the firearm during the offense.

[d]Includes prisoners who showed or pointed a firearm at anyone during the offense but did not discharge it.

[e]Includes prisoners who reported they carried or possessed a firearm. Excludes prisoners who were missing responses on firearm possession or use. For violent offense and non-violent offense, also excludes prisoners who were missing a controlling offense.

Source: Bureau of Justice Statistics, Survey of Prison Inmates, 2016.

Among prisoners who possessed a firearm during a violent offense, a large majority of both state (81%) and federal (73%) prisoners used the firearm during the offense, far more than the percentages for non-violent offenders (25% state, 13% federal). More than half (56%) of state prisoners serving time for a violent offense who possessed a firearm during the crime discharged it, compared to fewer than a sixth (15%) of non-violent offenders in state prison who possessed a firearm. Violent offenders (27%) in federal prison who possessed a firearm during the crime were about 3.5 times as likely to discharge it as non-violent offenders (8%). Among state prisoners who had possessed a firearm during their offense, however, non-violent offenders (25%) were more likely than violent offenders (18%) to have planned to use the firearm during the offense.

Type of firearm possessed by prisoners during offense

Handguns were by far the most common type of firearm possessed or used by prisoners during the crime for which they were sentenced. About 18% of all state and federal prisoners in 2016 reported that they had possessed a handgun during the crime for which they were serving a sentence **(table 3)**. Two percent or fewer possessed a rifle or a shotgun. Twelve percent of state and 5% of federal prisoners used a handgun during their offense. Most state (79%) and federal (80%) prisoners did not possess any type of firearm during the crime for which they were imprisoned.

TABLE 3

Firearm possession and use among state and federal prisoners during the offense for which they were serving time, by type of firearm, 2016

	Percent of prisoners who possessed a firearm			Percent of prisoners who used a firearm[a]		
Type of firearm	All prisoners	State*	Federal	All prisoners	State*	Federal
Total	100%	100%	100%	100%	100%	100%
Firearm[b]	20.8%	20.9%	20.0%	12.8%	13.9%	5.0% †
Handgun	18.4	18.4	18.3	11.2	12.2	4.6
Rifle	1.5	1.4	2.0 †	0.8	0.8	0.4 †
Shotgun	1.6	1.6	1.7	1.1	1.2	0.4 †
No firearm	79.2%	79.1%	80.0%	87.2%	86.1%	95.0%
Estimated number of prisoners (with valid data)[c]	1,378,200	1,208,100	170,100	1,378,200	1,208,100	170,100

Note: Details on type of firearm may not sum to totals because prisoners could report more than one type of firearm. Percentages exclude missing data. Excludes 3.0% of state prisoners and 1.7% of federal prisoners who were missing responses on firearm possession during the offense and an additional 0.3% of state prisoners and 0.2% of federal prisoners who were missing responses on type of firearm. See appendix table 4 for standard errors.

*Comparison group.

†Difference with comparison group is significant at the 95% confidence level.

[a]Percentages exclude 0.6% of state prisoners and 0.7% of federal prisoners who were missing responses on firearm use.

[b]Includes prisoners who reported a type of firearm that did not fit into one of the existing categories and those who did not provide enough information to categorize the type of firearm. About 0.1% of state prisoners and 0.2% of federal prisoners reported another type of firearm or did not report enough information to specify the type of firearm.

[c]Excludes prisoners who were missing responses on firearm possession or type of firearm. Counts are weighted to totals from the 2015 National Prisoner Statistics Program; see *Methodology: Survey of Prison Inmates, 2016* (NCJ 252210, BJS web, July 2019).

Source: Bureau of Justice Statistics, Survey of Prison Inmates, 2016.

Demographic characteristics

Male prisoners were more likely than female prisoners to have possessed a firearm during their crime. About a fifth of male state and federal prisoners serving a sentence in 2016 possessed a firearm during the crime (table 4). Males in state prisons in 2016 were about 2.5 times as likely (22%) as females in state prisons (9%) to have possessed a firearm during the crime for which they were imprisoned. In federal prisons, males (21%) were about three times as likely as females (7%) to have possessed a firearm during their crime. Almost 3 in 10 (29%) black prisoners serving a sentence in state prison in 2016 possessed a firearm during their crime. White (12%) and Hispanic (21%) state prisoners were less likely to have possessed a firearm during their crime. Similarly, white (17%) and Hispanic (13%) federal prisoners serving a sentence in 2016 were less likely to have possessed a firearm during the crime than black (29%) federal prisoners. State prisoners who served in the military were less likely to have possessed a firearm during their crime (16%) than state prisoners who had not served in the military (21%).

TABLE 4

Firearm possession among state and federal prisoners during the offense for which they were serving time, by demographic characteristics, 2016

	State		Federal	
Demographic characteristic	Number of prisoners	Percent of prisoners who possessed a firearm during the offense	Number of prisoners	Percent of prisoners who possessed a firearm during the offense
Sex				
Male*	1,124,200	21.8%	159,800	20.9%
Female	87,000	9.5 †	10,600	6.6 †
Race/Hispanic origin[a]				
White	383,300	12.4% †	35,400	16.6% †
Black*	401,500	29.4	53,800	29.2
Hispanic	247,200	21.5 †	62,600	12.6 †
American Indian/Alaska Native	17,200	14.8 †	2,800	23.8
Asian/Native Hawaiian/Other Pacific Islander	10,700	22.8	2,600	:
Two or more races	133,100	19.1 †	10,900	29.3
Age at time of survey				
18–24*	123,800	31.7%	8,200	30.1%
25–34	389,100	24.4 †	47,700	27.4
35–44	318,800	19.3 †	58,800	19.0 †
45–54	224,800	14.6 †	36,700	14.1 †
55 or older	154,800	16.0 †	19,000	12.2 †
Marital status				
Married*	168,500	16.7%	36,800	14.4%
Widowed/widowered	34,300	18.3	3,100	21.7
Separated	58,300	12.7 †	9,600	12.8
Divorced	233,300	14.5	30,900	15.2
Never married	715,900	24.8 †	90,000	24.6 †
Education[b]				
Less than high school*	750,500	23.1%	94,900	22.7%
High school graduate	273,700	19.6 †	36,500	19.4
Some college	133,900	14.7 †	23,100	18.8
College degree or more	43,600	11.0 †	12,700	6.3 †
Citizenship				
U.S. citizen*	1,156,800	21.0%	127,500	24.2%
Non-U.S. citizen	53,100	18.5	42,400	7.2 †
Military service				
Yes*	95,200	15.6%	9,200	15.9%
No	1,115,900	21.4 †	161,200	20.3

Note: Percentages and counts exclude missing data. Excludes 3.0% of state prisoners and 1.7% of federal prisoners who were missing responses on firearm possession during the offense. Details for counts may not sum to totals due to missing data. See appendix table 5 for standard errors.

*Comparison group.

†Difference with comparison group is significant at the 95% confidence level.

: Not calculated. Too few cases to provide a reliable estimate, or coefficient of variation is greater than 50%.

[a]Excludes persons of Hispanic/Latino origin, unless specified.

[b]Based on highest year of education completed.

Source: Bureau of Justice Statistics, Survey of Prison Inmates, 2016.

In general, the likelihood of state and federal prisoners having possessed a firearm during their crime decreased with age. Firearm possession among state prisoners ages 18 to 24 (32%) in 2016 was more common than among older prisoners. Federal prisoners ages 18 to 24 (30%) were more likely to possess a firearm than those age 35 or older (16%, not shown in table).

The difference in firearm possession between U.S. citizens (21%) and non-citizens (18%) in state prisons in 2016 was not statistically significant. Among federal prisoners serving a sentence in 2016, firearm possession was more than three times as high among U.S. citizens (24%) as non-citizens (7%).

Method, source, and process used to obtain the firearm

Among prisoners who possessed a firearm when they committed the offense for which they were imprisoned and who reported the source from which they obtained it, the most common source (43%) was off-the-street or the underground market **(table 5)**. Another 7% of state and 5% of federal prisoners stole the firearm, and 7% of state and 8% of federal prisoners reported that they obtained the firearm at the location of the crime.

TABLE 5

Among state and federal prisoners who had possessed a firearm during the offense for which they were serving time, sources and methods used to obtain a firearm, 2016

Source and method to obtain firearm	All prisoners	State	Federal
Purchased/traded at retail source	10.1%	9.7%	13.7%
Gun shop/store	7.5	7.2	9.6
Pawn shop	1.6	1.5	2.2
Flea market	0.4	:	:
Gun show	0.8	0.8	1.4
Obtained from individual	25.3%	26.0%	20.5%
Purchased/traded from family/friend	8.0	7.9	9.1
Rented/borrowed from family/friend	6.5	7.0	3.0
Gift/purchased for prisoner	10.8	11.2	8.4
Off the street/underground market[a]	43.2%	43.2%	42.9%
Theft[b]	6.4%	6.6%	4.7%
From burglary	1.5	1.5	:
From retail source	0.2	:	:
From family/friend	1.6	1.8	:
Unspecified theft[c]	3.1	3.3	1.8
Other source	17.4%	17.1%	20.1%
Found at location of crime/victim	6.9	6.7	7.9
Brought by someone else	4.6	4.7	3.6
Other[d]	5.9	5.6	8.5
Multiple sources[e]	2.5%	2.6%	2.0%
Estimated number of prisoners who possessed a firearm, excluding prisoners who did not report source[f]	256,400	227,100	29,300

Note: Prisoners were asked to report all sources and methods of obtaining any firearm they possessed during the offense, so details may not sum to totals. Each source is included in this table when multiple sources were reported. See *Methodology*. Percentages exclude missing data. Excludes 10.3% of state prisoners and 14.1% of federal prisoners who possessed a firearm during the offense and were missing responses on either source or method of obtaining the firearm. These prisoners were excluded either because they did not provide a valid response or they did not receive the questions due to providing an open-ended response to the previous question about type of weapon. See appendix table 6 for standard errors.

: Not calculated. Too few cases to provide a reliable estimate, or coefficient of variation is greater than 50%.

[a]Illegal sources of firearms that include markets for stolen goods, middlemen for stolen goods, criminals or criminal enterprises, or individuals or groups involved in sales of illegal drugs.

[b]Excludes theft from victim.

[c]Includes theft where the source could not be identified and theft other than from a burglary, retail location, family, or friend.

[d]Included if no source specified in the table was reported. Includes sources that did not fit into one of the existing categories, sources for which there were few responses such as bought online, or if there was not enough information to categorize the source. Examples of other sources include bought from an unknown source or obtained from a friend by an unknown method.

[e]Includes prisoners who reported multiple sources or methods that fit into more than one of the categories. Each reported source is included in the categories above.

[f]Includes prisoners who reported they carried or possessed a firearm and prisoners who reported a source or method.

Source: Bureau of Justice Statistics, Survey of Prison Inmates, 2016.

Among prisoners who possessed a firearm during the offense for which they were imprisoned, 7% of state and 10% of federal prisoners serving a sentence in 2016 bought or traded for the firearm from a gun shop or gun store. About 1% bought or traded for the firearm at a gun show. About a quarter (26%) of state prisoners and about a fifth (21%) of federal prisoners obtained a firearm that they possessed during their offense from an individual in a non-retail setting, such as a friend or family member.

Prisoners who reported that they had purchased or traded a firearm at a retail source were asked if they had obtained the firearm from a licensed dealer or private seller. Among prisoners who had possessed a firearm during the offense for which they were serving time, 8% of state and 11% of federal prisoners had purchased it from or traded with a licensed firearm dealer at a retail source **(table 6)**.

Prisoners who reported that they had purchased a firearm from a licensed firearm dealer at a retail source were further asked whether they bought the firearm under their own name and whether they knew a background check was conducted. Among those who had possessed a firearm during the offense for which they were imprisoned, 7% of state and 8% of federal prisoners had purchased it under their own name from a licensed firearm dealer at a retail source, while approximately 1% of state and 2% of federal prisoners had purchased a firearm from a licensed dealer at a retail source but did not purchase it under their own name (not shown in table).

Among all prisoners who purchased or traded a firearm from a licensed firearm dealer at a retail source (8.2%), the majority reported that a background check was conducted (6.7%).

TABLE 6

Among state and federal prisoners who had possessed a firearm during the offense for which they were serving time, processes used to obtain a firearm, 2016

Process to obtain firearm	All prisoners	State	Federal
Total	100%	100%	100%
Not purchased or traded at retail source	89.9%	90.3%	86.3%
Purchased or traded at retail source[a]	10.1%	9.7%	13.7%
Licensed firearm dealer at retail source	8.2	7.9	10.9
Purchased under own name[b]	6.9	6.8	8.4
Background check was reportedly conducted[c]	6.7	6.3	9.4
Private seller at retail source[d]	1.2	1.1	2.3
Unknown[e]	0.7	0.8	:
Estimated number of prisoners who possessed a firearm (with valid data)[f]	256,400	227,100	29,300

Note: Percentages exclude missing data. Excludes 10.3% of state prisoners and 14.1% of federal prisoners who possessed a firearm during the offense and were missing responses on source or method of obtaining the firearm. See appendix table 7 for standard errors.

: Not calculated. Too few cases to provide a reliable estimate, or coefficient of variation is greater than 50%.

[a]Includes prisoners who purchased or traded from a retail source, including a retail store, pawn shop, flea market, or gun show.

[b]Includes prisoners who purchased from a retail source, including a retail store, pawn shop, flea market, or gun show. Excludes prisoners who traded for a firearm from a retail source.

[c]Includes prisoners who purchased from a retail source, including a retail store, pawn shop, flea market, or gun show. Excludes prisoners who traded for a firearm from a retail source and prisoners who reported that a background check was not conducted or who were unaware as to whether one was conducted.

[d]Excludes private sellers other than at a retail source.

[e]Includes prisoners who purchased or traded a firearm from a retail source and were missing responses on whether a firearm was purchased or traded from a licensed firearm dealer or a private seller at a retail source.

[f]Includes prisoners who reported they carried or possessed a firearm and prisoners who reported a source or method.

Source: Bureau of Justice Statistics, Survey of Prison Inmates, 2016.

Use and source of firearms among all state and federal prisoners

About 1% of all state and federal prisoners used a firearm during the offense that they obtained from a retail source (table 7). About 2% of prisoners possessed a firearm that they obtained from a retail source, including a retail store, pawn shop, flea market, or gun show.

Thirteen percent of all state and federal prisoners used a firearm during the offense for which they were serving time in 2016.

TABLE 7

Firearm possession and use among all state and federal prisoners during the offense for which they were serving time, by type of controlling offense and source, 2016

	Percent of state and federal prisoners who—		Percent of state and federal prisoners who—	
Controlling offense[a]	Possessed a firearm[b]	Possessed a firearm that they obtained from a retail source[c]	Used a firearm[d]	Used a firearm that they obtained from a retail source[e]
Total	20.8%	1.9%	12.8%	1.3%
Violent*	29.3%	2.8%	23.1%	2.3%
Homicide[f]	43.5	5.9	37.0	5.2
Robbery	43.5	1.8	31.5	1.3
Property	4.8% †	0.5% †	1.9% †	:
Drug	9.6% †	1.0% †	0.8% †	0.1% †
Public order	23.6% †	1.7% †	5.5% †	0.6% †

Note: Percentages exclude missing data. Excludes 2.8% of prisoners who were missing responses on firearm possession during the offense and 1.2% of prisoners who had a valid response to firearm possession but were missing a controlling offense. Retail source includes purchasing or trading the firearm from a retail store, pawn shop, flea market, or gun show. Use includes prisoners who showed a firearm to anyone, pointed a firearm at anyone, or discharged a firearm during the controlling offense. See appendix table 8 for standard errors.

*Comparison group.

† Difference with comparison group is significant at the 95% confidence level across main categories, and no testing was done on subcategories (e.g., homicide).

: Not calculated. Too few cases to provide a reliable estimate, or coefficient of variation is greater than 50%.

[a]See *Methodology* for more information on how controlling offense was measured.

[b]Includes state and federal prisoners who reported a valid response to firearm possession.

[c]Includes state and federal prisoners who reported a valid response to firearm possession and source.

[d]Includes state and federal prisoners who reported a valid response to firearm possession and use.

[e]Includes state and federal prisoners who reported a valid response to firearm possession, source, and use.

[f]Includes murder and both non-negligent and negligent manslaughter.

Source: Bureau of Justice Statistics, Survey of Prison Inmates, 2016.

Methodology

Survey of Prison Inmates

The findings in this report are primarily based on data collected through the 2016 Survey of Prison Inmates (SPI). The SPI is a periodic, cross-sectional survey of the state and sentenced federal prison populations. Its primary objective is to produce national statistics of the state and sentenced federal prison populations across a variety of domains, including—but not limited to—demographic characteristics, current offense and sentence, incident characteristics, firearm possession and sources, criminal history, socioeconomic characteristics, family background, drug and alcohol use and treatment, mental and physical health and treatment, and facility programs and rule violations. RTI International served as BJS's data collection agent for the 2016 SPI under a cooperative agreement (award no. 2011-MU-MU-K070). From January through October 2016, data were collected through face-to-face interviews with prisoners using computer-assisted personal interviewing (CAPI).

Prior iterations of the SPI were known as the Survey of Inmates in State and Federal Correctional Facilities (SISFCF), which was renamed with the 2016 implementation. The first survey of state prisoners was fielded in 1974 and thereafter in 1979, 1986, 1991, 1997, and 2004. The first survey of federal prisoners was fielded in 1991, along with the survey of state prisoners, and since then both surveys have been conducted at the same time using the same questionnaire and administration.

The target population for the 2016 SPI was prisoners ages 18 and older who were held in a state prison or had a sentence to federal prison in the United States during 2016. Similar to prior iterations, the 2016 survey was a stratified two-stage sample design in which prisons were selected in the first stage and prisoners within sampled facilities were selected in the second stage. The SPI sample was selected from a universe of 2,001 unique prisons (1,808 state and 193 federal) that were either enumerated in the 2012 Census of State and Federal Adult Correctional Facilities or had opened between the completion of the census and July 2014 when the SPI sample of prisons was selected. A total of 364 prisons (306 state and 58 federal) participated in the 2016 survey out of the 385 selected (324 state and 61 federal) for interviewing. The first-stage response rate (i.e., the response rate among selected prisons) was 98.4% (98.1% among state prisons and 100% among federal prisons).[3] A total of 24,848 prisoners participated (20,064 state and 4,784 federal) in the 2016 SPI based on a sample of 37,058 prisoners (30,348 state and 6,710 federal). The second-stage response rate (i.e., the response rate among selected prisoners) was 70.0% (69.3% among state prisoners and 72.8% among federal prisoners).[4]

Responses from interviewed prisoners in the 2016 SPI were weighted to provide national estimates. Each interviewed prisoner was assigned an initial weight corresponding to the inverse of the probability of selection within each sampled prison. A series of adjustment factors were applied to the initial weight to minimize potential bias due to non-response and to provide national estimates.

For more information on the 2016 SPI methodology, see *Methodology: Survey of Prison Inmates, 2016* (NCJ 252210, BJS web, July 2019).

Standard errors and tests of significance

When national estimates are derived from a sample, as with the SPI, caution must be used when comparing one estimate to another or when comparing estimates between years. Although one estimate may be larger than another, estimates based on a sample rather than a complete enumeration of the population have some degree of sampling error. The sampling error of an estimate depends on several factors, including the size of the estimate, the number of completed interviews, and the intracluster correlation of the outcome within prisons. When the sampling error around an estimate is taken into account, estimates that appear different may not be statistically different. One measure of the sampling error associated with an estimate is the standard error. The standard error may vary from one estimate to the next. Standard errors in this report were estimated using Taylor Series Linearization to account for the complex design of the SPI in producing the variance estimates.

[3]A total of 15 prisons (12 state and 3 federal) that were sampled were deemed ineligible for the 2016 SPI. For more information, see *Methodology: Survey of Prison Inmates, 2016* (NCJ 252210, BJS web, July 2019).

[4]There were 10,661 sampled prisoners who were eligible for the survey but did not participate. Another 1,549 sampled prisoners were deemed ineligible for the survey. For more information, see *Methodology: Survey of Prison Inmates, 2016* (NCJ 252210, BJS web, July 2019).

Readers may use the estimates and standard errors of the estimates provided in this report to generate a 95% confidence interval around the estimates as a measure of the margin of error. Typically, multiplying the standard error by 1.96 and then adding or subtracting the result from the estimate produces the confidence interval. This interval expresses the range of values with which the true population parameter is expected to fall 95% of the time if the same method is used to select different samples.

For small samples and estimates close to 0%, the use of the standard error to construct the 95% confidence interval may not be reliable. Therefore, caution should be used when interpreting the estimates. Caution should also be used if constructing a 95% confidence interval, which would include zero in these cases, because the estimate may not be distinguishable from zero.

The standard errors have been used to compare estimates of firearm possession during the offense, firearm use during the crime, and type of firearm possessed. They have also been used to compare firearm possession among selected groups of prisoners that have been defined by demographic characteristics and controlling offense. To facilitate the analysis, rather than provide the detailed estimates for every standard error, differences in the estimates for subgroups in the relevant tables in this report have been tested and notated for significance at the 95% level of confidence. Readers should reference the tables for testing on specific findings. Unless otherwise noted, findings described in this report as higher, lower, or different passed a test at the 0.05 level of statistical significance (95% confidence level).

Measurement of firearm possession and source

The 2016 SPI was restricted to prisoners age 18 or older at the time of the survey. Firearms analyses in this report were restricted to state and federal prisoners who were sentenced or state prisoners who were convicted but were awaiting sentencing. This report excludes prisoners who were awaiting trial (i.e., unconvicted) or a revocation hearing or who were held for other reasons. Unconvicted prisoners, such as those awaiting trial or being held for other reasons like safekeeping or a civil commitment, were excluded from this report because they were not asked questions about firearm possession to protect against self-incrimination. (See appendix 1, *Questions related to firearms in the Survey of Prison Inmates, 2016*.) Of the estimated 1,421,700 state and federal prisoners in 2016, an estimated 287,400 were armed with a firearm, 1,094,200 were not armed with a firearm, 23,800 did not know or refused to answer the question, and 16,300 were not asked the question because they were not convicted or they stopped the interview before responding to the question.[5]

To determine whether prisoners possessed a firearm at the time of the offense for which they were serving time in prison, respondents were first asked whether they had carried, possessed, or used a weapon when the controlling offense occurred. Respondents could report that they carried, possessed, or used a firearm or another weapon such as a toy or BB gun, knife, other sharp object, or blunt object. Weapons other than firearms, including toy and BB guns, were excluded from this report. Multiple weapons and firearms could be reported by respondents.

Of the respondents who were asked about possessing a firearm during the offense for which they were imprisoned, about 3.0% of state and 1.7% of federal prisoners in 2016 were missing responses on firearm possession. These prisoners were excluded from the analyses in this report. All prisoners who reported they carried, possessed, or used a firearm during the offense were asked whether they had obtained the firearm because they were planning to carry, possess, or use it during the offense. They were also asked whether they showed, pointed, or fired the firearm during the offense. Respondents who reported that they fired the firearm were also asked whether they shot anyone and, if so, whether anyone they shot had died. Of the respondents who possessed a firearm during the offense, about 3.1% of state and 3.5% of federal prisoners in 2016 were missing responses on how they used the firearm. These prisoners were excluded from the analyses in figure 1, tables 1 through 3, and table 7.

To measure the type of firearm possessed by prisoners, respondents were asked whether they had carried, possessed, or used a handgun, rifle, shotgun, or some other type of firearm during the offense for which they were imprisoned. About 0.3% of state prisoners and 0.2% of federal prisoners in 2016 were missing responses on the type of firearm that they possessed. These prisoners, along with prisoners who were missing a response on firearm possession, were excluded from the analyses in table 3.

[5]The SPI sample was weighted to the state and federal prison populations that were eligible to be sampled in the survey. See *Methodology: Survey of Prison Inmates, 2016* (NCJ 252210, BJS web, July 2019).

To measure the source and method of obtaining the firearm possessed by prisoners during their crime, two separate questions were asked in the survey. The first question asked how the prisoners obtained the firearm, and multiple responses could be reported in the 2016 SPI. Possible responses included stole it, rented it, borrowed it from or were holding it for somebody, traded something for it, bought it, someone bought it for them, someone gave it as a gift, found it or it was at the location where the offense occurred, it was brought by someone else, or other. If respondents specified an "other" method of obtaining the firearm, then the field interviewers entered the respondents' answers into a text field. These responses originally reported as "other" were coded to one of the existing response categories if possible.

The second question asked where prisoners obtained the firearm, and multiple responses could be reported in the 2016 SPI. Respondents received this question if they reported that they stole, rented, borrowed from or were holding for somebody, traded something for, or bought the firearm. Possible responses included gun shop or gun store; pawn shop; flea market; gun show; from a victim, family member, or friend; from a fence (a middleman for stolen goods) or underground market; off the street or from a drug dealer; in a burglary; online or the internet; or other. Fewer than 1% of state and federal prisoners reported obtaining a firearm online. These responses were included in table 5 in the "other" category due to the small number of sample cases. If respondents specified an "other" source of obtaining a firearm, then the field interviewers entered the respondents' answers into a text field. Responses originally reported as "other" were coded to one of the existing response categories if possible.

The responses from these two questions were used to create the source and method categories in figure 1 and tables 5 through 7. Approximately 10.3% of state and 14.1% of federal prisoners in 2016 who possessed a firearm during the offense for which they were serving a sentence were missing responses on source or method of obtaining the firearm. These prisoners were excluded from figure 1 and tables 5 through 7.

Prisoners who reported purchasing or trading a firearm from a retail source (gun shop or gun store, pawn shop, flea market, or gun show) were asked if they purchased or traded it from a licensed firearm dealer or a private seller. Prisoners who reported they purchased a firearm from a retail source were further asked whether they bought the firearm under their own name and whether the seller did a firearm purchase background check before selling them the firearm. About 1% of the respondents who possessed a firearm during the offense purchased or traded it from a retail source and were missing responses on whether they bought the firearm from a licensed dealer or private seller. About 1% of respondents who possessed a firearm during the offense purchased it from a retail source and were missing responses on whether the firearm was purchased under their own name or whether a background check was conducted.

Measurement of controlling offense

The way controlling offense was measured through the 2016 SPI, and reflected in this report, varies by sentence status and the number of offenses of prisoners:

- For sentenced prisoners and those awaiting sentencing with one offense, that offense is the controlling offense.
- For sentenced prisoners with multiple offenses and sentences, the controlling offense is the one with the longest sentence.
- For sentenced prisoners with multiple offenses and one sentence and those awaiting sentencing with multiple offenses, the controlling offense is the most serious offense. For this report, violent offenses are considered most serious, followed by property, drug, public-order, and all other offenses.

For prisoners who were convicted but awaiting sentencing, the controlling offense is the most serious offense.

Appendix 1. Questions related to firearms in the Survey of Prison Inmates, 2016

This appendix includes the questions from the 2016 SPI that were used to measure the firearms' constructs in this report. Text that appears in capital letters in the questions was not read out loud to respondents. That text reflects programming instructions for the CAPI instrument, instructions to field interviewers who conducted the interviews, or response options that were not read out loud to respondents but were coded by the field interviewers during the interviews.

Questions

CJ39. (ASK IF RESPONDENT REPORTED BEING SENTENCED IN CJ1 OR CJ3 OR IF RESPONDENT REPORTED HE/SHE WAS AWAITING SENTENCING IN CJH2A.) Did you carry, possess, or use a weapon when the (INSERT CONTROLLING OFFENSE) occurred?

- YES
- NO (SKIP TO NEXT SECTION)

CJH1. How many weapons did you carry, possess, or use when the (INSERT CONTROLLING OFFENSE) occurred?

- ONE
- TWO OR MORE

CJH2. What (INSERT "kind of weapon was it?" OR "kinds of weapons were they?") CHECK ALL THAT APPLY.

- FIREARM
- TOY OR BB GUN (INCLUDE FAKE OR REPLICA GUNS)
- KNIFE
- OTHER SHARP OBJECT (SCISSORS, ICE PICK, AX, ETC.)
- BLUNT OBJECT (ROCK, CLUB, BLACKJACK, ETC.)
- ANOTHER WEAPON
  - What kinds of weapons were they?
    - INTERVIEWER: RECORD RESPONSE VERBATIM.

CJH3. (ASK IF RESPONDENT REPORTED "FIREARM" IN CJH2.) How many firearms did you carry, possess, or use when the (INSERT CONTROLLING OFFENSE) occurred?

- ENTER NUMBER OF FIREARMS

CJH4. (ASK IF RESPONDENT REPORTED "FIREARM" IN CJH2.) What (INSERT "type of firearm was it?" OR "types of firearms were they?") CHECK ALL THAT APPLY.

- A HANDGUN
- A RIFLE
- A SHOTGUN
- SOME OTHER TYPE OF FIREARM
  - What type of firearm?
    - INTERVIEWER: RECORD RESPONSE VERBATIM.

CJH5. (ASK IF RESPONDENT REPORTED "FIREARM" IN CJH2.) How did you obtain the (INSERT "firearm" OR "firearms") that you carried, possessed, or used during the (INSERT CONTROLLING OFFENSE)? Any others? CHECK ALL THAT APPLY.

- STOLE IT (GO TO CJH6)
- RENTED IT (GO TO CJH6)
- BORROWED FROM OR WAS HOLDING FOR SOMEBODY (GO TO CJH6)
- TRADED SOMETHING FOR IT (GO TO CJH6)
- BOUGHT IT (GO TO CJH6)
- SOMEONE BOUGHT IT FOR ME (GO TO CJH7)
- SOMEONE GAVE IT TO ME AS A GIFT (GO TO CJH9)
- FOUND IT/WAS AT LOCATION WHERE OFFENSE OCCURRED (GO TO CJH9)
- WAS BROUGHT BY SOMEONE ELSE (GO TO CJH9)
- OTHER
  - How did you obtain the firearm that you carried, possessed, or used during the offense?
    - INTERVIEWER: RECORD RESPONSE VERBATIM.

CJH6. (ASK IF RESPONDENT REPORTED "FIREARM" IN CJH2 AND REPORTED IN CJH5 HE/SHE "STOLE IT", "RENTED IT", "BORROWED FROM OR WAS HOLDING FOR SOMEBODY", "TRADED SOMETHING FOR IT", OR "BOUGHT IT".) Where did you obtain the (INSERT TYPE OF FIREARM REPORTED IN CJH4)? CHECK ALL THAT APPLY.

- GUN SHOP OR GUN STORE (GO TO CJH6A)
- PAWN SHOP (GO TO CJH6A)
- FLEA MARKET (GO TO CJH6A)
- GUN SHOW (GO TO CJH6A)
- FROM THE VICTIM(S) (GO TO CJH9)
- FROM A FAMILY MEMBER (GO TO CJH9)
- FROM A FRIEND (GO TO CJH9)
- FROM A FENCE/BLACK MARKET SOURCE (GO TO CJH9)
- OFF THE STREET/FROM A DRUG DEALER (GO TO CJH9)
- IN A BURGLARY (GO TO CJH9)
- ONLINE/THE INTERNET (GO TO CJH9)
- OTHER
  - Where did you obtain the (INSERT TYPE OF FIREARM REPORTED IN CJH4)?
    - INTERVIEWER: RECORD RESPONSE VERBATIM.

Continued on next page

Appendix 1. Questions related to firearms in the Survey of Prison Inmates, 2016 (continued)

CJH6a. (ASK IF RESPONDENT REPORTED IN CJH6 THAT THE FIREARM WAS FROM A "GUN SHOP OR GUN STORE", "PAWN SHOP", "FLEA MARKET", OR "GUN SHOW".) When you obtained the (INSERT TYPE OF FIREARM REPORTED IN CJH4) was it from a licensed firearm dealer or a private seller?

- LICENSED FIREARM DEALER
- PRIVATE SELLER

CJH6b. (ASK IF RESPONDENT REPORTED IN CJH5 THAT HE/SHE "BOUGHT IT" AND IN CJH6 REPORTED THAT THE FIREARM WAS FROM A "GUN SHOP OR GUN STORE", "PAWN SHOP", "FLEA MARKET", OR "GUN SHOW".) Did you buy the (INSERT TYPE OF FIREARM REPORTED IN CJH4) under your own name?

- YES
- NO
- NO PAPERWORK WAS REQUIRED

CJH6c. (ASK IF RESPONDENT REPORTED IN CJH5 THAT HE/SHE "BOUGHT IT" AND REPORTED IN CJH6 THAT THE FIREARM WAS FROM A "GUN SHOP OR GUN STORE", "PAWN SHOP", "FLEA MARKET", OR "GUN SHOW".) Did the seller do a firearm purchase background check before selling you the gun?

- YES
- NO

CJH6d. (ASK IF RESPONDENT REPORTED IN CJH5 THAT HE/SHE "BOUGHT IT" AND REPORTED IN CJH6 THAT THE FIREARM WAS FROM A "GUN SHOP OR GUN STORE", "PAWN SHOP", "FLEA MARKET", OR "GUN SHOW".) Did you buy the (INSERT TYPE OF FIREARM REPORTED IN CJH4) directly or did someone else buy it for you?

- INMATE BOUGHT
- SOMEONE ELSE BOUGHT

CJH7. (ASK IF RESPONDENT REPORTED "SOMEONE ELSE BOUGHT IT FOR ME" IN CJH5.) Where did that person obtain the (INSERT TYPE OF FIREARM REPORTED IN CJH4)?

- GUN SHOP OR GUN STORE
- PAWN SHOP
- FLEA MARKET
- GUN SHOW
- FROM THE VICTIM(S)
- FROM A FAMILY MEMBER
- FROM A FRIEND
- FROM A FENCE/BLACK MARKET SOURCE
- OFF THE STREET/FROM A DRUG DEALER
- IN A BURGLARY
- ONLINE/THE INTERNET
- OTHER
  - Where did that person obtain the (INSERT TYPE OF FIREARM REPORTED IN CJH4)?
    - INTERVIEWER: RECORD RESPONSE VERBATIM.

CJH8. (ASK IF RESPONDENT REPORTED "SOMEONE ELSE BOUGHT IT FOR ME" IN CJH5.) Why did someone else obtain the (INSERT TYPE OF FIREARM REPORTED IN CJH4) for you? CHECK ALL THAT APPLY.

- COULD NOT TRAVEL TO WHERE THE SELLER WAS
- NOT ALLOWED BECAUSE TOO YOUNG
- NOT ALLOWED BECAUSE OF CRIMINAL RECORD
- THEY COULD GET IT MORE QUICKLY OR EASILY
- DID NOT WANT TO BE LINKED TO FIREARM PURCHASE
- OTHER
  - Why did someone else obtain the (INSERT TYPE OF FIREARM REPORTED IN CJH4) for you?
    - INTERVIEWER: RECORD RESPONSE VERBATIM.

CJH9. Did you get the (INSERT TYPE OF FIREARM REPORTED IN CJH4) because you were **planning** to carry, possess, or use it during the (INSERT CONTROLLING OFFENSE)?

- YES
- NO

CJH10. Did you show or point (INSERT "the firearm" OR "any of the firearms") at anyone during the (INSERT CONTROLLING OFFENSE)?

- YES
- NO

CJH11. Did you fire (INSERT "the firearm" OR "any of the firearms") during the (INSERT CONTROLLING OFFENSE)?

- YES
- NO (SKIP TO NEXT SECTION)

CJH12. Did you shoot anyone?

- YES
- NO (SKIP TO NEXT SECTION)

CJH13. Did anyone you shot die?

- YES
- NO

APPENDIX TABLE 1

Standard errors for figure 1: Percent of all state and federal inmates who had possessed or used a firearm during their offense, 2016

Characteristic	Possessed	Used
Any gun	0.64%	0.51%
Handgun	0.59	0.46
Gun they obtained from retail source	0.13	0.12

Source: Bureau of Justice Statistics, Survey of Prison Inmates, 2016.

APPENDIX TABLE 2

Standard errors for table 1: Firearm possession and use among state and federal prisoners during the offense for which they were serving time, by type of controlling offense, 2016

		Percent of state prisoners who—			Percent of federal prisoners who—	
Controlling offense	Estimated number of state prisoners	Possessed a firearm	Used a firearm	Estimated number of federal prisoners	Possessed a firearm	Used a firearm
Total	31,100	0.69%	0.57%	8,300	1.76%	0.71%
Violent	22,400	0.90%	0.73%	2,700	2.87%	2.83%
Homicide	10,900	1.16	1.12	700	6.53	4.75
Rape/sexual assault	9,900	0.36	0.22	600	:	:
Robbery	6,700	1.32	1.28	1,600	3.73	3.80
Assault	5,900	1.34	1.24	700	5.15	4.52
Other violent	2,100	2.03	1.73	300	8.42	:
Property	7,800	0.53%	0.32%	2,000	0.83%	:
Burglary	3,900	0.80	0.54	100	:	:
Other property	5,800	0.58	0.33	2,000	0.81	:
Drug	11,400	0.68%	0.20%	5,400	0.87%	0.21%
Trafficking	9,700	0.83	0.24	5,000	0.88	0.21
Possession	3,400	1.06	:	600	:	:
Other/unspecified drug	700	:	:	600	:	:
Public order	8,400	1.35%	0.58%	3,600	3.55%	0.88%
Weapons	3,000	2.02	1.70	2,700	2.02	1.60
Other public order	7,200	0.70	0.42	3,800	0.89	:
Other	600	:	:	300	:	:
Unknown	1,400	1.61%	:	400	:	:

: Not calculated. Too few cases to provide a reliable estimate, or coefficient of variation is greater than 50%.
Source: Bureau of Justice Statistics, Survey of Prison Inmates, 2016.

APPENDIX TABLE 3

Standard errors for table 2: Among state and federal prisoners who possessed a firearm during the offense for which they were serving time, extent of firearm use, 2016

Firearm use	State prisoners	Federal prisoners	State prisoners		Federal prisoners	
			Violent offense	Non-violent offense	Violent offense	Non-violent offense
Obtained firearm because planned to use in controlling offense						
Yes	0.81%	1.57%	0.81%	2.00%	4.01%	1.88%
No	0.81	1.57	0.81	2.00	4.01	1.88
Used firearm	1.11%	1.92%	0.85%	1.83%	3.86%	1.57%
Discharged	1.34%	1.17%	1.36%	1.47%	3.58%	1.14%
Killed victim	1.28	0.75	1.40	:	2.49	:
Injured/shot victim but did not kill victim	0.73	0.55	0.86	0.89	:	:
Discharged firearm but did not shoot anyone	0.47	0.98	0.51	1.17	2.16	1.02
Did not discharge	0.97%	1.60%	1.21%	1.24%	4.99%	0.87%
Did not use firearm	1.11%	1.92%	0.85%	1.83%	3.86%	1.57%
Estimated number of prisoners who possessed a firearm (with valid data)	10,100	3,100	9,200	3,400	1,200	2,200

: Not calculated. Too few cases to provide a reliable estimate or coefficient of variation is greater than 50%.

Source: Bureau of Justice Statistics, Survey of Prison Inmates, 2016.

APPENDIX TABLE 4

Standard errors for table 3: Firearm possession and use among state and federal prisoners during the offense for which they were serving time, by type of firearm, 2016

Type of firearm	Percent of prisoners who possessed a firearm			Percent of prisoners who used a firearm		
	All prisoners	State	Federal	All prisoners	State	Federal
Firearm	0.64	0.69%	1.76%	0.51	0.57%	0.71%
Handgun	0.59	0.64	1.63	0.46	0.51	0.67
Rifle	0.10	0.10	0.28	0.07	0.08	0.13
Shotgun	0.11	0.12	0.22	0.09	0.10	0.09
No firearm	0.64	0.69	1.76	0.51	0.57	0.71
Estimated number of prisoners (with valid data)	32,100	31,000	8,300	32,100	31,000	8,300

Source: Bureau of Justice Statistics, Survey of Prison Inmates, 2016.

APPENDIX TABLE 5

Standard errors for table 4: Firearm possession among state and federal prisoners during the offense for which they were serving time, by demographic characteristics, 2016

	State		Federal	
Demographic characteristic	Number of prisoners	Percent of prisoners who possessed a firearm during the offense	Number of prisoners	Percent of prisoners who possessed a firearm during the offense
Sex				
Male	30,700	0.74%	8,200	1.88%
Female	5,200	0.96	1,300	1.00
Race/Hispanic origin				
White	16,500	0.64%	3,900	2.28%
Black	16,200	0.91	5,600	2.02
Hispanic	12,400	1.26	8,000	1.70
American Indian/Alaska Native	2,500	2.94	800	5.18
Asian/Native Hawaiian/Other Pacific Islander	1,600	4.69	600	:
Two or more races	5,000	1.19	1,200	3.50
Age at time of survey				
18–24	8,200	1.71%	1,000	5.69%
25–34	13,700	1.00	3,200	2.57
35–44	9,500	0.94	3,400	1.68
45–54	9,100	0.76	2,400	1.68
55 or older	7,700	1.02	2,200	2.02
Marital status				
Married	6,300	1.06%	3,100	1.77%
Widowed/widowered	2,000	2.10	400	5.93
Separated	2,700	1.34	1,200	3.11
Divorced	10,600	0.97	2,200	1.58
Never married	20,100	0.81	5,800	2.10
Education				
Less than high school	21,500	0.83%	6,000	2.18%
High school graduate	8,500	0.88	2,100	1.69
Some college	5,000	0.96	2,000	2.08
College degree or more	2,500	1.43	2,000	1.83
Citizenship				
U.S. citizen	30,000	0.69%	10,700	1.87%
Non-U.S. citizen	3,700	2.04	9,500	1.09
Military service				
Yes	4,800	1.07%	1,200	2.98%
No	28,700	0.72	8,200	1.80

: Not calculated. Too few cases to provide a reliable estimate, or coefficient of variation is greater than 50%.

Source: Bureau of Justice Statistics, Survey of Prison Inmates, 2016.

APPENDIX TABLE 6

Standard errors for table 5: Among state and federal prisoners who had possessed a firearm during the offense for which they were serving time, sources and methods used to obtain a firearm, 2016

Source and method to obtain firearm	All prisoners	State	Federal
Purchased/traded at retail source	0.66%	0.70%	2.07%
Gun shop/store	0.54	0.56	1.87
Pawn shop	0.27	0.29	0.62
Flea market	0.13	:	:
Gun show	0.16	0.17	0.44
Obtained from individual	0.87%	0.94%	2.02%
Purchased/traded from family/friend	0.59	0.65	1.27
Rented/borrowed from family/friend	0.47	0.52	0.54
Gift/purchased for prisoner	0.69	0.75	1.40
Off the street/underground market	1.07%	1.13%	3.26%
Theft	0.48%	0.53%	0.79%
From burglary	0.22	0.24	:
From retail source	0.07	:	:
From family/friend	0.26	0.29	:
Unspecified theft	0.31	0.34	0.53
Other source	0.78%	0.85%	1.80%
Found at location of crime/victim	0.50	0.53	1.31
Brought by someone else	0.45	0.49	0.87
Other	0.51	0.55	1.40
Multiple sources	0.27%	0.29%	0.50%
Estimated number of prisoners who possessed a firearm, excluding prisoners who did not report source	9,900	9,500	2,800

: Not calculated. Too few cases to provide a reliable estimate, or coefficient of variation is greater than 50%.

Source: Bureau of Justice Statistics, Survey of Prison Inmates, 2016.

APPENDIX TABLE 7

Standard errors for table 6: Among state and federal prisoners who had possessed a firearm during the offense for which they were serving time, processes used to obtain a firearm, 2016

Process to obtain firearm	All prisoners	State	Federal
Not purchased or traded at retail source	0.66%	0.70%	2.07%
Purchased or traded at retail source	0.66%	0.70%	2.07%
Licensed firearm dealer at retail source	0.60	0.63	2.08
Purchased under own name	0.54	0.57	1.89
Backgroundcheck was reportedly conducted	0.54	0.56	1.93
Private seller at retail source	0.19	0.20	0.63
Unknown	0.21	0.24	:
Estimated number of prisoners who possessed a firearm (with valid data)	9,900	9,500	2,800

: Not calculated. Too few cases to provide a reliable estimate, or coefficient of variation is greater than 50%.

Source: Bureau of Justice Statistics, Survey of Prison Inmates, 2016.

APPENDIX TABLE 8

Standard errors for table 7: Firearm possession and use among all state and federal prisoners during the offense for which they were serving time, by type of controlling offense and source, 2016

	Percent of state and federal prisoners who—		Percent of state and federal prisoners who—	
Controlling offense	Possessed a firearm	Possessed a firearm that they obtained from a retail source	Used a firearm	Used a firearm that they obtained from a retail source
Total	0.64%	0.13%	0.51%	0.12%
Violent	0.88%	0.23%	0.72%	0.21%
Homicide	1.14	0.63	1.10	0.62
Robbery	1.25	0.29	1.22	0.25
Property	0.50%	0.15%	0.30%	:
Drug	0.52%	0.17%	0.15%	0.04%
Public order	1.35%	0.27%	0.48%	0.17%

: Not calculated. Too few cases to provide a reliable estimate, or coefficient of variation is greater than 50%.

Source: Bureau of Justice Statistics, Survey of Prison Inmates, 2016.

The Bureau of Justice Statistics of the U.S. Department of Justice is the principal federal agency responsible for measuring crime, criminal victimization, criminal offenders, victims of crime, correlates of crime, and the operation of criminal and civil justice systems at the federal, state, tribal, and local levels. BJS collects, analyzes, and disseminates reliable statistics on crime and justice systems in the United States, supports improvements to state and local criminal justice information systems, and participates with national and international organizations to develop and recommend national standards for justice statistics. Jeffrey H. Anderson is the director.

This report was written by Mariel Alper and Lauren Glaze of BJS. Mariel Alper conducted statistical analyses. Marcus Berzofsky and John Bunker of RTI International provided statistical review. Danielle Kaeble, Laura Maruschak, Todd Minton, and Stephanie Mueller verified the report. Lauren Glaze was the BJS project manager for the 2016 Survey of Prison Inmates.

Eric Hendrixson and Jill Thomas edited the report. Tina Dorsey and Morgan Young produced the report.

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EXHIBIT "3"

U.S.

Rifles Used in San Bernardino Shooting Illegal Under State Law

Weapons were legally purchased with magazine locking devices but altered to make them more powerful, ATF finds

This photo provided by the San Bernardino County Sheriff's Department shows weapons carried by suspects at the scene of a shootout in San Bernardino, Calif. PHOTO: ASSOCIATED PRESS

By Ashby Jones and Dan Frosch
Updated Dec. 3, 2015 10:51 pm ET

The rifles used in the San Bernardino mass shooting were illegal under California law because they were modified and violated the state's ban on assault weapons, the Bureau of Alcohol, Tobacco, Firearms and Explosives determined on Thursday.

Syed Rizwan Farook and Tashfeen Malik, the suspects in the Wednesday shooting that left 14 people dead and 21 wounded, were armed with four guns. They carried two .223-caliber semiautomatic weapons and two 9mm semiautomatic pistols, according to San Bernardino Police Chief Jarrod Burguan.

The two semiautomatic rifles were versions of the popular AR-15 model, according to San Bernardino officials. One was made by DPMS Inc., and the other by Smith & Wesson.

While they were originally sold legally, with magazine locking devices commonly known as bullet buttons, the rifles were subsequently altered in different ways to make them more powerful, according to Meredith Davis, a special agent with the ATF.

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The Smith & Wesson rifle was changed in an attempt to enable it fire in fully automatic mode, while the DPMS weapon was modified to use a large-capacity magazine, she said.

Those alterations made the weapons unlawful under California's ban on assault weapons, which bans guns with magazines that can detach for quick reloading.

The state legally allows the sale and ownership of assault weapons that have fixed magazines.

Ms. Davis said authorities were working to determine who purchased the rifles, and whether they were connected to Mr. Farook. Ms. Davis said one handgun was made by Llama and the other was made by Springfield Armory.

The models of the handguns weren't specified. California approves the sale and use of certain handguns. Ms. Davis said the handguns were purchased by Mr. Farook.

FBI and police investigators are seen around a vehicle where two suspects were shot following a mass shooting in San Bernardino, Calif., on Wednesday. The rifles used in the attack were illegal under California law because they were modified and violated the state's ban on assault weapons, the ATF determined Thursday. REUTERS/Mike Blake PHOTO: MIKE BLAKE/REUTERS

The purchases of the two rifles and two handguns carried by the two suspects in the shootings all passed the background checks required by federal law, according to Ms. Davis.

Under federal law, all firearm purchases made through federally licensed dealers are subject to a review with the Federal Bureau of Investigation.

Such a review checks only limited categories of information, like whether a purchaser has a criminal history or has been involuntarily committed to a mental-health facility. Federal background checks wouldn't turn up other possible red flags, like whether a purchaser has ties to terrorist organizations.

Write to Ashby Jones at ashby.jones@wsj.com and Dan Frosch at dan.frosch@wsj.com

EXHIBIT "4"

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Mexico sets 1st half murder record, up 5.3%

July 22, 2019

MEXICO CITY (AP) — Mexico set a new record for homicides in the first half of the year as the number of murders grew by 5.3% compared to the same period of 2018, fueled partly by cartel and gang violence in several states.

Mexico saw 3,080 killings in June, an increase of over 8% from the same month a year ago, according to official figures. The country of almost 125

million now sees as many as 100 killings per day nationwide.

The 17,608 killings in the first half of 2019 is the most since comparable records began being kept in 1997, including the peak year of Mexico's drug war in 2011. Officials said 16,714 people were killed in the first half of 2018.

In particular, drug cartel turf wars have become increasingly bloody in the northern state of Sonora, where the number of homicides was up by 69% in the first half of the year. But in Sinaloa, where the cartel of convicted drug lord Joaquin "El Chapo" Guzman is based, homicides declined by 23% so far this year compared to last.

Given cutbacks and a widespread reorganization of security forces under President Andrés Manuel López Obrador, it is not clear who, if anyone, is doing the analysis and intelligence work to find out exactly which conflicts are causing the rise in homicides.

"I could give you 10 potential, plausible reasons, but the truth is we don't know, and that is perhaps the biggest problem," said security analyst Alejandro Hope. "There is very little systematic research that would allow us to conclude what is really happening."

And other types of crime, like extortion, have become increasingly frequent and violent.

As if to underscore that, officials said Monday the five men killed Sunday at a bar in the resort of Acapulco were allegedly part of a gang of extortionists who shook down business owners for protection payments.

Guerrero state prosecutor Jorge Zuriel "we now know that the members of this gang met daily at this bar to coordinate charging extortion payments and to collect the daily take."

One suspect has been arrested in the shootings, which left six people wounded. Zuriel said the killers were members of a rival gang.

Table 1

Crime in the United States

by Volume and Rate per 100,000 Inhabitants, 1998–2017

Year	Population[1]	**Violent crime[2]**	*Violent crime rate*	Murder and nonnegligent manslaughter	*Murder and nonnegligent manslaughter rate*	Rape (revised definition)[3]	*Rape (revised definition) rate*[3]	Rape (legacy definition)[4]	*Rape (legacy definition) rate*[4]	Robbery	*Robbery rate*	Aggravated assault	*Aggravated assault rate*	**Property crime**	*Property crime rate*	Burglary	*Burglary rate*	Larceny-theft	*Larceny-theft rate*	Motor vehicle theft	*Motor vehicle theft rate*
1998	270,248,003	1,533,887	567.6	16,974	6.3			93,144	34.5	447,186	165.5	976,583	361.4	10,951,827	4,052.5	2,332,735	863.2	7,376,311	2,729.5	1,242,781	459.9
1999	272,690,813	1,426,044	523.0	15,522	5.7			89,411	32.8	409,371	150.1	911,740	334.3	10,208,334	3,743.6	2,100,739	770.4	6,955,520	2,550.7	1,152,075	422.5
2000	281,421,906	1,425,486	506.5	15,586	5.5			90,178	32.0	408,016	145.0	911,706	324.0	10,182,584	3,618.3	2,050,992	728.8	6,971,590	2,477.3	1,160,002	412.2
2001[5]	285,317,559	1,439,480	504.5	16,037	5.6			90,863	31.8	423,557	148.5	909,023	318.6	10,437,189	3,658.1	2,116,531	741.8	7,092,267	2,485.7	1,228,391	430.5
2002	287,973,924	1,423,677	494.4	16,229	5.6			95,235	33.1	420,806	146.1	891,407	309.5	10,455,277	3,630.6	2,151,252	747.0	7,057,379	2,450.7	1,246,646	432.9
2003	290,788,976	1,383,676	475.8	16,528	5.7			93,883	32.3	414,235	142.5	859,030	295.4	10,442,862	3,591.2	2,154,834	741.0	7,026,802	2,416.5	1,261,226	433.7
2004	293,656,842	1,360,088	463.2	16,148	5.5			95,089	32.4	401,470	136.7	847,381	288.6	10,319,386	3,514.1	2,144,446	730.3	6,937,089	2,362.3	1,237,851	421.5
2005	296,507,061	1,390,745	469.0	16,740	5.6			94,347	31.8	417,438	140.8	862,220	290.8	10,174,754	3,431.5	2,155,448	726.9	6,783,447	2,287.8	1,235,859	416.8
2006	299,398,484	1,435,123	479.3	17,309	5.8			94,472	31.6	449,246	150.0	874,096	292.0	10,019,601	3,346.6	2,194,993	733.1	6,626,363	2,213.2	1,198,245	400.2
2007	301,621,157	1,422,970	471.8	17,128	5.7			92,160	30.6	447,324	148.3	866,358	287.2	9,882,212	3,276.4	2,190,198	726.1	6,591,542	2,185.4	1,100,472	364.9
2008	304,059,724	1,394,461	458.6	16,465	5.4			90,750	29.8	443,563	145.9	843,683	277.5	9,774,152	3,214.6	2,228,887	733.0	6,586,206	2,166.1	959,059	315.4
2009	307,006,550	1,325,896	431.9	15,399	5.0			89,241	29.1	408,742	133.1	812,514	264.7	9,337,060	3,041.3	2,203,313	717.7	6,338,095	2,064.5	795,652	259.2
2010	309,330,219	1,251,248	404.5	14,722	4.8			85,593	27.7	369,089	119.3	781,844	252.8	9,112,625	2,945.9	2,168,459	701.0	6,204,601	2,005.8	739,565	239.1
2011	311,587,816	1,206,005	387.1	14,661	4.7			84,175	27.0	354,746	113.9	752,423	241.5	9,052,743	2,905.4	2,185,140	701.3	6,151,095	1,974.1	716,508	230.0
2012	313,873,685	1,217,057	387.8	14,856	4.7			85,141	27.1	355,051	113.1	762,009	242.8	9,001,992	2,868.0	2,109,932	672.2	6,168,874	1,965.4	723,186	230.4
2013	316,497,531	1,168,298	369.1	14,319	4.5	113,695	35.9	82,109	25.9	345,093	109.0	726,777	229.6	8,651,892	2,733.6	1,932,139	610.5	6,019,465	1,901.9	700,288	221.3
2014	318,907,401	1,153,022	361.6	14,164	4.4	118,027	37.0	84,864	26.6	322,905	101.3	731,089	229.2	8,209,010	2,574.1	1,713,153	537.2	5,809,054	1,821.5	686,803	215.4
2015	320,896,618	1,199,310	373.7	15,883	4.9	126,134	39.3	91,261	28.4	328,109	102.2	764,057	238.1	8,024,115	2,500.5	1,587,564	494.7	5,723,488	1,783.6	713,063	222.2
2016[6]	323,405,935	1,250,162	386.6	17,413	5.4	132,414	40.9	96,970	30.0	332,797	102.9	802,982	248.3	7,928,530	2,451.6	1,516,405	468.9	5,644,835	1,745.4	767,290	237.3
2017	325,719,178	1,247,321	382.9	17,284	5.3	135,755	41.7	99,856	30.7	319,356	98.0	810,825	248.9	7,694,086	2,362.2	1,401,840	430.4	5,519,107	1,694.4	773,139	237.4

[1] Populations are U.S. Census Bureau provisional estimates as of July 1 for each year except 2000 and 2010, which are decennial census counts.

[2] The violent crime figures include the offenses of murder, rape (legacy definition), robbery, and aggravated assault.

[3] The figures shown in this column for the offense of rape were estimated using the revised Uniform Crime Reporting Program's (UCR) definition of rape. See data declaration for further explanation.

[4] The figures shown in this column for the offense of rape were estimated using the legacy UCR definition of rape. See data declaration for further explanation.

[5] The murder and nonnegligent homicides that occurred as a result of the events of September 11, 2001, are not included in this table.

[6] The crime figures have been adjusted.

NOTE: Although arson data are included in the trend and clearance tables, sufficient data are not available to estimate totals for this offense. Therefore, no arson data are published in this table.

Table 1A

Crime in the United States

Percent Change in Volume and Rate per 100,000 Inhabitants for 2 years, 5 years, and 10 years

Years	**Violent crime**[1]	*Violent crime rate*	Murder and nonnegligent manslaughter	*Murder and nonnegligent manslaughter rate*	Rape (revised definition)[2]	*Rape (revised definition) rate*[2]	Rape (legacy definition)[3]	*Rape (legacy definition) rate*[3]	Robbery	*Robbery rate*	Aggravated assault	*Aggravated assault rate*	**Property crime**	*Property crime rate*	Burglary	*Burglary rate*	Larceny-theft	*Larceny-theft rate*	Motor vehicle theft	*Motor vehicle theft rate*
2017/2016	-0.2	-0.9	-0.7	-1.4	+2.5	+1.8	+3.0	+2.2	-4.0	-4.7	+1.0	+0.3	-3.0	-3.6	-7.6	-8.2	-2.2	-2.9	+0.8	*
2017/2013	+6.8	+3.7	+20.7	+17.3	+19.4	+16.0	+21.6	+18.2	-7.5	-10.1	+11.6	+8.4	-11.1	-13.6	-27.4	-29.5	-8.3	-10.9	+10.4	+7.3
2017/2008	-10.6	-16.5	+5.0	-2.0			+10.0	+2.7	-28.0	-32.8	-3.9	-10.3	-21.3	-26.5	-37.1	-41.3	-16.2	-21.8	-19.4	-24.7

[1] The violent crime figures include the offenses of murder, rape (legacy definition), robbery, and aggravated assault.

[2] The figures shown in this column for the offense of rape were estimated using the revised Uniform Crime Reporting Program's (UCR) definition of rape. See data declaration for further explanation.

[3] The figures shown in this column for the offense of rape were estimated using the legacy UCR definition of rape. See data declaration for further explanation.

[*] Less than one-tenth of 1 percent.

Overview

Table 1—Crime in the United States, by Volume and Rate per 100,000 Inhabitants, 1998–2017

Table 1A—Crime in the United States, Percent Change in Volume and Rate per 100,000 Inhabitants for 2 years, 5 years, and 10 years

- In 2017, the estimated number of violent crime offenses was 1,247,321, a decrease of 0.2 percent from the 2016 estimate.

- The violent crime of murder and nonnegligent manslaughter decreased 0.7 percent in 2017 when compared with the 2016 estimate. Rape offenses (legacy definition) increased 3.0 percent, and aggravated assault offenses increased 1.0 percent. The violent crime of robbery decreased by 4.0 percent when compared with the 2016 estimate.

- The 2017 violent crime rate was 382.9 per 100,000 inhabitants, down 0.9 percent when compared with the 2016 violent crime rate.

- The murder rate was 5.3 per 100,000 inhabitants in 2017, a 1.4 percent decrease when compared with the estimated rate for the previous year.

- The estimated number of property crimes in 2017 was 7,694,086, a 3.0 percent decrease from the 2016 estimate.

- Of the property crimes, the estimated number of burglary offenses decreased 7.6 percent, and larceny-theft offenses declined 2.2 percent. The estimated number of motor vehicle thefts increased 0.8 percent.

- The 2017 property crime rate was 2,362.2 per 100,000, a 3.6 percent decrease when compared with the 2016 rate.

EXHIBIT "5"

Table 20

Murder

by State, Types of Weapons, 2018

Data Declaration (https://ucr.fbi.gov/crime-in-the-u.s/2018/crime-in-the-u.s.-2018/tables/table-20/table-20.xls/@@template-layout-view?override-view=data-declaration)

Download Excel (https://ucr.fbi.gov/crime-in-the-u.s/2018/crime-in-the-u.s.-2018/tables/table-20/table-20.xls/output.xls)

State	Total murders[1]	Total firearms	Handguns	Rifles	Shotguns	Firearms (type unknown)	Knives or cutting instruments	Other weapons	Hands, fists, feet, etc.[2]
Alabama[3]	2	2	2	0	0	0	0	0	0
Alaska	47	31	7	3	0	21	8	3	5
Arizona	339	203	139	12	6	46	45	87	4
Arkansas	218	156	66	6	5	79	17	38	7
California	1,739	1,177	834	24	27	292	252	223	87
Colorado	207	147	99	2	8	38	27	13	20
Connecticut	83	54	10	2	0	42	18	9	2
Delaware	48	40	14	1	2	23	4	3	1
District of Columbia	151	120	120	0	0	0	20	7	4
Georgia	568	460	410	11	10	29	44	62	2
Hawaii	33	11	6	1	0	4	10	6	6
Idaho	32	19	14	2	2	1	4	8	1
Illinois[3]	864	708	592	14	4	98	77	53	26
Indiana	371	294	136	10	7	141	33	29	15

State	Total murders[1]	Total firearms	Handguns	Rifles	Shotguns	Firearms (type unknown)	Knives or cutting instruments	Other weapons	Hands, fists, feet, etc.[2]
Iowa[4]	43	20	6	2	2	10	8	9	6
Kansas	110	78	47	0	2	29	7	19	6
Kentucky	237	179	112	12	6	49	17	32	9
Louisiana	521	436	233	12	5	186	30	44	11
Maine	23	11	6	0	1	4	2	6	4
Maryland	470	388	345	1	10	32	39	30	13
Massachusetts	136	93	37	0	1	55	25	13	5
Michigan	550	394	166	17	11	200	31	99	26
Minnesota	104	49	36	4	0	9	16	28	11
Mississippi	142	118	99	3	2	14	7	15	2
Missouri	555	473	235	16	9	213	40	32	10
Montana	34	17	9	3	0	5	2	12	3
Nebraska	43	26	22	0	1	3	5	9	3
Nevada	201	134	46	1	1	86	23	24	20
New Hampshire	21	12	6	0	0	6	3	4	2
New Jersey	286	202	152	0	2	48	37	28	19
New Mexico	137	87	39	3	0	45	23	22	5
New York	546	313	254	6	10	43	124	63	46
North Carolina	479	346	231	15	16	84	44	52	37
North Dakota	16	9	8	0	0	1	1	1	5
Ohio	546	383	184	3	7	189	49	87	27
Oklahoma	202	134	95	7	3	29	28	29	11
Oregon	81	48	30	3	1	14	12	18	3
Pennsylvania	787	580	464	17	7	92	83	99	25
Rhode Island	16	12	1	1	1	9	2	1	1
South Carolina	386	296	188	8	6	94	29	42	19
South Dakota	13	8	5	0	0	3	4	1	0
Tennessee	496	397	245	26	8	118	28	49	22
Texas	1,301	956	522	33	37	364	128	133	84
Utah	59	28	17	1	0	10	12	10	9
Vermont	10	3	3	0	0	0	0	5	2
Virginia	391	297	141	8	5	143	30	49	15
Washington	232	138	76	2	5	55	45	35	14

State	Total murders[1]	Total firearms	Handguns	Rifles	Shotguns	Firearms (type unknown)	Knives or cutting instruments	Other weapons	Hands, fists, feet, etc.[2]
West Virginia	57	34	21	1	1	11	5	14	4
Wisconsin	178	136	67	4	2	63	15	16	11
Wyoming	12	8	6	0	2	0	2	0	2

- [1] Total number of murders for which supplemental homicide data were received.
- [2] Pushed is included in hands, fists, feet, etc.
- [3] Limited supplemental homicide data were received.
- [4] Limited data for 2018 were available for Iowa.

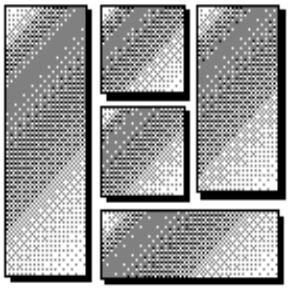
Data Declaration (https://ucr.fbi.gov/crime-in-the-u.s/2018/crime-in-the-u.s.-2018/tables/table-20/table-20.xls/@@template-layout-view?override-view=data-declaration)

Provides the methodology used in constructing this table and other pertinent information about this table.

EXHIBIT "6"

IMPACT EVALUATION OF THE PUBLIC SAFETY AND RECREATIONAL FIREARMS USE PROTECTION ACT OF 1994

Final Report



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March 13, 1997

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Any remaining errors or omissions are the responsibility of the authors. **Opinions expressed herein are those of the authors and not necessarily those of The Urban Institute, its trustees, or its sponsors.**

Table of Contents

List of Tables

List of Figures

1. OVERVIEW

Title XI of the Violent Crime Control and Law Enforcement Act of 1994 (the Crime Control Act) took effect on September 13, 1994. Subtitle A banned the manufacture, transfer, and possession of designated semiautomatic assault weapons. It also banned "large-capacity" magazines, which were defined as ammunition feeding devices designed to hold more than 10 rounds. Finally, it required a study of the effects of these bans, with particular emphasis on violent and drug trafficking crime, to be conducted within 30 months following the effective date of the bans. To satisfy the study requirement, the National Institute of Justice (NIJ) awarded a grant to The Urban Institute for an impact evaluation of Subtitle A. This report contains the study findings.

In defining assault weapons, Subtitle A banned 8 named categories of rifles and handguns. It also banned *exact copies* of the named guns, revolving cylinder shotguns, and guns with detachable magazines that were manufactured with certain features such as flash suppressors and folding rifle stocks. The ban specifically exempted *grandfathered* assault weapons and magazines that had been manufactured before the ban took effect. Implicitly, the ban exempts all other guns; several of these, which we treated as *legal substitutes,* closely resemble the banned guns but are not classified as exact copies.

Among other characteristics, ban proponents cited the capacity of these weapons, most of which had been originally designed for military use, to fire many bullets rapidly. While this capacity had been demonstrated in several highly publicized mass murders in the decade before 1994, ban supporters argued that it was largely irrelevant for hunting, competitive shooting, and self-defense. Therefore, it was argued, the ban could prevent violent crimes with only a small burden on law-abiding gun owners. Some of our own analyses added evidence that assault weapons are disproportionately involved in murders with multiple victims, multiple wounds per victim, and police officers as victims.

To reduce levels of these crimes, the law must increase the scarcity of the banned weapons. Scarcity would be reflected in higher prices not only in the *primary markets* where licensed dealers create records of sales to legally eligible purchasers, but also in *secondary markets* that lack such records. Although most secondary-market transfers are legal, minors, convicted felons, and other ineligible purchasers may purchase guns in them (usually at highly inflated prices) without creating records. In theory, higher prices in secondary markets would discourage criminal use of assault weapons, thereby reducing levels of the violent crimes in which assault weapons are disproportionately used.

For these reasons, our analysis considered potential ban effects on gun markets, on assault weapon use in crime, and on lethal consequences of assault weapon use. However, the statutory schedule for this study constrained our findings to short-run effects, which are not necessarily a reliable guide to long-term effects. The timing also limited the power of our statistical analyses to detect worthwhile ban effects that may have occurred. Most fundamentally, because the banned guns and magazines were never used in more than a fraction of all gun murders, even the maximum theoretically achievable preventive effect of the ban on gun murders is almost certainly too small to detect statistically with only one year of post-ban crime data.

With these cautions in mind, our analysis suggests that the primary-market prices of the banned guns and magazines rose by upwards of 50 percent during 1993 and 1994, while the ban was being debated, as gun distributors, dealers, and collectors speculated that the banned weapons would become expensive collectors' items. However, production of the banned guns also surged, so that more than an extra year's normal supply of assault weapons and legal substitutes was manufactured during 1994. After the ban took effect, primary-market prices of the banned guns and most large-capacity magazines fell to nearly pre-ban levels and remained there at

least through mid-1996, reflecting both the oversupply of grandfathered guns and the variety of legal substitutes that emerged around the time of the ban.

Even though the expected quick profits failed to materialize, we found no strong evidence to date that licensed dealers have increased "off the books" sales of assault weapons in secondary markets and concealed them with false stolen gun reports. Stolen gun reports for assault weapons did increase slightly after the ban took effect, but by less than reported thefts of unbanned large-capacity semiautomatic handguns, which began rising well before the ban.

The lack of an increase in stolen gun reports suggests that so far, the large stock of grandfathered assault weapons has remained largely in dealers' and collectors' inventories instead of leaking into the secondary markets through which criminals tend to obtain guns. In turn, this speculative stockpiling of assault weapons by law-abiding dealers and owners apparently reduced the flow of assault weapons to criminals, at least temporarily. Between 1994 and 1995, the criminal use of assault weapons, as measured by law enforcement agency requests for BATF traces of guns associated with crimes, fell by 20 percent, compared to an 11 percent decrease for all guns. BATF trace requests are an imperfect measure because they reflect only a small percentage of guns used in crime. However, we found similar trends in data on all guns recovered in crime in two cities. We also found similar decreases in trace requests concerning guns associated with violent and drug crimes.

At best, the assault weapons ban can have only a limited effect on total gun murders, because the banned weapons and magazines were never involved in more than a modest fraction of all gun murders. Our best estimate is that the ban contributed to a 6.7 percent decrease in total gun murders between 1994 and 1995, beyond what would have been expected in view of ongoing crime, demographic, and economic trends. However, with only one year of post-ban data, we cannot rule out the possibility that this decrease reflects chance year-to-year variation rather than a true effect of the ban. Nor can we rule out effects of other features of the 1994 Crime Act or a host of state and local initiatives that took place simultaneously. Further, any short-run preventive effect observable at this time may ebb in the near future as the stock of grandfathered assault weapons and legal substitute guns leaks to secondary markets, then increase as the stock of large-capacity magazines gradually dwindles.

We were unable to detect any reduction to date in two types of gun murders that are thought to be closely associated with assault weapons, those with multiple victims in a single incident and those producing multiple bullet wounds per victim. We did find a reduction in killings of police officers since mid-1995. However, the available data are partial and preliminary, and the trends may have been influenced by law enforcement agency policies regarding bullet-proof vests.

The following pages explain these findings in more detail, and recommend future research to update and refine our results at this early post-ban stage.

1.1. PRIMARY-MARKET EFFECTS

1.1.1. Prices and Production

1.1.1.1. Findings

We found clear peaks in legal-market prices of the banned weapons and magazines around the effective date of the ban, based on display ads in the nationally distributed periodical Shotgun News between 1992 and mid-1996. For example, a price index of banned SWD semiautomatic pistols rose by about 47 percent during the year preceding the ban, then fell by about 20 percent the following year, to a level where it remains. Meanwhile, the

prices of non-banned Davis and Lorcin semiautomatic pistols remained virtually constant over the entire period. Similarly, a price index for banned AR-15 rifles, exact copies, and legal substitutes at least doubled in the year preceding the ban, then fell after the ban nearly to 1992 levels, where they have remained. Prices of unbanned semiautomatic rifles (e.g., the Ruger Mini-14, Maadi, and SKS) behaved similarly to AR-15 prices, presumably due to pre-ban speculation that these guns would be included in the final version of the Crime Act.

Like assault weapon prices, large-capacity magazine prices generally doubled within the year preceding the ban. However, trends diverged after the ban depending on what gun the magazine was made for. For example, magazines for non-banned Glock handguns held their new high levels, while magazines for banned Uzi and unbanned Mini-14 weapons fell substantially from their peaks. AR-15 large-capacity magazine prices also fell to 1993 levels shortly after the ban took effect, but returned to their 1994 peak in mid-1996. We believe that demand for grandfathered Glock and AR-15 magazines was sustained or revived by continuing sales of legal guns that accept them.

Production of the banned assault weapons surged in the months leading up to the ban. Data limitations preclude precise and comprehensive counts. However, we estimate that the annual production of five categories of assault weapons (AR-15s and models by Intratec, SWD, AA Arms, and Calico) and legal substitutes rose by more than 120 percent, from an estimated 1989–93 annual average of 91,000 guns to about 204,000 in 1994 — more than an extra year's supply. In contrast, production of non-banned Lorcin and Davis pistols, which are among the guns most frequently seized by police, fell by about 35 percent, from a 1989–93 annual average of 283,000 to 184,000 in 1994.

Our interpretation of these trends is that the pre-ban price and production increases reflected speculation that grandfathered weapons and magazines in the banned categories would become profitable collectors' items after the ban took effect. Instead, however, assault weapon prices fell sharply within months after the ban took effect, apparently under the combined weight of the extra year's supply of grandfathered guns, along with legal substitute guns that entered the distribution chain around the time of the ban. While large-capacity magazine prices for several banned assault weapons followed similar trends, those for unbanned Glock pistols sustained their peaks, and those for the widely-copied AR-15 rifle rebounded at least temporarily to peak levels in 1996, after an immediate post-ban fall.

1.1.1.2. Recommendations

To establish our findings about legal-market effects more definitively, we have short-term (i.e., 12-month) and long-term research recommendations for consideration by NIJ. In the short term, we recommend entering and analyzing large-capacity magazine price data that we have already coded but not entered, in order to study how the prices and legal status of guns affect the prices of large-capacity magazines as economic complements. We also recommend updating our price and production analyses for both the banned firearms and large-capacity magazines, to learn about retention of the apparent ban effects we identified. For the long term, we recommend that NIJ and BATF cooperate in establishing and maintaining time-series data on prices and production of assault weapons, legal substitutes, other guns commonly used in crime, and the respective large and small capacity magazines; like similar statistical series currently maintained for illegal drugs, we believe such a price and production series would be a valuable instrument for monitoring effects of policy changes and other influences on markets for weapons that are commonly used in violent and drug trafficking crime.

1.2. SECONDARY-MARKET EFFECTS

1.2.1. Findings

In addition to the retail markets discussed above, there are secondary gun markets in which gun transfers are made without formal record keeping requirements. Secondary market transfers are by and large legal transactions. However, prohibited gun purchasers such as minors, felons, and fugitives tend to acquire most of their guns through secondary markets and pay premiums of 3 to 5 times the legal-market prices in order to avoid eligibility checks, sales records, and the 5-day waiting period required by the Brady Act. We were unable to observe secondary-market prices and quantities directly. Anecdotally, however, the channels through which guns "leak" from legal to secondary markets include gun thieves, unscrupulous licensed dealers who sell guns on the streets and in gun shows more or less exclusively to prohibited purchasers (who may resell the guns), as well as "storefront" dealers who sell occasionally in secondary markets, reporting the missing inventories to BATF inspectors as "stolen or lost." Since two of these channels may lead to theft reports to the FBI's National Crime Information Center (NCIC), we tested for an increase in reported assault weapon thefts after the ban.

To this point, there has been only a slight increase in assault weapon thefts as a share of all stolen semiautomatic weapons. Thus, there does not appear to have been much leakage of assault weapons from legal to secondary markets.

In order to assess the effects of the large-capacity magazine ban on secondary markets, we examined thefts of Glock and Ruger handgun models that accept these magazines. Thefts of these guns continued to increase after the ban, despite the magazine ban, which presumably made the guns less attractive. Yet we also did not find strong evidence of an increase in thefts of these guns relative to what would have been predicted based on pre-ban trends. This implies that dealers have not been leaking the guns to illegitimate users on a large scale.

1.2.2. Recommendations

To monitor possible future leakage of the large existing stock of assault weapons into secondary markets, we recommend updating our analyses of trends in stolen gun reports. We also recommend that BATF and NCIC encourage reporting agencies to ascertain and record the magazines with which guns were stolen. Also, because stolen gun reports are deleted from NCIC files when the guns are recovered, we recommend that analyses be conducted on periodic downloads of the database in order to analyze time from theft to recovery. For strategic purposes, it would also be useful to compare dealer patterns of assault weapon theft reports with patterns of occurrence in BATF traces of guns recovered in crime.

1.3. EFFECTS ON ASSAULT WEAPON USE IN CRIME

1.3.1. Findings

Requests for BATF traces of assault weapons recovered in crime by law enforcement agencies throughout the country declined 20 percent in 1995, the first calendar year after the ban took effect. Some of this decrease may reflect an overall decrease in gun crimes; total trace requests dropped 11 percent in 1995 and gun murders dropped 12 percent. Nevertheless, these trends suggest an 8–9 percent additional decrease due to substitution of other guns for the banned assault weapons in 1995 gun crimes. We were unable to find similar assault pistol reductions in states with pre-existing assault pistol bans. Nationwide decreases related to violent and drug crimes were at least as great as that in total trace requests in percentage terms, although these categories were quite small

in number. The decrease we observed was evidently not a spurious result of a spurt of assault-weapon tracing around the effective date of the ban, because there were fewer assault weapon traces in 1995 than in 1993.

Trace requests for assault weapons rose by 7 percent in the first half of 1996, suggesting that the 1995 effect we observed may be temporary. However, data limitations have prevented us from attributing this rebound to changes in overall crime patterns, leakage of grandfathered assault weapons to secondary markets, changes in trace request practices, or other causes. Data from two cities not subject to a pre-existing state bans suggested that assault weapon use, while rare in those cities both before and after the ban, also tapered off during late 1995 and into 1996.

With our local data sources, we also examined confiscations of selected unbanned handguns capable of accepting large-capacity magazines. Criminal use of these guns relative to other guns remained stable or was higher during the post-ban period, though data from one of these cities were indicative of a recent plateau. However, we were unable to acquire data on the magazines with which these guns were equipped. Further, trends in confiscations of our selected models may not be indicative of trends for other unbanned large-capacity handguns. It is therefore difficult to make any definitive statements about the use of large-capacity magazines in crime since the ban. Nevertheless, the contrasting trends for these guns and assault weapons provide some tentative hints of short-term substitution of non-banned large-capacity semiautomatic handguns for the banned assault weapons.

1.3.2. Recommendations

Although BATF trace request data provide the only national trends related to assault weapon use, our findings based on them are subject to limitations. Law enforcement agencies request traces on only a fraction of confiscated guns that probably does not represent the entire population. Therefore, we recommend further study of available data on all guns recovered in crime in selected cities that either were or were not under state assault weapon bans when the Federal ban took effect. Beyond that, we recommend analyzing BATF trace data already in-house to compare trends for specific banned assault weapon models with trends for non-banned models that are close substitutes. Most strongly, we also recommend updating our trend analysis, to see if the early 1996 rebound in BATF trace requests for assault weapons continued throughout the year and to relate any change to 1996 trends in gun crime and overall trace requests.

From a broader and longer-term perspective, we share others' concerns about the adequacy of BATF trace data, the only available national data, as a basis for assessing the effects of firearms policies and other influences on the use of assault weapons and other guns in violent and drug trafficking crime. Therefore, we commend recent BATF efforts to encourage local law enforcement agencies to request traces on more of the guns they seize from criminals. As a complement, however, we recommend short-term research on departmental policies and officers' decisions that affect the probability that a specific gun recovered in crime will be submitted for tracing.

Unfortunately, we have been unable to this point to assemble much information regarding trends in the criminal use of large-capacity magazines or guns capable of accepting these magazines. This gap is especially salient for the following reasons: the large-capacity magazine is perhaps the most functionally important distinguishing feature of assault weapons; the magazine ban affected more gun models than did the more visible bans on designated assault weapons; and based on 1993 BATF trace requests, non-banned semiautomatic weapons accepting large-capacity magazines were used in more crimes than were the banned assault weapons. For these reasons, we recommend that BATF and state/local law enforcement agencies encourage concerted efforts to record the magazines with which confiscated firearms are equipped — information that frequently goes unrecorded under present practice — and we recommend further research on trends, at both the national and local levels, on the

criminal use of guns equipped with large-capacity magazines. Finally, to support this research and a variety of strategic objectives for reducing the consequences of violent and drug trafficking crime, consideration should be given to studying the costs and benefits of legislative and administrative measures that would encourage recording, tracing, and analyzing magazines recovered in crimes, with or without guns.

1.4. CONSEQUENCES OF ASSAULT WEAPON USE

1.4.1. Findings

A central argument for special regulation of assault weapons and large-capacity magazines is that the rapid-fire/multi-shot capabilities they make available to gun offenders increase the expected number of deaths per criminal use, because an intended victim may receive more wounds, and more people can be wounded, in a short period of time. Therefore, we examined trends in three consequences of gun use: gun murders, victims per gun homicide incident, and wounds per gunshot victim.

Our ability to discern ban effects on these consequences is constrained by a number of facts. The potential size of ban effects is limited because the banned weapons and magazines were used in only a minority of gun crimes — based on limited evidence, we estimate that 25% of gun homicides are committed with guns equipped with large-capacity magazines, of which assault weapons are a subset. Further, the power to discern small effects statistically is limited because post-ban data are available for only one full calendar year. Also, a large stock still exists of grandfathered magazines as well as grandfathered and legal-substitute guns with assault weapon characteristics.

Our best estimate of the impact of the ban on state level gun homicide rates is that it caused a reduction of 6.7% in gun murders in 1995 relative to a projection of recent trends. However, the evidence is not strong enough for us to conclude that there was any meaningful effect (i.e., that the effect was different from zero). Note also that a true decrease of 6.7% in the gun murder rate attributable to the ban would imply a reduction of 27% in the use of assault weapons and large-capacity guns and no effective substitution of other guns. While we do not yet have an estimate of large-capacity magazine use in 1995, our nationwide assessment of assault weapon utilization suggested only an 8 to 20 percent drop in assault weapon use in 1995.

Using a variety of national and local data sources, we found no statistical evidence of post-ban decreases in either the number of victims per gun homicide incident, the number of gunshot wounds per victim, or the proportion of gunshot victims with multiple wounds. Nor did we find assault weapons to be overrepresented in a sample of mass murders involving guns (see Appendix A).

The absence of stronger ban effects may be attributable to the relative rarity with which the banned weapons are used in violent crimes. At the same time, our chosen measures reflect only a few of the possible manifestations of the rapid-fire/multi-shot characteristics thought to make assault weapons and large-capacity magazines particularly dangerous. For example, we might have found the use of assault weapons and large-capacity magazines to be more consequential in an analysis of the number of victims receiving any wound (fatal or non-fatal), in broader samples of firearm discharge incidents. Moreover, our comparisons did not control for characteristics of incidents and offenders that may affect the choice of weapon, the consequences of weapon use, or both.

Recommendations: First, we recommend further study of the impact measures examined in this investigation. Relatively little time has passed since the implementation of the ban. This weakens the ability of statistical tests — particularly those in our time-series analyses — to discern meaningful impacts. Moreover, the

ban's effects on the gun market are still unfolding. Hence, the long term consequences of the ban may differ substantially from the short term consequences which have been the subject of this investigation.

Therefore, we recommend updating the state-level analysis of gun murder rates as more data become available. Similarly, investigations of trends in wounds per gunshot victim could be expanded to include longer post ban periods, larger numbers of jurisdictions, and, wherever possible, data on both fatal and non-fatal victims. Examination of numbers of total wounded victims in both fatal and non-fatal gunshot incidents may also be useful. In some jurisdictions, it may also be possible to link trends in the types of guns seized by police to trends in specific weapon-related consequence measures.

Second, we recommend further research on the role of assault weapons and large-capacity magazines in murders of police officers. Our analysis of police murders has shown that the fraction of police murders involving assault weapons is higher than that for civilian murders. This suggests that gun murders of police should be more sensitive to the ban than gun murders in general. Yet, further research, considering such factors as numbers of shots fired, wounds inflicted, and offender characteristics, is necessary for a greater understanding of the role of the banned weaponry in these murders.

Along similar lines, we strongly recommend in-depth, incident-based research on the situational dynamics of both fatal and non-fatal gun assaults to gain greater understanding of the roles of banned and other weapons in intentional deaths and injuries. A goal of this research should be to determine the extent to which assault weapons and guns equipped with large-capacity magazines are used in homicides and assaults and to compare the fatality rates of attacks with these weapons to those with other firearms. A second goal should be to determine the extent to which the properties of the banned weapons influence the outcomes of criminal gun attacks after controlling for important characteristics of the situations and the actors. In other words, how many homicides and non-fatal gunshot wound cases involving assault weapons or large-capacity magazines would not occur if the offenders were forced to substitute other firearms and/or small capacity magazines? In what percentage of gun attacks, for instance, does the ability to fire more than 10 rounds without reloading influence the number of gunshot wound victims or determine the difference between a fatal and non-fatal attack? In this study, we found some weak evidence that victims killed with guns having large-capacity magazines tend to have more bullet wounds than victims killed with other firearms, and that mass murders with assault weapons tend to involve more victims than those with other firearms. However, our results were based on simple comparisons; much more comprehensive research should be pursued in this area.

Future research on the dynamics of criminal shootings, including various measures of the number of shots fired and wounds inflicted, would provide information on possible effects of the assault weapon and magazine ban that we were unable to estimate, as well as useful information on violent gun crime generally. Such research requires linking medical and law enforcement data sets on victim wounds, forensic examinations of recovered firearms and magazines, and police incident reports.

2. BACKGROUND FOR THE IMPACT ASSESSMENT

Title XI of the Violent Crime Control and Law Enforcement Act of 1994 (the Crime Control Act), took effect on its enactment date, September 13, 1994. Subtitle A, which is itself known as the Public Safety and Recreational Firearms Use Protection Act, contains three provisions related to "semiautomatic assault weapons." Section 110102 (the assault weapons ban) made unlawful the manufacture, transfer, or possession of such weapons under 18:922 of the United States Code. Section 110103 (the magazine ban) made unlawful the transfer or possession of "large-capacity ammunition feeding devices": detachable magazines that accept more than 10 rounds[1] and can be attached to semi- or automatic firearms. Section 110104 (the evaluation requirement) required the Attorney General to study the effect of these prohibitions and "in particular...their impact, if any, on violent and drug trafficking crime." The evaluation requirement specified a time period for the study: an 18-month period beginning 12 months after the enactment date of the Act. It also required the Attorney General to report the study results to Congress 30 months after enactment of the Crime Control Act — March 13, 1997. The National Institute of Justice awarded a grant to the Urban Institute to conduct the mandated study, and this report contains the findings.

This chapter first explains the legislation in additional detail, then discusses what is already known about the role of the banned weapons in crime, and finally explains certain relevant features of firearms markets.

2.1. THE LEGISLATION

Effective on its enactment date, September 13, 1994, Section 110102 of Title XI banned the manufacture, transfer, and possession of "semiautomatic assault weapons." It defined the banned items defined in four ways:

1) Named guns: specific rifles and handguns, available from ten importers and manufacturers: Norinco, Mitchell, and Poly Technologies (all models, popularly known as AKs); Israeli Military Industries UZI and Galil models, imported by Action Arms; Beretta Ar 70 (also known as SC-70); Colt AR-15; Fabrique National FN/FAL, FN/LAR, FN/FNC), SWD M-10, M-11, M-11/9, and M-12; Steyr AUG; and INTRATEC TEC-9, TEC-DC9, and TEC-22;

2) Exact copies: "Copies or duplicates of the [named guns] in any caliber";

3) Revolving cylinder shotguns: Large-capacity shotguns, with the Street Sweeper and Striker 12 named as examples; and

4) Features-test guns: semiautomatic weapons capable of accepting detachable magazines and having at least two named features.[2]

Several provisions of the ban require further explanation because they affected our approach to this study. First, the ban exempted several categories of guns: a long list of specific models specified in Appendix A to Sec.

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[1] Or "that can be readily restored or converted to accept."

[2] For rifles, the named features were: a folding or telescoping stock; a pistol grip that protrudes below the firing action; a bayonet mount; a flash suppresser or threaded barrel designed to accommodate one; a grenade launcher. For pistols, the features were a magazine outside the pistol grip; a threaded barrel (capable of accepting a barrel extender, flash suppresser, forward handgrip, or silencer); a heat shroud that encircles the barrel; a weight of more than 50 ounces unloaded; and a semiautomatic version of an automatic firearm. For shotguns, named features included the folding or telescoping stock, protruding pistol grip, fixed magazine capacity over 5 rounds, and ability to accept a detachable magazine.

110102; bolt- or pump-action, inoperable, and antique guns; semiautomatic rifles and shotguns that cannot hold more than 5 rounds; and firearms belonging to a unit of government, a nuclear materials security organization, a retired law enforcement officer, or an authorized weapons tester.

Second, the prohibitions exempted weapons and magazines that met the definitional criteria but were legally owned (by manufacturers, distributors, retailers, or consumers) on the effective date of the Act. Such "grandfathered" guns may legally be sold, resold, and transferred indefinitely. Estimates of their numbers are imprecise. However, a 1992 report by the American Medical Association reported an estimate of 1 million semiautomatic assault weapons manufactured for civilian use, plus 1.5 million semiautomatic M-1 rifles sold as military surplus (AMA Council, 1992). To distinguish grandfathered guns from exempt guns that might be stolen or diverted to illegal markets, the ban required the serial numbers of guns in the banned categories to clearly indicate their dates of manufacture.

Third, the ban on exact copies of the named guns did not prohibit the manufacture, sale, or transfer of legal substitutes, most of which first appeared around or after the effective date of the ban. Legal substitutes differ from banned exact copies by lacking certain named features or by incorporating minimal design modifications such as slight reductions of pistol barrel length, thumbholes drilled in a rifle stock, or the like. Manufacturers named some legal substitutes by adding a designation such as "Sporter," "AB," (After Ban), or "PCR" (Politically Correct Rifle) to the name of the corresponding banned weapon.

Section 110103 of Title XI banned large-capacity magazines, i.e., magazines that accept ten or more rounds of ammunition. Its effective date, exemptions, and grandfathering provisions correspond to those governing firearms under Section 110102. This provision exempts attached tubular devices capable of operating only with .22 caliber rimfire ammunition.

Section 110104 required the study that is the subject of this report: a study of the effect of the ban, citing impacts on violent crime and drug trafficking in particular. It also specified the time period of the study: to begin 12 months after enactment, to be conducted over an 18-month period, and to be reported to Congress after 30 months. Finally, Title XI included a "sunset provision" for the ban, repealing it 10 years after its effective date.

Subtitles B and C of Title XI are relevant to this study because they took effect at the same time, and so special efforts are needed to distinguish their effects from those effects of the assault weapon and magazine bans in Subtitle A. With certain exemptions, Subtitle B bans the sale, delivery, or transfer of handguns to juveniles less than 18 years old. This juvenile handgun possession ban applies, of course, to assault pistols and to other semiautomatic handguns that are frequently recovered in crimes. Subtitle C requires applicants for new and renewal Federal Firearms Licenses — the Federal dealers' licenses — to submit a photograph and fingerprints with their applications and to certify that their businesses will comply with all state and local laws pertinent to their business operations. These subtitles gave force of law to practices that BATF had begun early in 1994, to require the fingerprints and photographs, and to cooperate with local law enforcement agencies in investigations of Federal Firearms Licensees' (FFLs) compliance with local sales tax, zoning, and other administrative requirements. These BATF practices are believed to have contributed to an 11 percent reduction in licensees (from 281,447 to 250,833) between January and the effective date of the Crime Act, and a subsequent 50 percent reduction to about 124,286 by December 1996 (U.S. Department of Treasury, 1997). These practices and subtitles were intended to discourage license applications and renewals by the subset of licensees least likely to comply with laws governing sales to felons, juveniles, and other prohibited purchasers.

2.2. CONTEXT FOR THE ASSAULT WEAPONS BAN

At least three considerations appear to have motivated the Subtitle A bans on assault weapons and large-capacity magazines: arguments over particularly dangerous consequences of their use, highly publicized incidents that drew public attention to the widespread availability of military-style weapons, and the disproportionate use of the banned weapons in crime.

The argument over dangerous consequences is that the ban targets a large array of semiautomatic weapons capable of accepting large-capacity magazines (i.e., magazines holding more than 10 rounds). Semiautomatic firearms permit a somewhat more rapid rate of fire than do non-semiautomatics. When combined with large-capacity magazines, semiautomatic firearms enable gun offenders to fire more times and at a faster rate, thereby increasing the probability that offenders hit one or more victims at least once.

There is very little empirical evidence, however, on the direct role of ammunition capacity in determining the outcomes of criminal gun attacks (see Koper 1995). The limited data which do exist suggest that criminal gun attacks involve three or fewer shots on average (Kleck 1991, pp.78-79; McGonigal et al. 1993, p.534). Further, there is no evidence comparing the fatality rate of attacks perpetrated with guns having large-capacity magazines to those involving guns without large-capacity magazines (indeed, there is no evidence comparing the fatality rate of attacks with semiautomatics to those with other firearms). But in the absence of substantial data on the dynamics of criminal shootings (including the number of shots fired and wounds inflicted per incident), it seems plausible that offenders using semiautomatics, especially assault weapons and other guns capable of accepting large-capacity magazines, have the ability to wound more persons, whether they be intended targets or innocent bystanders (see Sherman et al. 1989). This possibility encouraged us to attempt to estimate the effect of the ban on both the number of murder victims per incident and the number of wounds per murder victim.

The potential of assault weapons to kill multiple victims quickly was realized in several dramatic public murder incidents that occurred in the decade preceding the ban and involved assault weapons or other semiautomatic firearms with large-capacity magazines (e.g., see Cox Newspapers 1989; Lenett 1995). In one of the worst mass murders ever committed in the United States, for example, James Huberty killed 21 persons and wounded 19 others in a San Ysidro, California, McDonald's on July 18, 1984, using an Uzi handgun and a shotgun. On September 14, 1989, Joseph T. Wesbecker killed seven persons and wounded thirteen others at his former workplace in Louisville, Kentucky before taking his own life. Wesbecker was armed with an AK-47 rifle, two MAC-11 handguns, and a number of other firearms. One of the most infamous assault weapon cases occurred on January 17, 1989, when Patrick Edward Purdy used an AK-47 to open fire on a schoolyard in Stockton, California, killing 5 children.

There were additional high profile incidents in which offenders using semiautomatic handguns with large-capacity magazines killed large numbers of persons. In October of 1991, a gunman armed with a Glock 17, a Ruger P89 (both the Glock and Ruger models are semiautomatic handguns capable of accepting magazines with more than 10 rounds), and several large-capacity magazines killed 23 people and wounded another 19 in Killeen, Texas. In a December 1993 incident, six people were killed and another 20 were wounded on a Long Island commuter train by a gunman equipped with a semiautomatic pistol and large-capacity magazines.

These events have been cited as jarring the public consciousness, highlighting the public accessibility of weapons generally associated with military use, and demonstrating the apparent danger to public health posed by semiautomatic weapons with large-capacity magazines. These considerations, along with the claim that large-capacity magazines were unnecessary for hunting or sporting purposes, reportedly galvanized public support for the initiative to ban these magazines (Lenett, 1995).

Debate over assault weapons raged for several years prior to the passage of the 1994 Crime Act. Throughout that time, different studies, news reports, policy debates, and legal regulations employed varying definitions of assault weapons. Yet, in general terms, the firearms targeted in these debates and those ultimately prohibited by the federal government's ban consist of various semiautomatic pistols, rifles, and shotguns, most of which accept detachable ammunition magazines and have military-style features. Mechanically, the most important features of these guns are their semiautomatic firing mechanisms and the ability to accept detachable magazines, particularly large-capacity magazines. However, these traits do not distinguish them from many other semiautomatic weapons used for hunting and target shooting. Therefore, some have argued that assault weapons differ only cosmetically from other semiautomatic firearms (Kleck 1991; Cox Newspapers 1989).

Nonetheless, proponents of assault weapons legislation argued that these weapons are too inaccurate to have much hunting or sporting value. Furthermore, they argued that various features of these weapons, such as folding stocks and shrouds surrounding their barrels, have no hunting or sporting value and serve to make these weapons more concealable and practical for criminal use (Cox Newspapers 1989). To the extent that these features facilitated criminal use of long guns or handguns with large-capacity magazines, one could hypothesize that there would be an increase in the deadliness of gun violence. Proponents also claimed that some of these weapons, such as Uzi carbines and pistols, could be converted rather easily to fully automatic firing.[3]

To buttress these arguments, proponents of assault weapons legislation pointed out that assault weapons are used disproportionately in crime. According to estimates generated prior to the federal ban, assault weapons represented less than one percent of the over 200 million privately-owned guns in the United States; yet they were reported to account for 8% of all firearms trace requests submitted to BATF from 1986 to 1993 (Lenett 1995; also see Zawitz 1995). Moreover, these guns were perceived to be especially attractive to offenders involved in drug dealing and organized crime, as evidenced by the relatively high representation of these weapons among BATF gun trace requests for these crimes. To illustrate, a late 1980s study of BATF trace requests reported that nearly 30% of the guns tied to organized crime cases were assault weapons, and 12.4% of gun traces tied to narcotics crimes involved these guns (Cox Newspapers 1989, p.4).

Further, most assault weapons combine semiautomatic firing capability with the ability to accept large-capacity magazines and higher stopping power (i.e., the ability to inflict more serious wounds).[4] Thus, assault weapons would appear to be a particularly lethal group of firearms. However, this is also true of many non-banned semiautomatic firearms. Moreover, there have been no studies comparing the fatality rate of attacks with assault weapons to those committed with other firearms.

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[3] Fully automatic firearms, which shoot continuously as long as the trigger is held down, have been illegal to own in the U.S. without a federal permit since 1934. BATF has the responsibility of determining whether particular firearm models are too easily convertible to fully automatic firing. Earlier versions of the SWD M series assault pistols made by RPB Industries were met with BATF disapproval for this reason during the early 1980s.

[4] Determinants of firearm stopping power include the velocity, size, shape, and jacketing of projectiles fired from a gun. Notwithstanding various complexities, the works of various forensic, medical, and criminological researchers suggest we can roughly categorize different types of guns as inflicting more or less lethal wounds (see review in Koper 1995). At perhaps the most general level, we can classify shotguns, centerfire (high-veolocity) rifles, magnum handguns, and other large caliber handguns (generally, those larger than .32 caliber) as more lethal firearms and small caliber handguns and .22 caliber rimfire (low velocity) rifles as less lethal firearms. Most assault weapons are either high velocity rifles, large caliber handguns, or shotguns.

Nonetheless, the involvement of assault weapons in a number of mass murder incidents such as those discussed above provided an important impetus to the movement to ban assault weapons. Commenting on Patrick Purdy's murder of five children with an AK-47 rifle in Stockton, California in 1989, one observer noted, "The crime was to raise renewed outcries against the availability of exotic military-style weapons in our society. This time police forces joined forces with those who have traditionally opposed the widespread ownership of guns" (Cox Newspapers 1989, p.i). Later that year, California became the first state in the nation to enact an assault weapons ban, and the federal government enacted a ban on the importation of several foreign military-style rifles.

2.3. ASSAULT WEAPONS AND CRIME

Table 2-1 describes the named guns banned by Subtitle A in terms of their design, price, pre-ban legal status, and examples of legal substitutes for the banned guns. The table also reports counts of BATF trace requests — law enforcement agency requests for BATF to trace the recorded purchase history of a gun. Trace counts are commonly used to compare the relative frequencies of gun model uses in crime, although they are subject to biases discussed in the next chapter. Together, the named guns and legal substitutes accounted for 3,493 trace requests in 1993, the last full pre-ban year. This represented about 6.3 percent of all 55,089 traces requested that year.

Of the nine types of banned weapons shown in Table 2-1, five are foreign-made: AKs, UZI/ Galil, Beretta Ar-70, FN models, and the Steyr AUG. Together they accounted for only 394 BATF trace requests in 1993, and 281 of those concerned Uzis. There are at least three reasons for these low frequencies. First, imports of all of them had been banned under the 1989 assault weapon importation ban. Second, the Blue Book prices of the UZI, FN models, and Steyr AUG were all high relative to the prices of guns typically used in crime. Third, the FN and Steyr models lack the concealability that is often desired in criminal uses.

Among the four domestically produced banned categories, two handgun types were the most frequently submitted for tracing, with 1,377 requests for TEC models and exact copies, and 878 traces of SWD's M-series. Table 2-1 also reports 581 trace requests for Colt AR-15 rifles, 99 for other manufacturers' exact copies of the AR-15, and a handful of trace requests for Street Sweepers and Berettas.

Table 2-1. Description of firearms banned in Title XI

Name of firearm	*Description*	*1993 Blue Book price*	*Pre-ban Federal legal status*	*1993 trace request count*	*Examples of legal substitutes*
Avtomat Kalashnikov (AK)	Chinese, Russian, other foreign and domestic: .223 or 7.62x39mm cal., semi-auto Kalashnikov rifle, 5, 10*, or 30* shot mag., may be supplied with bayonet.	$550 (plus 10-15% for folding stock models)	Imports banned in 1989	87	Norinco NHM 90/91
UZI, Galil	Israeli: 9mm, .41, or .45 cal. semi-auto carbine, mini-carbine, or pistol. Magazine capacity of 16, 20, or 25, depending on model and type (10 or 20 on pistols).	$550-$1050 (UZI) $875-$1150 (Galil)	Imports banned in 1989	281 UZI 12 Galil	
Beretta Ar-70	Italian: .222 or .223 cal., semi-auto paramilitary design rifle, 5, 8, or 30 shot mag.	$1050	Imports banned in 1989	1	
Colt AR-15	Domestic: .Primarily 223 cal. paramilitary rifle or carbine, 5-shot magazine, often comes with two 5-shot detachable mags. Exact copies by DPMS, Eagle, Olympic, and others.	$825-$1325	Legal (civilian version of military M-16)	581 Colt 99 Other manufacturers	Colt Sporter, Match H-Bar, Target. Olympic PCR Models.
FN/FAL, FN/LAR, FNC	Belgian design: .308 Winchester cal., semi-auto rifle or .223 Remington combat carbine with 30-shot mag. Rifle comes with flash hider, 4-position fire selector on automatic models. Manufacturing discontinued in 1988.	$1100-$2500	Imports banned in 1989	9	L1A1 Sporter (FN, Century)
SWD M-10, M-11, M-11/9, M-12	Domestic: 9mm paramilitary semi-auto pistol, fires from closed bolt, 32-shot mag. Also available in fully automatic variation.	$215	Legal	878	Cobray PM-11, PM12 Kimel AP-9, Mini AP-9
Steyr AUG	Austrian: .223 Remington/5.56mm cal., semi-auto paramilitary design rifle.	$2500	Imports banned in 1989	4	
TEC-9, TEC*DC-9, TEC-22	Domestic: 9mm semi-auto paramilitary design pistol, 10** or 32** shot mag.; .22 LR semi-auto paramilitary design pistol, 30-shot mag.	$145-$295	Legal	1202 Intratec 175 Exact copies	TEC-AB
Revolving Cylinder Shotguns	Domestic: 12 gauge, 12-shot rotary mag., paramilitary configuration, double action.	$525***	Legal	64 SWD Street Sweepers	

* The 30-shot magazine was banned by the 1994 Crime Act, and the 10-shot magazine was introduced as a result.
** The 32-shot magazine was banned by the 1994 Crime Act, and the 10-shot magazine was introduced as a result.
*** Street Sweeper
Source: *Blue Book of Gun Values*, 17th Edition, by S.P. Fjestad, 1996.

Although the banned weapons are more likely than most guns to be used in crime, they are so rare that only 5 models appeared among the BATF National Tracing Center list of the 50 most frequently traced guns in 1993: the SWD M-11/9 (659 trace requests, ranked 8), the TEC-9 (602 requests, ranked 9), the Colt AR-15 (581 requests, ranked 11), the TEC-DC9 (397 requests, ranked 21), and the TEC-22 (203, ranked 48). In addition, the list named eight unbanned guns that accept banned large-capacity magazines: the Glock 17 pistol (509 requests, ranked 13), the Ruger P85 pistol (403 requests, ranked 20), the Ruger P89 pistol (361 requests, ranked 24), the

Glock 19 pistol (339 requests, ranked 28), the Taurus PT92 (282 requests, ranked 31), the Beretta/FI Industries Model 92 pistol (270 requests, ranked 33), the Beretta Model 92 (264 requests, ranked 34), and the Ruger Mini-14 rifle (255 requests, ranked 36).

In contrast, the list of ten most frequently traced guns is dominated by inexpensive small-caliber semiautomatic handguns not subject to the ban. These included the Raven P-25 (1,674 requests, ranked 1), the Davis P380 (1,539 requests, ranked 2), the Lorcin L-380 (1,163 requests, ranked 3), the Jennings J-22 (714 requests, ranked 6), and the Lorcin L-25 (691 requests, ranked 7). Other guns among the 1993 top ten list were: the Norinco SKS, a Chinese-made semi-automatic rifle (786 requests, ranked 4); the Mossberg 500 .12-gauge shotgun (742 requests, ranked 5), and the Smith & Wesson .38 caliber revolver (596 requests, ranked 10). None of these are subject to the assault weapon ban.

The relative infrequency of BATF trace requests for assault weapons is consistent with other findings summarized in Koper (1995). During the two years preceding the 1989 import ban, the percentage of traces involving assault weapons reportedly increased from 5.5 to 10.5 percent for all crimes (Cox Newspapers, n.d., p.4), and was 12.4 percent for drug crimes. Because law enforcement agencies are thought to request BATF traces more frequently in organized crime and drug crime cases, many criminal researchers (including ourselves) believe that raw trace request statistics overstate the criminal use of assault weapons in crime. Based on more representative samples, Kleck (1991) reports that assault weapons comprised 3.6 percent or less of guns confiscated from most of the Florida agencies he surveyed, with only one agency reporting as high as 8 percent. Similarly, Hutson et al. (1994) report that assault weapons were involved in less than one percent of 1991 Los Angeles drive-by shootings with juvenile victims. Based on his reanalysis of 1993 New York City data, Koper (1995) concluded that assault weapons were involved in only 4 percent of the 271 homicides in which discharged guns were recovered and 6.5 percent of the 169 homicides in which ballistics evidence positively linked a recovered gun to the crime.

Koper (1995) also summarizes findings which suggest that criminal self-reporting of assault weapon ownership or use may have become “trendy” in recent years, especially among young offenders. The percentages of offenders who reported ever using weapons in categories that may have included assault weapons was generally around 4 percent in studies conducted during the 1980s, but rose to the 20- to 30-percent range in surveys of youth reported since 1993, when publicity about such weapons was high (see, e.g., Knox et al., 1994; Sheley and Wright, 1993).

2.4. MARKETS FOR ASSAULT WEAPONS AND OTHER FIREARMS

Predicting effects of the bans on assault weapons and large-capacity magazines requires some basic knowledge of firearms markets. The Federal Bureau of Alcohol, Tobacco and Firearms (BATF) licenses persons to sell or repair firearms, or accept them as a pawnbroker under the Gun Control Act of 1968. Cook et al. (1995, p.73) summarized the relevant characteristics of a Federal firearms licensee (FFL) as follows. Licenses are issued for three years renewable, and they allow Federal Firearm licensees to buy guns mail-order across state lines without a background check or a waiting period. Starting well before the 1994 Crime Act, applicants had to state that they were at least 21 years old and provide a Social Security number, proposed business name and location, and hours of operation. Since the 1968 Omnibus Crime Control and Safe Streets Act, FFL applicants have had to state that they were not felons, fugitives, illegal immigrants, or substance abusers, and that they had never renounced their American citizenship, been committed to a mental institution, or dishonorably discharged from the military.

The Gun Control Act of 1968 made these same categories of persons ineligible to purchase a gun from a licensee and required would-be purchasers to sign statements that they were not ineligible purchasers. The 1968

Act also requires FFLs to retain the records of each sale and a running log of acquisitions and dispositions of all guns that come into their possession. In 1993, the Brady Handgun Violence Prevention Act added several more requirements on handgun sales by FFLs; the focus on handguns reflected their disproportionate involvement in crime. Under the Brady Act, licensed dealers[5] became required to obtain a photo ID from each would-be handgun purchaser, to verify that the ID described the purchaser, to notify the chief law enforcement officer (CLEO) of the purchaser's home of the attempt to purchase, and to wait five business days before completing the sale, allowing the CLEO to verify eligibility and notify the seller if the purchaser is ineligible. The Brady Act also raised the fee for the most common license, Type 1 (retail), from $10.00 per year to $200.00 for the first three years and $90.00 for each three-year renewal.

Subtitle C of Title XI which took effect simultaneously with the 1994 assault weapons ban strengthened the requirements on FFLs and their customers in several ways, including the following. To facilitate fingerprint-based criminal history checks and to deter applicants who feared such checks, Subtitle C required FFL applicants to submit fingerprints and photographs; this ratified BATF practice that had begun in early 1994. To make FFLs more visible to local authorities, Subtitle C required applicants to certify that within 30 days they would comply with applicable local laws and required the Secretary of the Treasury to notify state and local authorities of the names and addresses of all new licensees. To help local law enforcement agencies recover stolen guns and to discourage licensees from retroactively classifying firearms they had sold without following Federally required procedures as "stolen," Subtitle C introduced requirements for FFLs to report the theft or loss of a firearm to BATF and to local authorities within 48 hours.

Assault weapons and other firearms are sold in primary and secondary markets whose structure was described by Cook et al. (1995). Primary markets include transactions by FFLs. At the wholesale level, licensed importers and distributors purchase firearms directly from manufacturers and advertise them through catalogs and display ads in nationally distributed publications such as *Shotgun News*. Under the law, purchasers may include walk-ins who reside in the distributor's state and FFLs from anywhere who can order guns by telephone, fax, or mail. Primary-market retailers include both large discount stores and smaller-volume independent firearms specialists who offer advice, gun service, sometimes shooting ranges, and other professional services of interest to gun enthusiasts. Some 25,000 independent dealers are organized as the National Alliance of Stocking Gun Dealers. At both the wholesale and retail level, primary-market sellers are legally required to verify that the purchaser is eligible under Federal laws, to maintain records of sales for possible future use in BATF traces of guns used in crime, and, since the effective date of the Crime Act, to report thefts of guns to BATF.

Cook et al. (1995, p.68) also designated "secondary markets," in which non-licensed persons sell or give firearms to others. Sellers other than FFLs include collectors or hobbyists who typically resell used guns through classified ads in newspapers or "consumer classified sheets," through newsletters oriented toward gun enthusiasts, or through word of mouth to family and friends. The secondary market also includes gun shows, "street sales", and gifts or sales to family, friends, or acquaintances. Secondary transfers are not subject to the record-keeping requirements placed on FFLs.

Gun prices in the primary markets are widely publicized, and barriers to entry are few, so that the market for legal purchasers is fairly competitive. For new guns, distributors' catalogs and publications such as *Shotgun News* disseminate wholesale prices. Prices of used guns are reported annually in a *Blue Book* catalog (Fjestad, 1996). Based on interviews with gun market experts, Cook et al. (1995, p.71) report that retail prices track

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[5] The Brady Act exempted sellers in states that already had similar requirements to verify the eligibility of would-be gun purchasers.

wholesale prices quite closely. They estimate that retail prices to eligible purchasers generally exceed wholesale (or original-purchase) prices by 3–5 percent in the large chain stores, by about 15 percent in independent dealerships, and by about 10 percent at gun shows because overhead costs are lower.

In contrast, purchasers who wish to avoid creating a record of the transaction and ineligible purchasers, including convicted felons who lack convincing false identification and wish to avoid the Brady Act eligibility check or waiting period, must buy assault weapons and other guns in the secondary markets, which are much less perfect. Prices for banned guns with accurate and complete descriptions are rarely advertised, for obvious reasons. Sellers do not supply catalogues and reference books that would help an untrained buyer sort out the bewildering array of model designations, serial numbers, and detachable features that distinguish legal from illegal guns. And competition is limited because sellers who are wary of possible undercover purchases by law enforcement agencies prefer to limit "off-the-books" sales either to persons known or personally referred to them, or to settings such as gun shows and streets away from home, where they themselves can remain anonymous.

In general, ineligible purchasers face premium prices some 3 to 5 times legal retail prices.[6] Moreover, geographic differentials persist that make interstate arbitrage, or trafficking, profitable from "loose regulation" states to "tight regulation" states. Among the banned assault weapons, for example, Cook et al. (1995, p.72, note 56) report TEC-9s with an advertised 1991 price of $200 in the Ohio legal retail market selling for $500 on the streets of Philadelphia. By 1995, they report a legal North Carolina price of $300 compared to a street price of $1,000 in New York City. In 1992 interviews with Roth (1992), local and state police officers reported even higher premiums in secondary submarkets in which ineligible purchasers bartered drugs for guns: prices in terms of the street value of drugs reportedly exceeded street cash prices by a factor of about 5.

The attraction that the higher premiums hold for FFLs as sellers has been noted by both researchers and market participants. Cook et al. (1995, p.72) note that licensed dealers willing to sell to ineligible purchasers or without Federal paperwork offer buyers the combined advantages of the primary and secondary markets: "they have the ability to choose any new gun in the catalog, but without the paperwork, delays, fees, and restrictions on who can buy." Their data raise the possibility that up to 78 percent of FFLs in the Raleigh/Durham/Chapel Hill area of North Carolina may operate primarily or exclusively in secondary markets, since 40 percent had not given BATF a business name on their application, and an additional 38 percent provided "business" numbers that turned out to be home numbers (Cook et al., 1995:75). They note the consistency of their findings with a national estimate by the Violence Policy Center (1992 — More Gun Dealers than Gas Stations) that 80 percent of dealers nationwide do not have storefront retail firearms businesses. Jacobs and Potter (1995, p.106) note that because resource constraints have restricted BATF inspections to storefronts, dealers without storefronts may operate without regard to the Brady Act requirements, or presumably to other requirements as well.

The opportunities for FFLs, whether operating from storefronts or not, to sell firearms in both the primary and secondary markets, were colorfully described in the 1993 statement of the National Alliance of Stocking Gun Dealers (NASGD) to the House and Senate Judiciary Committees regarding Subtitle C. After noting the substantial price premium for selling guns directly felons to and others on the street, the statement continues:

> Should you feel a little queasy about the late night hours and the face-to-face negotiations with the street folk, then you can become a "gun-show cowboy." Simply drive by your friendly "distributor"..., load up 250 handguns, and hit the weekend circuit of gun shows...If you choose

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[6] There are exceptions. Guns fired in crimes may sell at substantial discounts on the street because ballistic "fingerprints" may incriminate the subsequent owner. Drug addicts who find and steal guns during burglaries may sell or trade them for drugs at prices far below market.

> to do the "cash and carry" routine then you will command higher prices than those who insist on selling lawfully with all the attendant ID and paperwork. However, since you will most probably be selling at gun shows in states other than where you are licensed, it is unlawful for you to sell and deliver on the spot, so you will not want to identify yourself either. Attendees (purchasers) at gun shows include the entire spectrum of the criminal element — felons, gangs who don't have their own armorer, underage youth, buyers for underage youth, multistate gun runners and such...Though the gun show cowboy won't achieve quite as high a profit as the street seller, he can sell in very high volume and easily earn the same dollar amount and feel a lot safer. (NASGD, 1993:2-3).

Pierce et al. (1995) made an initial effort to investigate the extent and distribution of FFLs' transactions in secondary submarkets through which firearms flow to criminal uses. Using the automated Firearms Tracing System (FTS) recently developed by BATF's National Tracing Center, they explored several covariates of the distribution of traces in which a given FFL holder is named. They reported the highest mean number of traces for dealers in Maryland, Vermont, and Virginia. Other cross-tabulations indicated that currently active dealers operating at the addresses previously used by out-of-business dealers were more likely than average to be named in traces, which suggests that dealers who are active in secondary markets tend to reapply for licenses under new names. Finally, they reported a very high concentration of dealers in trace requests. While 91.6 percent of the dealers in the FTS database had never been named in a trace, 2,133 dealers, 0.8 percent of the total, had been named in 10 or more traces. Together, they were named in 65.7 percent of all traces conducted. An even smaller handful of 145 dealers' names surfaced in 30,850 traces — 25.5 percent of the entire trace database. These findings indicated that the channels through which guns flow from FFLs to criminal users are more heavily concentrated than previously recognized.

The channels described above through which firearms flow from licensed dealers (FFLs) and eligible purchasers to ineligible purchasers vary in terms of visibility.[7] In primary markets, ineligible purchasers may buy guns from FFLs using fake identification themselves or using "straw purchasers" (eligible buyers acting as agents for ineligible buyers, unbeknownst to the FFL). In Cook and Leitzel's (1996) terminology, these are "formal" transactions that create official records, but the records do not identify the actual consumer.

We use the term "leakage" to designate channels through which guns flow from legal primary and secondary markets to ineligible purchasers. No leakage channel creates valid sales records; however, at least since 1994, all are likely to generate stolen gun reports to BATF. Ineligible purchasers may buy guns informally (i.e., without paperwork) from unethical FFLs at gun shows or through "street" or "back door" sales. To prevent informal sales from creating discrepancies between actual inventories and the acquisition/disposition records, the FFL may report them as stolen. Such transactions are indistinguishable from actual thefts, the other leakage channel.

Guns may also leak from eligible non-FFL gun owners to ineligible owners through direct sales on the street or at gun shows, or through thefts. While non-FFL owners are not required to record sales or transfers of their guns, they may also wish to report a gun that they sell to an ineligible purchaser as stolen if they suspect it may be recovered in a future crime. Therefore, leakage in secondary markets may also be reflected in theft reports.

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[7] While the law presumes ineligible purchasers to be more likely than eligible purchasers to use guns during crimes, eligible purchasers have, in fact, committed viable crimes with large-capacity firearms.

3. ANALYSIS PLAN

Subtitle A of Title XI banned the manufacture, transfer, and possession of assault weapons and large-capacity magazines. We hypothesized that the ban would produce direct effects in the primary markets for these weapons, that related indirect effects in secondary markets would reduce the frequency of their criminal use, and that the decrease in use would reduce such consequences as gun homicides, especially incidents involving multiple victims, multiple wounds, and killings of law enforcement officers. In this chapter, we explain our general strategy testing these hypotheses.

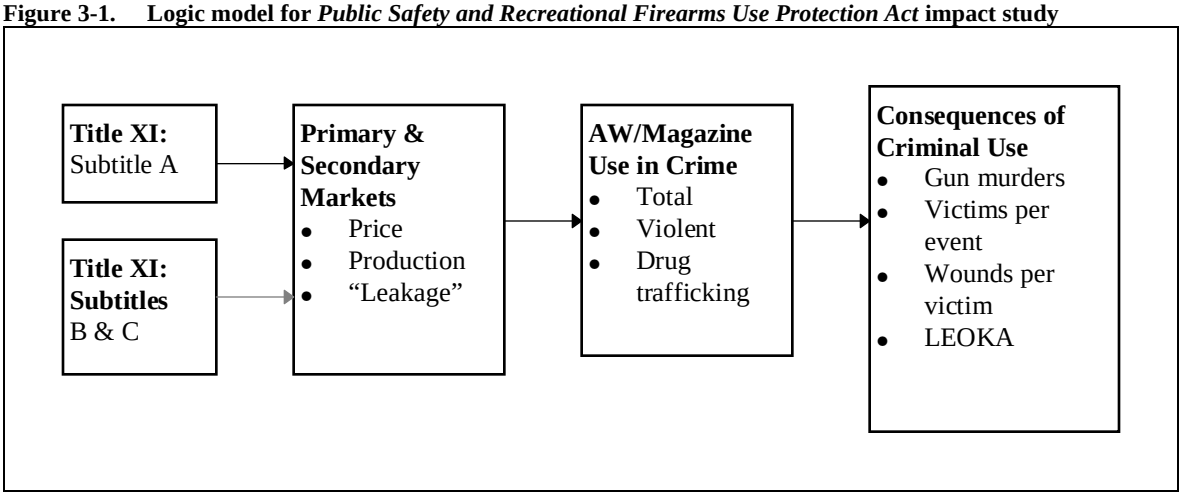
3.1. POTENTIAL BAN EFFECTS

Figure 3-1 displays the ban effects that we hypothesized and the measures that we used to test those effects. As shown there, we anticipated potential effects on primary and secondary markets for the banned guns and magazines, potential reductions in their use in crime, and subsequent reductions in the consequences of criminal use. Although the available measures of any single effect are problematic, the problems differ by measure. Therefore, our approach was to conduct several small studies, each subject to different error sources, and then to integrate the findings of the separate studies.

As shown in Figure 3-1, the **market effects** of interest included indicators of price, production, and "leakage" between primary and secondary markets. If the Subtitle A bans are to be effective in reducing criminal uses of the banned weapons and magazines, they must increase the prices of those items. Our **price** indicators were collected for banned guns, selected legal substitutes, large-capacity magazines, and, as comparison groups, comparable guns that should not have been directly affected by the ban. The data were the nationally advertised prices of distributors who ran display ads in *Shotgun News* continuously from January 1992 through mid-1996. Because these distributors sell guns simultaneously at the wholesale and retail levels, and because primary-market retail margins are small, we believe these prices offer a useful index of primary-market prices. We used hedonic price analysis to study trends. Annual **production** data were obtained from the Violence Policy Research Project, an organization that compiles BATF manufacturing data. We lacked post-ban data because release of the production statistics is delayed two years by law. Also, we had to make certain approximations because production statistics are not reported for specific models. Therefore, findings from our tabular analyses of production are less complete and more tentative than those about price. Finally, as discussed in Section 3.2, we defined **"leakage"** as the transfer of firearms to ineligible purchasers from licensed dealers and eligible purchasers. Because we argued there that leakage is likely to generate theft reports (either because the guns were transferred by theft or because a false theft report was used to conceal a sale to an ineligible purchaser), we measured leakage using counts of stolen gun reports to the FBI's National Crime Information Center (NCIC).

Our primary indicator of assault weapon **use in crime** is the volume of requests for BATF traces of guns recovered in crime. **Trace request** data have the advantage of providing a national picture, and they allow us to focus on two of the Congressional priorities for this study, violent crime and drug trafficking crime. They require special caution in interpretation, however, since trace requests are a small and unrepresentative sample of guns recovered in crime. We believe that our tabular analyses provide a defensible estimate of the short-term effects of Title XI on criminal use of the banned weapons. We attempted to supplement the national analysis with analyses of **local trends in recovered assault weapons** in representative samples of recovered guns from a number of law enforcement agencies, but could obtain the necessary data for only a few cities.

Figure 3-1. Logic model for *Public Safety and Recreational Firearms Use Protection Act* impact study



Finally, as shown in Figure 3-1, we used four indicators of the **consequences** of criminal use of assault weapons and semiautomatic weapons with large-capacity magazines: total gun murders by state, victims per criminal event involving gun murder, entry wounds per gunshot wound victim, and law enforcement officers killed in action. While these indicators all have logical relationships to use of the banned items, all have difficulties. Total gun murders is an insensitive indicator because attacks with assault weapons and other semiautomatics with large-capacity magazines account for only a fraction of all murders. Other consequences such as victims per event and wounds per victim are more specific to the banned weapons and magazines, as supporters argued during the ban debates, and assault weapons are more disproportionately used in killings of law enforcement officers than in other murders. However, available databases for measuring those impacts are difficult to analyze because they contain such small numbers of cases. And, for all the indicators, the existence of only one full post-ban year in available data may make the estimates too imprecise to discern short-run impacts even if they are large enough to be of policy interest. As a result, our findings about ban effects on consequences are especially tentative.

We anticipated that market effects during the short-term period allowed for this study would be heavily influenced by expectations. Enactment of the ban was preceded by extensive publicity and debate, which afforded time for manufacturers, distributors, retailers, and collectors to speculate that the firearms being considered for ban coverage would eventually become expensive collectors' items. Analogous experience from 1989 seemed instructive, because that year saw both a Federal ban on importation of assault rifles and a California ban analogous to Title XI. During the three months leading up to the importation ban, import license requests for assault rifles, which had numbered 40,000 in 1987 and 44,000 in 1988, swelled 10-fold to an annual rate of 456,000 (AMA Council, 1992). It is not clear how rapidly the import surge flowed through the distribution chain from importers to consumers in the primary and secondary markets. Yet six months later, during the period leading up to a California ban and sentence enhancement, several police agencies reported sharp decreases in criminal use of assault rifles. At the time, observers attributed this seeming paradox to advance publicity that may have left the misimpression that the ban took effect when enacted, judicial anticipation of the enhancements in setting bond and imposing sentence, tips to police from law-abiding gun dealers sensitive to the criminal gun use that motivated the ban, and owners' reluctance to risk confiscation for misuse of their assault weapons, which had become more valuable in anticipation of the ban (Mathews, 1989). However, it is equally plausible that the speculative price increases for the banned weapons in formal markets at least temporarily bid assault weapons

away from ineligible purchasers who would more probably have used them in crimes (Cook and Leitzel, 1996).[8] Whether these short-run conditions would hold for the long run would depend on the extent to which grandfathered guns in the banned categories leaked into secondary markets over time through gun shows, "back door" sales, and thefts.

Therefore, our objectives became to estimate ban-related effects on price, supply responses, and leakage from formal to informal markets; to estimate how these market effects influenced criminal assault weapon use; and to estimate trends in the consequences of that use. In accordance with the statutory study requirement, we placed special emphasis on the use of assault weapons in violent crime and drug trafficking crime wherever available data permitted.

3.2. GENERAL DESIGN STRATEGY

Our general design strategies are to test whether the assault weapon and magazine bans interrupted trends over time in the outcome measures listed above. A variety of techniques exist for this general problem. They differ in terms of desirable qualities such as statistical power, robustness against various threats to the validity of findings, and precision; unfortunately, the techniques with more desirable properties are generally more demanding in terms of data requirements. Because of different data constraints, we employed a variety of methods, including various forms of time series and multiple regression analysis (i.e., pooled, cross-sectional time series analysis, hedonic price analysis, and Box-Jenkins interrupted time series models), simple before and after comparisons, and graphical displays. As a result, our conclusions about some measures are stronger than about others.

Because we anticipated these circumstances, our approach to the Congressional mandate was to conduct a number of small-scale analyses of more-or-less readily available data, then to synthesize the results into our best judgment concerning the impacts of Title XI.[9] We carried out three kinds of analyses of market effects:

- Hedonic price analyses of 1992–96 primary-market price trends for banned semiautomatic firearms, comparable unbanned firearms, and large-capacity magazines, using national distributors' prices;
- Tabular analyses of gun production data through 1994, the latest available year;
- Pre-ban/post-ban comparisons and time series analyses of 1992–96 trends in "leakage" to illegal markets, as measured by guns reported stolen to FBI/NCIC.

We carried out two kinds of analyses of assault weapon use:

- Graphical and tabular analyses of 1992–96 trends in requests for BATF traces of assault weapons recovered in crime, in both absolute terms and as a percentage of all requests;

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[8] While unbanned, widely available, inexpensive semiautomatic pistols made by Lorcin, Davis, and other manufacturers are good (and perhaps superior) substitutes for the banned assault weapons in most criminal uses, they are not substitutes for speculative purposes.

[9] During the project, we abandoned early plans for several additional impact studies that we had contemplated. It proved impossible to analyze trends in enforcement of the ban because of the small numbers of matters referred to U.S. Attorneys and cases filed in U.S. District Court. We were forced to abandon plans to measure secondary-market prices of banned weapons from classified advertisements for two reasons: back issues of consumer classifieds proved unavailable, and the ads describe the weapons too imprecisely for consistent classification. Finally, we dropped plans to analyze multi-city assault weapon use data from the gun module of the Drug Use Forecasting (DUF) program for two reasons. Data exist only for the post-ban period, and we had concerns about the validity of respondents' reports of assault weapon ownership and use.

- Pre-ban/post-ban comparisons and time series analyses of 1992–96 trends in counts of guns recovered in crime by selected local law enforcement agencies.

We carried out the following analyses of the consequences of using assault weapons and semiautomatics with large-capacity magazines in crime:

- An analysis of state-level time-series data on gun murders which controls for potential influences of legal, demographic, and criminological importance;
- Pre-ban/post-ban comparisons and time series analyses of 1980–95 trends in victims per gun-homicide incident as measured nationally from Supplementary Homicide Reports;
- Descriptive analysis of the use of assault weapons in mass murders in the U.S. from 1992-present (see Appendix A);
- Graphical analyses and pre-ban/post-ban comparisons of 1992–96 trends in the number of wounds per gunshot victim using medical data from medical examiners and one hospital emergency department in selected cities, following Webster et al. (1992) and McGonigal et al. (1993);
- A tabular analysis of 1992–96 trends in law enforcement officers killed in action (LEOKA) with assault weapons.

3.2.1. Threats to Validity and Use of Comparison Groups

The validity of the techniques we applied depends on comparisons of trends between meaningful treatment and comparison groups, and we used two approaches to defining comparison groups. In general, to estimate ban effects on markets and uses, we compared trends between types of guns and magazines that were differentially affected by the ban. To estimate effects on the consequences of assault weapon use, we used pre-existing state-level bans on assault weapons and juvenile handgun possession to define comparison groups, because we assumed that such laws would attenuate the effects of the Federal ban.[10]

Table 3-1 describes our general classification scheme for types of guns affected by the ban and the corresponding comparison groups.[11] The comparisons are not always precise, and, as later chapters will make clear, they differ from measure to measure depending on the gun descriptors used in available databases.

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[10] Although in theory, comparisons of markets and uses could be made simultaneously by weapon and jurisdiction, the disaggregation often leaves too little data for meaningful analysis.

[11] To be considered a potential comparison gun, we had to have at least anecdotal evidence that it had appeal beyond the community of sportsmen and collectors and/or evidence that it was among the 50 guns most commonly submitted for BATF traces. Without that constraint, it would have been unreasonable to consider it as being functionally similar to any banned gun, and data on prices and uses would have involved numbers too small to analyze. The trade-off is that the comparison guns may well have been subject to indirect substitution effects from the ban.

Table 3-1. Banned weapons and examples of unbanned comparison weapons

Banned weapon	*Examples of Comparison weapon*
Named Domestic Assault Pistols	
-SWD M-10, M-11, M-11/9, M-12, exact copies under other names, legal substitutes -TEC-9, TEC-DC9,TEC-22, exact copies by AA Arms, legal substitutes	-Lorcin, Davis semiautomatic pistols (less expensive) -Glock, Ruger semiautomatic pistols (more expensive)
Named Domestic Assault Rifles	
-Colt AR-15, exact copies and legal substitutes	-Ruger Mini-14 (unbanned domestic) -Maadi (legal import)
Named Foreign Assault Weapons	
-UZI carbines and pistols -AK models	-SKS (recently restricted, widely available import)
"Features Test" Guns	
Calico Light Weapons pistols and rifles Feather rifles	See pistols and rifles above.
Rare Banned Weapons	
Beretta Ar-70, FN models, Steyr AUG, revolving cylinder shotguns	No comparisons defined.

Of the banned weapons named in Table 3-1, the named domestic assault pistols are of greatest interest because they are more widely used in crime than rifles. We used two categories of pistols as comparison groups: the cheap small-caliber pistols by Lorcin and Davis that are among the most widely used guns in crime, and the more expensive Glock and Ruger pistols. The Glock and Ruger models took on additional significance by serving as indicators of non-banned handguns capable of accepting large-capacity magazines. For the AR-15 family of assault rifles, we used the Ruger Mini-14, SKS, and/or Maadi rifles in various comparisons. All are legally and widely available.

We performed relatively few comparative analyses of named foreign assault weapons, the UZI, Galil, and AK weapons, because the 1989 import ban limited their availability during our observation period, and their legal status was unchanged by the Title XI ban. Nevertheless, because these guns remain in criminal use, we performed price analyses for their large-capacity magazines, which are also widely available from foreign military surplus. The SKS semiautomatic rifle, which was imported from China and Russia in fairly large numbers[12] until recently, served as an unbanned comparison weapon for the banned foreign rifles. We carried out no analyses concerning the rarest assault weapons shown in Table 3-1.

Because few available databases relate the consequences of assault weapon use to the make and model of weapon, most of our analyses of consequences are based on treatment and comparison jurisdictions defined in terms of their legal environments. Four states — California, Connecticut, Hawaii, and New Jersey — already

[12] Although a 1994 ban on Chinese imports of many goods including firearms nominally covered SKS rifles, large numbers continued to enter the country under Craig Amendment exemptions for goods already "on the water" at the time of the import ban.

banned assault weapons before the Federal ban was enacted. Although state bans can be circumvented by interstate traffickers, we hypothesized that their existence would reduce the effects of the Federal ban in their respective states.

The following chapters report findings of the analyses described here. Each chapter also explains in detail the tailoring of this general analysis plan to data constraints associated with each comparison.

4. GUN AND MAGAZINE MARKET EFFECTS

The discussion of gun markets in Chapter 2 led us to several hypotheses. First, assuming that the primary and secondary markets were in equilibrium before Congress took up serious discussion of a ban on assault weapons and large-capacity magazines, we hypothesized that the opening of debate would stimulate speculative demand for the banned guns and magazines, leading to price increases in primary markets well in advance of the effective date of the ban. Second, we hypothesized that for the makes and models of assault weapons whose prices increased, quantities produced would also increase before the ban took effect. These "grandfathered guns" were exempted from the ban.

Having been advised by a gun market expert[13] that legal substitutes for many of the banned weapons appeared in primary markets around the effective date of the ban, it seemed doubtful that the speculative pre-ban price increases could hold under the combined weight of stockpiled grandfathered guns and the flows of new legal substitute models. Therefore, our third hypothesis was that the post-ban prices of banned guns and their legal substitutes would return to their pre-debate equilibrium levels.

We presumed that assault weapons and large-capacity magazines are economic complements, so that, like bread and butter, an increase in the supply of either one should decrease its price and increase the price of the other. Therefore, our fourth hypothesis was that, for the oversupplied assault weapons and legal substitutes whose prices fell from their speculative peaks, their magazine prices[14] should rise over time, as the stock of grandfathered magazines dwindled.

Finally, we believed that for banned makes and models whose prices experienced a speculative price bubble around the time of the ban and then returned to pre-ban levels, speculative demand would fall eventually in both primary and secondary markets as expectations receded for a price "rebound" in primary markets. In contrast, demand by ineligible purchasers intending to use the banned weapons in crime should be relatively unaffected. Therefore, at least in the short run, relative prices should rise in secondary markets, where such "crime demand" is concentrated. We could not directly observe secondary-market prices. However, a price rise in secondary relative to primary markets should cause increased "leakage" to secondary markets, reflected in rising theft reports of assault weapons during post-ban periods of low prices in primary markets.

The following sections report the methods we used to test these hypotheses about market effects of the ban, and our findings.

4.1. FINDINGS OF PRICE ANALYSIS

4.1.1. Collection of Price Data

To test our hypotheses about price trends, we sought to approximate the prices at which the banned items could be legally purchased throughout the country. After considering available data sources, we decided that monthly data would be sufficient and that the distributors' prices advertised in national publications would offer a

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[13] William R. Bridgewater, personal communication, September 1995.

[14] Magazines are make and model-specific, so that in general a magazine made for a specific rifle will not fit other rifles. However, a magazine made for a banned assault rifle like the Colt AR-15 will fit an exact copy like the Olympic Arms AR-15 and a legal substitute like the Colt AR-15 Sporter, which has the same receiver.

suitable index. Those prices are available to any FFL, and, as discussed in Chapter 2, primary-market FFLs generally re-sell within 15 percent of the distributors' price.

To collect the necessary data, we developed two forms. The first was designed to collect data on base price and accessorized price on 47 makes and models of guns. These included all guns named in Subtitle A along with selected legal substitutes and functional substitutes (e.g., low-capacity semiautomatic pistols that are commonly used in crimes). The second form recorded make, model, capacity, and price of any advertised large-capacity magazines. Both forms also recorded the distributors' names and, for verification purposes, a citation to the location of the advertisements.

We selected twelve gun and magazine distributors that had display ads on a monthly basis in *Shotgun News* throughout the entire period from April 1992 through June 1996. This period was selected to permit observation of rumored "Clinton election" price effects (i.e., increased speculative demand based on concern over possible new gun controls under a Democratic administration) as well as the entire period of debate over Subtitle XI and as long a post-ban period as possible. Display ad prices were coded on a monthly basis throughout the period except immediately around the ban, from August 1994 to October 1994, when prices were coded on a weekly basis to maximize statistical power during the period when we expected the largest price variances. The Shotgun News issue to be coded for each month was selected randomly, to avoid any biases that might have occurred if a particular part of the month was coded throughout the period. The number of advertised-price observations for any given gun varied from month to month over the period, as distributors chose to feature different makes and models. The number of price observations for a given make and model bears an unknown relationship to the number of transactions occurring at that price. The advertised prices should be considered approximations for at least three reasons. Advertised prices simultaneously represent wholesale prices to retail dealers and retail prices to "convenience dealers" who hold licenses primarily to receive guns for personal use by mail from out-of-state sources. There is anecdotal evidence of discounts from advertised prices for purchases in large quantities or by long-time friends of the distributors. Finally, the ads did not permit us to accurately record such price-relevant features as finish, included gun cases, and included magazines.

4.1.2. Analysis

Price trends for a number of firearms and large-capacity magazines were analyzed using hedonic price analysis (Berndt 1990, pp.102-149; also see Chow 1967). This form of analysis examines changes over time in the price of a product while controlling for changes over time in the characteristics (i.e., quality) of the product. Hedonic analysis employs a model of the form:

$$Y = a + b * X + c_1 * T_1 + \ldots c_n * T_n + e$$

where Y is the logarithmic price of the product, X represents one or more quality characteristics affecting the price of the product, T_1 through T_n are dummy variables for the time periods of interest, a is an intercept term, and e is an error term with standard properties. The coefficients c_1 through c_n provide quality-adjusted estimates of changes over time in the price of the product.

In the analysis that follows, all price data were first divided by quarterly values of the gross domestic product price deflator as provided in *Economic Indicators* (August 1996). This quantity was then logged. In all models, we have omitted the time dummy for the period when the ban went into effect. Thus, the time coefficients are interpreted relative to the prices at the time of ban implementation. Because the outcome variable is logged, the coefficients on the time period indicators can be interpreted as multiplier effects (we illustrate this in more

detail below). Whenever possible, we examined quarterly price trends. In a number of instances, however, sample size considerations required us to use semi-annual or annual periods.

Our quality variables correspond to factors such as manufacturer, model, distributor, and, in some cases, weapon caliber. In addition, some of the models include an indicator variable denoting whether the firearm had special features or enhancements or was a special edition of any sort.[15] We have used these variables as proxy variables for quality characteristics in the absence of more detailed measures of weapon characteristics. Further, we cannot fully account for the meaning of significant distributor effects. Distributor effects may represent unmeasured quality differentials in the merchandise of different distributors, or they may represent other differences in stock volume or selling or service practices between the distributors.[16] Nevertheless, we included distributor because it was often a significant predictor of price. Thus, our models provide price trends after controlling for the mix of products and distributors advertised during each time period. Finally, the models presented below are parsimonious models in which we have retained only those quality indicators which proved meaningful in preliminary analyses.[17]

4.1.2.1. Gun Prices

For the analysis of firearm prices, we chose groups of weapons based on both theoretical importance and data availability (a number of the guns included on our coding form appeared infrequently in the ads examined by project staff). We examined price trends in banned assault pistols and compared them to price trends for unbanned semiautomatic handguns commonly used in crime. In addition, we analyzed the price trend for the banned AR-15 assault rifle and its variations and compared it to trends for a number of similar semiautomatic rifles not subject to the ban.

Our findings for handguns were consistent with our hypotheses. For the banned SWD group of assault pistols, the average advertised price peaked at the time the ban took effect, having risen from 68 percent of the peak a year earlier; within a year, the mean price fell to about 79 percent of peak. In contrast, advertised prices of unbanned Davis and Lorcin semiautomatic pistols commonly used in crime were essentially constant over the entire period.

Rifle price trends were only partially consistent with our hypotheses. For semiautomatic rifles, prices of both the banned AR-15 family of assault rifles and a comparison group of unbanned semiautomatic rifles showed evidence of speculative peaks around the time the ban took effect, followed by a decrease to approximately pre-speculation levels.

We interpret these findings as evidence of substantial speculative pre-ban demand for guns that were expected to be banned as assault weapons, while the underlying primary market for guns more commonly used in crime remained stable. While no plausible definition of assault weapon was ever likely to include the Davis and

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[15] We note, however, that recording special features of the weapons was a secondary priority in the data collection effort; for this reason, and because the ads do not follow a consistent format, this information may not have been recorded as consistently as other data elements.

[16] We have heard speculations but have no evidence that distributors' prices for a given quantity of a specific gun may be inversely related to the rigor of their verification of purchasers' eligibility.

[17] We eliminated control variables that had t values less than one in absolute value. This generally improved the standard errors for the coefficients of interest (i.e., the coefficients for the time period indicators).

Lorcin pistols, Lenett (1995) describes considerable uncertainty during the Crime Act debate over precisely which rifles were to be covered.

Assault pistols: The analysis of assault pistol prices focused on the family of SWD M10/M11/M11-9/M12 weapons.[18] [19] Our coders did not find enough ads for these weapons to conduct a quarterly price trend analysis; therefore, we examined semi-annual prices. Results are shown in Table 4-1. In general, the M10, M11, and M11/9 models were significantly more expensive than the M12 model and the new PM11 and PM12 models. Models with the Cobray trademark name had lower prices, while weapons made in .380 caliber commanded higher prices. Finally, two distributors selling these weapons had significantly lower prices than did the other distributors.

[18] Over the years, this class of weapons has been manufactured under a number of different names (i.e., Military Armaments Corp., RPB Industries, Cobray, SWD, and FMJ).

[19] Initially, we had also wished to analyze the prices of banned Intratec weapons and their copies. However, project staff found few ads for these guns among the chosen distributors, particularly in the years prior to the ban's implementation.

Table 4-1. Regression of SWD handgun prices on time indicators, controlling for product characteristics and distributors

Analysis of Variance

Source	*DF*	*Sum of squares*	*Mean square*	*F value*	*Prob>F*
Model	16	16.26086	1.01630	13.376	0.0001
Error	132	10.02900	0.07598		
C Total	148	26.28986			

Root MSE	0.27564	R–square	0.6185
Dep Mean	0.87282	Adj R–square	0.5723

Parameter Estimates

Variable	*DF*	*Parameter estimate*	*Standard error*	*T for H0 parameter = 0*	*Prob>|T|*
INTERCEP	1	1.00876	0.073205	13.78	0.0001
T1	1	-0.17097	0.130798	-1.307	0.1935
T2	1	-0.29236	0.109943	-2.659	0.0088
T3	1	-0.26949	0.078477	-3.434	0.0008
T4	1	-0.38309	0.086909	-4.408	0.0001
T5	1	-0.1881	0.12957	-1.452	0.1489
T7	1	-0.04368	0.076185	-0.573	0.5674
T8	1	-0.23376	0.108602	-2.152	0.0332
T9	1	0.108787	0.205848	0.528	0.5981
CAL380	1	0.200609	0.06946	2.888	0.0045
DIST 3	1	-0.26216	0.128954	-2.033	0.0441
DIST 5	1	0.331378	0.224065	1.479	0.1415
DIST 6	1	-0.18987	0.059367	-3.198	0.0017
COBRAY	1	-0.18832	0.053756	-3.503	0.0006
M10	1	0.771313	0.131932	5.846	0.0001
M11	1	0.308675	0.057351	5.382	0.0001
M119	1	0.110174	0.077347	1.424	0.1567

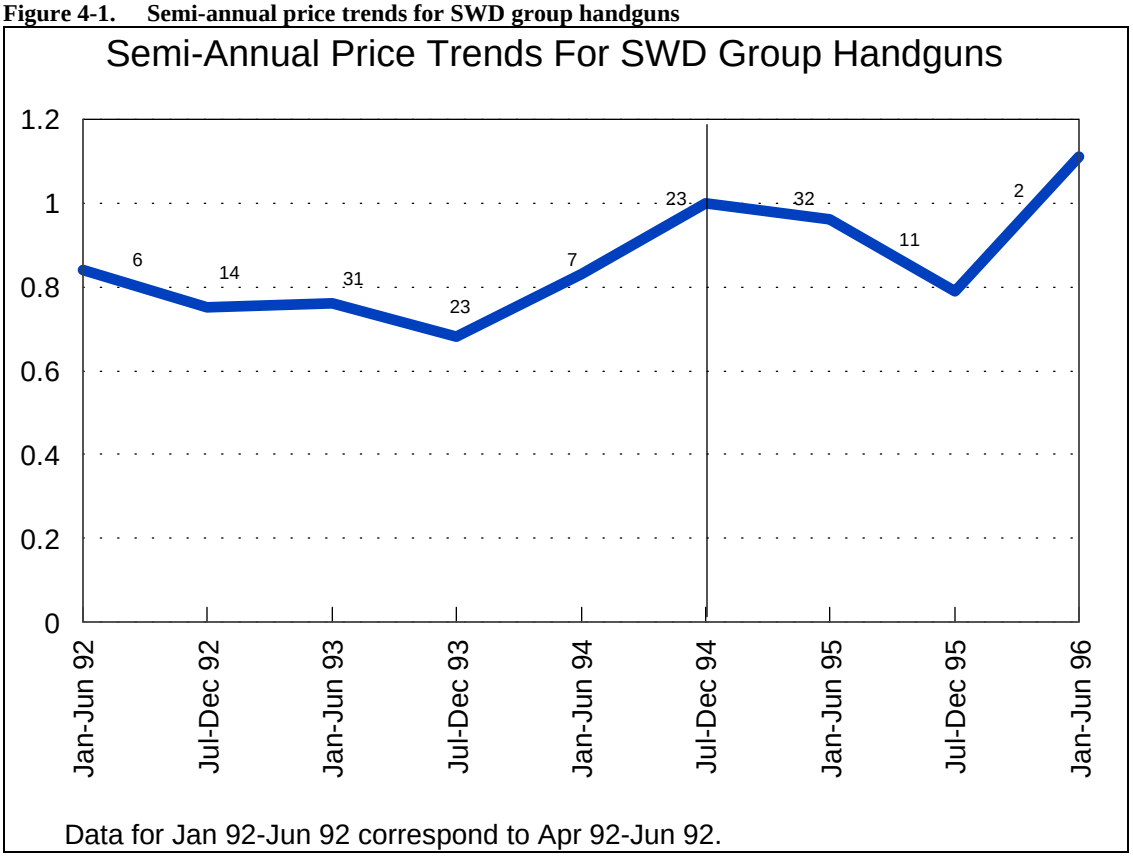
The coefficients for the time indicator variables provide quality-adjusted price trends. The time indicator t6 has been omitted from the equation.[20] This indicator corresponds to the period of July 1994 through December 1994 which encompasses the ban implementation date of September 13, 1994. The coefficients on the time dummy variables are all negative and most are significant, indicating that prices for these weapons were at their highest during the six month period when the ban took effect. To interpret the time variables, we exponentiate the coefficients (i.e., take their antilogs). To illustrate, the coefficient for the first time period (January 1992 through June 1992) is -0.170966.[21] Exponentiating this coefficient yields approximately 0.84, indicating that the average price of these weapons at time 1 (January 1992 through June 1992) was 84 percent of the average price at time 6

[20] In this and all other price analyses, time dummies are defined to omit the time period that includes the effective date of the ban. This restricts the coefficient to 0 and exp(0) = 1. Therefore, the effective date is the reference period for prices in all other periods.

[21] Data collection began with April 1992 issues of Shotgun News. Consequently, the first data point is based on data for April through June of 1992 rather than a full six-month period.

(July 1994 through December 1994). Conversely, the average quality-adjusted price of these firearms was 17 percent less during the January 1992-June 1992 period than during the July 1994-December 1994 period.

Figure 4-1. Semi-annual price trends for SWD group handguns



The time effects are displayed graphically in Figure 4-1 (sample sizes are shown for each time period).[22] During the semi-annual periods prior to the ban's implementation, prices of these weapons ranged from 68 to 83 percent of their price during the period of the ban's implementation. Prices peaked when the ban became effective in the latter part of 1994 and remained high through the first half of 1995. In the second half of 1995, however, the prices dropped off dramatically, falling to levels comparable to the pre-ban period. Prices may have rebounded again during the first half of 1996, but the apparent "rebound" was based on only two advertisements and should be treated very cautiously. If one assumes that wholesale markets were in equilibrium before debates about the ban started, then these data reflect a ban-related, speculative peak of up to 47 percent in price, followed by a decline of about 20 percent. Parenthetically, we note that contrary to some anecdotes, we found no evidence of speculation related to the 1992 election.

Comparison handguns: For comparison, we also examined price trends for a number of unbanned semiautomatic handgun models: the Davis P32 and P380 and the Lorcin L25 and L380. By a number of accounts, these models are among the guns most frequently used in crime (BATF 1995; Kennedy et al. 1996; Wintemute 1994, Chapter 2 supra). Because of small sample size, this model was estimated using semi-annual data spanning from 1992 through 1995. Referring to Table 4-2, two of the handgun models were significantly less expensive than the others, and one distributor offered statistically significant discounts for these guns.

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[22] Sample sizes are defined in terms of number of price observations available during the period. The number of transactions that took place at each recorded price is, of course, unavailable to us.

Table 4-2. Regression of Lorcin and Davis handgun prices on time indicators, controlling for product characteristics and distributors

Analysis of Variance

Source	*DF*	*Sum of squares*	*Mean square*	*F value*	*Prob>F*
Model	11	3.60246	0.32750	30.678	0.0001
Error	81	0.86469	0.01068		
C Total	92	4.46716			

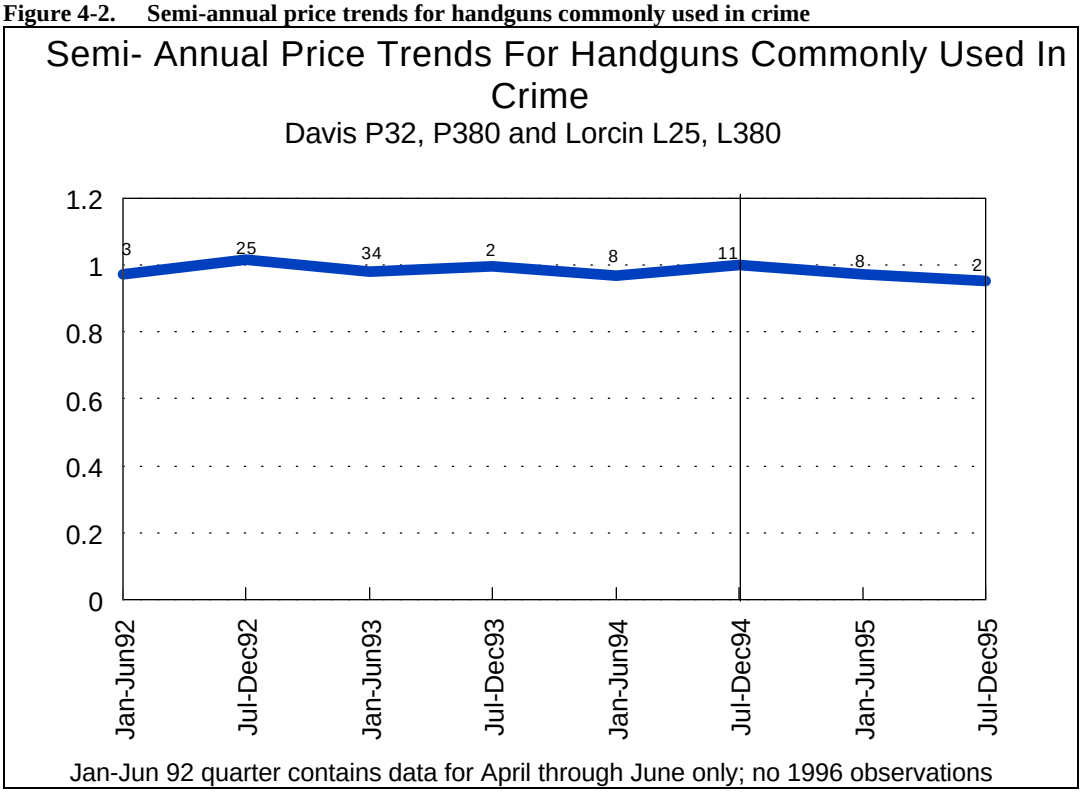
Root MSE	0.10332	R–square	0.8064
Dep Mean	-0.60396	Adj R–square	0.7801
C.V.	-17.10713		

Parameter Estimates

Variable	*DF*	*Parameter estimate*	*Standard error*	*T for H0 parameter = 0*	*Prob>|T|*
INTERCEP	1	-0.44243	0.034043	-12.996	0.0001
T1	1	-0.03004	0.069877	-0.43	0.6684
T2	1	0.014817	0.040258	0.368	0.7138
T3	1	-0.0198	0.037239	-0.532	0.5964
T4	1	-0.00259	0.082314	-0.031	0.975
T5	1	-0.03162	0.048582	-0.651	0.517
T7	1	-0.02753	0.048576	-0.567	0.5724
T8	1	-0.05041	0.082314	-0.612	0.542
P32	1	-0.22559	0.033404	-6.753	0.0001
L25	1	-0.55562	0.034119	-16.285	0.0001
DIST 2	1	-0.06434	0.030256	-2.127	0.0365
DIST 6	1	-0.05723	0.042414	-1.349	0.181

The time period coefficients indicate that prices for these weapons were unaffected by the assault weapons ban. Most of the time dummies have negative signs, but their t score values are very small, indicating that prices during these periods did not differ meaningfully from those at the time when the ban was implemented. This is underscored graphically in Figure 4-2.

Figure 4-2. Semi-annual price trends for handguns commonly used in crime



Assault rifles: To investigate the ban's effect on assault rifle prices, we examined quarterly price trends for the Colt AR15 family, which includes the AR15 as well as Colt's Sporter, H-Bar, and Target models.[23] Referring to Table 4-3, the AR15 model was more expensive than other models. Further, guns which had special features/enhancements or a special designation of some sort had somewhat higher prices. Models in 7.62mm caliber were lower in price than other models, though this effect was not quite statistically significant. Finally, one distributor stood out as having lower prices than other distributors.

[23] A number of other manufacturers also made exact copies of the Colt AR15 (e.g., Essential Arms, Olympic Arms, and SGW Enterprises). We included a number of these copies on our price coding form before the ban and legal substitutes thereafter, but we did not find advertisements for these non-Colt versions in *Shotgun News*.

Table 4-3. Regression of Colt AR15 group prices on time indicators, controlling for product characteristics and distributors

Analysis of Variance

Source	*DF*	*Sum of squares*	*Mean square*	*F value*	*Prob>F*
Model	23	21.67729	0.94249	18.161	0.0001
Error	235	12.19537	0.05190		
C Total	258	33.87266			

Root MSE	0.22781	R–square	0.6400
Dep Mean	2.13335	Adj R–square	0.6047
C.V.	10.67826		

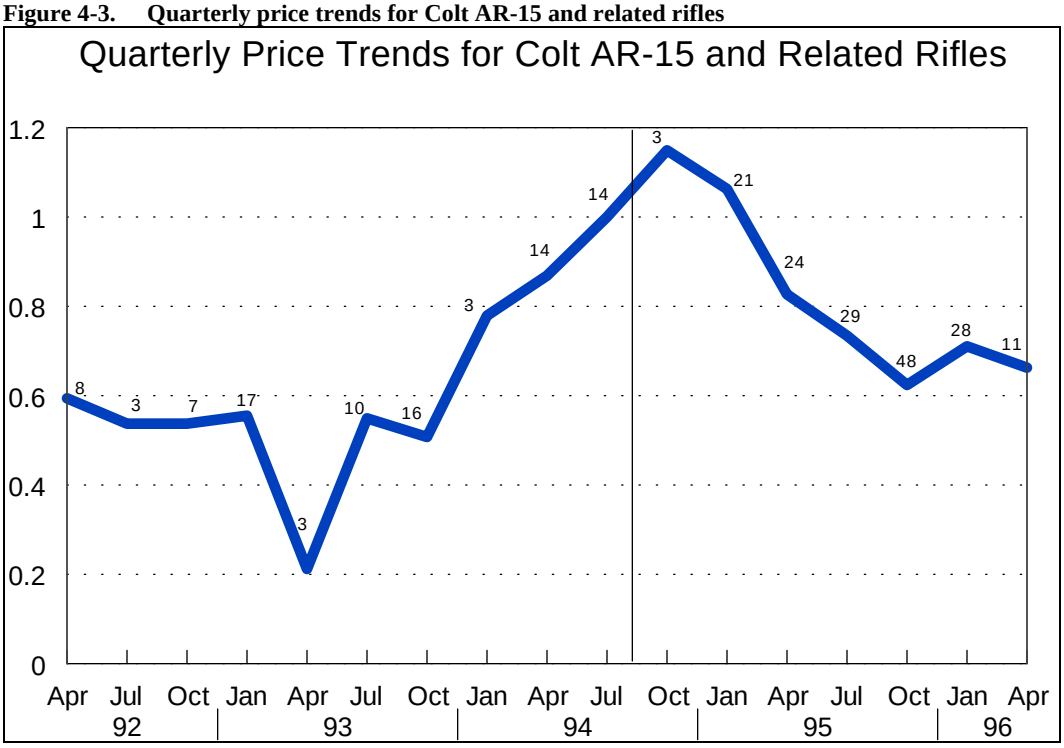
Parameter Estimates

Variable	*DF*	*Parameter estimate*	*Standard error*	*T for H0 parameter = 0*	*Prob>|T|*
INTERCEP	1	2.714668	0.066599	40.762	0.0001
Q1	1	-0.52079	0.107749	-4.833	0.0001
Q2	1	-0.62023	0.149137	-4.159	0.0001
Q3	1	-0.62368	0.116786	-5.34	0.0001
Q4	1	-0.58506	0.083154	-7.036	0.0001
Q5	1	-1.54569	0.150793	-10.25	0.0001
Q6	1	-0.60339	0.095035	-6.349	0.0001
Q7	1	-0.68488	0.084707	-8.085	0.0001
Q8	1	-0.25158	0.14673	-1.715	0.0877
Q9	1	-0.14066	0.087217	-1.613	0.1081
Q11	1	0.143282	0.148951	0.962	0.3371
Q12	1	0.059189	0.082263	0.72	0.4725
Q13	1	-0.18904	0.07715	-2.45	0.015
Q14	1	-0.3144	0.075984	-4.138	0.0001
Q15	1	-0.46528	0.069595	-6.686	0.0001
Q16	1	-0.33741	0.079461	-4.246	0.0001
Q17	1	-0.40788	0.093078	-4.382	0.0001
DIST 5	1	-0.16586	0.044717	-3.709	0.0003
SPORTERL	1	-0.26691	0.042783	-6.239	0.0001
SPORTERC	1	-0.27709	0.057987	-4.778	0.0001
MATCH H-BAR	1	-0.28594	0.041454	-6.898	0.0001
TARGET	1	-0.30664	0.05565	-5.51	0.0001
FEATURE	1	0.1039	0.040315	2.577	0.0106
CAL762	1	-0.14924	0.092373	-1.616	0.1075

Turning to the quarterly indicator variables, the omitted period is quarter ten (July 1994 through September 1994). Most of the quarterly dummy variables have coefficients which are negative and significant, indicating that prices rose significantly at the time of the ban's implementation. Indeed, prices during the 1992–93 period were 41 to 79 percent lower than those at the time of the ban. The prices then began rising during 1994 and peaked during the quarter after the ban's implementation (however, prices during the latter period were not significantly different from those when the ban went into effect). These data reflect price increase of 69 to 100 percent over typical quarters during the 1992–93 period, and a 376 percent increase over the lowest price quarter during that period.

Quality-adjusted prices began to fall significantly during the second quarter of 1995. During the first two quarters of 1996, prices were 29 to 33 percent less than at the time of the ban.[24] These trends are illustrated in Figure 4-3.[25]

Figure 4-3. Quarterly price trends for Colt AR-15 and related rifles



Other Semiautomatic Rifles: A comparison price series was constructed for a small number of semiautomatic rifles not prohibited by the ban. The rifles selected for this analysis, the Ruger Mini-14 and Maadi rifles are arguably useful substitutes for the banned rifles for many purposes. The Mini-14 is a semiautomatic rifle which is relatively common among guns submitted to ATF for tracing.[26] The Maadi is an Egyptian semiautomatic rifle which is loosely patterned after the AK-47, but it is a legal gun, according to BATF experts.

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[24] Colt has discontinued its AR15 models, but the company has continued to make post-ban, modified versions of other weapons in the AR15 family (e.g., the Sporter). We considered the possibility that the AR15 model would follow a different pre/post ban trend from the other Colt models. Based on the number of available observations, we estimated a yearly model for the AR15. Yearly prices for the AR15 followed the same basic pattern as did the entire AR15 group. Relative to 1994, prices for the AR15 were 57 percent lower in 1993 ($p<.01$), 39 percent lower in 1995 ($p=.02$), and 37 percent lower in 1996 ($p=.06$). In addition, we estimated a model containing dummy variables for the AR15 and the post-ban period and an interaction term between these dummy variables (no other time period dummies were included in the model). The interaction term was very small and insignificant, leading us to include that the price differential between the AR15 model and the other Colt models remained constant throughout the period under study.

[25] Because some quarterly estimates were based on very small numbers of advertisements, the exact values of the quarterly coefficients should be treated cautiously. Nevertheless, a semi-annual model produced the same pattern of results.

[26] Based upon figures provided by ATF, the Mini-14 ranked as the 23rd most common firearm submitted to ATF for tracing in 1992 and the 36th most common firearm submitted in 1993. The Ruger Mini-14 was also featured as a common assault weapon in an early study of assault weapons published by *Cox Newspapers* (1989). However, the Crime Act specifically exempts Mini-14's without folding stocks from assault weapons status.

Further, the Maadi rifle has not been affected by import restrictions as have a number of other potential substitute rifles.

Table 4-4 and Figure 4-4 present trends for prices of these rifles (N=156) measured on a quarterly basis. The Ruger Mini-14 was significantly more expensive than was the Maadi, and a number of distributors had substantially lower or higher prices for these weapons. Guns having some sort of special feature or classification were somewhat less expensive than were other weapons.

Table 4-4. Regression of Ruger Mini-14 and Maadi rifle prices on time indicators, controlling for product characteristics and distributors

Analysis of Variance

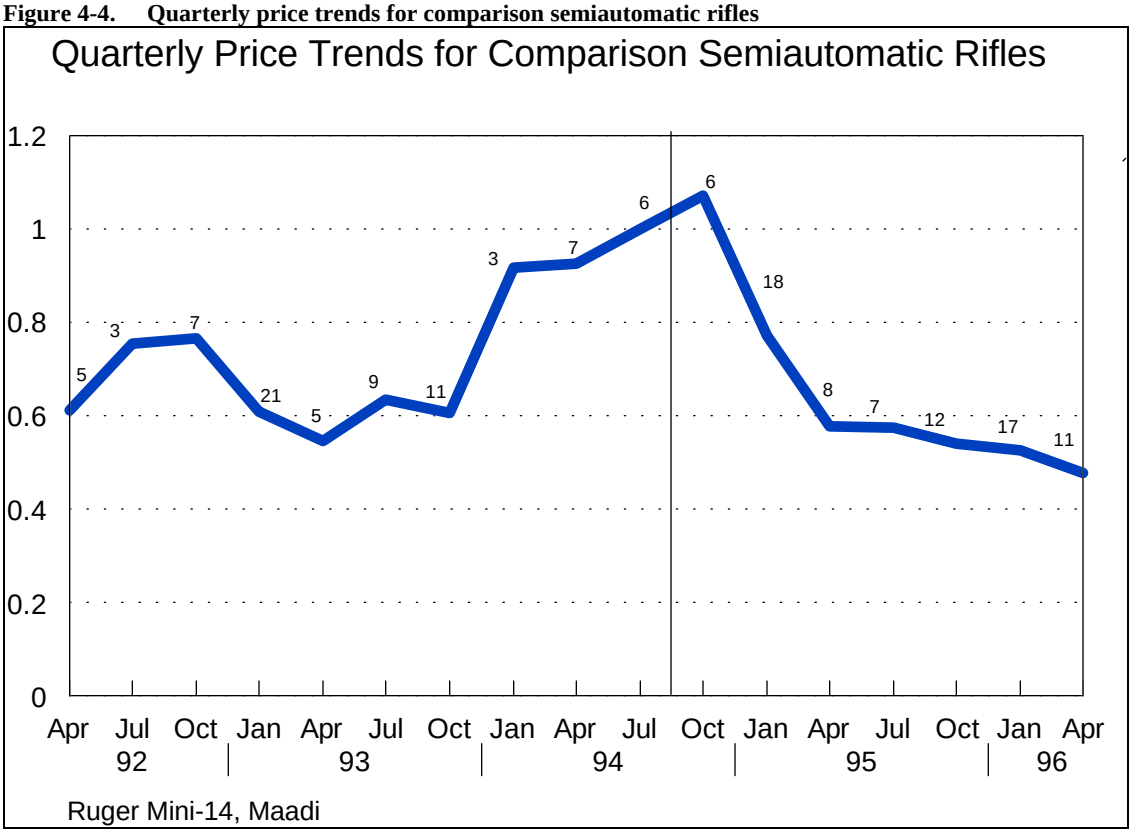
Source	*DF*	*Sum of squares*	*Mean square*	*F value*	*Prob>F*
Model	23	15.72251	0.68359	12.468	0.0001
Error	132	7.23741	0.05483		
C Total	155	22.95993			

Root MSE	0.23416	R–square	0.6848
Dep Mean	1.11132	Adj R–square	0.6299
C.V.	21.06999		

Parameter Estimates

Variable	*DF*	*Parameter estimate*	*Standard error*	*T for H0 parameter = 0*	*Prob>|T|*
INTERCEP	1	1.348039	0.096025	14.038	0.0001
Q1	1	-0.49339	0.150985	-3.268	0.0014
Q2	1	-0.28143	0.170394	-1.652	0.101
Q3	1	-0.26618	0.145198	-1.833	0.069
Q4	1	-0.49586	0.1189	-4.17	0.0001
Q5	1	-0.60429	0.149813	-4.034	0.0001
Q6	1	-0.45337	0.12651	-3.584	0.0005
Q7	1	-0.50108	0.123093	-4.071	0.0001
Q8	1	-0.08801	0.166538	-0.528	0.598
Q9	1	-0.07736	0.131103	-0.59	0.5561
Q11	1	0.06801	0.139693	0.487	0.6272
Q12	1	-0.26056	0.114103	-2.284	0.024
Q13	1	-0.55108	0.128193	-4.299	0.0001
Q14	1	-0.5565	0.137519	-4.047	0.0001
Q15	1	-0.61763	0.120067	-5.144	0.0001
Q16	1	-0.64124	0.119303	-5.375	0.0001
Q17	1	-0.73806	0.123765	-5.963	0.0001
RUGER	1	0.672197	0.055061	12.208	0.0001
DIST 2	1	-0.17779	0.079666	-2.232	0.0273
DIST 3	1	-0.08717	0.054575	-1.597	0.1126
DIST 4	1	-1.66399	0.242712	-6.856	0.0001
DIST 5	1	-0.19243	0.0727	-2.647	0.0091
DIST 7	1	0.235402	0.131826	1.786	0.0764
FEATURES	1	-0.08813	0.047131	-1.87	0.0637

Figure 4-4. Quarterly price trends for comparison semiautomatic rifles



The temporal price trends for these weapons mirror those found for the AR15 family rifles. Relative to the period of the ban's implementation, prices were significantly lower during periods before and after the ban's implementation. During 1992 and 1993, prices ranged from 23 to 45 percent lower than during the reference period. Prices were at their highest during 1994, with the peak occurring during the quarter following the ban's effective date, reflecting an increase of 82 percent from the 1992–93 low point to the immediate post-ban period. However, prices for the first, second, and fourth quarters of 1994 were not discernibly different from those during the third quarter. Prices began to fall significantly in 1995, and by the second quarter of 1996, prices were approximately 52 percent lower than during the quarter when the ban took effect.[27]

Alternative Comparison for Semiautomatic Rifles: As a final test of price trends for potential substitute semiautomatic rifles, we added the SKS rifle to the semiautomatic rifles model. The SKS rifle is imported (there are Russian and Chinese versions) and is occasionally mistaken for an AK-47. The SKS was not covered by either the 1989 import ban or the Crime Act. We initially excluded it as a comparison semiautomatic rifle because importation was nominally restricted in 1994 as part of U.S. trade sanctions directed against China. However, SKS rifles have continued to enter the U.S. under the Craig Amendment exemption for goods already "on the water" when the trade sanctions were imposed. We added it to subsequent analysis because it has been relatively

[27] Because some of the quarterly periods yielded few observations, we also estimated a semi-annual model for these gun prices. The results of this model paralleled those of the quarterly model; prices were at their highest during the latter half of 1994 and were significantly lower throughout 1992, 1993, 1995, and early 1996.

common among gun traces submitted to BATF[28] and because our coders found over 550 ads for SKS rifles, making that gun the most frequently advertised weapon in *Shotgun News* from among those guns chosen for the analysis.

Results from a quarterly price trend model for 698 SKS, Ruger Mini-14, and Maadi AK-type advertisements are presented in Table 4-5 and Figure 4-5. Again, the results indicate that prices were highest during 1994 and peaked during the quarter of the ban's implementation (quarter ten). Prices during the 1992–93 period were generally 32 to 25 percent less than they were during the quarter of the ban's implementation. Following the ban, however, prices fell rather quickly, and by 1996 they were approximately 35 percent less than they had been at the time of the ban.

[28] Figures provided to us by BATF show that the SKS was the 10th most common firearm traced in 1992 and the 4th most common in 1993.

Table 4-5. Regression of Ruger Mini-14, Maadi, and SKS rifle prices on time indicators, controlling for product characteristics and distributors

Analysis of Variance

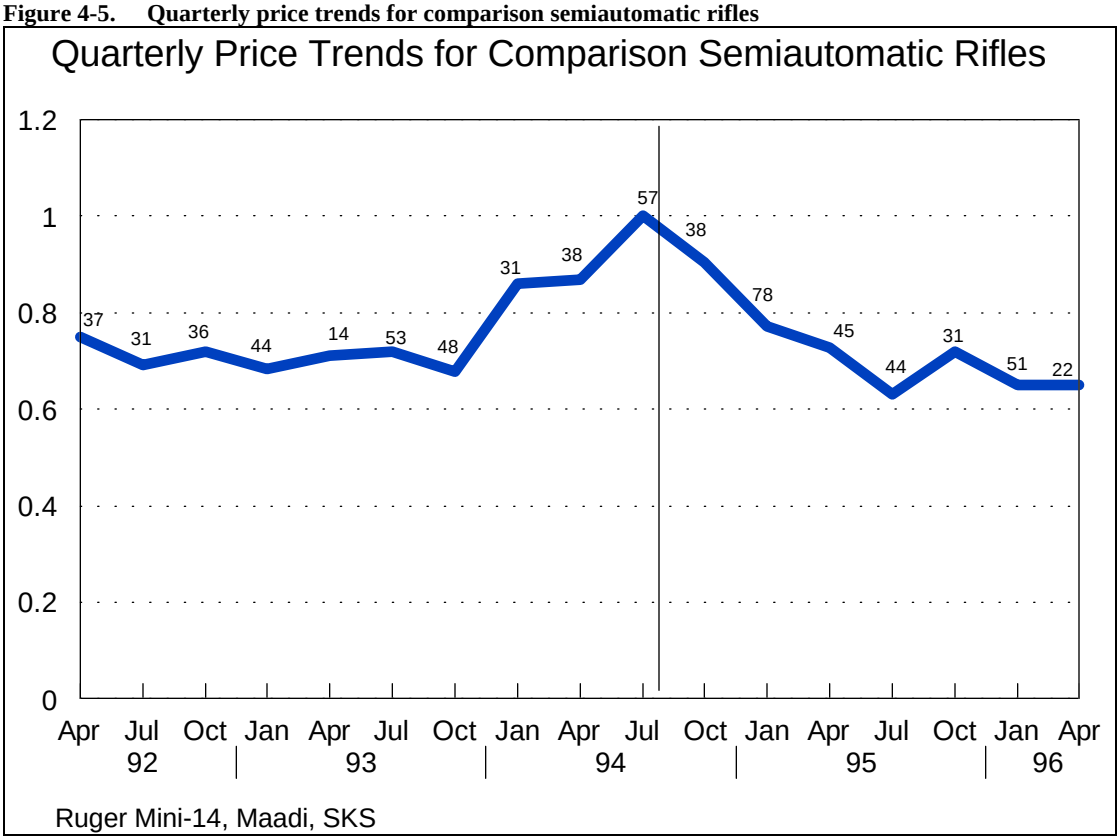
Source	*DF*	*Sum of squares*	*Mean square*	*F value*	*Prob>F*
Model	19	145.53206	7.65958	105.960	0.0001
Error	678	49.01094	0.07229		
C Total	697	194.54300			

Root MSE	0.26886	R–square	0.7481
Dep Mean	0.32139	Adj R–square	0.7410
C.V.	83.65546		

Parameter Estimates

Variable	*DF*	*Parameter estimate*	*Standard error*	*T for H0 parameter = 0*	*Prob>|T|*
INTERCEP	1	0.320571	0.037047	8.653	0.0001
Q1	1	-0.29288	0.056985	-5.14	0.0001
Q2	1	-0.36758	0.060234	-6.103	0.0001
Q3	1	-0.32732	0.057937	-5.65	0.0001
Q4	1	-0.37657	0.056037	-6.72	0.0001
Q5	1	-0.33581	0.08099	-4.146	0.0001
Q6	1	-0.32629	0.051373	-6.351	0.0001
Q7	1	-0.39266	0.052767	-7.441	0.0001
Q8	1	-0.15306	0.060298	-2.538	0.0114
Q9	1	-0.13647	0.056349	-2.422	0.0157
Q11	1	-0.09587	0.056591	-1.694	0.0907
Q12	1	-0.25553	0.047168	-5.417	0.0001
Q13	1	-0.32473	0.053753	-6.041	0.0001
Q14	1	-0.457	0.054492	-8.387	0.0001
Q15	1	-0.32702	0.06053	-5.403	0.0001
Q16	1	-0.43303	0.052708	-8.216	0.0001
Q17	1	-0.42588	0.068581	-6.21	0.0001
MAADI	1	0.855348	0.032324	26.462	0.0001
RUGER	1	1.363013	0.036904	36.934	0.0001
FEATURES	1	0.093431	0.02203	4.241	0.0001

Figure 4-5. Quarterly price trends for comparison semiautomatic rifles



4.1.3. Magazine Prices

Since the Crime Act permanently capped the stock of large-capacity magazines at the number produced before September 13, 1994, our long-run expectations about price trends for the banned magazines depend on whether or not the ban prevented increases in the supply of "compatible" guns that accept the magazine. For compatible guns whose supply continued to increase — such as the unbanned Ruger Mini-14 rifle and Glock pistols and the AR-15 family of rifles, for which legal substitutes emerged — we expect a gradual long-run increase in the price of the large-capacity magazines. Only for compatible guns such as Uzi models, whose supply was capped because legal substitutes did not emerge, do we expect stable or declining long-run magazine prices as the operational stock of banned guns gradually declines.

In the short run, which is all we can observe at this time, we expect at least three confounding factors to divert large-capacity magazine prices from these trends. First, as with the banned guns, speculative demand for the banned magazines may have caused prices to rise and then fall around the time of the ban. Second, because guns and magazines are economic complements, their prices may be likely to move in opposite directions. Third, for banned guns such as the AR-15 and Uzi models, which are mechanically identical to military weapons, there are military surplus supplies that we believe are huge relative to civilian demand. For these reasons, short-run price trends are a poor guide to long-run price trends for large-capacity magazines.

With these reservations in mind, we examined price trends for large-capacity magazines (i.e., magazines holding more than 10 rounds) manufactured for use with banned firearms and compared them to trends for large-capacity magazines made for unbanned semiautomatic weapons. Selection of firearm models was based on both theoretical relevance and available sample sizes. To improve the generalizeability of the results, we attempted to

analyze magazine prices for both handguns and long guns and for both banned and non-banned weapons. The methodology for the magazine price analysis was essentially the same as that used in the firearm price analysis.[29] As in the firearm price analysis, our quality control variables consisted primarily of indicator variables corresponding to manufacturers and distributors. An additional key variable for the magazine analysis was the number of rounds held by the magazine (logged).[30]

Assault weapon handgun magazines–Uzi: Our analysis of large-capacity magazines prices for assault weapons focused upon the 9mm Uzi handgun.[31] Though importation of the Uzi handgun had been discontinued in 1993 (Fjestad 1996, p.1049), our coders found ads for Uzi magazines (N=117) more frequently than for other assault weapon handguns.[32] Even so, the number of observations was as low as 1-2 for some quarterly periods, and we therefore grouped the data into semi-annual time periods. There is no legal substitute for the banned Uzis that accepts the same magazine.

Regression results for Uzi magazine prices are presented in Table 4-6 and price trends are displayed in Figure 4-6. Controlling for the number of rounds held by the magazine, semi-annual prices during the January 1992 through June 1994 period ranged from approximately 52 to 62 percent of their value during the latter half of 1994. Prices peaked in the first half of 1995, rising another 56 percent, to a tripling of their 1992–94 lowest prices. Prices began to fall in the latter half of 1995 and the first half of 1996, but they did not differ significantly from prices during the latter half of 1994.

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[29] Project staff recorded information on all advertisements for magazines holding more than 10 rounds which appeared in the selected issues of *Shotgun News*. However, the volume of collected data required us to pursue a data reduction strategy. Based on informal inspection of the hardcopy data, therefore, we chose a group of magazines which appeared relatively more frequently and which had relevance as a banned weapon or legal substitute.

[30] Other potentially important characteristics are whether the magazine was new or used and the type of metal from which the magazine was made. Ads often did not state whether magazines were new or used, and our research staff did not record this information. Our working assumption is that the magazines were new or in good working condition. If an ad featured the same magazine manufactured with different types of metals, we used the base price magazine. If the coding form indicated that the advertisement featured only magazines made from special materials (e.g., stainless steel), we made note of this characteristic. There were very few such cases, and preliminary analyses using an indicator variable for the presence of a special metal showed the variable to have no impact in any of the models discussed in the main text.

[31] The Uzi was previously manufactured and imported to the U.S. in both carbine and handgun versions, but the carbine versions were banned from importation in 1989.

[32] The relative frequency of Uzi magazine advertisements is probably due to the fact that the Uzi is a military weapon. Firearms experts have informed us that good quality, military surplus magazines are commonly available and are often sold cheaply.

Table 4-6. Regression of Uzi large-capacity magazine prices on time indicators, controlling for product characteristics and distributors

Analysis of Variance

Source	*DF*	*Sum of squares*	*Mean square*	*F value*	*Prob>F*
Model	9	12.80484	1.42276	9.670	0.0001
Error	107	15.74298	0.14713		
C Total	116	28.54782			

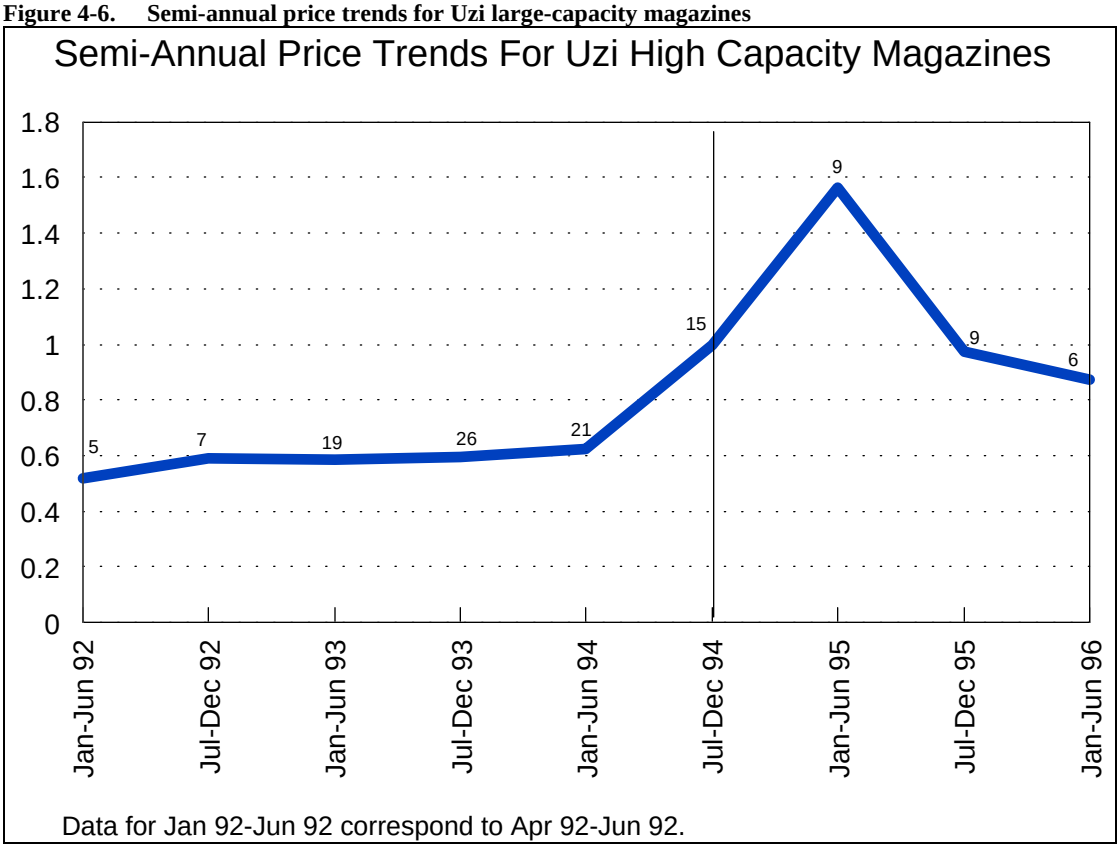
Root MSE	0.38358	R–square	0.4485
Dep Mean	-1.65739	Adj R–square	0.4022
C.V.	-23.14337		

Parameter Estimates

Variable	*DF*	*Parameter estimate*	*Standard error*	*T for H0 parameter = 0*	*Prob>|T|*
INTERCEP	1	-3.835055	0.54716949	-7.009	0.0001
ROUNDS	1	0.729783	0.15350538	4.754	0.0001
T1	1	-0.661263	0.19914123	-3.321	0.0012
T2	1	-0.525479	0.17560540	-2.992	0.0034
T3	1	-0.536934	0.13325422	-4.029	0.0001
T4	1	-0.515880	0.12659037	-4.075	0.0001
T5	1	-0.474834	0.12970256	-3.661	0.0004
T7	1	0.447430	0.16646042	2.688	0.0083
T8	1	-0.027967	0.16286070	-0.172	0.8640
T9	1	-0.137577	0.18908164	-0.728	0.4684

Exhibit 6
0206

Figure 4-6. Semi-annual price trends for Uzi large-capacity magazines



Other Handgun Magazines: To provide price trends for large-capacity magazines manufactured for non-banned handguns, we examined large-capacity magazines for Glock 9mm handguns. Prior to the Crime Act, Glock sold several handgun models with large-capacity magazines. The most common, the Glock 17, was among the ten firearm models submitted most frequently to ATF for tracing in 1994 (BATF 1995a). Guns currently manufactured by Glock are capable of accepting Glock's pre-ban large-capacity magazines, but the supply is limited to magazines made before the ban.

Project staff found 74 advertisements for Glock magazines, but the large majority of these ads were placed after the ban (only nine ads were pre-ban) and there were no ads for 1992. It was therefore necessary to group the advertisements into yearly periods rather than quarterly or semi-annual periods. Regression results and price trends for 1993 through 1996 are shown in Table 4-7 and Figure 4-7 respectively. In general, magazines with greater numbers of rounds were more expensive. In addition, a number of distributors had higher prices for these magazines, and magazines for one particular model were more expensive at a moderate level of statistical significance.[33]

[33] For the model dummy variables, the excluded category included magazines for which no model was indicated.

Table 4-7. Regression of Glock large-capacity handgun magazine prices on time indicators, controlling for product characteristics and distributors

Analysis of Variance

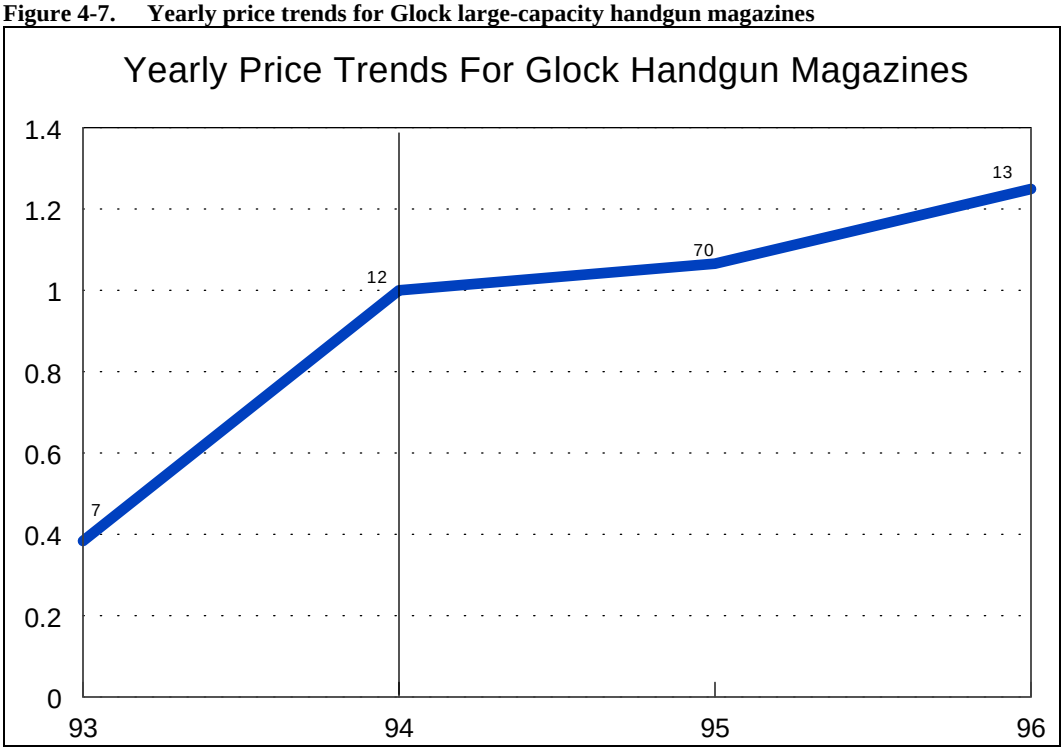
Source	*DF*	*Sum of squares*	*Mean square*	*F value*	*Prob>F*
Model	10	29.85755	2.98575	28.020	0.0001
Error	91	9.69680	0.10656		
C Total	101	39.55434			

Root MSE	0.32643	R–square	0.7548
Dep Mean	-0.86656	Adj R–square	0.7279
C.V.	-37.66991		

Parameter Estimates

Variable	*DF*	*Parameter estimate*	*Standard error*	*T for H0 parameter = 0*	*Prob>|T|*
INTERCEP	1	-3.37422	0.56384	-5.984	0.0001
ROUNDS	1	0.618327	0.197724	3.127	0.0024
Y93	1	-0.95884	0.17246	-5.56	0.0001
Y95	1	0.064606	0.108817	0.594	0.5542
Y96	1	0.2227	0.143595	1.551	0.1244
DIST 10	1	0.529244	0.279526	1.893	0.0615
DIST 12	1	0.601322	0.162505	3.7	0.0004
DIST 3	1	0.37606	0.17071	2.203	0.0301
DIST 5	1	0.980483	0.101626	9.648	0.0001
M17	1	0.198804	0.108878	1.826	0.0711
M19	1	0.169323	0.112614	1.504	0.1362

Figure 4-7. Yearly price trends for Glock large-capacity handgun magazines



Most importantly, prices for large-capacity Glock magazines were 62 percent lower in 1993 than they were in 1994. Prices remained high through 1995, and they increased another 25 percent in 1996 (relative to 1994), though this increase was not statistically significant by conventional standards.

Assault rifle magazines — AR15 Family: Pre-ban large-capacity magazines manufactured by Colt for their AR15's and related rifles can be utilized with the post-ban, modified versions of these rifles. Consequently, we expected that there would be a continuing demand for these magazines.

Project staff recorded 364 ads for large-capacity magazines (.223 caliber) made to fit the AR15 and related rifles. Results from our analysis of quarterly price trends for these magazines are shown in Table 4-8 and Figure 4-8. Magazines having larger ammunition capacities were more expensive as were those magazines for which Colt was listed explicitly as the manufacturer.[34] In addition, prices tended to differ significantly between distributors.

During the quarters of 1992 and 1993, prices were anywhere from 33 to 56 percent lower than during the third quarter of 1994. Prices rose further during the last quarter of 1994 and remained high through the first three quarters of 1995. In the last quarter of 1995 and the first quarter of 1996, prices fell though they remained higher than their pre-ban levels. Prices then rebounded in the second quarter of 1996, reaching a peak value comparable to the last quarter of 1995 (prices were approximately 29 percent higher than during the quarter when the ban took effect). Gun market experts have suggested to us that these short-run fluctuations reflect intermittent availability of military surplus M-16 magazines, which are compatible with the AR-15 family of rifles.

[34] Though firearms usually require magazines made by the same manufacturer, a number of manufacturers other than Colt make magazines which can fit Colt rifles.

Table 4-8. Regression of Colt AR15 group large-capacity magazine prices on time indicators, controlling for product characteristics and distributors

Analysis of Variance

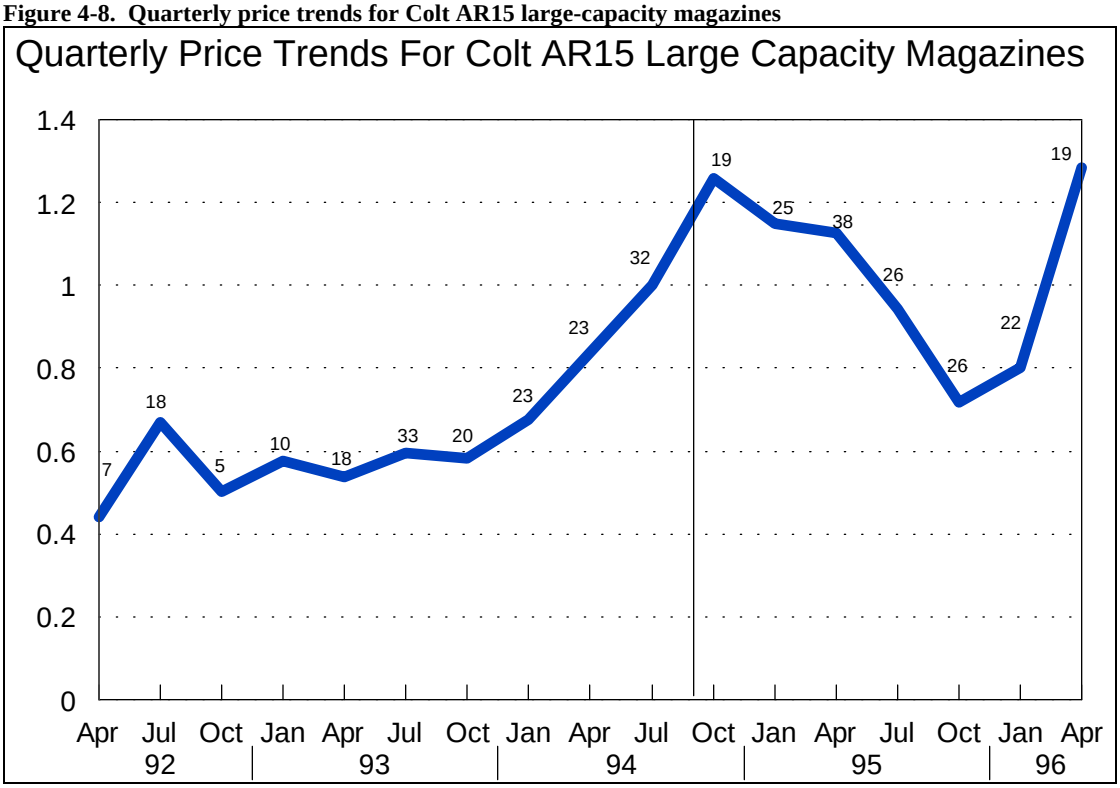
Source	*DF*	*Sum of squares*	*Mean square*	*F value*	*Prob>F*
Model	26	122.28012	4.70308	33.836	0.0001
Error	337	46.84153	0.13900		
C Total	363	169.12165			

Root MSE	0.37282	R–square	0.7230
Dep Mean	-1.65183	Adj R–square	0.7017
C.V.	-22.57021		

Parameter Estimates

Variable	*DF*	*Parameter estimate*	*Standard error*	*T for H0 parameter = 0*	*Prob>|T|*
INTERCEP	1	-5.34744	0.194896	-27.437	0.0001
ROUNDS	1	1.025757	0.046243	22.182	0.0001
CLT	1	0.184123	0.063507	2.899	0.004
DIST 2	1	0.385288	0.283893	1.357	0.1756
DIST 3	1	0.10778	0.078807	1.368	0.1723
DIST 4	1	-0.40188	0.129797	-3.096	0.0021
DIST 5	1	0.134623	0.068759	1.958	0.0511
DIST 7	1	-0.41214	0.13435	-3.068	0.0023
DIST 10	1	0.137861	0.080196	1.719	0.0865
DIST 11	1	-0.36298	0.168942	-2.149	0.0324
DIST 12	1	0.215247	0.085722	2.511	0.0125
Q1	1	-0.82099	0.158248	-5.188	0.0001
Q2	1	-0.39767	0.115668	-3.438	0.0007
Q3	1	-0.68998	0.181038	-3.811	0.0002
Q4	1	-0.55199	0.137727	-4.008	0.0001
Q5	1	-0.61893	0.115858	-5.342	0.0001
Q6	1	-0.52304	0.093025	-5.623	0.0001
Q7	1	-0.54396	0.107619	-5.055	0.0001
Q8	1	-0.38921	0.102709	-3.789	0.0002
Q9	1	-0.17713	0.104247	-1.699	0.0902
Q11	1	0.229259	0.11575	1.981	0.0484
Q12	1	0.13716	0.107928	1.271	0.2047
Q13	1	0.115077	0.099774	1.153	0.2496
Q14	1	-0.05869	0.106556	-0.551	0.5821
Q15	1	-0.32639	0.107409	-3.039	0.0026
Q16	1	-0.21758	0.109759	-1.982	0.0482
Q17	1	0.252132	0.117683	2.142	0.0329

Figure 4-8. Quarterly price trends for Colt AR15 large-capacity magazines



Comparison Semiautomatic Rifle Magazines — Ruger Mini-14: Quarterly price regression results for large-capacity magazines made for the Ruger Mini-14 rifle are shown in Table 4-9. Magazines with the Ruger name and larger magazines were more expensive than other magazines.[35] Further, prices differed significantly among distributors.

[35] A number of manufacturers besides Ruger made large-capacity magazines to fit the Mini-14.

Table 4-9. Regression of Ruger Mini-14 large-capacity magazine prices on time indicators, controlling for product characteristics and distributors

Analysis of Variance

Source	*DF*	*Sum of squares*	*Mean square*	*F value*	*Prob>F*
Model	26	64.39474	2.4672	34.029	0.0001
Error	303	22.05342	0.07278		
C Total	329	86.44816			

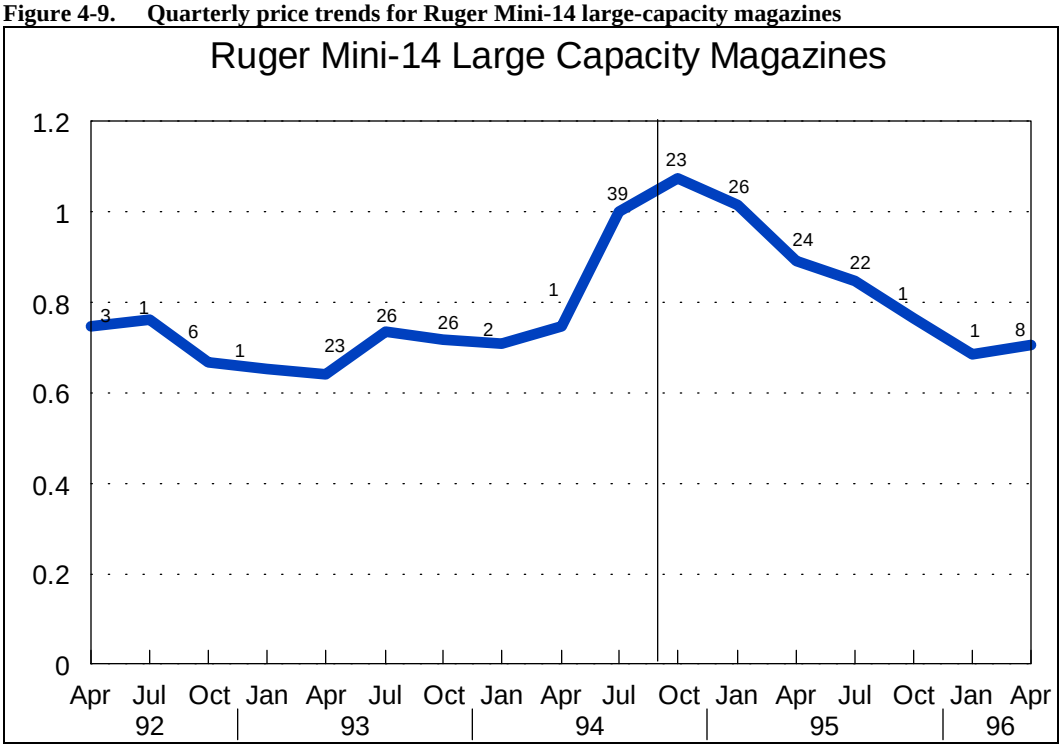
Root MSE	0.26978	R–square	0.7449
Dep Mean	-1.72827	Adj R–square	0.7230
C.V.	-15.61009		

Parameter Estimates

Variable	*DF*	*Parameter estimate*	*Standard error*	*T for H0 parameter = 0*	*Prob>|T|*
INTERCEP	1	-4.41607	0.145547	-30.341	0.0001
ROUNDS	1	0.836435	0.036639	22.829	0.0001
RUG	1	0.264903	0.061061	4.338	0.0001
DIST 2	1	-0.3889	0.17264	-2.253	0.025
DIST 3	1	-0.13012	0.072105	-1.805	0.0721
DIST 4	1	-0.57328	0.126483	-4.532	0.0001
DIST 5	1	-0.40885	0.066235	-6.173	0.0001
DIST 7	1	-0.5319	0.278193	-1.912	0.0568
DIST 10	1	-0.26988	0.074589	-3.618	0.0003
DIST 11	1	-0.1793	0.164002	-1.093	0.2751
DIST 12	1	0.324892	0.094116	3.452	0.0006
Q1	1	-0.29169	0.178205	-1.637	0.1027
Q2	1	-0.27167	0.08733	-3.111	0.002
Q3	1	-0.40486	0.122507	-3.305	0.0011
Q4	1	-0.425	0.082811	-5.132	0.0001
Q5	1	-0.44577	0.073027	-6.104	0.0001
Q6	1	-0.30726	0.070368	-4.366	0.0001
Q7	1	-0.33086	0.069189	-4.782	0.0001
Q8	1	-0.34428	0.074365	-4.63	0.0001
Q9	1	-0.29213	0.078927	-3.701	0.0003
Q11	1	0.071176	0.074263	0.958	0.3386
Q12	1	0.013922	0.07447	0.187	0.8518
Q13	1	-0.11436	0.073432	-1.557	0.1204
Q14	1	-0.1658	0.075341	-2.201	0.0285
Q15	1	-0.26924	0.081055	-3.322	0.001
Q16	1	-0.37783	0.084169	-4.489	0.0001
Q17	1	-0.34628	0.111216	-3.114	0.002

The quarterly indicators in Table 4-9 and the graphic illustration in Figure 4-9 show that quarterly prices prior to the ban were 64 to 76 percent of their level at the time of the ban. By late 1995, prices of these magazines were falling significantly, and by 1996 they had fallen to levels comparable to pre-ban prices.

Figure 4-9. Quarterly price trends for Ruger Mini-14 large-capacity magazines



4.1.4. Summary of Large-Capacity Magazine Price Trends

In summary, short-run price trends for four examples of banned large-capacity magazines appeared to depend on the legal status of the guns they fit, speculative demand for the guns and magazines, and the availability of military surplus magazines. All four magazine prices rose substantially during the period of debate over the ban, reflecting anticipatory demand. However, their price trends diverged substantially after that point. For a banned assault pistol (the 9mm Uzi) for which no legal substitute emerged, the post-ban magazine price fell to a level between its peak and its pre-speculation level and remained there. For a banned rifle (Colt AR-15) for which legal substitutes emerged and the gun price fell sharply after the ban, post-ban magazine prices fluctuated dramatically, apparently because of variations in the availability of military surplus M-16 magazines. For unbanned Glock pistols, whose supply continued to grow, the post-ban magazine price continued to rise throughout the post-ban period, though at a slower rate than during the pre-ban speculation; this is consistent with the expected long-term price trend. Finally, prices for large-capacity Ruger Mini-14 magazines appear to have followed speculative trends similar to those for the rifles themselves.

4.2. PRODUCTION TRENDS

Analyses reported in Section 4.1 found substantial pre-ban price increases for two major categories of assault weapons that were examined: SWD and related handguns (+47 percent), the AR-15 assault rifle family (+69 percent to +100 percent, at minimum). A comparison group of unbanned semiautomatic rifles including the domestically produced Ruger Mini-14 showed a pre-ban price increase of 82 percent. But strikingly, a comparison group of inexpensive Davis and Lorcin semiautomatic handguns showed no discernible price change during the 4-year period that included the effective date of the ban.

In the introduction to this chapter, we hypothesized that weapons whose prices increased during the pre-ban period would also show increases in production. To test that hypothesis, we were able to obtain annual

production data from the Violence Policy Center for three of the four weapon categories above: the SWD, AR-15, and Davis/Lorcin groups.[36] The data extend through 1994, the year of the ban and the last year for which production data are available.

The production data for these three groups are shown in Figure 4-10, Figure 4-11, and Figure 4-12, and they strongly support the hypothesis that pre-ban price speculation was associated with increases in production. As shown there, the SWD and AR-15 groups show substantial increases in production in 1993 and 1994, the years when prices were increasing in advance of the ban. Production increases of similar magnitude appear for two other categories of banned assault weapons that could not be included in the price analysis: the Intratec/AA Arms group, and Calico and Feather Industries rifles, which are banned by the features test.[37] In contrast, the Davis/Lorcin handgun group showed decreased production relative to both 1993 and their 1989–93 average.

Table 4-10 summarizes production data for five typical groups of banned assault weapons and the Lorcin/Davis comparison group of small-caliber semiautomatic pistols. For each weapon type, the table reports 1994 production, average 1989–93 production, and the ratio of 1994 production to the average over the period. On average, 1994 assault weapon production exceeded the 1989–93 average by a ratio of 2.233 during the nine months before the ban took effect. In contrast, 1994 production for the Lorcin/Davis comparison group was only 65.2 percent of the 1989–93 average.

Table 4-10. Production trends for banned assault weapons and comparison guns

Firearm type	*(1)* *1994 production*	*(2)* *1989–93 average production*	*(3)* *Ratio [(1)/(2)]*	*(4)* *"Excess" production [(1)-(2)]*
AR-15 group	66,042	38,511	1.714	27,531
Intratec 9mm, 22	102,682	33,578	3.058	69,104
SWD family (all) & MAC (all)	14,380	10,508	1.368	3,872
AA Arms	17,280	6,561	2.633	10,719
Calico 9mm, 22	3,194	1,979	1.613	1,215
Lorcin, Davis	184,139	282,603	0.652	
Assault Weapon Total*	203,578	91,137	2.233	112,441

*Assault weapon total excludes Lorcin/Davis group

Table 4-10 also displays "excess" production, the difference between 1994 production and 1989–93 average production. Excess 1994 production for the five assault weapon types shown in the table was approximately 112,000, which were added to the stock of grandfathered assault weapons eligible for resale after the ban took effect.

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[36] BATF production data for rifles are not disaggregated by model or caliber. While we could be confident that nearly all Colt's rifles belong to the AR-15 family and could therefore use Colt's rifle production data as an index of AR-15 production, Sturm, Ruger produces too many rifles besides the Mini-14 for us to have a reliable index of Mini-14 production.

[37] It may be of interest that the Intratec, SWD, and Calico/Feather groups, but not the AR-15 group, also had production peaks in 1989, the year of the assault weapon import ban.

Figure 4-10. Annual production data, Colt and Olympic Arms AR-15 type (years with complete data only)

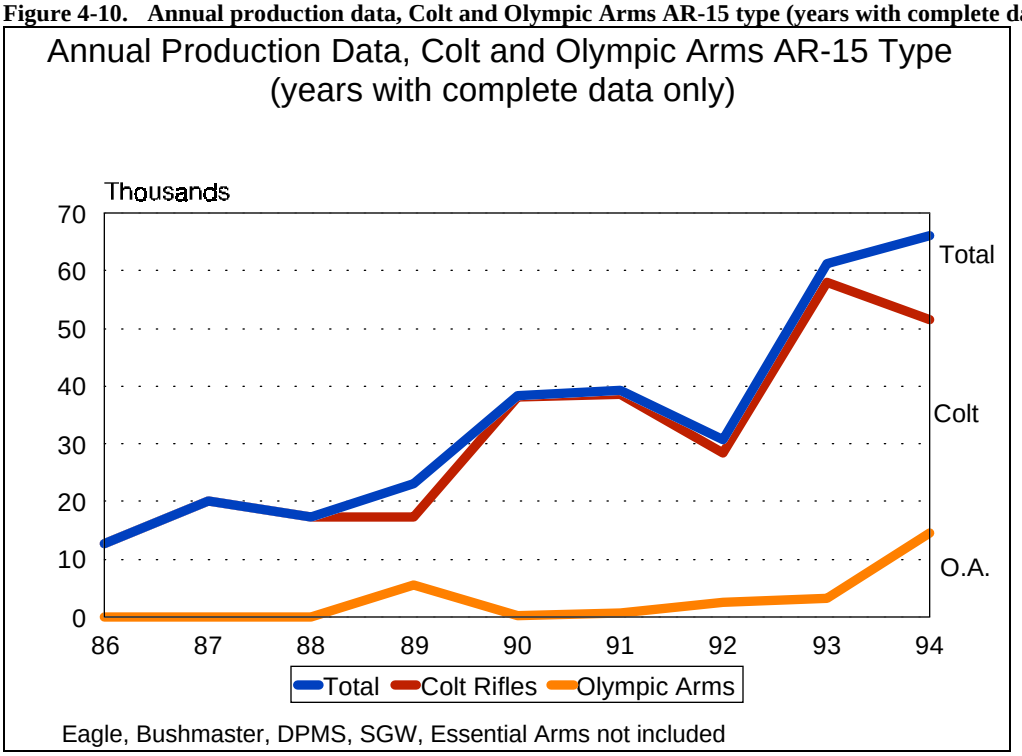


Figure 4-11. Annual production data, SWD group (missing data in some early years)

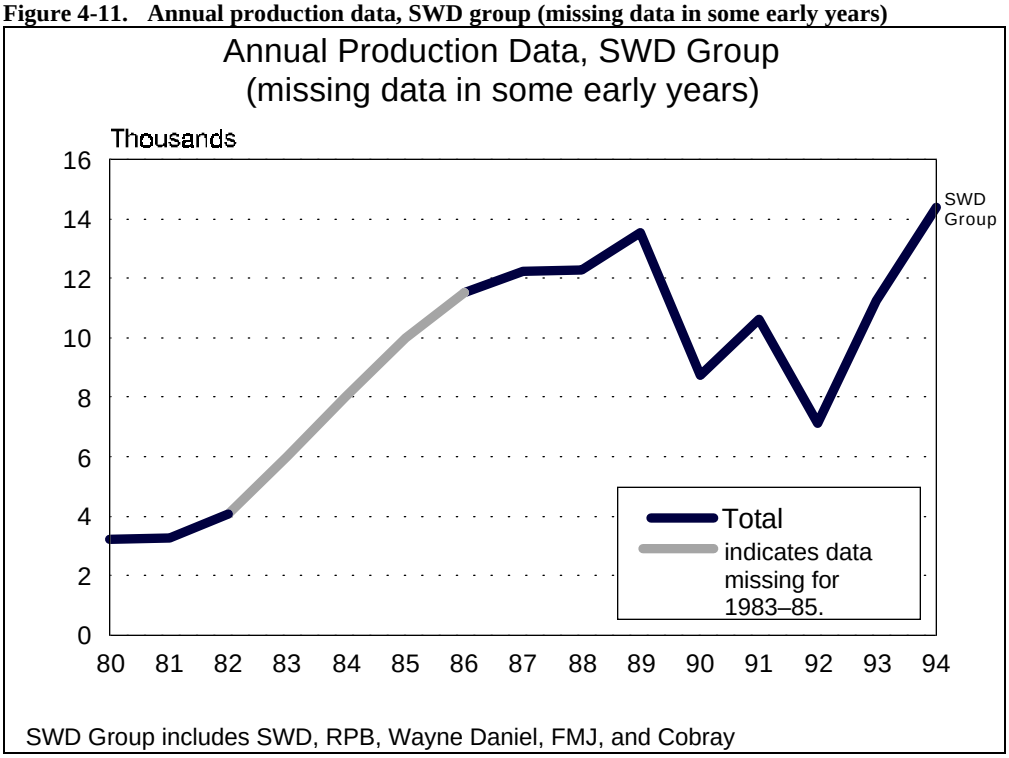
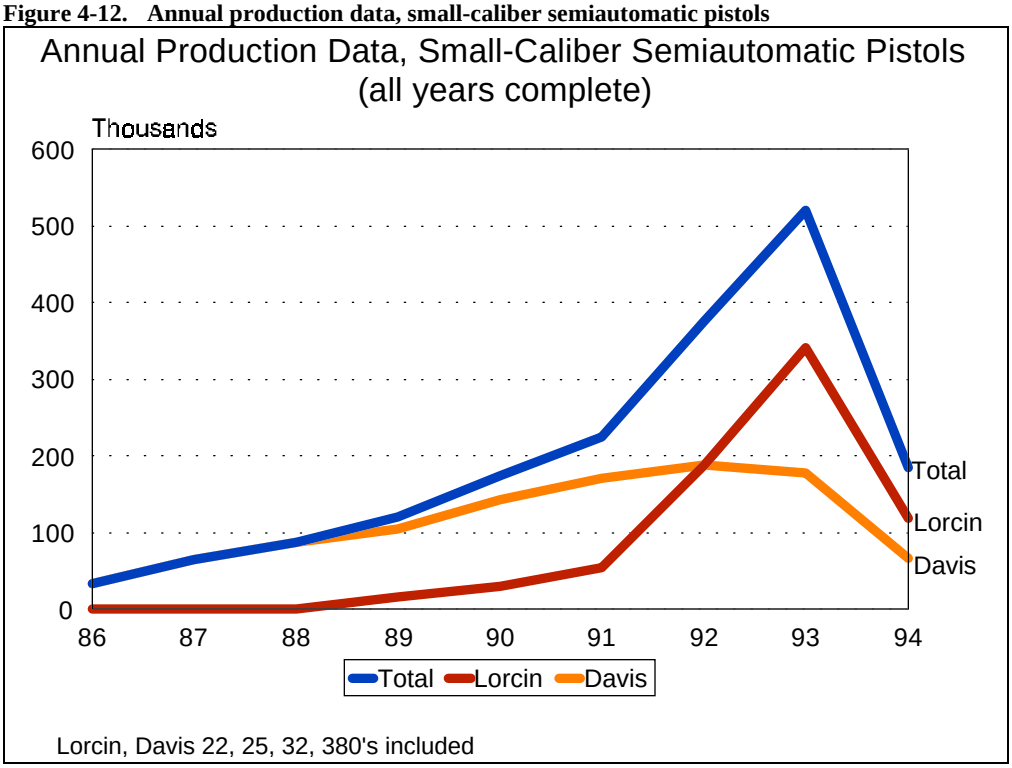


Figure 4-12. Annual production data, small-caliber semiautomatic pistols



4.3. UNINTENDED CONSEQUENCES: GUN THEFTS AND "LEAKAGE"

4.3.1. Introduction

As a final consideration of the ban's impact on gun markets, we investigated trends in stolen firearms. Given the boom in production of the banned weapons prior to the assault weapon ban, there would appear to be a substantial stockpile of banned weapons, some of which may "leak" from gun dealers and carriers into the hands of criminals and other violence-prone individuals after the ban through a combination of recorded transfers, unrecorded transfers, and thefts.

Indeed, we hypothesized that the Crime Act might have the unintended consequence of increasing reported thefts of the banned weapons for two reasons. Short-term price increases in primary markets might temporarily keep assault weapons from entering the sales distribution channels to criminals, who might be tempted to steal them instead. In addition, dealers who had paid high speculative prices for grandfathered assault weapons around the time of the of the ban but then suffered the post-ban price decline prices might be encouraged to sell their to ineligible purchases and then report the weapons as stolen to BATF, who in turn would enter them into the Federal Bureau of Investigation's national database on stolen firearms. Our tests of these hypotheses had to recognize that any observed rise in assault weapon thefts could be due, at least in part, to new theft reporting requirements established for firearm dealers by Subtitle C of Title XI. In the sections below, we describe the tests and findings.

4.3.2. Data and Analysis Strategy

Since 1967, the Federal Bureau of Investigation has stored law enforcement agency reports of stolen and recovered guns in a database maintained by the National Crime Information Center (NCIC). This database contains records on guns which have been reported stolen to participating agencies. It also includes a relatively small number of guns which have been recovered by law enforcement agencies but which have not been reported stolen to the FBI. The latter category of guns accounts for about 6 percent of the guns in the database, and we removed them from our analysis. Weapons which are stolen and later recovered are removed from the database by the NCIC. Thus, the file contains only guns which have been stolen and not recovered. Among other items, the database contains entries for the following: the date the gun was reported stolen ; the weapon type, make, model, caliber, and serial number of the gun; and the agency to which the weapon owner reported the theft.

For our analysis, we utilized data on guns stolen between January 1992 and May 1996. Our analysis of assault weapon thefts focused upon our select group of domestic assault weapons. Unfortunately, weapon model is missing for the majority of the records in the file. Therefore we used the following operational definitions to approximate thefts of assault weapons and other guns:[38]

1) Colt AR15 group: all .223 caliber firearms made by Colt, Eagle, Olympic/SGW, Essential Arms, Bushmaster, and Sendra.

2) Intratec group: all 9mm and .22 caliber semiautomatic weapons made by Intratec and all 9mm semiautomatic handguns made by AA Arms.

3) SWD group: all 9mm, .380, and .45 caliber semiautomatic weapons made by SWD, Ingram, Military Armaments Corp., and RPB Industries.

4) Features test group: all semiautomatic handguns and rifles made by Calico and all 9mm and .22 caliber semiautomatic rifles made by Feather.

5) Non-banned large-capacity handguns: Based on the relative frequency of the Glock 17 and Ruger P89 among guns traced by BATF (see Chapter 2), we used Glock and Ruger 9mm semiautomatic handguns to operationalize this count.

4.3.3. Trends in Stolen Assault Weapons

Statistics in Table 4-11 show that the number of assault weapons reported stolen per month was higher during the post-ban period than during the pre-ban period. These figures combine all of the assault weapons in our select group. As is shown in

[38] We arrived at these operational definitions by examining the varieties of gun types, makes, models, and calibers contained in the *Blue Book of Gun Values* (Fjestad 1996). The largest approximation error is probably that Group 2 includes the Protect .22, which is not banned and does not accept large-capacity magazines.

Figure 4-13, this post-ban increase continued an upward trend which began before the assault weapon ban. Interpreting the raw numbers of assault weapons thefts is problematic even with time series methods, however, because the Subtitle C theft reporting requirement for FFL's may have caused an artificial increase in reported thefts. The monthly average of total reported gun thefts did increase from approximately 11,602 for the January 1992 through August 1994 period to 12,806 during the September 1994 through May 1996 period, although we did not make systematic attempts to explain the increase.

Table 4-11. Pre-ban (Jan. 1992-Aug. 1994) to post-ban (Sept. 1994-May 1996) changes in counts of stolen assault weapons and unbanned semiautomatic handguns capable of accepting large-capacity magazines

Stolen gun type	*Pre-ban monthly mean*	*Post-ban monthly mean*
Assault weapons	2,334	2,642
Unbanned large-capacity semiautomatic handguns	235	343

Table 4-12. Pre-ban (Jan. 1992-Aug. 1994) to post-ban (Sept. 1994-May 1996) changes in ratios of stolen assault weapons and unbanned semiautomatic handguns capable of accepting large-capacity magazines

	Pre-ban	*Post-ban*	*Change*
Ratio: Assault weapons ÷ automatic and semiautomatic guns	.449	.463	+3%
Ratio: Unbanned large-capacity semiautomatic handguns ÷ All semiautomatic handguns	.054	.073	+35%

To control for possible confounding effects of the Subtitle C reporting requirement, we examined assault weapon thefts as a proportion of all reported thefts of semiautomatic and automatic weapons. A post-ban increase in this proportion would suggest a rise in assault weapon thefts which occurred independently of any Subtitle C effect. We used semiautomatic and automatic weapons as our baseline rather than all reported thefts in order to control for changes in the composition of the gun stock; semiautomatic firearms, of which assault weapons are a subset, have grown dramatically since the late 1980s as a share of the firearms market. Relatedly, some law enforcement personnel have suggested to us that gun theft victims are more likely to report thefts of recently purchased firearms because it is easier for victims to assemble information necessary for a theft report (such as serial numbers) when dealing with a newer firearm. Finally, expressing assault weapons as a proportion of semiautomatic/automatic weaponry may correct potential bias stemming from the NCIC's removal of recovered weapons from their data system. Some evidence suggests that semiautomatic handguns tend to move more quickly from retail sale to crime than do other firearms (Kennedy et al. 1996). If this process works the same way for the time from theft to use in crime and recovery by police, then assault weapons and other semiautomatic firearms may tend to drop out of the system at a faster rate than other firearms.

Figures in Table 4-12 reveal that between 1992 and 1996 automatic and semiautomatic assault weapon thefts increased only very slightly (about 3%) as a proportion of thefts of rapid fire weapons. A contingency table chi-square test indicated that this was a statistically significant increase ($p<.01$).[39] However, an interrupted time series analysis of monthly trends (see Figure 4-14) failed to provide any strong evidence that the ban caused a change in the proportion of semiautomatic/automatic firearm thefts involving assault weapons.[40] Either way, the relative Increase in assault weapon thefts appears to have been very modest.

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[39] The proportion of semiautomatic/automatic gun thefts accounted for by assault weapons is strikingly large in light of the generally low prevalence of these guns among confiscated and traced weapons. Due to the manner in which we approximated assault weapon thefts, our figures probably overstate assault weapon thefts to some degree. In addition, BATF agents have suggested to us that assault weapon thefts may be more likely to be reported to NCIC than thefts of other firearms due to owners' insurance claims on assault weapons and owners' concerns about how stolen assault weapons may be used.

Errors in the data submitted by law enforcement agencies may also be relevant. The NCIC uses character and numeric codes to identify manufacturers, weapon types, and calibers. To assess coding error in the data, we ran a number of crude reliability tests with guns made by selected manufacturers. To illustrate, if a particular handgun manufacturer makes only semiautomatic handguns, one can examine all guns made by that company which appear in the database and determine what percentage were coded as weapon types other than semiautomatic handguns. If 5% of the guns produced by this manufacturer have other weapon type codes, then the manufacturer and/or weapon type must be incorrect for that 5% of cases.

We chose guns made by Davis Industries and Intratec for our tests. Davis Industries makes only derringers and semiautomatic pistols (Fjestad 1996, pp.412-413). Davis derringers are made in .22, .25, .32, .38, and 9mm calibers. The company's semiautomatic pistols are produced in calibers .32 and .380. Of the several thousand guns in the data coded as Davis Industries firearms, about 10% were coded as weapon types other than derringers or semiautomatic handguns (most of these were coded as revolvers). Virtually 100% of the Davis Industries derringers had calibers in the proper range, as did 95% of the semiautomatic handguns.

Intratec, a prominent maker of assault weapons, makes derringers in .38 caliber and produces semiautomatic handguns in .22, .25, .380, .40, .45, and 9mm calibers (Fjestad 1996, pp.577-579). Approximately 89% of the several thousand guns coded as Intratecs were coded as semiautomatic handguns or derringers. Nearly 100% of the Intratec semiautomatic handguns had caliber codes in the proper range, while 97% of the derringers had the proper caliber.

In light of the various coding errors which are present in the NCIC data, we constructed our counts of assault weapons and semiautomatic/automatic guns using a broad array of weapon type codes corresponding to various semiautomatic and fully automatic weapon types. The analyses described above seem to indicate that errors in the numerator and denominator of our assault weapon measure are roughly proportional. Finally, our analysis assumes that any biases in the data resulting from the various issues discussed above have remained relatively constant from the pre-ban to post-ban periods.

[40] Due to ambiguity regarding the form of the ban's hypothesized impact on assault weapon thefts, we tested a number of impact models (see McCleary and Hay 1980). The temporary increase in assault weapon prices which occurred around the time of the ban may have raised the incentive for criminals to steal assault weapons, thereby creating an abrupt, temporary impact on thefts of assault weapons. However, an abrupt temporary impact was inconsistent with the data.

The eventual fall in assault weapon prices, on the other hand, could have increased the incentive for dealers to "leak" the guns to illegitimate buyers. The gradual decline of assault weapon prices documented in the price analysis would suggest a gradual, permanent impact on assault weapon thefts. However, an abrupt, permanent impact also seems plausible. Further, abrupt, permanent impact models are less demanding on the data and sometimes provide a better fit and more accurate results even when the true form of the impact is not of this type (see McDowall et al. 1996). In this case, a gradual, permanent impact model yielded insignificant results and provided a worse fit to the data than did an abrupt, permanent impact model.

Assessment of the abrupt, permanent impact model was complicated by the presence of an outlier observation corresponding to March 1993, during which time there was an unusually low proportion of thefts involving assault weapons (see Figure 4-14). We therefore estimated models with and without this observation. In the first model, we retained the outlier observation and logged the data series. This model suggested that the ban produced a moderately significant ($p<.10$) positive impact on the proportion of semiautomatic/automatic gun thefts that involved assault weapons. (After adding the intervention component, this model did not require any autoregressive or moving average parameters for the noise component). When the outlier observation was removed, however, the model failed to yield evidence of an impact from the ban. (The noise

component for this model included a fourth order autoregressive subset model [see SAS Institute 1993] in which all parameters except the fourth were set to zero).

Figure 4-13. Stolen assault weapons count, January 1992–May 1996

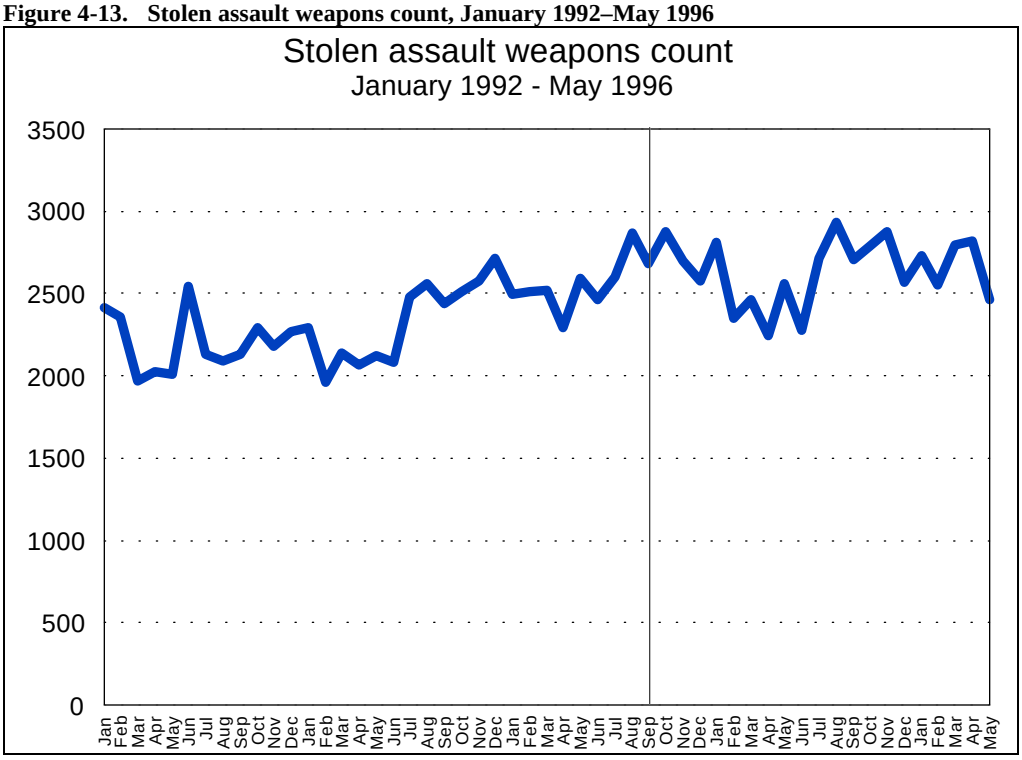

Figure 4-14. Assault weapons as a proportion of stolen semiautomatic and automatic guns, January 1992–June 1996

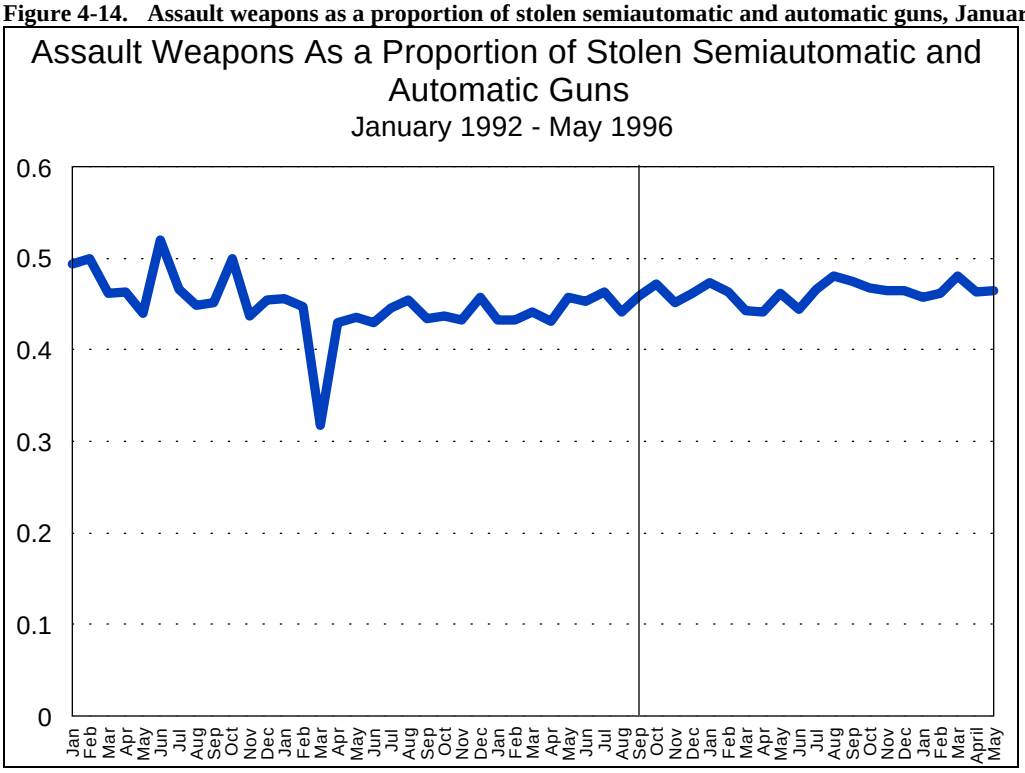

Exhibit 6
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Additional analyses (not shown) revealed that the assault weapon trends were driven entirely by assault pistols. Thefts of the AR15 group weapons, for example, were rather few in number both before and after the ban, and they decreased both in numbers and as a proportion of stolen weapons during the post-ban months.

4.3.4. Trends in Thefts of Non-Banned Semiautomatic Handguns Capable of Accepting Large-capacity Magazines

In another set of analyses, we investigated whether the ban affected thefts of non-banned semiautomatic handguns capable of handling banned, large-capacity magazines. A number of effects seem plausible. If the magazine ban has been effective in decreasing the availability of large-capacity magazines, one might hypothesize a decrease in offenders' demand for handguns capable of accepting these magazines and a decrease in thefts of these weapons from primary-market dealers and eligible owners. Alternatively, if a similar decrease in the demand for these guns drove down their prices in the primary market, it might increase the incentive for dealers to leak the guns to the illegal market and report the guns as stolen or missing. However, recent years' Blue Book values for Glock pistols suggest that their primary-market prices have been quite stable, when adjusted for inflation. Therefore, if these magazines are still widely available in secondary markets, some offenders might desire to substitute unbanned large-capacity handguns for banned assault weapons. In that case, we might also expect to see a rise in thefts of these guns.

Average monthly thefts of these weapons were higher in the months following the ban (Table 4-11). Moreover, thefts of these guns increased by about a third during the post ban period as a fraction of all semiautomatic handgun thefts (Table 4-12). However, Figure 4-15 and Figure 4-16 show that thefts of these guns were trending upwards in both numbers and as a proportion of semiautomatic handgun thefts both before and after the ban. A time series analysis did not provide conclusive evidence that handguns accepting large-capacity magazines increased significantly after the ban as a fraction of semiautomatic handgun thefts.[41] (We did not employ contingency table chi-square tests due to the clear upward trend in this variable.) At any rate, the Crime Act does not appear to have decreased criminal demand for these guns, as approximated by theft reports.

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[41] We tested a variety of potential impact forms for this time series, though we considered an abrupt, permanent impact or a gradual, permanent impact to be most plausible in light of the steadily increasing prices for Glock magazines documented in the price analysis. A model with an abrupt, permanent intervention component and a first order autoregressive process for the noise component provided an adequate fit to the data. However, this model yielded an impact estimate virtually identical to the change in the proportion measure shown in Table 4-12 (an increase of approximately one third). In light of the clear pre-ban upward trend in this measure shown in Figure 4-16, we find this effect to be implausible and suspect that the data series is too short to provide a rigorous test of the ban's impact using this methodology.

We ran a crude alternative test in which we regressed the proportion measure on a time trend and a pre-ban/post-ban indicator variable. The time trend variable was significant, while the post ban variable suggested a positive, but statistically insignificant, increase of about 7% in the proportion measure.

Figure 4-15. Stolen unbanned large-capacity semiautomatic handgun counts, January 1992–May 1996

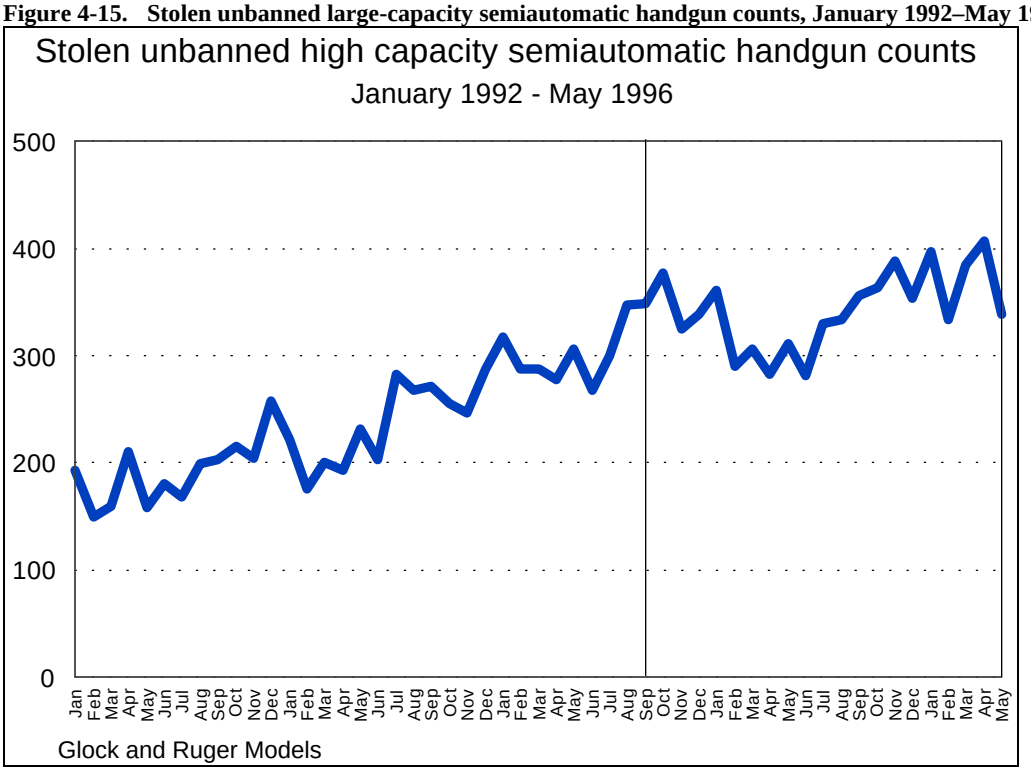
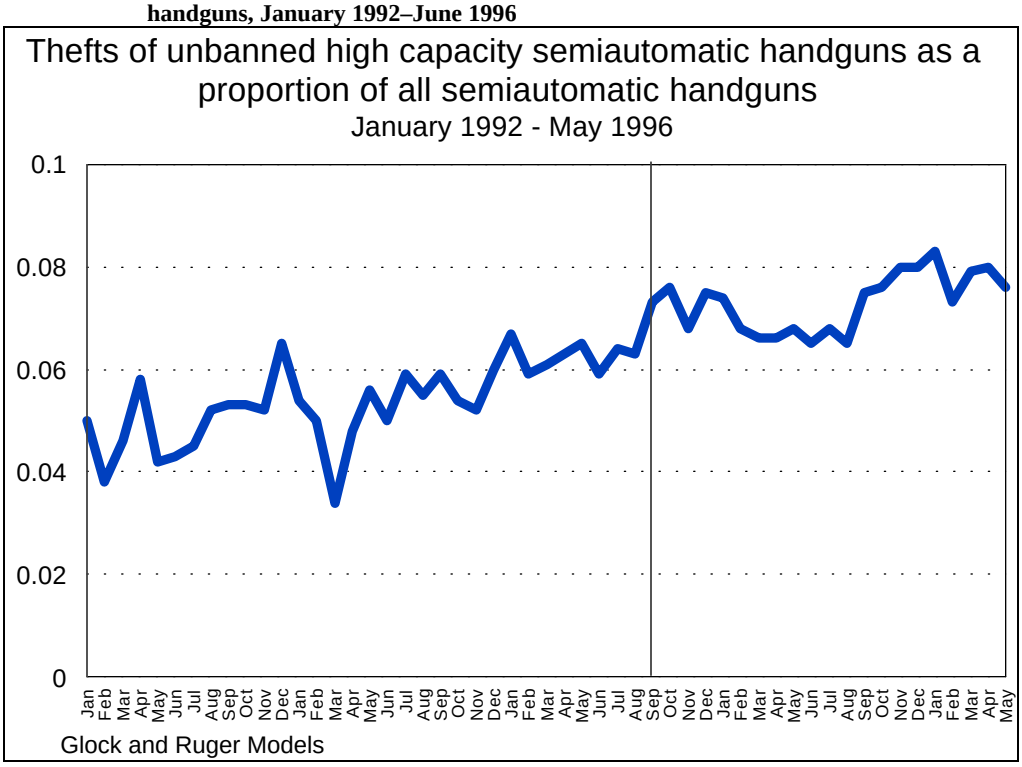


Figure 4-16. Thefts of unbanned large-capacity semiautomatic handguns as a proportion of all semiautomatic handguns, January 1992–June 1996



5. UTILIZATION EFFECTS

5.1. BATF NATIONAL FIREARM TRACE DATA

5.1.1. Introduction: Data and Limitations

To provide national level estimates of the use of assault weapons, we obtained data on firearm trace requests submitted to the U.S. Bureau of Alcohol, Tobacco and Firearms (BATF) by Federal, State, and local law enforcement personnel throughout the nation from January 1993 through May 1996. BATF maintains a firearm tracing center in West Virginia. Upon request, personnel at this center can trace firearms to their last point of recorded sale in a primary market. BATF makes this service available to police departments throughout the country to assist in criminal investigations.

The assault weapon trace file provided by BATF contains the make, model, and caliber of all models subject to the assault weapons ban (the designations are discussed in more detail below). Further, the file includes the month and year when BATF received the request, the state from which the request originated, and type of crime with which the firearm was associated. Our data for total traces consist of aggregate counts of traces broken down by month, year, state, weapon type,[42] and offense.

BATF trace data are the only available national-level sample of guns used in crime. Nevertheless, BATF trace data have significant limitations for research purposes. As Zawitz (1995, p.4) has noted, trace requests represent an unknown fraction of all guns used in crime. In terms of general limitations, BATF cannot trace military surplus weapons, imported guns without the importer name, stolen guns, or guns without a legible serial number (Zawitz 1995, p.4). Tracing guns manufactured before 1968 is also difficult because FFL's were not required to keep records of their transactions prior to that time. BATF does not generally trace guns having a manufacturing date more than six years old (such guns are likely to be many transfers removed from the original retail purchaser), though BATF can and does trace these guns in response to special requests.

Moreover, trace data are based on requests from law enforcement agencies; yet not all guns used in crime are seized by authorities, and agencies, particularly local ones, do not submit all guns they seize for tracing. Consequently, firearms submitted to BATF for tracing may not be a representative sample of firearms used in crime. Previous studies of trace data have suggested that only about 10 percent of gun crimes and 2 percent of violent crimes result in trace requests to BATF (Cox Newspapers 1989, p.3; Kleck 1991, p.75).[43]

The vast majority of weapons submitted to BATF for tracing are associated with weapons offenses, drug offenses, or violent crimes. In 1994, 72% of traces were for weapons offenses, 12% were for drug-related offenses, 12% were for the combined violent crimes of homicide, assault, and robbery, and 2% were for burglary

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[42] The weapon categories consist of revolver, pistol, derringer, rifle, shotgun, combination rifle/shotgun, and a few other miscellaneous categories.

[43] A prior study of BATF trace data by *Cox Newspapers* (1989) suggested that police are more likely to request gun traces for organized crime and drug trafficking. Further, the study indicated that these were the types of crimes with which assault weapons were most likely to be associated. Nearly 30 percent of the gun traces tied to organized crime were for assault weapons as defined by the Cox study (their definition did not match that in the 1994 Crime Act), and 12.4 percent of gun traces for drug crimes involved these guns. In contrast, assault weapons accounted for only 8 percent of gun trace requests for assaults and homicides.

(BATF 1995a, p.43). The high representation of weapons offenses was probably due to the fact that 57% of the trace requests were made by BATF field offices (BATF 1995a, p.45).

Because of the predominance of weapons offenses, BATF trace data might not appear to be a good indicator of guns used in violent and/or drug-related crime. However, the fact that a gun was not seized in association with a specific violent crime does not rule out the possibility that it had been used or would have been used in violent crime. Substantial percentages of adult and juvenile offenders carry firearms on a regular basis for protection and to be prepared for criminal opportunities (Sheley and Wright 1993; Wright and Rossi 1986). In Kansas City, Missouri, for example, about 60% of the guns seized as a result of regular police enforcement activity in high crime beats in 1992 were seized in conjunction with pedestrian checks, car checks, and other traffic violations (Shaw 1994, p.263).[44] Moreover, drug offenders tend to be disproportionately involved in violence and illegal gun traffic (National Institute of Justice 1995; Sheley and Wright 1993). Thus, guns seized in association with weapons offenses and violent offenses — in addition to those seized for drug-related crimes — may serve as a good indicator of guns possessed by drug offenders.

Despite their limitations, guns confiscated by law enforcement agencies are a reasonable index of guns used in violent and drug-related crime, and they are the best available indicator of changes over time in the types of guns used in crime and possessed and/or carried by criminal and otherwise deviant or high risk persons. BATF trace data are the only such national sample.

Yet, another important limitation to national trace data is that the process by which state and local law enforcement agencies decide to submit guns for tracing is largely unknown, and there are undoubtedly important sources of variation between agencies in different states and localities (and perhaps regions). For instance, a state or local agency may be less likely to need the tracing services of BATF if its state or city maintains its own firearms registration system. Knowledge of BATF's tracing capabilities and participation in federal/state/local law enforcement task forces are some additional factors that can affect an agency's tracing practices. Further, these conditions will vary over time; for example, BATF has been actively trying to spread this knowledge and encourage trace requests since 1994. For all of these reasons, BATF trace data should be interpreted cautiously.

Finally, prior studies have suggested that assault weapons are more likely than other guns to be submitted for tracing.[45] However, this generalization may no longer be valid, for, as is discussed below, police appear to be requesting traces for increasing proportions of confiscated firearms.

5.1.2. Trends in Total Trace Requests

Table 5-1 presents yearly changes in trace requests for all firearms for 1993 through early 1996. Total traces grew 57 percent from 1993 to 1994, decreased 11 percent from 1994 to 1995, and then increased 56 percent from 1995 to 1996. In contrast, Table 5-2 indicates that gun crimes declined throughout the 1993–95 period (national gun crime figures are not yet available for 1996). The increase in gun trace requests that occurred in 1994 was not attributable to an increase in gun crime and thus appears to have reflected a change in police trace request behavior and/or BATF initiatives. The large growth in traces in early 1996 also seems to be unrelated to gun crime (national gun crime figures for 1996 are not yet available, but we are not aware of any data suggesting

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[44] This calculation excludes guns seized by special crime hot spots patrols which were proactively targeting guns. Thus, the figure reflects normal police activity.

[45] Prior estimates have indicated that approximately 5 to 11 percent of trace requests are for assault weapons (*Cox Newspapers* 1989; Lenett 1995; Zawitz 1995), though these estimates have not all been based on the 1994 Crime Act definition of assault weapons.

that gun crime has increased over 50 percent since 1995). On the other hand, the decline in trace requests in 1994 mirrored the decline in gun crime, particularly gun homicides (the most accurately measured gun crime category), suggesting that tracing practices were fairly stable from 1994 to 1995.

Table 5-1. Total traces, January 1993–May 1996

Year	*Total*	*Monthly average*	*Percent change from previous year*
1993	55,089	4,591	N/A
1994	86,216	7,185	+ 57
1995	76,924	6,410	- 11
1996 (Jan.-May)	54,254	10,851	+56*

* Change is expressed relative to January through May of 1995.

Table 5-2. National trends in gun crime, 1993–95

Year	*Offense*	*Number*	*Percent change from previous year*
1993	Gun murders	16,136	N/A
1994	Gun murders	15,463	- 4
1995	Gun murders	13,673	- 12
1993	Gun robberies	279,737	N/A
1994	Gun robberies	257,428	- 8
1995	Gun robberies	238,023	- 8
1993	Gun aggrav. assaults	284,910	N/A
1994	Gun aggrav. assaults	268,788	- 6
1995	Gun aggrav. assaults	251,712	- 6

Sources: FBI Uniform Crime Reports, *Crime in the United States* (1996, pp.18, 26-29, 31-32; 1995, pp.18, 26-29, 31; 1994, pp.27-29, 31-32).

As a comparison to national trends, Table 5-3 presents gun confiscation figures for the cities of Boston and St. Louis, two cities for which we have data on all confiscated firearms.[46] The Boston data are consistent with national trends in gun violence in that they show decreases in gun seizures for each year.[47] In St. Louis, gun confiscations increased slightly in 1994, but in 1995, they decreased by an amount comparable to the nationwide

[46] These Boston data were provided to us by the Boston Police Department via researchers at Harvard University. The St. Louis data are from the St. Louis Police Department and were provided by researchers at the University of Missouri, St. Louis.

[47] The sharp decrease in gun confiscations from 1995 to 1996 may be due in part to recent youth gun violence initiatives being undertaken by the Boston Police Department in collaboration with a number of other agencies and researchers from Harvard University (Kennedy et al. 1996; Kennedy 1996).

decreases in gun murders and gun robberies. Of course, trends in Boston and St. Louis may not be indicative of those in the rest of the nation. Nevertheless, the contrast between the Boston and St. Louis figures and the national tracing figures provide further evidence that changes in national gun traces in 1994 and early 1996 were driven largely by police practices and BATF initiatives rather than changes in gun crime.

Table 5-3. Gun confiscations/traces, January 1993–May 1996

Year	*Total*	*Monthly average*	*Percent change from previous year*
Gun confiscations/traces for Boston, MA, January 1993–May 1996			
1993	866	72	N/A
1994	762	64	- 12%
1995	712	59	- 7%
1996 (Jan.-May)	241	48	- 28%*
Gun confiscations in St. Louis, MO, 1993–95			
1993	3,544	295	N/A
1994	3.729	311	5%
1995	3,349	279	-10%

*Change is expressed relative to January-May of 1995.

In sum, the changes in national trace requests which occurred in 1994 and early 1996 appear to have stemmed from BATF initiatives. Although we have little documentation of these changes, our consultations with BATF agents have suggested that the surge in trace requests from 1993 to 1994 was due largely to internal BATF initiatives that now require agents to submit all confiscated firearms for tracing. In addition, BATF has made efforts to encourage more police departments to submit trace requests and to encourage police departments to request traces for greater fractions of their confiscated weapons. One example is BATF's national juvenile firearms tracing initiative launched in late 1993 (BATF 1995b, p.21). Greater cooperation between BATF and local agencies (through, for example, special task forces) has also resulted in more trace requests according to BATF officials, and a few states and localities have recently reached 100 percent tracing. Beginning in the fall of 1995, moreover, agents from the tracing center began visiting BATF's field divisions to inform federal, state, and local law enforcement personnel about the tracing center's services and capabilities, including the implementation of computerized on-line tracing services. This would appear to be a major factor behind the growth in trace requests from 1995 to 1996.

For the 1994–95 period, however, tracing practices seem to have remained steady. The decline in traces in 1995 matched a real decrease in gun crimes. These developments have important ramifications for the analysis of assault weapon traces.[48]

[48] We made limited efforts to further disentangle federal and state/local trends by obtaining annual data on traces from a number of states broken down by requesting agency. We examined trace requests from a number of cities where, according to informal judgments by BATF agents, cooperative efforts between local law enforcement agencies and BATF had resulted in the submission of trace requests for a relatively high percentage of confiscated firearms over an extended period. We anticipated that trace requests from BATF field offices in these locations would show substantial increases from 1993 to

5.1.3. Total Assault Weapon Traces

During the period from January 1993 through May 1996, BATF received 12,701 trace requests for assault weapons. This count covers specific makes and models listed in the 1994 Crime Act, exact copies of those makes and models, and other firearms failing the Crime Act's features test for assault weapons.[49] The requests include all states, Washington, D.C., Puerto Rico, and Guam.[50]

Table 5-4 shows the number, monthly averages, and percentage changes of assault weapon traces for each year. Assault weapon traces increased 9 percent from 1993 to 1994, declined 20 percent from 1994 to 1995, and then increased 7 percent from 1995 to 1996. While one cannot entirely dismiss the possibility that the use of assault weapons rose in 1994 and 1996, it seems likely that these increases were due partially or entirely to the general increase in police trace requests which occurred during those years. Yet assault weapon traces increased by amounts much smaller than did total traces in 1994 and 1996, a finding which supports the conjecture that police have been more consistently diligent over time in requesting traces for confiscated assault weapons.[51]

1994, and that requests from the local law enforcement agencies would rise from 1995 to 1996. However, the figures from these locations did not reveal any clearly interpretable patterns. Any patterns which might have existed may be obscured by the fact that local agencies may submit traces directly to the tracing center or submit them indirectly through local ATF field offices. In 1994, for example, 17% of trace requests were from outside (i.e., non-BATF) agencies directly, while 26% were from outside agencies through BATF offices (BATF 1995, p.45). Our judgment is that analyzing trace requests according to submitting agency will not necessarily illuminate the ambiguities in interpreting trace request trends without extensive research into both the processes by which guns are selected for tracing and submitted by local agencies and BATF field offices and the impact of special BATF/local initiatives on these processes.

[49] The guns designated as "features test" guns consist of makes and models that fail the features test based on manufacturer specifications. The file does not generally include guns which were legal as manufactured but were later modified in ways which made them illegal. (Firearms which are traced by BATF are not actually sent to BATF for inspection). Further, firearms are often manufactured and sold with various options, and the legal/illegal status of some models is contingent upon the particular features with which the gun was manufactured. For example, a Franchi Spas 12 shotgun may or may not be an assault weapon depending upon the size of its ammunition magazine (prior to the ban, the gun was sold with 5 shot and 8 shot tube magazines - see Fjestad [1996, p.471]). Unfortunately, this level of detail is not available in the BATF data. Potential assault weapon models like the Franchi Spas 12 were included in the assault weapon file, but, as is discussed later in the text, we did not utilize them in all analyses.

[50] It should be noted that the firearm make and model designations in BATF trace data are made by the law enforcement officers who submit the requests. Undoubtedly, there exists some level of error in these designations, though we do not have any data with which to estimate the error rate.

[51] The 1996 assault weapon traces include 89 observations identified as "duplicate traces." Although these trace requests can sometimes represent instances in which the same gun was used in multiple crimes, they usually represent instances in which, for various administrative reasons, a particular trace request was entered into the computer system more than once. Unfortunately, it is not possible to identify duplicate trace requests for years prior to 1996. In order to treat data from all years in a consistent manner, we therefore retained all of the 1996 trace requests for the analysis. Consequently, the total and assault weapon trace numbers presented in this report overstate the true numbers of trace requests. Our analysis of the trace data rests on the assumption that the rate of duplicate tracing has remained relatively constant over the 1993–96 period.

Table 5-4. Assault weapons traces, January 1993–May 1996

Year	*Total*	*Monthly average*	*Percent change from previous Year*
1993	3,748	312	N/A
1994	4,077	340	+ 9%
1995	3,268	272	- 20%
1996 (Jan.-May)	1,608	322	+ 7%*

*Change is expressed relative to January through May of 1995.

Traces for assault weapons dropped more markedly from 1994 to 1995 (20 percent) than did overall traces (11 percent). In a t-test of 1994 and 1995 monthly means, the drop in assault weapon traces was statistically significant (p=.01, two-tailed test), while the drop in total traces was not (p=.22, two-tailed test). Moreover, the drop in assault weapon traces was substantially greater than the declines in gun murder (12 percent), gun robbery (8 percent), and gun assault (6 percent) for the same period. This suggests that criminal use of assault weapons decreased from 1994 to 1995, both in absolute terms and relative to crime trends generally. In addition, utilization of assault weapons in crime was less in 1995 than in 1993.

5.1.4. Analysis of Select Assault Weapons

As noted in Chapter 2, many of the foreign makes and models banned by Title XI were banned from importation prior to the passage of that legislation. Thus, any recent decrease in the use of those weapons cannot be attributed unambiguously to the effects of the Crime Act. For this reason, we concentrated our analyses below on a select group of domestic assault weapons whose availability was not affected by legislation or regulations predating the 1994 Crime Act. These guns include the AR15 family (including the various non-Colt copies), the Intratec family (including the AA Arms AP-9), and the SWD handgun family.

In addition, we selected a small number of firearm models which, as manufactured, fail the features test of the assault weapons legislation. These weapons had to meet three selection criteria: 1) the weapon had to be in production at the time of the Crime Act (if the weapon was a foreign weapon, its importation could not have been discontinued prior to the Crime Act);[52] 2) there had to be 30 or more trace requests for assault weapons made by that manufacturer during the period January 1993 through April 1994; and 3) the weapon had to have an unambiguous assault weapon designation as it was manufactured prior to the ban (i.e., its status could not be conditional on optional features).[53] These criteria ensured that we would capture the most prevalent assault weapons that were still being sold in primary markets just prior to the effective date of Title XI. We used January 1993 through April 1994 as the selection period in order to minimize effects on the gun market which may have resulted from the passage of the assault weapons legislation by the U.S. House of Representatives in May of 1994.

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[52] Heckler and Koch, for example, manufactured a number of rifle and handgun models which were relatively common among assault weapon traces (i.e., the HK91, HK93, HK94, and SP89). However, these models were all discontinued between 1991 and 1993 (Fjestad 1996, p.531).

[53] BATF officials assisted us in these designations. The only weapon which passed the first two criteria but not the third was the Franchi Spas 12 shotgun. The assault weapon trace file contained 53 trace requests for this model prior to May 1994.

The features test weapons selected for the analysis were: Calico M950 and M110 model handguns; Calico M100, M900, and M951 model rifles; and Feather AT9 and AT22 model rifles.

This select group of assault weapons accounted for 82 percent of assault weapon traces submitted to BATF during the study period. Yearly trends in trace requests for these weapons (see Table 5-5) were virtually identical to those for all assault weapons. Most importantly, average monthly traces were 20 percent lower in 1995 than in 1994 (p=.01, two-tailed test). Figure 5-1 displays the trend in monthly traces for these firearms.

Figure 5-1. National ATF trace data: Traces for select assault weapons, January 1993–May 1996

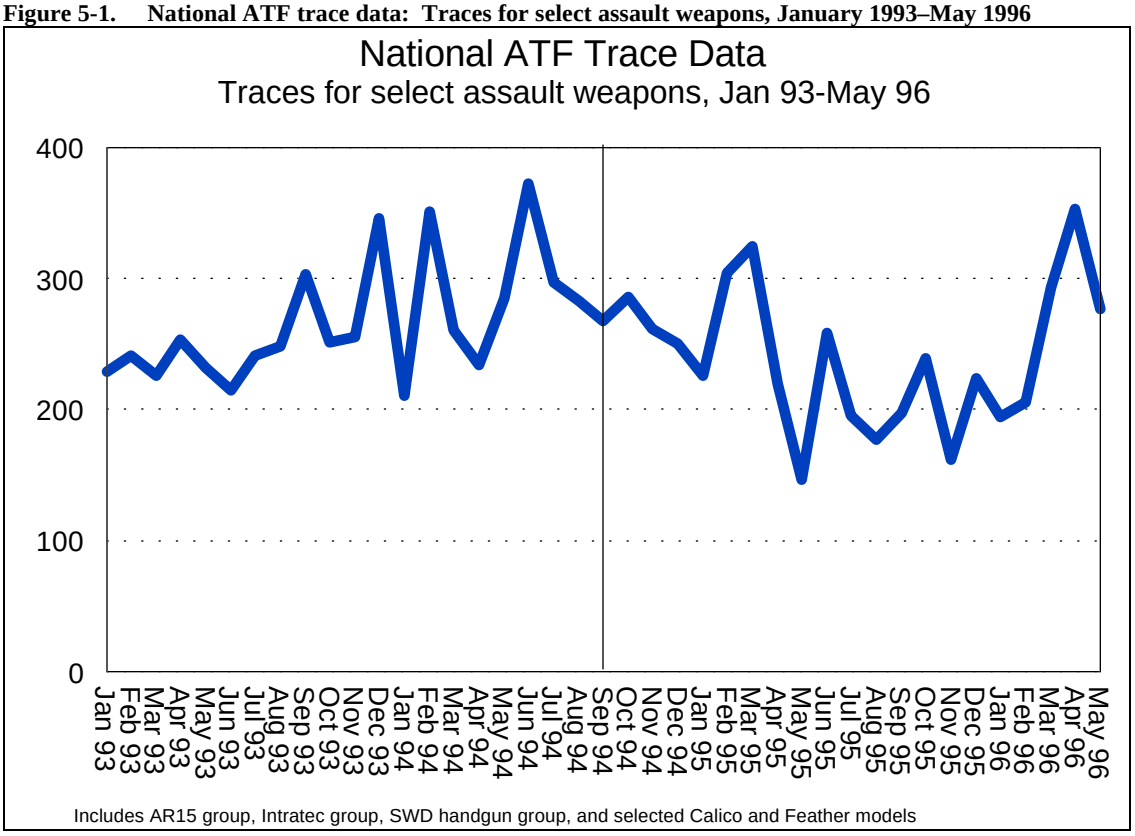


Table 5-5. Traces for select assault weapons,† January 1993–May 1996

Year	*Total*	*Monthly average*	*Percent change from previous year*
1993	3,040	253	N/A
1994	3,358	280	+ 10%
1995	2,673	223	- 20%
1996 (Jan.-May)	1,323	265	+ 8%*

*Change is expressed relative to January through May of 1995.

†Includes traces for AR15 group, Intratec group, SWD handgun group, and selected Calico and Feather models.

5.1.5. Assault Weapon Traces for Violent Crimes and Drug-Related Crimes

To fulfill Title XI's mandate to assess the effects of the ban on violent and drug-related crime, we also analyzed assault weapon traces associated with violent crimes (murder, assault, and robbery) and drug-related crimes. We used our select group of assault weapons for this analysis. Yearly trends for these traces are presented in Table 5-6. Monthly trends are graphed in Figure 5-2 and Figure 5-3. A striking feature of these numbers is their small magnitude. On average, the monthly number of assault weapon traces associated with violent crimes across the entire nation ranged from approximately 30 in 1995 to 44 in 1996. For drug crimes, the monthly averages ranged from 34 in 1995 to 50 in 1994.

Figure 5-2. National ATF trace data: Traces for select assault weapons (violent crimes)

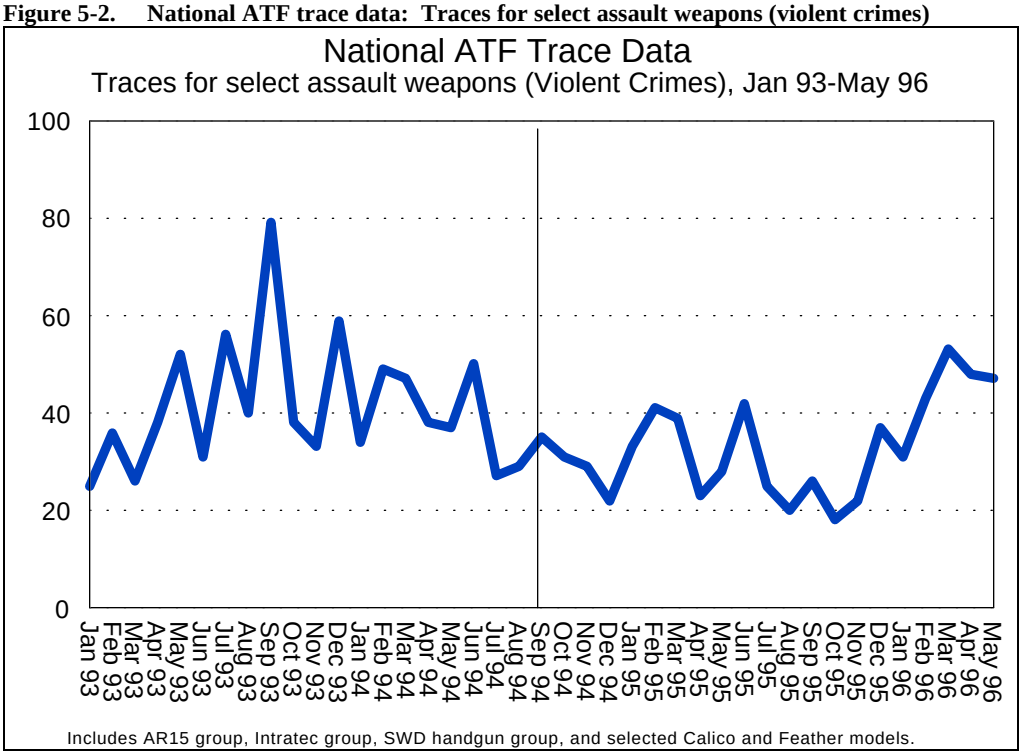


Figure 5-3. National ATF trace data: traces for select assault weapons (drug crimes)

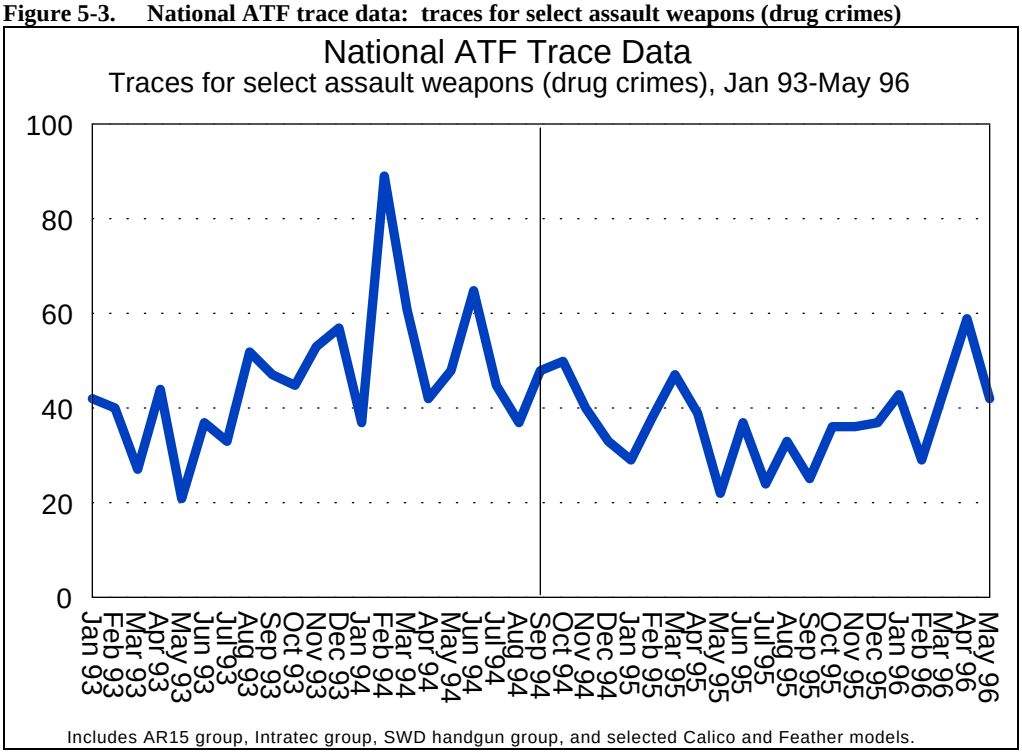


Table 5-6. Traces for select assault weapons,† January 1993–May 1996 (violent and drug-related crimes)

Violent Crimes:

Year	*Total*	*Monthly average*	*Percent change from previous year*
1993	513	43	N/A
1994	428	36	- 17%
1995	354	30	- 17%
1996 (Jan.-May)	222	44	+ 35%*

Drug-Related Crimes:

Year	*Total*	*Monthly average*	*Percent change from previous year*
1993	498	42	N/A
1994	595	50	+ 19%
1995	403	34	- 32%
1996 (Jan.-May)	217	43	+ 24%*

*Change is expressed relative to January through May of 1995.

†Includes AR15 group, Intratec group, SWD handgun group, and selected Calico and Feather models.

Traces for assault weapons associated with violent crimes dropped 17 percent in both 1994 and 1995. Both decreases were greater than the decreases which occurred for violent gun crimes in each of those years. However, assault weapon traces for violent crime rebounded 35 percent in 1996 to a level comparable with that in 1993.

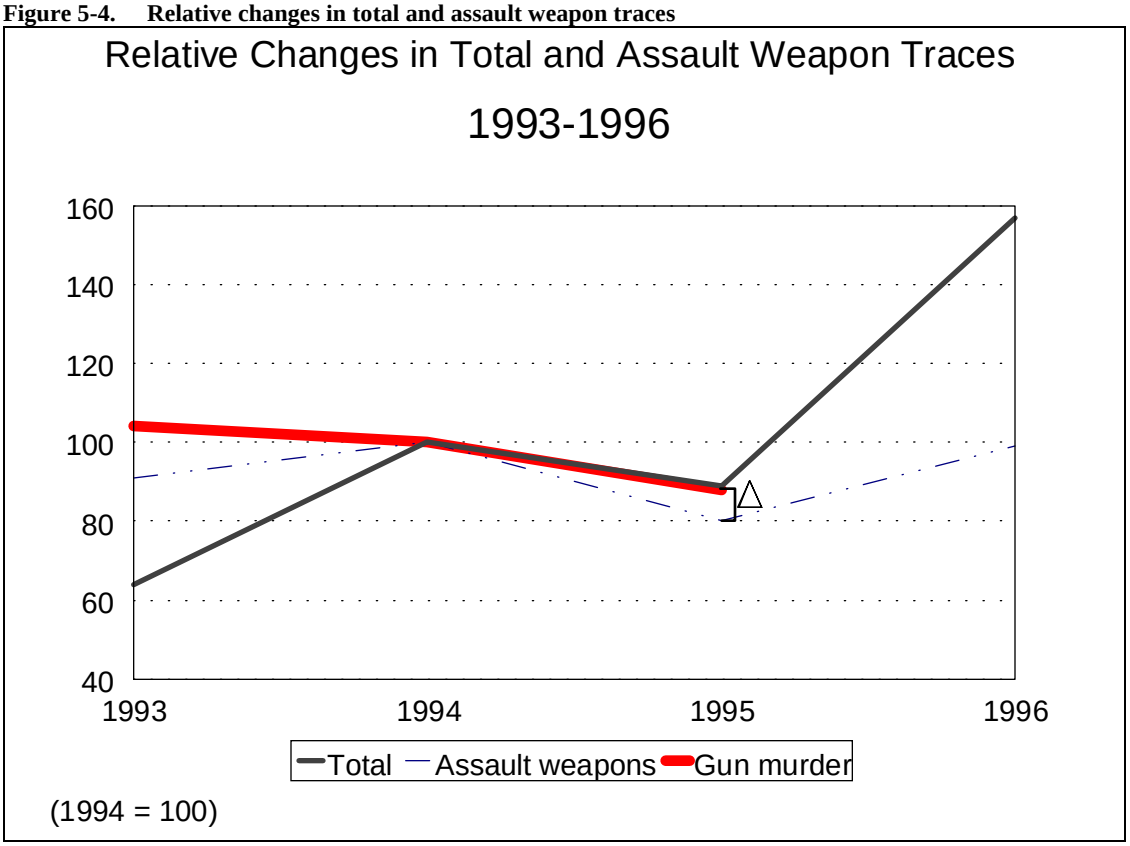
Assault weapon traces for drug crimes followed patterns similar to those for all assault weapons. Assault weapon traces increased 19 percent from 1993 to 1994, decreased 32 percent from 1994 to 1995, and then increased 24 percent from 1995 to 1996. The yearly fluctuations of these traces were greater than those for all assault weapons, but the drug trace numbers may be relatively more unstable due to the small number of weapons under consideration.

5.1.6. Conclusions on National Trends in the Use of Assault Weapons

National-level data suggest that the use of assault weapons, as measured by trace requests to BATF, declined in 1995 in the wake of the Crime Act. The 20 percent decrease in assault weapon trace requests from 1994 to 1995 was greater than occurred overall, and it was greater than the 6 to 12 percent national drop in violent gun crime. This is demonstrated graphically in Figure 5-4. Assault weapon traces for violent crimes and drug-related crimes also decreased in 1995 by amounts comparable to or greater than the overall drop in assault weapon

traces. Further, there were approximately 13 percent fewer assault weapon trace requests in 1995 than during the pre-ban year of 1993.[54]

Figure 5-4. Relative changes in total and assault weapon traces



Another indication that this was an effect from the ban is that assault weapon traces declined less in 1995 in states which had their own bans prior to the Federal legislation. Table 5-7 presents combined yearly traces for our select assault pistol group in the four states with assault weapon bans: California, New Jersey, Connecticut, and Hawaii. In general, assault weapon traces in these states followed the same pattern as did the national figures. The increases in 1994 and 1996 were larger than the national increases which occurred during those years, but the 1995 decrease was smaller than the national assault weapon decrease. Further, the decline in these ban states was consistent in magnitude with the national drop in gun crime.[55]

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[54] The data also do not show any obvious substitution of non-banned long guns for assault weapons. Trace requests for shotguns decreased 10 percent in 1995. Total rifle traces increased 3.5 percent in 1995, but our select group of assault weapon rifles (AR15 group and selected Calico and Feather models) also increased 3 percent. Thus, banned and non-banned rifles did not follow divergent trends. With currently available data, we have not been able to assess whether the assault weapon ban led to displacement to other categories of weapons, such as non-banned semiautomatic handguns capable of carrying pre-ban large-capacity magazines.

[55] We chose to examine only assault weapon pistols because assault rifles are rarely used in crime and Hawaii's assault weapons legislation covers only handguns. Maryland passed an assault pistol ban in 1994, but the legislation was passed only a few months prior to the Federal ban, so we did not include Maryland as a ban state.

All of the assault pistol ban states outlawed one or more of the handguns in our select group of assault pistols. However, the coverage of these state laws varied, and our select assault pistols were not banned in all of these states. We therefore conducted a supplemental analysis focusing on the Intratec TEC-9 series and the M10/M11 series made by SWD and others. As far as we can determine, these guns were covered by all of the state assault pistol bans. Trace requests for TEC-9's,

Table 5-7. Assault pistol traces, ban states (CA, NJ, CT, and HI), January 1993–May 1996

Year	*Total*	*Monthly mean*	*Percent change from previous year*
1993	204	17	N/A
1994	228	19	+12%
1995	210	18	- 8%
1996 (Jan.-May)	106	21	+15%

*Change is expressed relative to January through May of 1995.

Nationally, traces for assault weapons rebounded in 1996 to a level higher than that of 1993 but lower than that of 1994. This could represent leakage into illegal channels from the stockpile of legal, grandfathered assault weapons manufactured prior to the implementation of Title XI. Production of assault weapons increased considerably in 1994, and prices of these weapons fell to pre-ban levels in late 1995 and early 1996 (see Chapter 3). Over the next few years, it is possible that more, rather than fewer, of the grandfathered weapons will make their way into the hands of criminals through secondary markets.

On the other hand, the increase for 1996 may be an artifact of recent BATF initiatives to increase trace requests from local police. The rebound in assault weapon traces might also reflect an as yet undocumented rebound in gun crime in 1996. Unfortunately, we cannot disentangle these possibilities with data available at this time, and it is not yet clear whether the 1995 decrease in our indicator of assault weapon use was temporary or permanent.[56]

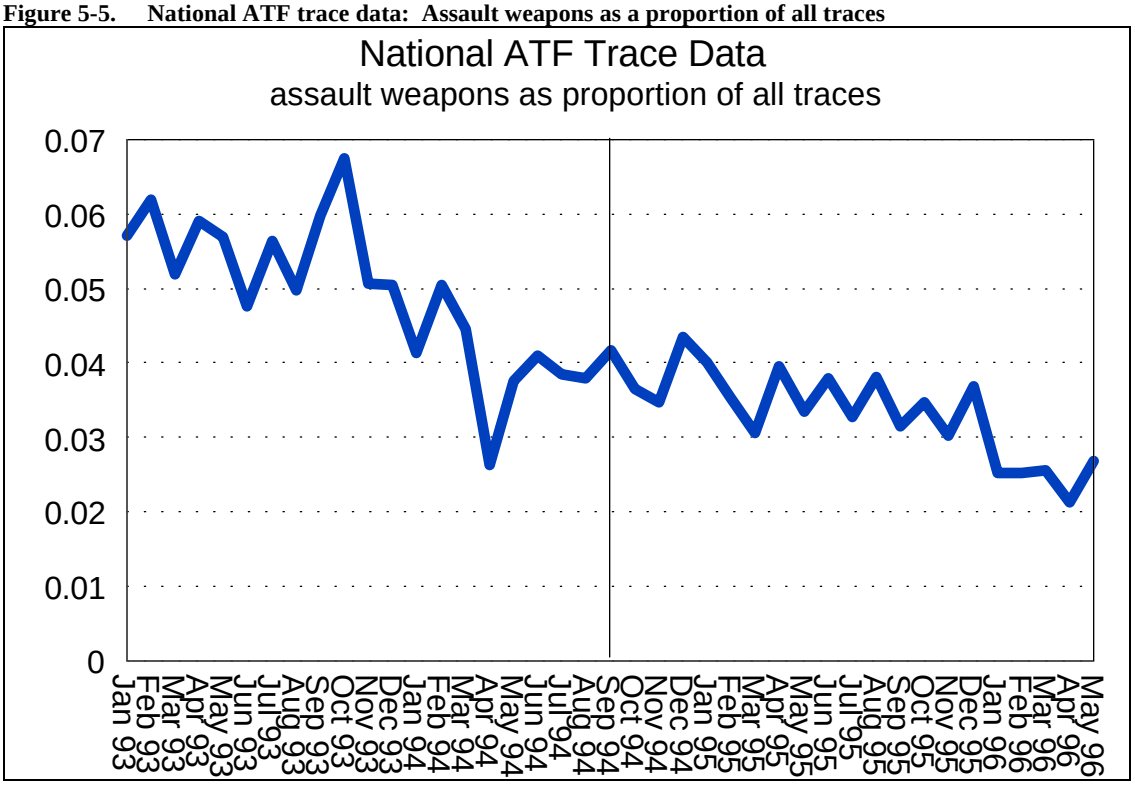
5.1.7. The Prevalence of Assault Weapons Among Crime Guns

As is shown in Figure 5-5, assault weapon traces decreased as a proportion of all traces throughout the entire study period. While Title XI may have contributed to this trend, it is apparent that the trend began before implementation of Title XI, and, to a large degree, must reflect the disproportionate growth in trace requests for non-assault weapons rather than a continual decline in the prevalence of assault weapons.

M10's, and M11's from the ban states rose 1% from 1993 to 1994, decreased 6% from 1994 to 1995, and remained steady from 1995 to early 1996. The 6% drop in 1995 seems to confirm that assault weapon trace requests dropped in the ban states after implementation of the federal law but by smaller percentages than assault weapon trace requests nationwide.

[56] In light of the substantial instrumentation problems with these data and the threat which such problems pose to quasi-experimental time series designs (Campbell and Stanley 1963, pp.40-41), we elected not to pursue more sophisticated methods, such as an interrupted time series analysis, with these data.

Figure 5-5. National ATF trace data: Assault weapons as a proportion of all traces



Despite this problem with interpreting trends in the prevalence of assault weapon traces, the 1996 trace figures arguably provide the best available estimate of the prevalence of assault weapons among crime guns. Firearm tracing should now be more complete and less biased than at any time previously. For January through May of 1996, assault weapons accounted for 3 percent of all trace requests. Our group of select domestic assault weapons represented 2.5 percent of all traces. Traces for the select assault weapon group accounted for 2.6 percent of traces for guns associated with violent crimes and 3.5 percent of traces for guns associated with drug crimes. This is consistent with previous research indicating that assault weapons are more likely to be associated with drug crimes than with violent crime (Cox Newspapers 1989; Kleck 1991). At the same time, these numbers reinforce the conclusion that assault weapons are rare among crime guns.

5.1.8. Crime Types Associated with Assault Weapons

Table 5-8 displays the types of offenses with which assault weapons were associated. For each year, approximately two-thirds of assault weapons were tied to weapons offenses. Drug offenses were the next most common, accounting for 16 to 18 percent of assault weapon traces for each year. Violent offenses ranged from 13 to 17 percent of assault weapon traces. For comparison, the percentage of total traces associated with drug offenses varied between 12 and 13 percent during this period. Violent offenses accounted for 12 to 16 percent of total traces. Hence, assault weapons were more likely to be associated with drug offenses than were other traces.

Table 5-8. Assault weapon trace requests to BATF by crime type

Offense type*	*1993* (N=3,725)	*1994* (N=4,048)	*1995* (N=3,226)	*1996 (Jan–May)* (N=1,500)
Murder/Homicide	.097	.069	.063	.072
Aggravated assaults	.048	.040	.051	.076
Robbery	.027	.018	.020	.022
Drug abuse violations	.167	.182	.161	.174
Weapons; carrying, possessing, etc.	.647	.665	.661	.581
Other offenses	.015	.025	.046	.075

*Offense type could not be determined for 1 percent of assault weapon traces in 1993, 1994, and 1995. Offense type could not be determined for 7 percent of assault weapon traces in 1996.

5.2. ASSAULT WEAPON UTILIZATION: LOCAL POLICE DATA SOURCES

5.2.1. Introduction and Data Collection Effort.

Because of our concerns over the validity of national BATF trace data for measuring the distribution of guns used in crime, we attempted to collect and analyze data from a number of police departments around the country. We sought to acquire data on all firearms confiscated in these jurisdictions, rather than just firearms for which BATF trace requests were made. Analyzing all guns confiscated in a jurisdiction provides a more complete and less biased picture of weapons used in crime than does analysis of guns selected for BATF traces. The disadvantage of using local agency gun seizure data is that trends in any given jurisdiction may not be indicative of those elsewhere in the nation. Of course, local agency data are still subject to general limitations regarding police gun confiscation data which were raised in the last section (i.e., not all guns confiscated by police are used in violent or drug-related crime and not all guns used in crime are seized by police).

Unfortunately, the attempt to collect local gun data fell short of our expectations. Our intention was to collect data from cities in states both with and without their own assault weapon bans. Further, we concentrated our data collection effort on cities in states which had relatively high rates of gun violence. To this end, we contacted several police departments around the country. However, most of the departments that we contacted either did not have their property records computerized or had only computerized their records a few months prior to the implementation of the Crime Act, thus precluding the collection of meaningful pre-ban baseline data.[57]

Ultimately, we obtained data from two cities, St. Louis and Boston, neither of which is subject to a State assault weapon ban. From St. Louis, we acquired a database on all firearms confiscated by police from 1992 through 1995 (N=13,863). Our Boston data consist of monthly counts of various categories of firearms confiscated by Boston police from 1992 through August of 1996 (total confiscations numbered 3,840 for this period). For both locations, we examined trends in confiscations of our select domestic assault weapon group (i.e., the AR15, Intratec, and SWD families and selected Calico and Feather models). In addition, we approximated trends in confiscations of semiautomatic handguns capable of accepting large-capacity magazines by analyzing confiscations of selected Glock and Ruger pistols.

[57] Time, cost, and personnel considerations limited our ability to implement on-site data collection efforts.

The patterns we discovered were relatively consistent in both cities. Assault weapon confiscations were rare both before and after the ban. In both cities, the data were suggestive of a decrease in assault weapon confiscations after the ban. As a fraction of all confiscated guns, assault weapons decreased roughly 25% in these cities. Thus, these data sources provide some confirmation of our inferences regarding assault weapon trends from the national trace data. Further, we were able to examine the crimes with which assault weapons were associated in St. Louis and found that, as in the national data, assault weapons are overrepresented in drug offenses but not in violent offenses. Finally, confiscations of non-banned semiautomatic handguns capable of accepting large-capacity magazines increased or remained stable after the ban as a fraction of all confiscated handguns in both St. Louis and Boston.[58]

5.2.2. Assault Weapons in St. Louis and Boston

St. Louis police confiscated 180 weapons in the select assault weapon group between 1992 and 1995.[59] The vast majority of these weapons were from the Intratec and SWD assault pistol groups. Average monthly confiscations of assault weapons dropped from 4 to 3 after the ban's implementation (see Table 5-9). Total gun seizures also dropped during the post-ban months. In order to control for the general downward trend in gun confiscations, we examined assault weapons as a fraction of all confiscated guns. Prior to the ban, assault weapons accounted for about 1.4% of all guns. After the ban they decreased to 1% of confiscated guns, a relative decrease of approximately 29%. A contingency table chi-square test indicated that this was a statistically meaningful drop (p=.05). In addition, assault weapons represented a lower fraction of all guns confiscated during 1995 (.009) than

Table 5-9. Summary data on guns confiscated in St. Louis, January 1992 – December 1995

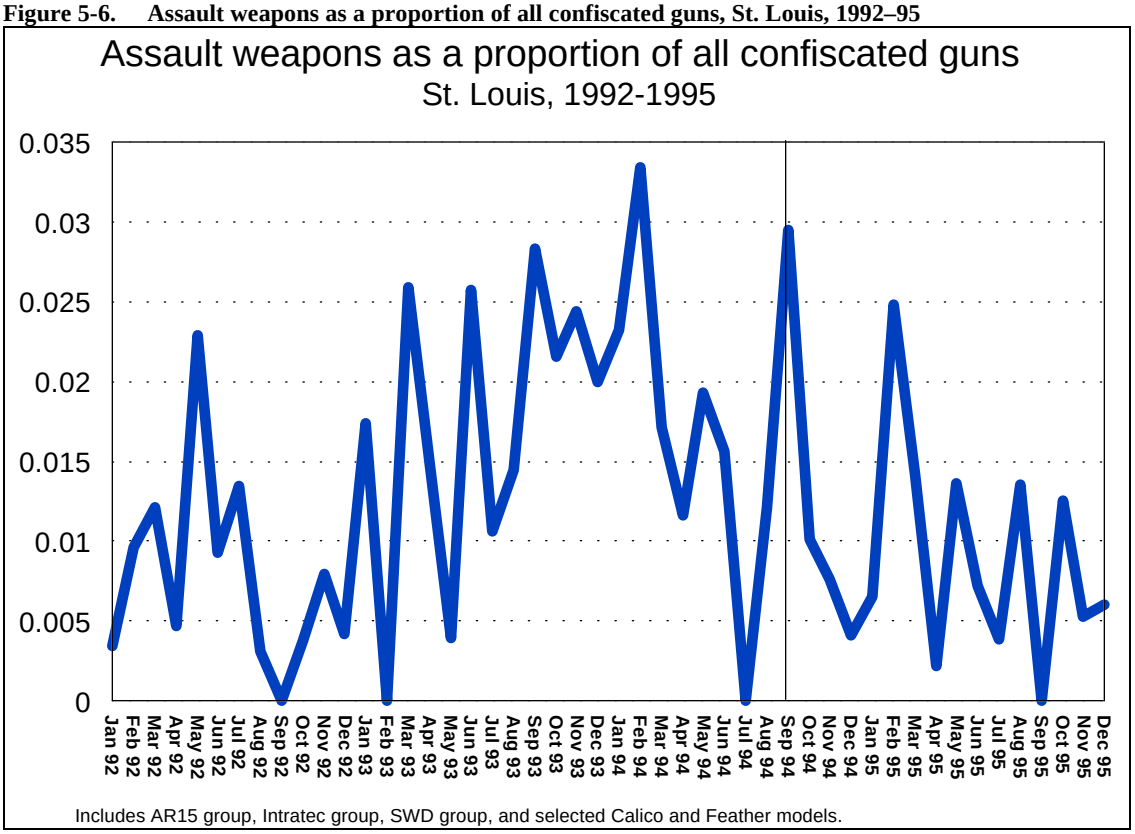
	Pre-ban (Jan. '92–Aug. '94)	*Post-ban (Sept. '94–Dec. '95)*	*Change*
Total guns confiscated			
Total	9,372	4,491	
Monthly mean	293	281	-4%
Assault guns			
Total	134	46	
Monthly mean	4	3	-25%
Proportion of confiscated guns	.014	.010	-29%
Large-capacity handguns (Ruger and Glock)			
Total	118	93	
Monthly mean	4	6	+50%
Proportion of all handguns	.018	.031	+72%

[58] As stated above, analyses of local data sources have the limitation that they are not necessarily indicative of those elsewhere in the nation. We cannot address the various local conditions which may have impacted recent gun trends in the selected cities. However, we should note that youth gun violence initiatives sponsored by the National Institute of Justice have been ongoing in each city during recent years. It is not clear at this time what impact, if any, these initiatives have had upon the gun trends that are the subjects of our investigation.

[59] The St. Louis data contain a few SWD streetsweeper shotguns in addition to SWD assault pistols.

during 1993 (.018), the last full calendar year prior to the passage and implementation of the ban. A monthly trend line for assault weapons as a fraction of all guns is shown in Figure 5-6.[60] [61]

Figure 5-6. Assault weapons as a proportion of all confiscated guns, St. Louis, 1992–95



A similar picture emerged from Boston. From 1992 through August of 1996, Boston police seized only 74 of these weapons. As in St. Louis, the vast majority were Intratec and SWD assault pistols. Table 5-10 shows

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[60] We also estimated interrupted time series models to test the post intervention change in the monthly trend for the assault weapons proportion measure. As in the NCIC analysis reported in Section 4.3 (p.50) we considered various models of impact. An abrupt, temporary impact model might seem appropriate, for example, based on the price trends presented in Section 4.1 (p.24). Both abrupt, permanent and gradual, permanent impacts are also plausible and seem to better match the pattern displayed in the St. Louis data. At any rate, these analyses failed to confirm that there was a significant change in assault weapons as a fraction of all guns. (The best fitting model was an abrupt, permanent impact model with an autoregressive parameter at the third lag).

However, we have emphasized the chi-square proportions test because the monthly series is rather short (N=48) for interrupted time series analysis (McCleary and Hay 1980) and because the monthly trend line provides no strong indication that the post ban drop was due to a preexisting trend.

[61] Average monthly confiscations of long guns (rifles and shotguns) increased somewhat from 88 in the pre-ban months to 92 after the ban. As a proportion of all confiscated guns, long guns rose from .299 before the ban to .326 after the ban. Thus, the decrease in assault weapons may have been offset by an increase in the use of long guns. However, we did not have the opportunity to investigate the circumstances under which long guns were seized. The post-ban increase could have been due, for example, to an increase in the proportion of confiscated guns turned in voluntarily by citizens. In addition, the ramifications of a long gun substitution effect are somewhat unclear. If, for instance, the substituted long guns were .22 caliber, rimfire (i.e., low velocity) rifles (and in addition did not accept large-capacity magazines), then a substitution effect would be less likely to have demonstrably negative consequences. If, on the other hand, offenders substituted shotguns for assault weapons, there could be negative consequences for gun violence mortality.

the respective numbers of total firearms and assault weapons seized before and after the Crime Act. The average number of assault weapons seized per month dropped from approximately 2 before the ban to about 1 after the ban, but total gun seizures were also falling. As a fraction of all guns, assault weapons decreased from .021 before the ban to .016 after the ban, a relative decrease of about 24%. A contingency table chi-square test indicated that this change was not statistically meaningful (p=.38), but the numbers provide some weak indication that assault weapons were dropping at a faster rate than were other guns. Quarterly trends for the proportions variable shown in Figure 5-7 suggest that assault weapons were relatively high as a proportion of confiscated guns during the quarters immediately following the ban, but then dropped off notably starting in the latter part of 1995.[62] [63]

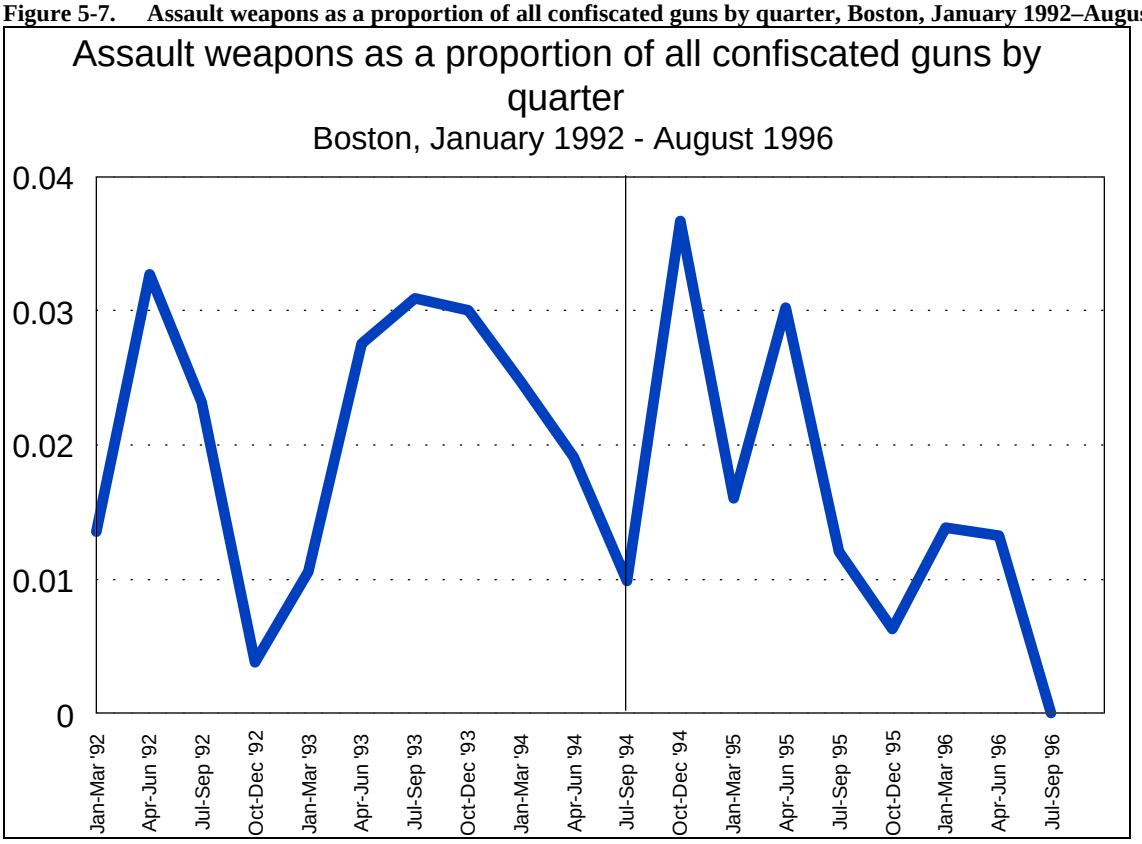
Table 5-10. Summary data on guns confiscated in Boston, January 1992 – August 1996

	Pre-ban ***Jan. '92–Aug. '94)***	***Post-ban*** ***(Sept. '94–Aug. '96)***	***Change***
Total guns confiscated			
Total	2,567	1,273	
Monthly mean	80	53	-34%
Assault guns			
Total	53	21	
Monthly mean	2	1	-50%
Proportion of confiscated guns	.021	.016	-24%
Large-capacity handguns (Ruger and Glock)			
Total	28	17	
Monthly mean	1	1	0%
Proportion of all handguns	.015	.016	+7%

[62] We did not estimate time series models with the Boston data due to the rarity with which assault weapons were confiscated during the study period.

[63] In other analyses, we found that long guns decreased as a proportion of gun confiscations throughout the period, suggesting that there was not substitution of long guns for assault weapons in Boston.

Figure 5-7. Assault weapons as a proportion of all confiscated guns by quarter, Boston, January 1992–August 1996



5.2.3. Assault Weapons and Crime

Using the data from St. Louis, we were able to investigate the types of crimes with which assault weapons were associated. Approximately 12% of the assault weapons seized in St. Louis during the study period were associated with the violent crimes of homicide, aggravated assault, and robbery. Overall, about 12% of all confiscated guns were associated with these crimes. Hence, assault weapons do not appear to be used disproportionately in violent crime relative to other guns in these data, a finding consistent with our conclusions about national BATF trace data (see previous section). Overall, assault weapons accounted for about 1% of guns associated with homicides, aggravated assaults, and robberies.

However, 27% of the assault weapons seized in St. Louis were associated with drug offenses. This figure is notably higher than the 17% of all confiscated guns associated with drug charges.[64] This finding is also consistent with our national trace data analysis showing assault weapons to be more heavily represented among drug offenders relative to other firearms. Nevertheless, only 2% of guns associated with drug crimes were assault weapons.

5.2.4. Unbanned Handguns Capable of Accepting Large-capacity Magazines

We could not directly measure criminal use of pre-ban large-capacity magazines. Therefore, in order to approximate pre-ban and post-ban trends, we examined confiscations of a number of Glock and Ruger handgun models which can accept large-capacity magazines. These guns are not banned by the Crime Act, but they can

[64] Some of the guns associated with drug charges were also tied to weapons charges.

accept banned large-capacity magazines. We selected Glock and Ruger models because they are relatively common in BATF trace data (BATF 1995a, p.35). A caveat to the analysis is that we were not able to obtain data on the magazines recovered with these guns. Consequently, we cannot say whether Glock and Ruger pistols confiscated after the ban were equipped with pre-ban large-capacity magazines. It is also possible that trends corresponding to Glocks and Rugers are not indicative of trends for other unbanned, large-capacity handguns.

As was discussed in Chapter 4 (see the NCIC stolen gun analysis), the hypothesized effects of the ban on this group of weapons is ambiguous. If large-capacity handgun magazines have become less available since the ban as intended (indeed, recall that the magazine price analysis in Chapter 4 indicated that prices of large-capacity magazines for Glock handguns remained at high levels through our last measurement period in the spring of 1996), one might hypothesize that offenders would find large-capacity handguns like Glocks and Rugers to be less desirable, particularly in light of their high prices relative to other handguns. If, on the other hand, large-capacity magazines for these unbanned handguns are still widely available, offenders seeking high-quality rapid-fire capability might substitute them for the banned assault weapons.

With the St. Louis data, we investigated trends in confiscations of all Glock handguns and Ruger P85 and P89 models. Police confiscated 118 of these handguns during the pre-ban months and 93 during the post-ban months (see Table 5-9). The monthly average increased from approximately 4 in the pre-ban months to 6 in the post-ban period. As a fraction of all confiscated handguns, moreover, the Glock and Ruger models rose from .018 before the ban to .031 after the ban, a relative increase of 72%. (These handguns also increased from .037 to .065 — a 76% change — as a fraction of all semiautomatic handguns; thus, the upward trend for these guns was not simply a result of a general increase in the use of semiautomatic handguns). However, Figure 5-8 shows that these handguns were trending upward as a fraction of all handguns well before the ban was implemented. (For this reason, we did not conduct contingency table chi-square tests for the pre-ban and post-ban proportions). Visually, it appears that the ban may have caused this trend to level off. Nevertheless, an interrupted time series analysis failed to provide evidence of a ban effect on the proportion of handguns which were unbanned large-capacity semiautomatics.[65]

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[65] In preliminary analysis, we found that the noise component of this time series was substantially affected by a modest outlier value at the last data point. We were able to estimate a better fitting model with more stable parameters with the outlier removed. After removing this data point (N=47), the final noise component consisted of a moving average parameter at the third lag, autoregressive parameters at lags two and four, and a seasonal autoregressive parameter at the twelfth lag. As in the time series analyses reported elsewhere, we examined a variety of impact models. The most appropriate impact model for the data was an abrupt, permanent impact. The impact parameter was positive (.006) but statistically insignificant (t value=1.13).

Figure 5-8. Unbanned large-capacity handguns as a proportion of all confiscated handguns, St. Louis, 1992–95

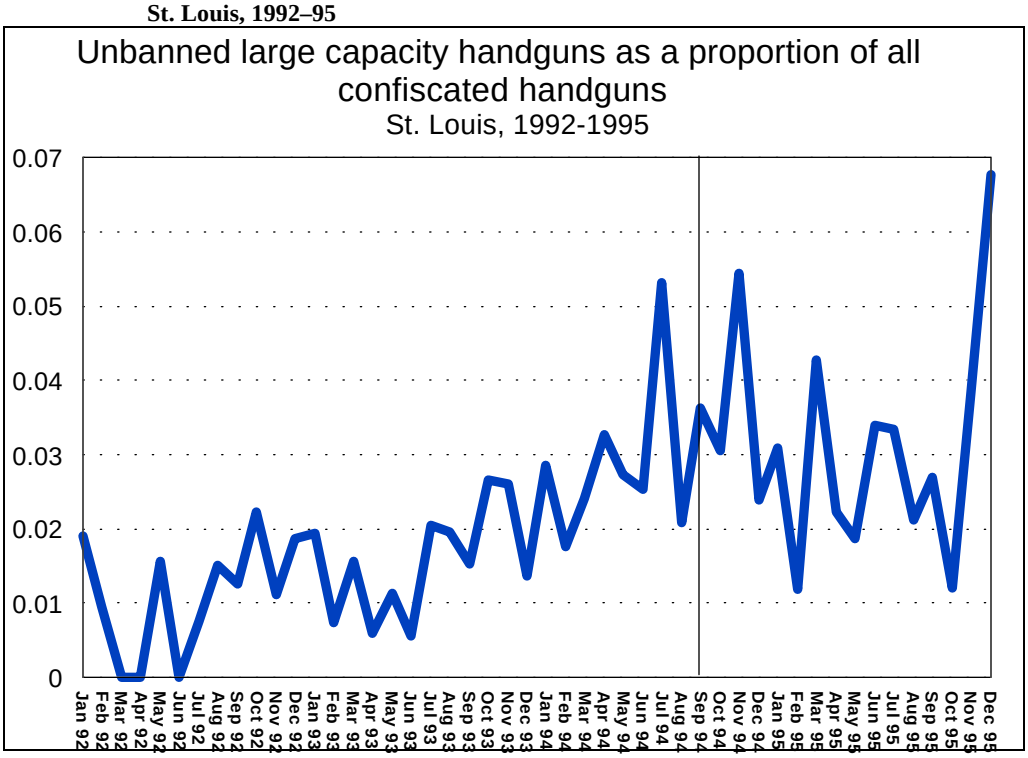
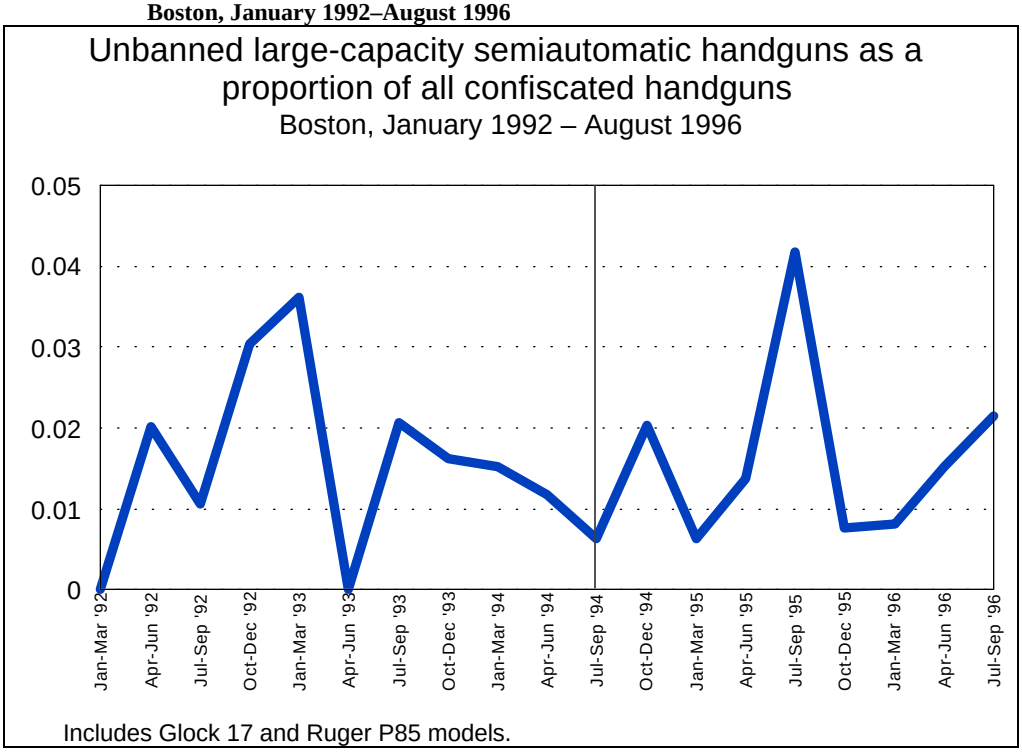


Figure 5-9. Unbanned large-capacity semiautomatic handguns as a proportion of all confiscated handguns, Boston, January 1992–August 1996



The data we acquired from Boston included counts for two specific unbanned, large-capacity handgun models, the Glock 17 and Ruger P85. Police in Boston confiscated 28 of these guns from January 1992 through August of 1994 and 17 from September 1994 through August 1996 (see Table 5-10). As a proportion of all

confiscated handguns, these models increased slightly from .015 before the ban to .016 after the ban. However, a contingency table chi-square test indicated that this difference was not statistically meaningful (p=.83).[66] The quarterly trend for the proportion measure is displayed in Figure 5-8. The pattern does not suggest any meaningful trends over time.[67]

In sum, the data from St. Louis and Boston do not warrant any strong conclusions one way or the other with respect to the use of large-capacity magazines, as crudely approximated by confiscations of a few relatively popular unbanned handgun models which accept such magazines. The ban on large-capacity magazines does not seem to have discouraged the use of these guns. At the same time, the assault weapon ban has not caused a clear substitution of these weapons for the banned large-capacity firearms.

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[66] We did not attempt any time series analyses with these data due to the rarity with which these guns were confiscated in Boston.

[67] A caveat to this analysis is that the Ruger P85 was discontinued in 1992 and replaced with a new version called the P89 (Fjestad 1996, p.996). The P89 was one of the ten most frequently traced guns nationally in 1994 (BATF 1995a, p.35). Unfortunately, we did not acquire data on confiscations of P89's in Boston (the P89 was included in our St. Louis figures). Had we been able to examine P89's in Boston, we may have found a greater increase in the use of unbanned, large-capacity handguns after the ban. Accordingly, the most prudent conclusion from the Boston data may be that there are no signs of a decrease in the use of unbanned, large-capacity handguns.

6. POTENTIAL CONSEQUENCES OF ASSAULT WEAPON USE

The Congressional mandate for this study required us to study how the Subtitle A bans on assault weapons and large-capacity magazines affected two consequences of using those weapons: specifically, violent and drug-related crime. Among violent crimes, we devoted most attention to gun murders, because it is the best measured. However, the total gun murder rate is an insensitive indicator of ban effects, because only a fraction of gun murders involve large-capacity magazines, and only about 25 percent of those murders involve the banned assault weapons. Therefore, we carried out supplementary analyses of certain categories of gun murders that more commonly involve the banned guns and magazines: events that involve multiple gun murder victims, gun murders involving multiple wounds, and killings of law enforcement officers. Unlike the BATF trace data analyzed in Chapter 5, available data sources did not permit us to categorize these events on the basis of relationship to drugs.

6.1. TRENDS IN STATE-LEVEL GUN HOMICIDE RATES

To estimate the impact of the Subtitle A bans on gun homicide rates, we estimated multivariate regression models using data from all states with reasonably consistent Supplementary Homicide Reporting over the sixteen-year period 1980 through 1995. We closely followed the approach used by Marvell and Moody (1995) to analyze the impact of enhanced prison sentences for felony gun use. Marvell and Moody generously provided their database, which we updated to cover the post-ban period.

Any effort to estimate how the ban affected the gun murder rate must confront a fundamental problem, that the maximum achievable preventive effect of the ban is almost certainly too small to detect statistically. Although our statistical model succeeded in explaining 92 percent of the variation in State murder rates over the observation period, a post hoc power analysis revealed that it lacks the statistical power to detect a preventive effect smaller than about 17 percent of all gun murders under conventional standards of statistical reliability.[68] A reduction that large would amount to preventing at least 2.4 murders for every one committed with an assault weapon before the ban, or, alternatively, preventing two-thirds of all gun murders committed with large-capacity magazines — obviously impossible feats given the availability of substitutes for the banned weapons.[69] While there are substantially smaller reductions that would benefit society by more than the cost of the ban, they would be impossible to detect in a statistical sense, at least until the U.S. accumulates more years of post-ban data.

Within this overall constraint, our strategy was to begin with a "first-approximation" estimate of the ban effect on murders, then to produce a series of re-estimates intended to rule out alternative explanations of the estimated effect. Based on these efforts, our best estimate of the short-run effect is that the ban produced a 6.7 percent reduction in gun murders in 1995. However, we caution that for the reasons just explained, we cannot statistically rule out the possibility that no effect occurred. Also, we expect any short-run 1995 preventive effect on gun murders to ebb, then flow, in future years, as the stock of grandfathered assault weapons makes its way to offenders patronizing secondary markets, while the stock of large-capacity magazines dwindles over time.

The following sections first describe our data set, then explain our analyses.

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[68] By conventional standards, we mean statistical power of 0.8 to detect a change, with .05 probability of a Type 1 error.

[69] Moreover, no evidence exists on the lethality effect of limiting magazine capacity.

6.1.1. Data

Data for gun homicides are available for the entire 1980–95 period of the study. We obtained data from "Crime in the United States" Uniform Crime Reports for the years 1994 and 1995, and from Marvell and Moody for the years 1980 through 1993. (Marvell and Moody used "Crime in the United States" Uniform Crime Reports for years 1991 to 1993, and unpublished data from the FBI for the earlier years.)

Since the fraction of homicides for which weapon use was reported by states varied from state to state and even year to year over the period, it was necessary to adjust and filter the data. To address this reporting problem, we adopted Marvell and Moody's (1995) approach to compile what they call a "usable" data series, consisting of observations (each year for each state) for which homicide weapon-use reporting is at least 75 percent complete (See Marvell and Moody, 1995).[70] On this basis we had to eliminate a certain portion of the gun homicide data (see Table 6-2) For each observation that met this requirement, the number of gun homicides was multiplied by a correction factor defined as the ratio of the FBI estimate for the total number of reported homicides in the state to the number of homicides for which the state reported weapon data.

We used Marvell and Moody's rule of retaining states in the analysis only if they had data for seven or more consecutive years[71] and added the additional requirement that states must have had gun homicide data for the post-intervention year, 1995. (This additional requirement caused us to eliminate four states entirely from the analysis: Delaware, Kansas, Nebraska, and New Mexico.) In addition, Marvell and Moody made allowances for otherwise adequate seven-year series that contained a single year of data that did not meet the above requirements. Provided the reporting rate was at least 50 percent and the corrected figure did not "depart greatly"[72] from surrounding years, the state was not dropped from the analysis. (These are: Louisiana 1987, South Carolina 1991, Tennessee 1991, and Wyoming 1982.) A further allowance was, that if the reporting rate was below 50 percent, or if the adjusted number did depart from surrounding years, the percentage of gun homicides was revised as the average of that for the four surrounding years. (These are: Alaska 1984, Arizona 1989, Idaho 1991, Iowa,1987, Kentucky 1983, Maryland 1987, Minnesota 1990, North Dakota 1991, Texas 1982, and Vermont, 1993.) In the end, "usable data" remained for 42 states for the analysis (see Table 6-2).

To allow us to account for intervening influences on gun homicide rates, we gathered data for several time-varying control variables that proved statistically significant in Marvell and Moody's analysis. Two economic variables (state per capita personal income and state employment rate) and two age structure variables were included. State per capita personal income was available from the Bureau of Economic Analysis for all years; we obtained data for 1991–95 directly from the Department of Commerce, while Marvell and Moody provided us the data for earlier years. State employment rates were available from the Bureau of Labor Statistics, Department of Labor for 1994 and 1995 and from the Bureau of Economic Analysis (via Marvell and Moody) for year 1980–93. Data on the age structures of state populations were available from the Bureau of the Census

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[70] An alternative approach would have been to use mortality data available from the National Center for Health Statistics through 1992, then to append NCR data for the subsequent years. We were concerned about possible artifactual effects of combining medical examiners' and police data into a single time series, but recommend this approach for future replication.

[71] However, we departed from Marvell and Moody by including observations for years that followed a gap in a series of "usable" data and were therefore not part of a seven-year string. The state was treated as a missing observation during the gap.

[72] According to Marvell and Moody, a single year of data does not "depart greatly" from surrounding years if either the percentage of gun murders falls within the percentages for the prior and following years, or if it is within three percentage points of the average of the four closest years.

unadjusted estimates of total resident population of each state as of July 1 of each year. (We obtained these data directly for years 1994–95, while Marvell and Moody generously provided us with the data for earlier years).

6.1.2. Research Design

As a first approximation for estimating effects of the assault weapon ban, we specified Model 1 as loglinear in state gun homicide rate (adjusted as described above) and a series of regressors.[73] The regressors were:

- A third-degree polynomial trend in the logarithm of time;
- A dummy variable for each state;
- State per-capita income and employment rates for each year (logged);
- Proportions of the population aged 15-17 and 18-24 (logged);
- D95, a 1995 dummy variable, which represented ban effects in this first-approximation model; and
- PREBAN, a dummy variable set to represent states with assault weapon bans during their pre-ban years.

We represented time with the polynomial trend instead of a series of year dummies for two reasons. First, by reducing the number of time parameters to estimate from 15 to 3, we improved statistical efficiency. Second, during sensitivity analyses after Model 1 was fit, we discovered that it produced more conservative estimates of ban effects than a model using time dummies (that model implicitly compares 1995 levels to 1994 levels instead of to the projected trend for 1995), because the estimated trend began decreasing at an increasing rate in the most recent years. We included the economic and demographic explanatory variables because Marvell and Moody (1995) had found them to be significant influences on state-level homicide rates using the same data set. PREBAN was included so that for states with their own assault weapon bans, the D95 coefficient would reflect differences between 1995 and only those earlier years in which the state's gun ban was in place.

As shown in Table 6-1, Model 1 estimated a 9.0 percent reduction in gun murder rates in the year following the Crime Act, based on a statistically significant estimated coefficient for the 1995 dummy variable.[74] This estimated coefficient, of course, reflects the combined effect of a package of interventions that occurred nearly simultaneously with the Subtitle A bans on assault weapons and large-capacity magazines. These include: the Subtitle B ban on juvenile handgun possession and the new Subtitle C FFL application and reporting requirements, other Crime Act provisions, the Brady Act, and a variety of State and local initiatives.

We reasoned that if the Model 1 estimate truly reflected assault weapon ban effects, then by disaggregating the states we would find a larger reduction in gun murders in the states without pre-existing assault weapon bans than in the four states with such bans prior to 1994 (California, Connecticut, Hawaii, and New Jersey). To test this hypothesis, we estimated Model 2, in which D95 was replaced by two interaction terms that indicated whether or not a State ban was in place in 1995. As shown in Table 6-1, disaggregating the states using

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[73] We weighted the regression by state population to adjust for heteroskedasticity and to avoid giving undue weight to small states.

[74] In our sensitivity analyses of models in which the polynomial time trend was replaced with year dummies, the corresponding Model 1 estimated reduction was 11.2 percent, and the estimated coefficient was statistically significant at the .05 level. Similarly, for alternatives to Models 2-4, the estimated ban effects were 2 to 3 percent larger than those shown in Table 6-1 and were statistically significant at the .05 level.

Model 2 did produce a larger estimated ban effect, a statistically significant reduction of 10.3 percent in the states without their own bans.

Table 6-1. Estimated Coefficients and Changes in Gun Murder Rates from Title XI Interventions

Model	*Subgroup for 1995 impact*	*Coefficient*	*Percent change*	*test statistic*
1	All Usable (N = 42)	-0.094 +	-9.0%	-1.67
2	States without AW ban (N = 38)	-0.108 +	-10.3	-1.88
	States with AW ban (N = 4)	-0.001	-0.1	-0.01
3	States without AW or JW ban (N = 22)	-0.102	-9.7	-1.56
	States without AW, with JW ban (N = 16)	-0.115	-10.9	-1.64
	States with AW, without JW ban (N = 2)	-0.076	-7.3	-0.41
	States with AW and JW ban (N = 2)	0.044	4.5	0.39
4	California and New York excluded: States without AW or JW ban (N = 22)	-0.103	-9.8	-1.58
	States without AW, with JW ban (N = 15)	-0.069	-6.7	-0.95
	States with AW, without JW ban (N = 2)	-0.079	-7.6	-0.43
	States with AW and JW ban (N = 1)	0.056	5.8	0.30

+ Statistically significant at 10-percent level

To isolate the hypothesized Subtitle A bans from the Subtitle B ban on juvenile handgun possession, we estimated Model 3, in which D95 was used in four interaction terms with dummy variables indicating whether a state had its own assault weapon ban, juvenile handgun possession ban, both, or neither at the time of the Crime Act.[75] We also added a term, PREJBAN, which represented states with juvenile bans during their pre-ban years, for reasons analogous to the inclusion of PREBAN. The estimates of most interest are those for the 38 states without their own assault weapon bans. Among those, the estimated ban effect was slightly larger in states that

[75] A more restrictive alternative to Model 3 is based on the assumption that the impacts for states without assault weapon bans and the impacts for states without juvenile handgun possession bans are additive. A model estimate under this assumption yielded very similar point estimates and slightly smaller standard errors than Model 3. We preferred the more flexible Model 3 for two reasons. First, the less restrictive model helps us interpret the estimates clearly in light of some of the legislative changes that occurred in late 1994. Model 3 allows the reader to assess the consequences of the assault weapon ban under each set of conditions that existed at the time the ban was implemented. Second, because a juvenile handgun possession ban a fortiori prohibits the most crime-prone segment of the population from possessing the assault weapons most widely used in crime, we hesitated to impose an additivity assumption.

already had a juvenile handgun possession ban than in those that did not. We interpret the former estimate as a better estimate of the assault weapon ban effect because the State juvenile ban attenuates any confounding effects of the Federal juvenile ban. In any event, however, the estimates are not widely different, and they imply a reduction in the 10 to 11 percent range.

We were also concerned that our estimates might be distorted by the effects of relevant State and local initiatives. Therefore, we reestimated Model 3 excluding 1995 data for California and New York. We filtered out these two because combined they account for nearly one-fourth of all U.S. murders and because they were experiencing potentially relevant local interventions at the time of the ban: California's "three strikes" law and New York City's "Bratton era" in policing, coming on the heels of several years of aggressive order maintenance in that city's subway system.

The estimation results with California and New York omitted appear as Model 4 in Table 6-1. While dropping these states leaves three of the estimated coefficients largely unaffected, it has a substantial effect on New York's category, states with a juvenile handgun possession ban but no assault weapon ban. The estimated ban effect in this category drops from a nearly significant 10.9 percent reduction to a clearly insignificant 6.7 percent reduction, which we take as our best estimate.

To conclude our study of state-level gun homicide rates, we performed an auxiliary analysis. We were concerned that our Model 4 estimate of 1995 ban effects could be biased by failure to control for the additional requirements on FFL applicants that were imposed administratively by BATF in early 1994 and included statutorily in Subtitle C of Title XI, which took effect simultaneously with the assault weapon ban. These requirements were intended to discourage new and renewal applications by scofflaw dealers who planned to sell guns primarily to ineligible purchasers presumed to be disproportionately criminal. Indeed, they succeeded in decreasing the number of FFLs by some 37 percent during 1994 and 1995, from about 280,000 to about 180,000 (U.S. Department of Treasury, 1997). We were concerned that if the FFLs who left the formal market during that period were disproportionately large suppliers of guns to criminals, then failure to control for their disappearance could cause us to impute any resulting decrease in gun murder rates mistakenly to the Subtitle A ban.

Unfortunately, we could use only the 1989–95 subset of our database to test this possibility, because we could not obtain state-level FFL counts for years before 1989. Therefore, we modified Model 4 by replacing the time trend polynomial with year dummies. We then estimated the modified Model 4 both with and without a logged FFL count and an interaction term between the logged count and a 1994–95 dummy variable. Although the estimated coefficient on the interaction term was significantly negative, the estimated 1995 ban effect was essentially unchanged.

Table 6-2. Years for which gun-related homicide data are not available

	Gun homicide data 1980–95
Alabama	✔
Alaska	✔
Arizona	✔
Arkansas	✔
California	✔
Colorado	✔
Connecticut	✔

	Gun homicide data 1980–95
Delaware	No usable data
District of Columbia	No usable data
Florida	1988–91
Georgia	1980–81
Hawaii	✔
Idaho	✔
Illinois	No usable data
Indiana	1989–1991
Iowa	1991–1993
Kansas	No usable data
Kentucky	1987-89; 1994
Louisiana	1990–91
Maine	1990–92
Maryland	✔
Massachusetts	1988–90
Michigan	✔
Minnesota	✔
Mississippi	No usable data
Missouri	✔
Montana	No usable data
Nebraska	No usable data
Nevada	✔
New Hampshire	✔
New Jersey	✔
New Mexico	No usable data
New York	✔
North Carolina	✔
North Dakota	1994
Ohio	✔
Oklahoma	✔
Oregon	✔

	Gun homicide data 1980–95
Pennsylvania	✔
Rhode Island	✔
South Carolina	✔
South Dakota	No usable data
Tennessee	✔
Texas	✔
Utah	✔
Vermont	1980-83
Virginia	✔
Washington	✔
West Virginia	✔
Wisconsin	✔
Wyoming	✔
✔ indicates usable data are available for all years (1980–95) in the period	

6.2. ASSAULT WEAPONS, LARGE-CAPACITY MAGAZINES, AND MULTIPLE VICTIM/MASS MURDERS

6.2.1. Trends in Multiple-Victim Gun Homicides

The use of assault weapons and other firearms with large-capacity magazines is hypothesized to facilitate a greater number of shots fired per incident, thus increasing the probability that one or more victims are hit in any given gun attack. Accordingly, one might expect there to be on average a higher number of victims per gun homicide incident for cases involving assault weapons or other firearms with large-capacity magazines. To the extent that the Crime Act brought about a permanent or temporary decrease in the use of these weapons (a result tentatively but not conclusively demonstrated for assault weapons in Chapter 5), we can hypothesize that the number of victims per gun homicide incident may have also declined.

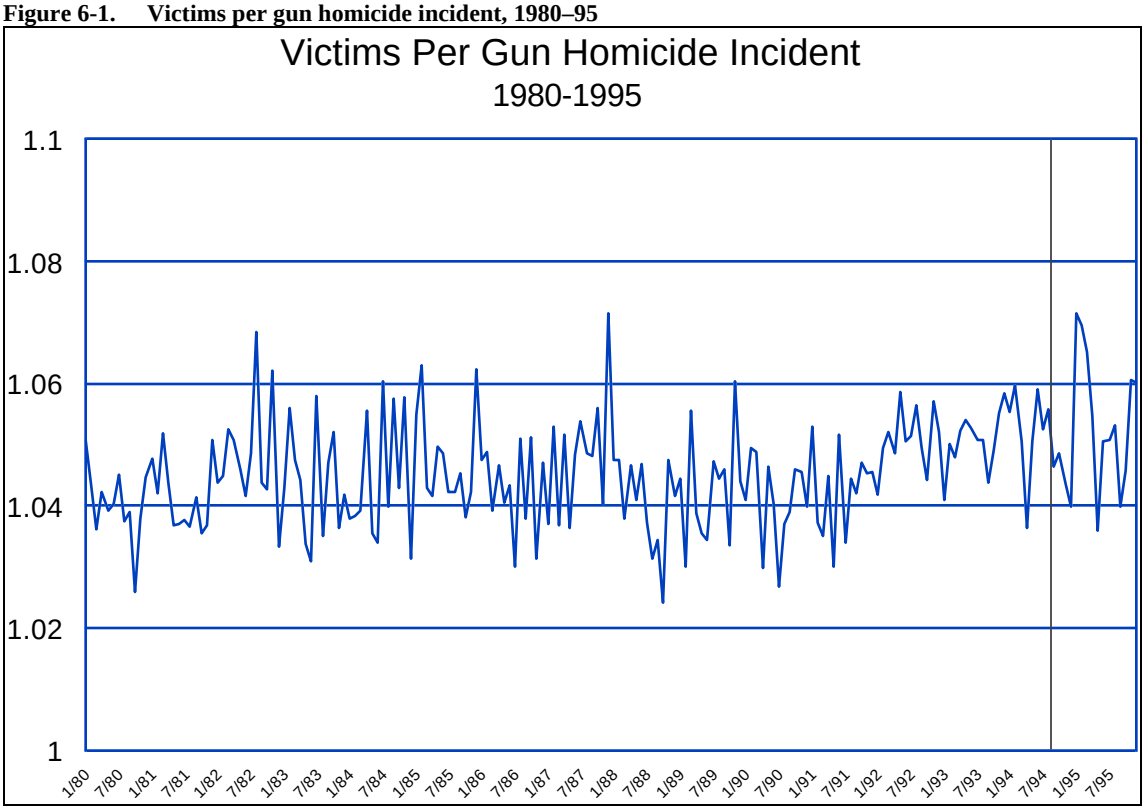
We investigated this hypothesis using data from the Federal Bureau of Investigation's Supplemental Homicide Reports (SHR) for the years 1980 through 1995. We constructed a monthly database containing the number of gun homicide incidents and victims throughout the nation.[76] The SHR does not contain information

[76] The SHR is compiled annually by the FBI based on homicide incident reports submitted voluntarily by law enforcement agencies throughout the country (see the FBI's *Uniform Crime Reports* for more information about reporting to the Uniform Crime Reports and the Supplemental Homicide Reports). Though the SHR contains data on the vast majority of homicides in the nation, not all agencies report homicide incident data to the SHR, and those agencies which do report may fail to report data for some of the homicides in their jurisdiction. In this application, it is not clear how any potential bias from

about the makes, models, and magazine capacities of firearms used in homicides. Consequently, these results rely on indirect, inferred links between expected changes in the use of banned weapons and trends in the victim per incident measure.

From 1980 through August of 1994 (the pre-ban period), there were 184,528 gun homicide incidents reported to the SHR. These cases involved 192,848 victims, for an average of 1.045 victims per gun homicide incident. For the post-ban months of September 1994 through December 1995, there were 18,720 victims killed in 17,797 incidents, for an average of 1.052 victims per incident. Thus, victims per incident increased very slightly (less than 1 percent) after the Crime Act. A graph of monthly means presented in Figure 6-1 suggests that this increase predated the assault weapon ban. Nevertheless, an interrupted time series analysis also failed to produce any evidence that the ban reduced the number of victims per gun homicide incident.[77]

Figure 6-1. Victims per gun homicide incident, 1980–95



Considering the rarity with which assault weapons are used in violent crime (for example, assault weapons are estimated to be involved in 1 to 7 percent of gun homicides),[78] this result is not unexpected. At the same time, an important qualifier is that the data available for this study have not produced much evidence regarding pre-ban/post-ban trends in the use of large-capacity magazines in gun crime. In the next section, we offer a tentative estimate, based on one city, that approximately 20 to 25 percent of gun homicides are committed

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missing cases would operate. That is, we are unaware of any data indicating whether reported and non-reported cases might differ with respect to the number of victims killed.

[77] We tested the data under different theories of impact suggested by the findings on assault weapon utilization reported in Chapter 5, but failed to find evidence of a beneficial ban effect. If anything, our time series analysis suggested that the post-ban increase in victims per gun murder incident was a meaningful change.

[78] See discussion in Chapters 2 (p.8) and 5 (p.58) and in Section 6.3 (p.87) of this chapter.

with gun equipped with large-capacity magazines banned by the Crime Act.[79] Hence, trends in the use of large-capacity magazines would seem to have more potential to produce measurable effects on gun homicides. It is not yet clear as to whether the use of large-capacity magazines has been substantially affected by the Crime Act.

Despite these ambiguities, we can at least say that this examination of SHR data produced no evidence of short term decreases in the lethality of gun violence as measured by the mean number of victims killed in gun homicide incidents.[80]

6.3. CONSEQUENCES OF TITLE XI: MULTIPLE WOUND GUN HOMICIDES

To provide another measure of the consequences of the assault weapon/large-capacity magazine ban on the lethality of gun violence, we analyzed trends in the mean number of gunshot wounds per victim of gun homicides in a number of sites. In one jurisdiction, we were able to examine trends in multiple wound non-fatal gunshot cases. The logic of these analyses stems from the hypothesis that offenders with assault weapons or other large-capacity firearms can fire more times and at a more rapid rate, thereby increasing both the probability that they hit one or more victims and the likelihood that they inflict multiple wounds on their victims. One manifestation of this phenomenon could be a higher number of gunshot wounds for victims of gun homicides committed with assault weapons and other large-capacity firearms. To the extent that Title XI decreased the use of assault weapons and large-capacity magazines, we hypothesize a decrease in the average number of wounds per gun murder victim.

To test this hypothesis, we collected data from police and medical sources on gunshot murders (justifiable homicides were excluded) in Milwaukee County, Seattle and King County, Jersey City (New Jersey), Boston, and San Diego County. Selection of the cities was based on both data availability and theoretical relevance. Jersey City and San Diego were chosen as comparison series for the other cities because New Jersey and California had their own assault weapons bans prior to the Federal ban. The New Jersey and California laws did not ban all large-capacity magazines, but they did ban several weapons capable of accepting large-capacity magazines. Thus, we hypothesized that any reduction in gunshot wounds per gun homicide victim due to the Federal ban might be smaller in magnitude in Jersey City and San Diego.

The data from Seattle and San Diego were collected from the respective medical examiners' offices of those counties.[81] The Milwaukee data were collected from both medical and police sources by researchers at the Medical College of Wisconsin. The Jersey City data were collected from the Jersey City Police Department. Finally, the Boston data were provided by the Massachusetts Department of Public Health. From each of these sources, we were able to collect data spanning from January 1992 through at least the end of 1995. In some cities we were able to obtain data on the actual number of gunshot wounds inflicted upon victims, while in other cities we were able to classify cases only as single wound or multiple wound cases. Depending on data available, we analyzed pre-ban and post-ban data in each city for either the mean number of wounds per victim or the proportion

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[79] A New York study estimated this figure to be between 16 percent and 25 percent (New York State Division of Criminal Justice Services 1994, p.7).

[80] See Appendix A for an investigation of assault weapon use in mass murders.

[81] The Seattle data were collected for this project by researchers at the Harborview Injury Prevention and Research Center in Seattle. The San Diego County Medical Examiner's Office provided data from San Diego.

of victims with multiple wounds. We concluded this investigation with an examination of the mean number of gunshot wounds for victims killed with assault weapons and other firearms with large-capacity magazines, based on data from one city.

6.3.1. Wounds per Incident: Milwaukee, Seattle, and Jersey City

From the Milwaukee, Seattle, and Jersey City data, we were able to ascertain the number of gunshot wounds suffered by gun murder victims. Relevant data comparing pre-ban and post-ban cases are displayed in Table 6-3. The average number of gunshot wounds per victim did not decrease in any of these three cities. Gunshot wounds per victim actually increased in all these cities, but these increases were not statistically significant.[82] [83]

Table 6-3. Gunshot wounds per gun homicide victim, Milwaukee, Seattle, and Jersey City

	Cases	*Average*	*Standard deviation*	*T value*	*P level*
Milwaukee County (N = 418)					
Pre-ban: January '92 - August '94	282	2.28	2.34		
Post-ban: September '94 - December '95	136	2.52	2.90		
Difference		+ 0.24		0.85*	.40
Seattle and King County (N = 275)					
Pre-ban: January '92 - August '94	184	2.08	1.78		
Post-ban: September '94 - June '96	91	2.46	2.22		
Difference		+ 0.38		1.44*	.15
Jersey City (N =44)					
Pre-ban: January '92 - August '94	24	1.58	1.56		
Post-ban: September '94 - May '96	20	1.60	1.79		
Difference		+ 0.02		0.03	.97

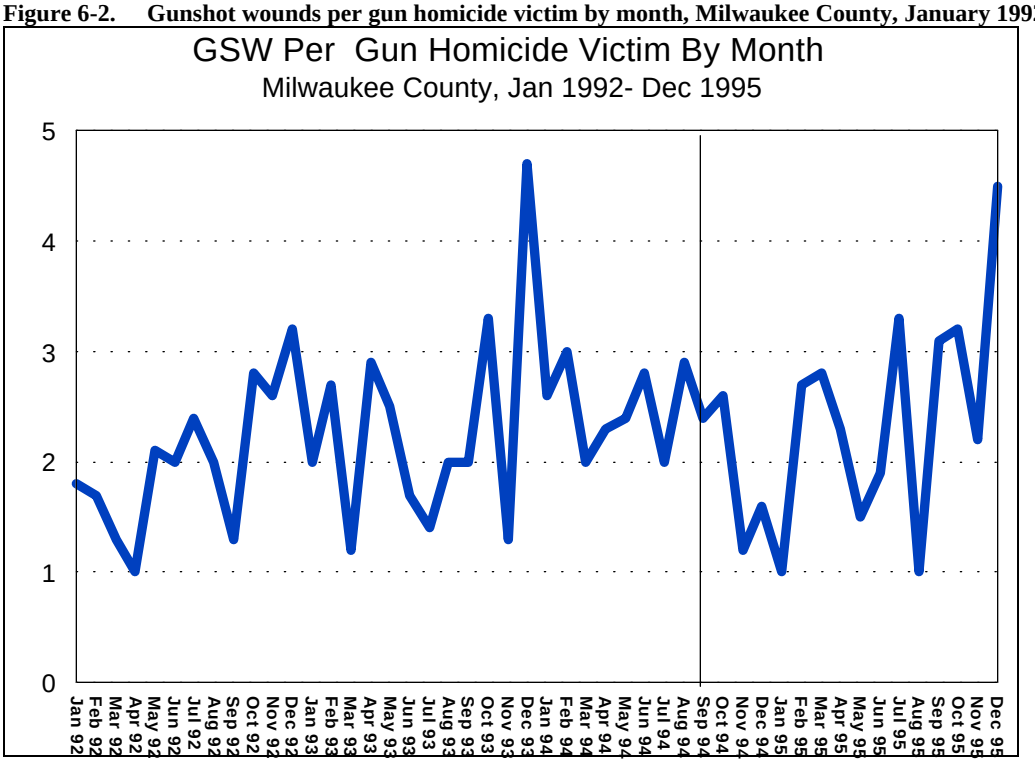
* T values were computed using formula for populations having unequal variances

[82] Our comparisons of pre-ban and post-ban cases throughout this section are based on the assumption that the cases in each sample are independent. Technically, this assumption may be violated by incidents involving multiple victims and/or common offenders. Violation of this assumption has the practical consequence of making test statistics larger, thus making it more likely that differences will appear significant. Since the observed effects in these analyses are insignificant and usually in the wrong direction, it does not appear that violation of the independence assumption is a meaningful threat to our inferences.

[83] We also ran tests comparing only cases from 1993 (the last full year prior to passage and implementation of Title XI) and 1995 (the first full year following implementation of Title XI). These tests also failed to yield evidence of a post-ban reduction in the number of wounds per case.

Time trends in the monthly average of wounds per victim for Milwaukee and Seattle are displayed in Figure 6-2 and Figure 6-3. Figure 6-4 presents quarterly time trends for Jersey City. None of the graphs provide strong visual evidence of trends or changes in trends associated with the implementation of Title XI, but the Milwaukee and Seattle graphs are somewhat suggestive of upward pre-ban trends that may have been affected by the ban. We made limited efforts to estimate interrupted time series models (McCleary and Hay 1980) for these two series. The Milwaukee model provided no evidence of a ban effect,[84] and the efforts to model the Seattle data were inconclusive.[85] Because the ban produced no effects in Milwaukee or Seattle, it was not necessary to draw inferences about Jersey City as a comparison site.

Figure 6-2. Gunshot wounds per gun homicide victim by month, Milwaukee County, January 1992–December 1995



[84] We tested the Milwaukee data under various theories of impact but failed to find evidence of an effect from the ban.

[85] The Seattle data produced an autocorrelation function (see McCleary and Hay 1980) that was uninterpretable, perhaps as a result of the small number of gun murders per month in Seattle. Aggregating the data into larger time periods (such as quarters) would have made the series substantially shorter than the 40-50 observations commonly accepted as a minimum number of observations necessary for Box-Jenkins (i.e., ARIMA) modeling techniques (e.g., see McCleary and Hay 1980, p.20).

Figure 6-3. Gunshot wounds per gun homicide victim by month, King County (Seattle), January 1992–June 1996

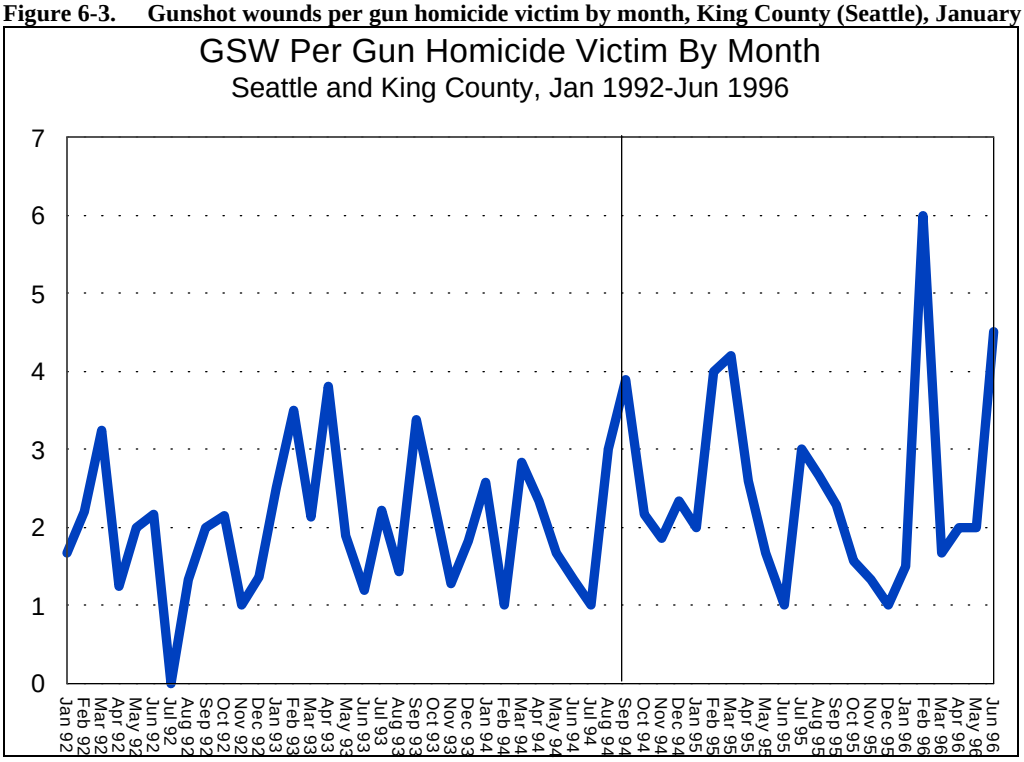
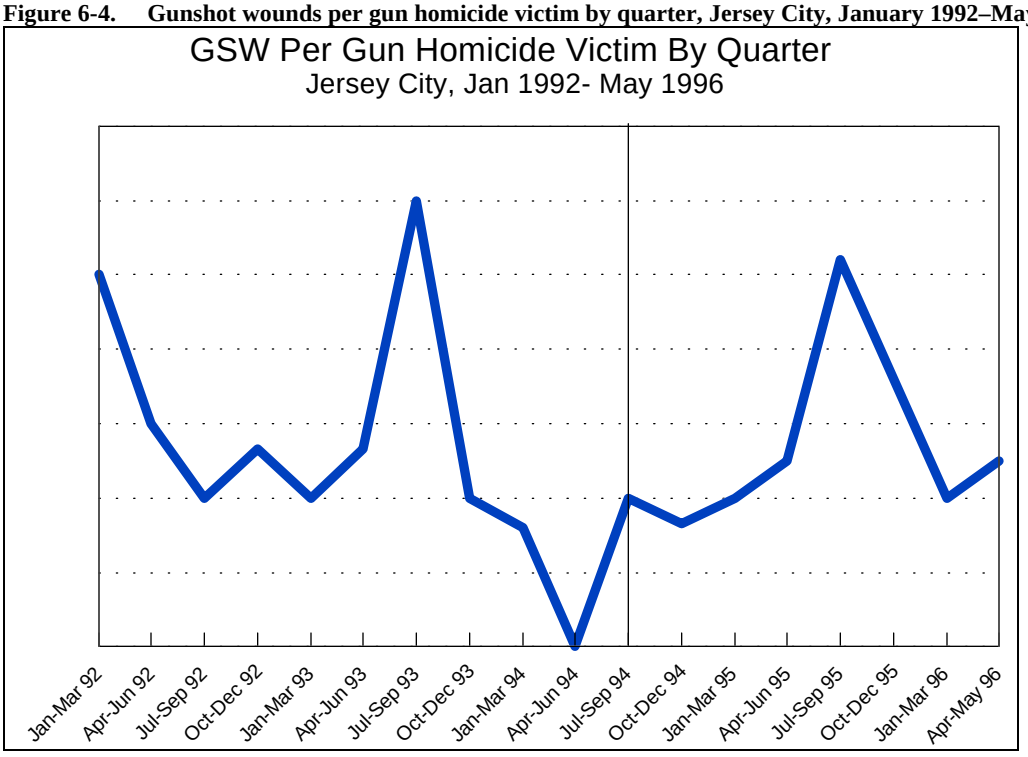


Figure 6-4. Gunshot wounds per gun homicide victim by quarter, Jersey City, January 1992–May 1996

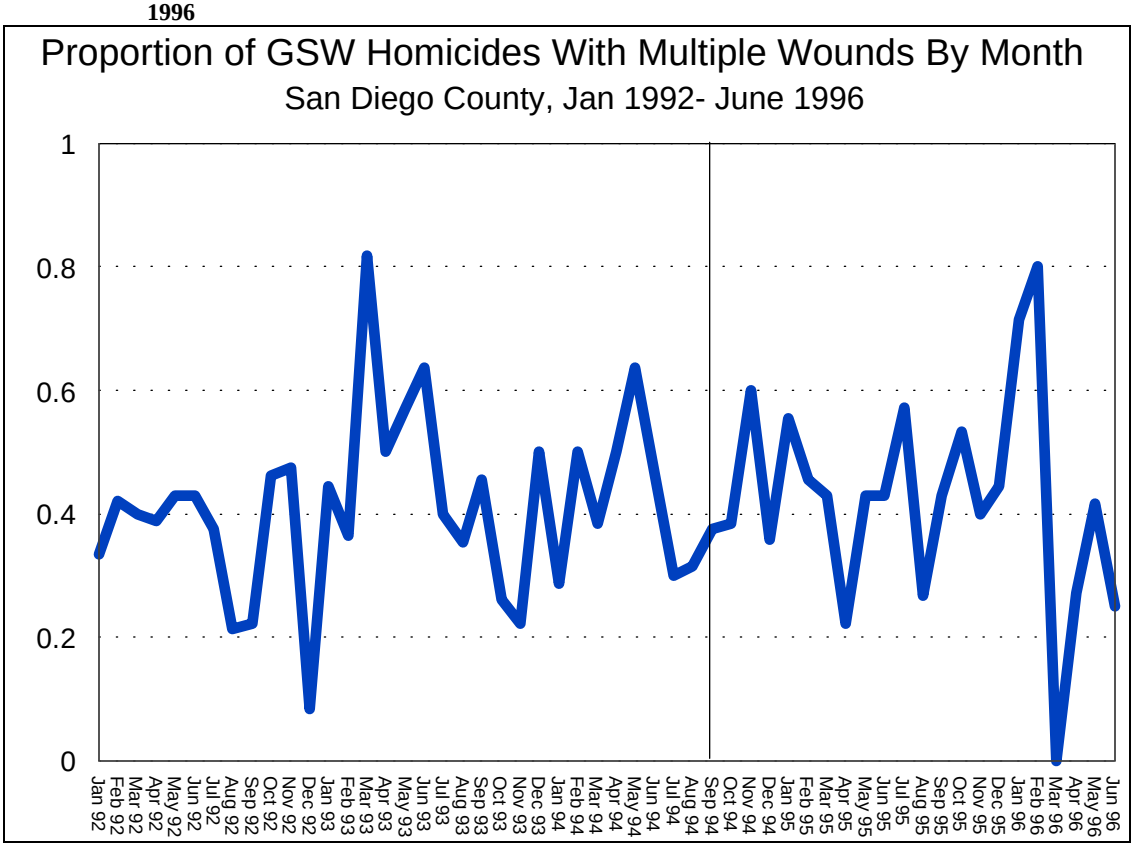


6.3.2. Proportion of Cases With Multiple Wounds: San Diego and Boston

The data from San Diego and Boston identified cases only as being single or multiple wound cases. We examined the proportions of pre-ban and post-ban cases involving multiple wounds and utilized contingency tables with chi-square tests to determine whether pre-ban and post-ban cases differed significantly.[86]

The proportion of San Diego County's gun homicide victims sustaining multiple wounds increased very slightly after the ban (see Table 6-4), thus providing no evidence of a ban impact. Nor do there appear to have been any significant temporal trends before or after the ban (see Figure 6-5).

Figure 6-5. Proportion of gunshot homicides with multiple wounds by month, San Diego County, January 1992–June 1996



The Boston data require further explanation and qualification. The data were taken from the Weapon-Related Injury Surveillance System (WRISS) of the Massachusetts Department of Public Health (MDPH). WRISS tracks gunshot and stabbing cases treated in acute care hospital emergency departments throughout the state.[87] These data have the unique advantage of providing trends for non-fatal victimizations, but they represent a biased sample of gunshot homicide cases because gun homicide victims found dead at the scene are not tracked by WRISS.[88] Since multiple wound victims can be expected to have a greater chance of dying at the scene, WRISS

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[86] Monthly and quarterly averages in the fraction of cases involving multiple wounds did not appear to follow discernible time trends for any of these series (see Figure 6-5 through Figure 6-8). Therefore, we did not analyze the data using time series methods.

[87] For a discussion of error rates in the determination of wound counts by hospital staff, see Randall (1993).

[88] The MDPH also maintains a database on all homicide victims, but this database does not contain single/multiple wound designations and data for 1995 are not complete as of this writing.

data are likely to underestimate the fraction of gun homicide victims with multiple wounds. While it is possible that this bias has remained constant over time, the gun homicide trends should be treated cautiously.

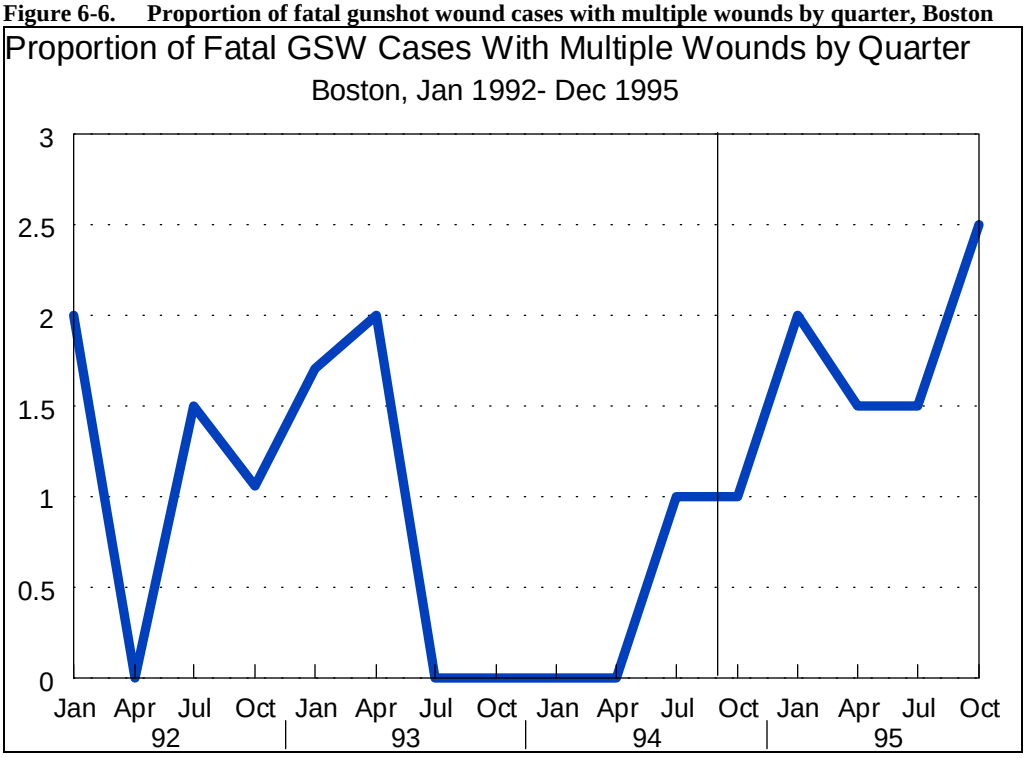
Table 6-4. Proportion of gunshot victims receiving multiple wounds, San Diego and Boston

	Cases	*Proportion with multiple wounds*	*Standard deviation*
San Diego homicides (N = 668)			
Pre-ban: January '92 - August '94	445	.41	.49
Post-ban: September '94 - June '96	223	.43	.50
Difference		.02	
ξ^2 *= 0.177*			
P level = .674			
Boston Gun homicides (N = 53)			
Pre-ban: January '92 - August '94	32	.50	.50
Post-ban: September '94 - December '95	21	.38	.50
Difference		-.12	
ξ^2 *= 0.725*			
P level = .39			
Boston non-fatal gunshot victims (N = 762)			
Pre-ban: January '92 - August '94	518	.18	.39
Post-ban: September '94 - December '95	244	.24	.43
Difference		.06	
ξ^2 *= 3.048*			
P level = .08			
Boston total gunshot victims (N = 815)			
Pre-ban: January '92 - August '94	550	.20	.40
Post-ban: September '94 - December '95	265	.27	.44
Difference		.07	
ξ^2 *= 4.506*			
P level = .03			

An additional concern with WRISS data is that system compliance is not 100 percent. Based on figures provided by MDPH, yearly hospital reporting rates in Boston during the study period were as follows: 63 percent for 1992; 69 percent for 1993; 75 percent for 1994; and 79 percent for 1995. It is thus possible that gunshot cases treated in non-reporting hospitals differ significantly from those treated in reporting hospitals with respect to single/multiple wound status. For all of these reasons, the Boston data should be interpreted cautiously. Overall, the WRISS captured 18 to 33 percent of Boston's gun homicides for the years 1992–94.

Pre-ban/post-ban comparisons for fatal, non-fatal, and total gunshot cases from WRISS are presented in Table 6-4. The proportion of multiple wound cases decreased only for gun homicides. This decrease was not statistically significant, but the sample sizes were very small and thus the statistical power of the test is rather low. Nonetheless, the non-fatal wound data, which are arguably less biased than the fatal wound data, show statistically meaningful increases in the proportion of cases with multiple wounds.[89] Figure 6-6 through Figure 6-8 present monthly or quarterly trends for each series. These trends fail to provide any visual evidence of a post-ban reduction in the proportion of multiple wound gunshot cases.[90] Thus, overall, the Boston data appear inconclusive.

Figure 6-6. Proportion of fatal gunshot wound cases with multiple wounds by quarter, Boston



[89] Further, the decrease for homicide cases could have been due to an increase in the proportion of multiple wound victims who died at the scene and were not recorded in the WRISS.

[90] As with the Milwaukee and Seattle data, we also ran supplemental tests with the San Diego and Boston data using only cases from 1993 and 1995. These comparisons also failed to produce evidence of post-ban reductions in the proportion of gunshot cases with multiple wounds.

Figure 6-7. Proportion of non-fatal gunshot wound cases with multiple wounds by month, Boston, January 1992–December 1995

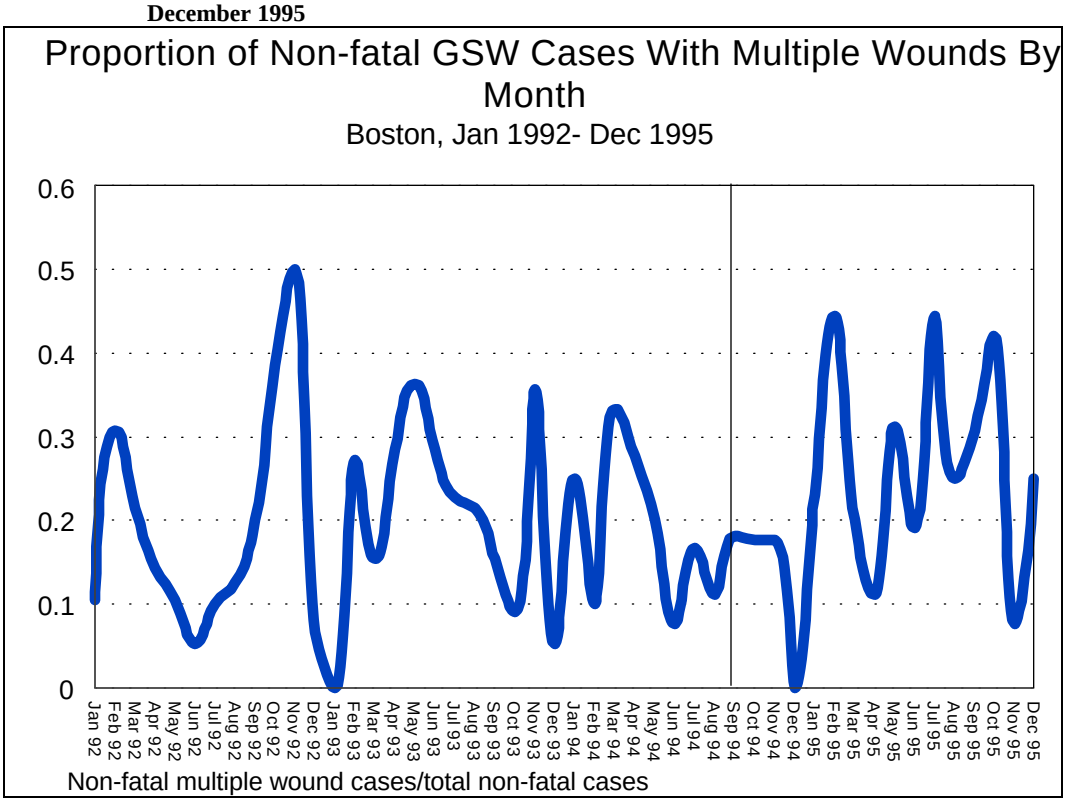
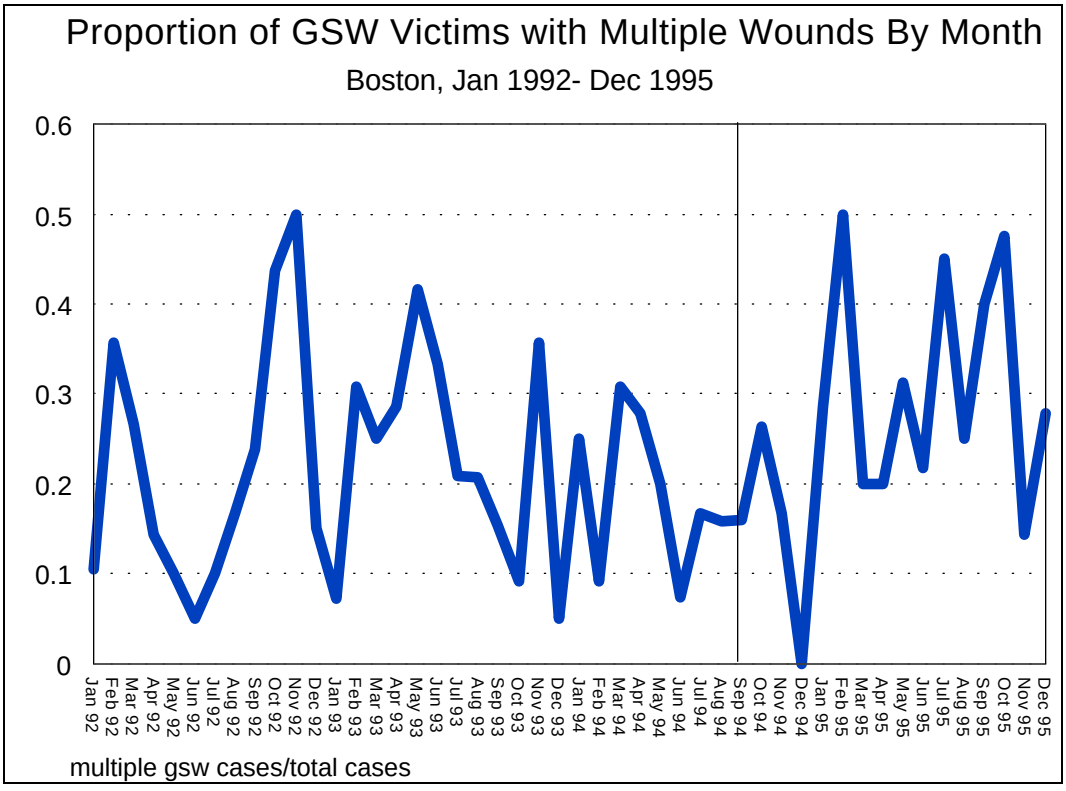


Figure 6-8. Proportion of gunshot wound victims with multiple wounds by month, Boston, January 1992–December 1995



6.3.3. Assault Weapons, Large-Capacity Magazines, and Multiple Wound Cases: Milwaukee

Most of the data sources used in this investigation contain little or no detailed information regarding weapon makes and models. Consequently, the validity of the previous analyses rest on indirect, inferred links between multiple wound gun homicides and expected changes in the use of assault weapons and large-capacity magazines.

However, we were able to make more explicit links between the banned weapons and gunshot wound counts by performing a cross-sectional analysis with the data from Milwaukee. Complete weapon make and model data were obtained for 149 guns associated with the 418 gun murders which occurred in Milwaukee County from 1992 through 1995. Eight of these firearms, or 5.4 percent, were assault weapons named in Title XI or copies of firearms named in Title XI (all of the assault weapons were handguns).[91] Table 6-5 shows the mean number of wounds for gun homicide victims killed with assault weapons and other guns. Note that in Table 6-5 we screened out two cases in which the victim appeared to have been shot with multiple firearms. One of these cases involved an assault weapon. The results in Table 6-5 indicate that victims killed with assault weapons were shot a little over three times on average, while victims killed with other firearms were shot slightly over two times on average. This difference was not statistically significant, but the small number of cases involving assault weapons makes the test rather weak.

Table 6-5. Gunshot wounds per gun homicide victim: Assault weapon and large-capacity magazine cases, Milwaukee

	Cases	*Average*	*Standard deviation*	*T value*	*P level*
Assault weapons v. other firearms (N = 147)					
Assault weapons	7	3.14	3.08		
Other firearms	140	2.21	2.87		
Difference		0.93		0.83	.41
Firearms with banned large-capacity magazines v. other firearms (N = 132)					
Large-capacity firearms	30	3.23	4.29		
Other firearms	102	2.08	2.48		
Difference		1.15		1.41*	.17

*T values were computed using formula for populations having unequal variances.

We also conducted a more general examination of cases involving any firearm with a large-capacity magazine. There were 132 cases in which a victim was killed with a firearm for which make, model, and magazine capacity could be determined (the magazine capacity variable corresponds to the magazine actually recovered with the firearm). This analysis also excluded cases in which the victim was shot with more than one firearm. In 30 of these cases (23 percent), the victim was killed with a firearm carrying a large-capacity magazine

[91] It is possible that other firearms in the database were assault weapons according to the features test of Title XI, but we did not have the opportunity to fully assess this issue.

banned by Title XI. As is shown in the bottom of Table 6-5, offenders killed with guns having banned large-capacity magazines received over three wounds on average. In contrast, persons killed with firearms having non-banned magazines received an average of two wounds. Despite the relatively small number of large magazine cases, the t statistic is moderately large and could be considered statistically meaningful with a one-tailed test.[92] In addition, we constructed a regression model in which wound counts were regressed upon magazine capacity and the number of perpetrators involved in the incident.[93] The large-capacity magazine coefficient was 1.24 with a two-tailed p level equal to 0.05 (however, the equation explained only 3 percent of the variance in wound counts). These admittedly crude comparisons support the hypothesis that large-capacity magazines are linked to higher numbers of shots fired and wounds inflicted.

6.3.4. Conclusions

Our multi-site analysis of gunshot wounds inflicted in fatal and non-fatal gunshot cases failed to produce evidence of a post-ban reduction in the average number of gunshot wounds per case or in the proportion of cases involving multiple wounds. These results are perhaps to be expected. Available data from national gun trace requests to BATF (see Chapter 5), Milwaukee (this chapter), and other cities (see Chapters 2 and 5) indicate that assault weapons account for only 1 to 7 percent of all guns used in violent crime. Likewise, our analysis of guns used in homicides in Milwaukee suggests that a substantial majority of gun homicides (approximately three-quarters) are not committed with guns having large-capacity magazines. Further, victims killed with large-capacity magazines in Milwaukee were shot three times on average, a number well below the ten-round capacity permitted for post-ban magazines. This does not tell us the actual number of shots fired in these cases, but other limited evidence also suggests that most gun attacks involve three or fewer shots (Kleck 1991; McGonigal et al. 1993). Finally, a faster rate of fire is arguably an important lethality characteristic of semiautomatics which may influence the number of wounds inflicted in gun attacks; yet one would not expect the Crime Act to have had an impact on overall use of semiautomatics, of which assault weapons were a minority even before the ban.

On the other hand, the analysis of Milwaukee gun homicides did produce some weak evidence that homicide victims killed with guns having large-capacity magazines tended to have more bullet wounds than did victims killed with other firearms. This may suggest that large-capacity magazines facilitate higher numbers of shots fired per incident, perhaps by encouraging gun offenders to fire more shots (a phenomenon we have heard some police officers refer to as a "spray and pray" mentality). If so, the gradual attrition of the stock of pre-ban large-capacity magazines could have important preventive effects on the lethality of gun violence. However, our analysis of wounds inflicted in banned and non-banned magazine cases was crude and did not control for potentially important characteristics of the incidents, victims, and offenders. We believe that such incident-based analyses would yield important information about the role of specific firearm characteristics in lethal and non-lethal gun violence and provide further guidance by which to assess this aspect of the Crime Act legislation.

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[92] Note that two cases involving attached tubular .22 caliber large-capacity magazines were included in the non-banned magazine group because these magazines are exempted by Title XI. In one of these cases, the victim sustained 13 wounds. In a second comparison, these cases were removed from the analysis entirely. The results were essentially the same; the two-tailed p level for the comparison decreased to .13.

[93] The regression model (N=138) included cases in which the victim was shot with more than one gun. Separate variables were included for the number of victims and the use of more than one firearm. Both variables proved insignificant, but the perpetrator variable had a somewhat larger t statistic and was retained for the model discussed in the main text.

6.4. LAW ENFORCEMENT OFFICERS KILLED IN ACTION

6.4.1. Introduction and Data

As a final measure of consequences stemming from the assault weapons ban, we examined firearm homicides of police officers. Assault weapons and other high capacity firearms offer substantial firepower to offenders and may be especially attractive to very dangerous offenders. Further, the firepower offered by these weapons may facilitate successful gun battles with police. We hypothesized that these weapons might turn up more frequently in police homicides than in other gun homicides, and that the Crime Act might eventually decrease their use in these crimes.

To investigate this issue, we obtained data from the Federal Bureau of Investigation (FBI) on all gun murders of police officers from January 1992 through May 1996.[94] The data include the date of the incident, the state in which the incident occurred, the agency to which the officer belonged, and the make, model, and caliber of the firearm reportedly used in the murder. During this period, 276 police officers were killed by offenders using firearms. Gun murders of police peaked in 1994 (see Table 6-6). Data for 1995 and early 1996 suggest a decline in gun murders of police. However, any drop in gun murders of police could be due to more officers using bullet-proof vests, changes in policing tactics for drug markets, or other factors unrelated to the assault weapons ban. Moreover, the 1995 and 1996 data we received are preliminary and thus perhaps incomplete. For these reasons, we concentrated on the use of assault weapons in police homicides and did not attempt to judge whether the assault weapon ban has caused a decline in gun murders of police.

Table 6-6. Murders of police officers with assault weapons

Year	*Total gun murders of police officers*	*Officers killed with assault weapons*	*Proportion of victims killed with assault weapons (minimum estimate)*	*Proportion of victims killed with assault weapons for cases in which gun make is known*
1992	54	0	0%	0%
1993	67	4	6%	8%
1994	76	9	12%	16%
1995*	61	7	11%	16%
1996* (Jan–May)	18	0	0%	0%

*Data for 1995 and 1996 are preliminary

Even this more limited task was complicated by the fact that complete data on the make, model, and caliber of the murder weapon were not reported for a substantial proportion of these cases. The number of cases by year for which at least the gun make is known are 43 (80%) for 1992, 49 (73%) for 1993, 58 (76%) for 1994, 44 (72%) for 1995, and 10 (56%) for 1996.

6.4.2. Assault Weapons and Homicides of Police Officers

We focused our investigation on all makes and models named in Title XI and their exact copies. We also included our selected features test guns (Calico and Feather models), although we did not make a systematic

[94] These data are compiled annually by the FBI based on reports submitted by law enforcement agencies throughout the country.

assessment of all guns which may have failed the features test of the Crime Act as produced by their manufacturers.[95] Using these criteria, our estimate is that 20 officers were murdered by offenders using assault weapons during this period. (In some of these cases, it appears that the same weapon was used to murder more than one officer). Of these cases, 3 involved Intratec models, 6 were committed with weapons in the SWD family, 3 involved AR15's or exact AR15 copies, 2 cases involved Uzi's, and 6 cases identified AK-47's as the murder weapons.[96] [97] These cases accounted for about 7% of all gun murders of police during this period. This 7% figure serves as a minimum estimate of assault weapon use in police gun murders. A more accurate estimate was obtained by focusing on those cases for which, at a minimum, the gun make was reported. Overall, 10% of these cases involved assault weapons, a figure higher than that for gun murders of civilians.[98]

All of the assault weapon cases took place from 1993 through 1995 (see Table 6-6). For those three years, murders with assault weapons ranged from 6% of the cases in 1993 to 12% in 1994. Among those cases for which firearm make was reported, assault weapons accounted for 8% in 1993 and 16% in both 1994 and 1995. All of these cases occurred prior to June 1995. From that point through May of 1996, there were no additional deaths of police officers attributed to assault weapons. This is perhaps another indication of the temporary or permanent decrease in the availability of these weapons which was suggested in Chapter 5.

In sum, police officers are rarely murdered with assault weapons. Yet the fraction of police gun murders perpetrated with assault weapons is higher than that for civilian gun murders. Assault weapons accounted for about 10% of police gun murders from 1992 through May of 1996 when considering only those cases for which the gun make could be ascertained. Whether the higher representation of assault weapons among police murders is due to characteristics of the weapons, characteristics of the offenders who are drawn to assault weapons, or some

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[95] With the available data, it is not possible for us to determine whether otherwise legal guns were modified so as to make them assault weapons.

[96] There is a discrepancy between our data and those provided elsewhere with respect to a November 1994 incident in which two FBI agents and a Washington, D.C. police officer were killed. In a study of police murders from January 1994 through September 1995, Adler et al. (1995) reported that the offender in this case used a TEC9 assault pistol. The FBI data identify the weapon as an M11. (The data actually identify the gun as a Smith and Wesson M11. However, Smith and Wesson does not make a model M11. We counted the weapon as an SWD M11.)

In addition, Adler et al. identified one additional pre-ban incident in which an officer was killed with a weapon which may have failed the features test (a Springfield M1A). We are not aware of any other cases in our data which would qualify as assault weapon cases based on the features test, but we did not undertake an in-depth examination of this issue. There were no cases involving our select features test guns (Calico and Feather models).

[97] The weapon identifications in these data were made by the police departments reporting the incidents, and there is likely to be some degree of error in the firearm model designations. In particular, officers may not always accurately distinguish banned assault weapons from legal substitutes or look-alike variations. We note the issue here due to the prominence of AK-47's among guns used in police homicides. There are numerous AK-47 copies and look-alikes, and firearm experts have informed us that legal guns such as the SKS rifle and the Norinco NHM-90/91 (a modified, legal version of the AK-47) are sometimes, and perhaps commonly, mistakenly identified as AK-47's.

[98] In consultation with BATF officials, we developed a list of manufacturers who produced models listed in the Crime Act and exact copies of those firearms. We were thus able to determine whether all of the identified makes in the FBI file were assault weapons.

combination of both is unclear. However, there have been no recorded murders of police with assault weapons since the early part of 1995.[99]

These findings have important ramifications for future research on the impact of the assault weapons ban. The relatively high use of assault weapons in murders of police suggests that police gun murders should be more sensitive to the effects of the ban than gun murders of civilians. That is, if the disproportionate representation of assault weapons among gun homicides of police is attributable to the objective properties of these firearms (i.e., the greater lethality of these firearms), then a decrease in the availability of these guns should cause a notable reduction of police gun murders because other weapons will not be effective substitutes in gun battles with police. At this point, however, it is not clear whether the high representation of assault weapons among police murder cases is due to the greater stopping power of assault weapons (most assault weapons are high velocity rifles or high velocity handguns and thus inflict more serious wounds), their rate of fire and ability to accept large-capacity magazines, some combination of these weapon characteristics, or simply the traits of offenders who prefer assault weapons. A variety of non-banned weapons may serve as adequate substitutes for offenders who engage in armed confrontations with police.

As more data become available, we encourage the study of trends in police gun murders before and after the Crime Act. Furthermore, we believe that research on these issues would be strengthened by the systematic recording of the magazines with which police murder weapons were equipped and the numbers of shots fired and wounds inflicted in these incidents.

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[99] We did not examine police murders committed with firearms capable of accepting large-capacity magazines because the available data do not enable us to determine whether any guns used after the ban were actually equipped with pre-ban large-capacity magazines, nor do the data indicate the number of shots fired in these incidents. Moreover, in recent years many police departments have adopted large-capacity semiautomatic handguns as their standard firearm. Since about 14% of police officers murdered with guns are killed with their own firearms (FBI 1994, p.4), this could create an apparent increase in police murders with large-capacity firearms. (We did not acquire data on whether the officers were killed with their own firearms.) For a discussion of large-capacity firearms used in killings of police from January 1994 through September 30, 1995, see Adler et al. (1995).

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Appendix A
Assault Weapons and Mass Murder

INTRODUCTION: MASS MURDERS AS AN IMPACT MEASURE

As another indicator of ban effects on the consequences of assault weapon use, we attempted to analyze pre- and post-ban trends in mass murders, which we defined as the killing of four or more victims at one time and place by a lone offender. Although we lacked advance information on the proportion of mass murders involving assault weapons, we had two reasons for believing that assault weapons were more prevalent in mass murders than in events involving smaller numbers of victims:

1) A weapon lethality/facilitation hypothesis, that assault weapon characteristics, especially high magazine capacities, would enable a rational but intent killer to shoot more people more rapidly with an assault weapon than with many other firearms.

2) A selection hypothesis, that certain deranged killers might tend to select assault weapons to act out "commando" fantasies (e.g., see Holmes and Holmes 1994, pp.86-87).

In addition, we believed that newspaper reports of mass murders might carry more detail than reports of other murders, and that these reports might provide insights into the situational dynamics of mass murders involving assault weapons.

Our attempt to construct and analyze a 1992–96 trend line in mass murders using Nexis searches of U.S. news sources foundered, for two primary reasons. First, apparent variations in reporting or indexing practices forced us to alter our search parameters over the period, and so all three kinds of variation introduce validity problems into the trends. Second, newspaper accounts were surprisingly imprecise about the type of weapon involved. In some cases, the offender had not yet been apprehended and thus the make and model of the weapon was probably unknown. In other instances, there was apparent inattention or confusion regarding the make, model, and features. Finally, some offenders were armed with multiple weapons when they committed their crimes or when they were captured, and it was unclear to the reporter which weapon accounted for which death(s).[1]

Nevertheless, our mass murder analysis produced several interesting, though tentative, findings. First, SHR and news media sources both appear to undercount mass murders under our definition, and our capture-recapture analysis suggests that their true number may exceed the count based on either source by something like 50 percent. Second, contrary to our expectations, only 2 — 3.8 percent — of the 52 mass murders we gleaned from the Nexis search unambiguously involved assault weapons. This is about the same percentage as for other murders. Third, media accounts lend some tenuous support to the notion that assault weapons are more deadly than other weapons in mass murder events, as measured by victims per incident.

Our search methodology and the findings above are explained more fully in the following sections, which conclude with recommendations for further related research.

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[1] It is also not unusual for news accounts to use imprecise terms like "assault rifle" when describing a military-style firearm. However, we did not encounter any such cases in our particular sample.

DEFINING MASS MURDERS AND SAMPLE SELECTION

In general terms, a mass murder is the killing of a number of people at one time and place. The time requirement in particular sets mass murders apart from serial murders, which take place over a very long timeframe. We focused our analysis upon mass murders committed with firearms, and we chose four victims for our operational definition of mass murder.[2] In addition, we focused upon cases in which the murders were committed by one offender. We selected the victim and offender criteria based on practicality and because they arguably fit better with the weapon lethality/weapon facilitation argument. If assault weapons do contribute to mass murder, we hypothesized that they will enable a single offender to murder greater numbers of people at one time. Thus, we selected a subset of mass murders for which we felt assault weapons might plausibly play a greater role.

Project staff conducted Nexis searches for multiple-victim firearm murder stories appearing in U.S. news sources from 1992 through the early summer of 1996. Fifty-two stories meeting our firearm mass murder criteria were found. A breakdown of these cases by year is shown in the bottom row of table A-1.[3] Cases ranged from a low of 3 in 1994 and 1996 to a high of 20 in 1995. We urge caution in the interpretation of these numbers. Although project staff did examine well over a thousand firearm murder stories, we do not claim to have found all firearm mass murders occurring during this time. Rather, these cases should be treated as a possibly unrepresentative sample of firearm mass murders. Further, we do not recommend using these numbers as trend indicators. We refined our search parameters several times during the course of the research, and we cannot speak to issues regarding changes in journalistic practices (or Nexis coverage) which may have occurred during this period and affected our results. This portion of the evaluation was more exploratory in nature, and the primary goal was to assess the prevalence of assault weapons among a sample of recent mass murder incidents.

Table A-1. Mass murder newspaper reports, by weapon type and year of event

	1992	*1993*	*1994*	*1995*	*1996*	*Total*
Semiautomatics						
Handgun	4	3	1	7	1	16
Rifle	0	0	0	2	0	2
Generic weapon types						
Revolver	0	0	0	1	0	1
Other non-semiautomatic handgun	0	0	0	0	0	0
Handgun, type unknown	2	2	0	1	0	5
Non-semiautomatic rifle	0	0	0	1	0	1
Rifle, type unknown	1	1	0	0	0	2
Non-semiautomatic shotgun	0	0	0	1	0	1
Shotgun, type unknown	2	3	0	1	0	6
Unknown firearm	5	2	2	6	2	17

[2] As Holmes and Holmes (1994, pp.71-73) have noted, most scholars set the victim criterion for mass murder at three or four victims.

[3] Table A-1 excludes 1 of the 52 for which we were unable to ascertain the date of the mass murder.

Total cases	14	11	3	20	3	51

ESTIMATING TOTAL FIREARM MASS MURDERS: A METHODOLOGICAL NOTE

Our investigation of multiple/mass murders utilized both the SHR and news media as data sources. Both of these sources have limitations for this task. Though the SHR is widely accepted as an accurate source of homicide data, not all agencies in the country report homicides to the SHR, and agencies that do report to the SHR program may not report all of their homicides. Likewise, some mass murders may not be reported accurately in media sources, or the stories may differ in their accessibility depending on where they occurred and the publication(s) which carried the story. Family-related mass murders, for example, seem less likely to be reported in national sources (Dietz 1986), although the availability of national electronic searches through services such as Nexis would seem to lessen this problem.[4] Our experience suggests that both sources underestimate the number of true mass murders.

Capture-recapture methods (e.g., see Mastro et al. 1994; Neugebauer and Wittes 1994) offer one potential way of improving estimation of mass murders. Capture-recapture methods enable one to estimate the true size of a population based on the number of overlapping subjects found in random samples drawn from the population. Mastro et al. (1994), for example, have used this methodology to estimate the number of HIV-infected drug users in the population of a foreign city. Similarly, researchers in the biological sciences have used this methodology to estimate the size of different wildlife populations.

Given two samples from a population, the size of the population can be estimated as:

$$N = n1 * n2 / m$$

where N is the population estimate, n1 is the size of the first sample, n2 is the size of the second sample, and m is the amount of overlap in the samples (i.e., the number of subjects which turned up in the first sample and that were subsequently recaptured in the second sample). Neugebauer and Wittes (1994, p.1068) point out that this estimate is biased but that the "bias is small when the capture and recapture sizes are large." The reliability of the estimate depends on four assumptions (Mastro et al. 1994, pp.1096-1097). First, the population must be closed (in our case, this is not a problem because our samples are drawn from the same geographic area and time period). Second, the capture sources must be independent (if more than two sources are used, log-linear modeling can be used to account for dependence between the sources, and the assumption of independence is not necessary). Third, members of the population must have an equal probability of being captured. Finally, the matching procedure must be accurate — all matches must be identified and there can be no false matches.

As mentioned previously, our work with the SHR and media sources suggests that both sources underestimate the true number of firearm mass murders occurring in the nation. That being the case, we offer a tentative illustration of how capture-recapture methods might be used to estimate the true number of mass murders occurring in the nation based on the SHR and media source numbers. We add a number of qualifiers

[4] In our experience, one factor making mass murder cases more difficult to locate is that many of these stories are not labeled with dramatic terms such as "mass murder" or "massacre." Despite the rarity and tragedy of these events, they are often described in commonplace terms (headlines may simply state something like, "Gunman shoots five persons during robbery"). Thus, it becomes necessary to develop Nexis search parameters broad enough to capture various sorts of multiple-victim incidents. This, in turn, requires one to examine a much greater number of stories.

throughout this exercise. To begin with, the SHR and media sources might not seem independent because, generally speaking, news organizations are reliant upon police for information about crime. Once a homicide is discovered, on the other hand, the reporting apparatuses for the SHR and news organizations are distinct.

With that caveat in mind, we used the year 1992 for this demonstration. For that year, we identified all cases from both sources in which one offender killed four or more persons using a firearm. The SHR search turned up 15 cases, and the Nexis search yielded 14 cases.

Next, we attempted to match these cases. Tentatively, we determined that nine cases were common to both sources (see Table A-2). Our estimate for the number of incidents during 1992 in which one offender killed four or more persons using a firearm(s) thus becomes:

$$N = (15 * 14)/9 = 23.$$

Table A-2. 1992 HR/Nexis comparisons

NEXIS	***SHR***	***NEXIS & SHR***
14	15	9
NEXIS ONLY		***NUMBER OF VICTIMS***
2/16/92	Mobile, AL	4
5/1/92	Yuba County, CA	4
6/15/92	Inglewood, CA	5
9/13/92	Harris County, TX	4
11/13/92	Spring Branch, TX	5
FBI ONLY		***NUMBER OF VICTIMS***
8/92	Dade, FL	4
9/92	Chicago, IL	4
5/92	Detroit, MI	4
3/92	New York, NY	4
1/92	Burleigh, ND	4
7/92	Houston, TX	4
NEXIS & FBI		***NUMBER OF VICTIMS***
2/12/92	Seattle, WA	4
3/21/92	Sullivan, MO	6
3/26/92	Queens, NY	5
7/23/92	Fairmont, WV	4
10/4/92	Dallas, TX	4
10/15/92	Schuyler County	4
11/1/92	Rancho Santa Fe, CA	4
12/13/92	King County, WA	4
12/24/92	Prince William County, VA	4

A number of cautionary notes are required. Obviously, our sample sizes are quite small, but, apparently, so is the population which we are trying to estimate. In addition, our matches between the sources were based on matching the town (determined from the police department's name), month of occurrence, number of victims, and number of offenders. In a more thorough investigation, one would wish to make the matches more carefully. If,

for instance, the victims were not all immediately killed, one may find a news story referring to the initial number of deaths, and that count might not match the final count appearing in the SHR. Moreover, we have focused on cases in which one offender committed the murders. However, the SHR might list two or more offenders if there were other accomplices who did not do the shooting. Finally, there could be ambiguity regarding the exact location of the SHR cases because we used the police department name to match the locations with the Nexis cases (city or town name does not appear in the file). We did not investigate these issues extensively, but they would seem to be manageable problems.

Another issue is whether each incident's probability of being captured is the same for each sample. Our tentative judgment is that this is not the case, or at least it does not appear to have been true for our sample. Referring to Table A-2, it seems that the SHR-only cases were more likely to appear in urban areas, whereas the Nexis-only cases appear to have taken place in more rural areas. We can speculate that rural police departments are somewhat less likely to participate in the SHR, and that cases in rural areas are thus less likely to be reported to the SHR. In contrast, the greater number of murders and violent acts which occur in urban areas may have the effect of making any given incident less newsworthy, even if that incident is a mass murder. A mass murder taking place among family members in an urban jurisdiction, for instance, might get less prominent coverage in news sources and might therefore be more difficult to locate in a national electronic search.

But even if we accept these biases as real, we can at least estimate the direction of the bias in the capture-recapture estimate. Biases such as those discussed above have the effect of lessening the overlap between our sources. Therefore, they decrease the denominator of the capture-recapture equation and bias the population estimate upwards. With this in mind, our 1992 estimate of 23 cases should be seen as an upper estimate of the number of these incidents for that year.

In this section, we have provided a very rough illustration of how capture-recapture models might be utilized to more accurately estimate the number of mass murders in the U.S. or any portion of the U.S. If additional homicide sources were added such as the U.S. Public Health Service's Mortality Detail Files, moreover, researchers could model any dependencies between the sources. With further research into past years and ahead into future years, researchers could build time series to track mass murders and firearm mass murders over time. This may be a worthwhile venture because though these events are only a small fraction of all homicides, they are arguably events which have a disproportionately negative impact on citizens' perceptions of safety.

Firearms Used in Mass Murders

Table A-1 displays information about the weapons used in our sample of mass murders. One of the major goals behind the Nexis search was to obtain more detailed information on the weapons used in firearm mass murders. Yet a substantial proportion of the articles said nothing about the firearm(s) used in the crime or identified the gun(s) with generic terms such as "handgun," "rifle," or "shotgun." Overall, 18 stories identified the murder weapon(s) as a semiautomatic weapon, and 16 of these guns were semiautomatic handguns. Only eight stories named the make and model of the murder weapon.

Despite the general lack of detailed weapon information, our operating assumption was that, due to their notoriety, assault weapons would draw more attention in media sources. That is, we assumed that reporters would explicitly identify any assault weapons that were involved in the incident and that unidentified weapons were most likely not assault weapons. This assumption is most reasonable for cases in which the offender was apprehended. Overall, 37 cases (71 percent) were solved and another 6 (11.5 percent) had known suspects.

Of the total 52 cases in our sample, 2, or 3.8 percent, involved assault weapons as the murder weapon. If we focus on just the 37 solved cases, assault weapons were involved in 5.4 percent (both assault weapon cases were solved). One of the assault weapon cases took place in 1993 and the other took place in 1995 after the ban's implementation. The accounts of those cases are as follows:

> Case 1 (July 3, 1993, San Francisco, California). A 55-year-old man bearing a grudge against his former attorneys for a lawsuit in which he lost 1 million dollars killed 8 persons, wounded 6 others, and then killed himself during a 15-minute rampage in which he fired 50-100 rounds. The offender was armed with two TEC-9 assault pistols, a .45 caliber semiautomatic pistol, and hundreds of rounds of ammunition.[5]

> Case 2 (June 20, 1995, Spokane, Washington). A military man assigned to Fairchild Air Force Base entered the base hospital with an AK-47 assault rifle and opened fire, killing 4 and wounding 19. The gunman was killed by a military police officer. At the time of the story, no motive for the killing had been discovered.

In addition, our search uncovered two other cases in which the offender possessed an assault weapon but did not use it in the crime. In one of these cases, the additional weapon was identified only as a "Chinese assault rifle," so there is the possibility that the gun was an SKS rifle or other firearm that was not an assault weapon by the criteria of Title XI.

LETHALITY OF ASSAULT WEAPONS USED IN MASS MURDERS

Although assault weapons appeared rarely in our sample of firearm mass murder cases, there are some indications that mass murders involving assault weapons are more deadly than other mass murders with guns. The two unambiguous assault weapon cases in our sample involved a mean of 6 victims, a number 1.5 higher than the 4.5 victims killed on average in the other cases. Further, each assault weapon case involved a substantial number of other victims who were wounded but not killed. Other notorious mass murders committed with assault weapons also claimed particularly high numbers of victims (Cox Newspapers 1989). The numbers of victims in these cases suggests that the ability of the murder weapons to accept large-capacity magazines was probably an important factor. We offer this observation cautiously, however, for several reasons besides the small number of cases in our sample. We did not make detailed assessments of the actors or circumstances involved in these incidents. Relevant questions, for example, might include whether the offender had a set number of intended targets (and, relatedly, the relationship between the offender and victims), the number of different guns used, whether the offender had the victims trapped at the time of the murders, and the amount of time the offender had to commit the crime.

In order to refine our comparison somewhat further, we examined the number of victims in assault weapon and non-assault weapon cases after removing 19 family-related cases from consideration. This did not change the results; the average number of victims in assault weapon cases was still approximately 1.5 higher than that of non-assault weapon cases.

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[5] The story indicated that the offender had modified the firearms to make them fire more rapidly than they would have otherwise. Presumably, this means that he converted the guns to fully automatic fire, but this is not entirely clear from the article.

RECOMMENDATIONS FOR FURTHER RELATED RESEARCH

There are a number of related questions that could be pursued in future research. One concerns a more explicit examination of the role of large-capacity magazines in mass murder, particularly for incidents involving non-assault weapon firearms. Based on our experience, this information is rarely offered in media sources and would require contacting police departments which investigated mass murder incidents. Another issue concerns non-fatal victims. This was not an express focus of our research, but if the assault weapon/large-capacity semiautomatic hypothesis has validity, we can hypothesize that shootings involving these weapons will involve more total victims. Along similar lines, Sherman and his colleagues (1989) documented a rise in bystander shootings in a number of cities during the 1980s and speculated that the spread of semiautomatic weaponry was a factor in this development. Due to time and resource limitations, we did not pursue the issue of bystander shootings for this study, but further research might shed light on whether assault weapons and large-capacity magazines have been a factor in any such rise.

EXHIBIT "7"

An Updated Assessment of the Federal Assault Weapons Ban: Impacts on Gun Markets and Gun Violence, 1994-2003

Report to the National Institute of Justice, United States Department of Justice

By

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TABLE OF CONTENTS

TABLE OF CONTENTS (CONTINUED)

PREFACE

Gun violence continues to be one of America's most serious crime problems. In 2000, over 10,000 persons were murdered with firearms and almost 49,000 more were shot in the course of over 340,000 assaults and robberies with guns (see the Federal Bureau of Investigation's annual *Uniform Crime Reports* and Simon et al., 2002). The total costs of gun violence in the United States – including medical, criminal justice, and other government and private costs – are on the order of at least $6 to $12 billion per year and, by more controversial estimates, could be as high as $80 billion per year (Cook and Ludwig, 2000).

However, there has been good news in recent years. Police statistics and national victimization surveys show that since the early 1990s, gun crime has plummeted to some of the lowest levels in decades (see the *Uniform Crime Reports* and Rennison, 2001). Have gun controls contributed to this decline, and, if so, which ones?

During the last decade, the federal government has undertaken a number of initiatives to suppress gun crime. These include, among others, the establishment of a national background check system for gun buyers (through the Brady Act), reforms of the licensing system for firearms dealers, a ban on juvenile handgun possession, and Project Safe Neighborhoods, a collaborative effort between U.S. Attorneys and local authorities to attack local gun crime problems and enhance punishment for gun offenders.

Perhaps the most controversial of these federal initiatives was the ban on semiautomatic assault weapons and large capacity ammunition magazines enacted as Title XI, Subtitle A of the *Violent Crime Control and Law Enforcement Act of 1994*. This law prohibits a relatively small group of weapons considered by ban advocates to be particularly dangerous and attractive for criminal purposes. In this report, we investigate the ban's impacts on gun crime through the late 1990s and beyond. This study updates a prior report on the short-term effects of the ban (1994-1996) that members of this research team prepared for the U.S. Department of Justice and the U.S. Congress (Roth and Koper, 1997; 1999).

ACKNOWLEDGMENTS

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The author wishes to thank several people and organizations that assisted this effort in numerous ways. Daniel Woods assisted with data analysis. Jeffrey Roth, who directed our first study of the assault weapons ban, provided advice and editorial input. Additional research assistance was provided by the following former employees of the Urban Institute: Gretchen Moore, David Huffer, Erica Dinger, Darin Reedy, Kate Bunting, Katie Gorie, and Michele Waul. The following persons and organizations provided databases, information, or other resources utilized for this report: Glenn Pierce (Northeastern University), Pamela Shaw and Edward Koch (Baltimore Police Department), Robert Shem (Alaska State Police), Bill McGill and Mallory O'Brien (currently or formerly of the Firearm Injury Center, Medical College of Wisconsin), Rick Ruddell (California State University, Chico), Scott Doyle (Kentucky State Police), Terrence Austin and Joe Vince (currently or formerly of the Bureau of Alcohol, Tobacco, Firearms, and Explosives), Carlos Alvarez and Alan Lynn (Metro-Dade Police Department), Charles Branas (Firearm and Injury Center, University of Pennsylvania), Caroline Harlow (Bureau of Justice Statistics), and Rebecca Knox (Brady Center to Prevent Handgun Violence). Robert Burrows (Bureau of Alcohol, Tobacco, Firearms, and Explosives) and Wain Roberts (Wain Roberts Firearms) shared technical expertise on firearms. Anonymous reviewers for the National Institute of Justice provided thorough and helpful comments on earlier versions of this report, as did Terrence Austin and Robert Burrows of the Bureau of Alcohol, Tobacco, Firearms, and Explosives. Finally, I thank Lois Mock, our National Institute of Justice grant monitor, for her advice and encouragement throughout all of the research that my colleagues and I have conducted on the assault weapons ban.

1. IMPACTS OF THE FEDERAL ASSAULT WEAPONS BAN, 1994-2003: KEY FINDINGS AND CONCLUSIONS

This overview presents key findings and conclusions from a study sponsored by the National Institute of Justice to investigate the effects of the federal assault weapons ban. This study updates prior reports to the National Institute of Justice and the U.S. Congress on the assault weapons legislation.

The Ban Attempts to Limit the Use of Guns with Military Style Features and Large Ammunition Capacities

- Title XI, Subtitle A of the Violent Crime Control and Law Enforcement Act of 1994 imposed a 10-year ban on the "manufacture, transfer, and possession" of certain semiautomatic firearms designated as assault weapons (AWs). The ban is directed at semiautomatic firearms having features that appear useful in military and criminal applications but unnecessary in shooting sports or self-defense (examples include flash hiders, folding rifle stocks, and threaded barrels for attaching silencers). The law bans 18 models and variations by name, as well as revolving cylinder shotguns. It also has a "features test" provision banning other semiautomatics having two or more military-style features. In sum, the Bureau of Alcohol, Tobacco, Firearms, and Explosives (ATF) has identified 118 models and variations that are prohibited by the law. A number of the banned guns are foreign semiautomatic rifles that have been banned from importation into the U.S. since 1989.

- The ban also prohibits most ammunition feeding devices holding more than 10 rounds of ammunition (referred to as large capacity magazines, or LCMs). An LCM is arguably the most functionally important feature of most AWs, many of which have magazines holding 30 or more rounds. The LCM ban's reach is broader than that of the AW ban because many non-banned semiautomatics accept LCMs. Approximately 18% of civilian-owned firearms and 21% of civilian-owned handguns were equipped with LCMs as of 1994.

- The ban exempts AWs and LCMs manufactured before September 13, 1994. At that time, there were upwards of 1.5 million privately owned AWs in the U.S. and nearly 25 million guns equipped with LCMs. Gun industry sources estimated that there were 25 million pre-ban LCMs available in the U.S. as of 1995. An additional 4.7 million pre-ban LCMs were imported into the country from 1995 through 2000, with the largest number in 1999.

- Arguably, the AW-LCM ban is intended to reduce gunshot victimizations by limiting the national stock of semiautomatic firearms with large ammunition capacities – which enable shooters to discharge many shots rapidly – and other features conducive to criminal uses. The AW provision targets a relatively small number of weapons based on features that have little to do with the weapons'

operation, and removing those features is sufficient to make the weapons legal. The LCM provision limits the ammunition capacity of non-banned firearms.

The Banned Guns and Magazines Were Used in Up to A Quarter of Gun Crimes Prior to the Ban

- AWs were used in only a small fraction of gun crimes prior to the ban: about 2% according to most studies and no more than 8%. Most of the AWs used in crime are assault pistols rather than assault rifles.

- LCMs are used in crime much more often than AWs and accounted for 14% to 26% of guns used in crime prior to the ban.

- AWs and other guns equipped with LCMs tend to account for a higher share of guns used in murders of police and mass public shootings, though such incidents are very rare.

The Ban's Success in Reducing Criminal Use of the Banned Guns and Magazines Has Been Mixed

- Following implementation of the ban, the share of gun crimes involving AWs declined by 17% to 72% across the localities examined for this study (Baltimore, Miami, Milwaukee, Boston, St. Louis, and Anchorage), based on data covering all or portions of the 1995-2003 post-ban period. This is consistent with patterns found in national data on guns recovered by police and reported to ATF.

- The decline in the use of AWs has been due primarily to a reduction in the use of assault pistols (APs), which are used in crime more commonly than assault rifles (ARs). There has not been a clear decline in the use of ARs, though assessments are complicated by the rarity of crimes with these weapons and by substitution of post-ban rifles that are very similar to the banned AR models.

- However, the decline in AW use was offset throughout at least the late 1990s by steady or rising use of other guns equipped with LCMs in jurisdictions studied (Baltimore, Milwaukee, Louisville, and Anchorage). The failure to reduce LCM use has likely been due to the immense stock of exempted pre-ban magazines, which has been enhanced by recent imports.

It is Premature to Make Definitive Assessments of the Ban's Impact on Gun Crime

- Because the ban has not yet reduced the use of LCMs in crime, we cannot clearly credit the ban with any of the nation's recent drop in gun violence. However, the ban's exemption of millions of pre-ban AWs and LCMs ensured that the effects

of the law would occur only gradually. Those effects are still unfolding and may not be fully felt for several years into the future, particularly if foreign, pre-ban LCMs continue to be imported into the U.S. in large numbers.

The Ban's Reauthorization or Expiration Could Affect Gunshot Victimizations, But Predictions are Tenuous

- Should it be renewed, the ban's effects on gun violence are likely to be small at best and perhaps too small for reliable measurement. AWs were rarely used in gun crimes even before the ban. LCMs are involved in a more substantial share of gun crimes, but it is not clear how often the outcomes of gun attacks depend on the ability of offenders to fire more than ten shots (the current magazine capacity limit) without reloading.

- Nonetheless, reducing criminal use of AWs and especially LCMs could have non-trivial effects on gunshot victimizations. The few available studies suggest that attacks with semiautomatics – including AWs and other semiautomatics equipped with LCMs – result in more shots fired, more persons hit, and more wounds inflicted per victim than do attacks with other firearms. Further, a study of handgun attacks in one city found that 3% of the gunfire incidents resulted in more than 10 shots fired, and those attacks produced almost 5% of the gunshot victims.

- Restricting the flow of LCMs into the country from abroad may be necessary to achieve desired effects from the ban, particularly in the near future. Whether mandating further design changes in the outward features of semiautomatic weapons (such as removing all military-style features) will produce measurable benefits beyond those of restricting ammunition capacity is unknown. Past experience also suggests that Congressional discussion of broadening the AW ban to new models or features would raise prices and production of the weapons under discussion.

- If the ban is lifted, gun and magazine manufacturers may reintroduce AW models and LCMs, perhaps in substantial numbers. In addition, pre-ban AWs may lose value and novelty, prompting some of their owners to sell them in undocumented secondhand markets where they can more easily reach high-risk users, such as criminals, terrorists, and other potential mass murderers. Any resulting increase in crimes with AWs and LCMs might increase gunshot victimizations for the reasons noted above, though this effect could be difficult to measure.

2. PROVISIONS OF THE ASSAULT WEAPONS BAN

2.1. Assault Weapons

Enacted on September 13, 1994, Title XI, Subtitle A of the *Violent Crime Control and Law Enforcement Act of 1994* imposes a 10-year ban on the "manufacture, transfer, and possession" of certain semiautomatic firearms designated as assault weapons (AWs).[1] The AW ban is not a prohibition on all semiautomatics. Rather, it is directed at semiautomatics having features that appear useful in military and criminal applications but unnecessary in shooting sports or self-defense. Examples of such features include pistol grips on rifles, flash hiders, folding rifle stocks, threaded barrels for attaching silencers, and the ability to accept ammunition magazines holding large numbers of bullets.[2] Indeed, several of the banned guns (e.g., the AR-15 and Avtomat Kalashnikov models) are civilian copies of military weapons and accept ammunition magazines made for those military weapons.

As summarized in Table 2-1, the law specifically prohibits nine narrowly defined groups of pistols, rifles, and shotguns. A number of the weapons are foreign rifles that the federal government has banned from importation into the U.S. since 1989. Exact copies of the named AWs are also banned, regardless of their manufacturer. In addition, the ban contains a generic "features test" provision that generally prohibits other semiautomatic firearms having two or more military-style features, as described in Table 2-2. In sum, the federal Bureau of Alcohol, Tobacco, Firearms, and Explosives (ATF) has identified 118 model and caliber variations that meet the AW criteria established by the ban.[3]

Figures 2-1 and 2-2 illustrate a few prominent AWs and their features. Figure 2-1 displays the Intratec TEC-9 assault pistol, the AW most frequently used in crime (e.g., see Roth and Koper 1997, Chapter 2). Figure 2-2 depicts the AK-47 assault rifle, a weapon of Soviet design. There are many variations of the AK-47 produced around the world, not all of which have the full complement of features illustrated in Figure 2-2.

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[1] A semiautomatic weapon fires one bullet for each squeeze of the trigger. After each shot, the gun automatically loads the next bullet and cocks itself for the next shot, thereby permitting a somewhat faster rate of fire relative to non-automatic firearms. Semiautomatics are not to be confused with fully automatic weapons (i.e., machine guns), which fire continuously as long as the trigger is held down. Fully automatic weapons have been illegal to own in the United States without a federal permit since 1934.

[2] Ban advocates stress the importance of pistol grips on rifles and heat shrouds or forward handgrips on pistols, which in combination with large ammunition magazines enable shooters to discharge high numbers of bullets rapidly (in a "spray fire" fashion) while maintaining control of the firearm (Violence Policy Center, 2003). Ban opponents, on the other hand, argue that AW features also serve legitimate purposes for lawful gun users (e.g., see Kopel, 1995).

[3] This is based on AWs identified by ATF's Firearms Technology Branch as of December 1997.

Table 2-1. Firearms Banned by the Federal Assault Weapons Ban

Firearm	Description	1993 Blue Book Price	Pre-Ban Federal Legal Status	Examples of Legal Substitutes
Avtomat Kalashnikov (AK) (by Norinco, Mitchell, Poly Technologies)	Chinese, Russian, other foreign and domestic: .223 or 7.62x39mm caliber, semiauto. rifle; 5, 10, or 30 shot magazine, may be supplied with bayonet	$550 (generic import); add 10-15% for folding stock models	Imports banned in 1989.	Norinco NHM 90/91 [1]
Uzi, Galil	Israeli: 9mm, .41, or .45 caliber semiauto. carbine, mini-carbine, or pistol. Magazine capacity of 16, 20, or 25, depending on model and type (10 or 20 on pistols).	$550-$1050 (Uzi) $875-$1150 (Galil)	Imports banned in 1989	Uzi Sporter [2]
Beretta AR-70	Italian: .222 or .223 caliber semiauto. paramilitary design rifle; 5, 8, or 30 shot magazine.	$1050	Imports banned in 1989.	
Colt AR-15	Domestic: primarily .223 caliber paramilitary rifle or carbine; 5 shot magazines, often comes with two 5-shot detachable magazines. Exact copies by DPMS, Eagle, Olympic, and others.	$825-$1325	Legal (civilian version of military M-16)	Colt Sporter, Match H-Bar, Target models
Fabrique National FN/FAL, FN/LAR, FNC	Belgian design: .308 caliber semiauto. rifle or .223 combat carbine with 30 shot magazine. Rifle comes with flash hider, 4 position fire selector on automatic models. Discontinued in 1988.	$1100-$2500	Imports banned in 1989.	L1A1 Sporter (FN, Century) [2]
Steyr AUG	Austrian: .223/5.56mm caliber semiauto. paramilitary design rifle.	$2500	Imports banned in 1989	
SWD M-10, 11, 11/9, 12	Domestic: 9mm, .380, or .45 caliber paramilitary design semiauto. pistol; 32 shot magazine. Also available in semiauto. carbine and fully automatic variations.	$215 (M-11/9)	Legal	Cobray PM11, 12
TEC-9, DC9, 22	Domestic: 9mm caliber semiauto. paramilitary design pistol, 10 or 32 shot magazine.; .22 caliber semiauto. paramilitary design pistol, 30 shot magazine.	$145-$295	Legal	TEC-AB
Revolving Cylinder Shotguns	Domestic: 12 gauge, 12 shot rotary magazine; paramilitary configuration	$525 (Street Sweeper)	Legal	

[1] Imports were halted in 1994 under the federal embargo on the importation of firearms from China.
[2] Imports banned by federal executive order, April 1998.

Table 2-2. Features Test of the Federal Assault Weapons Ban

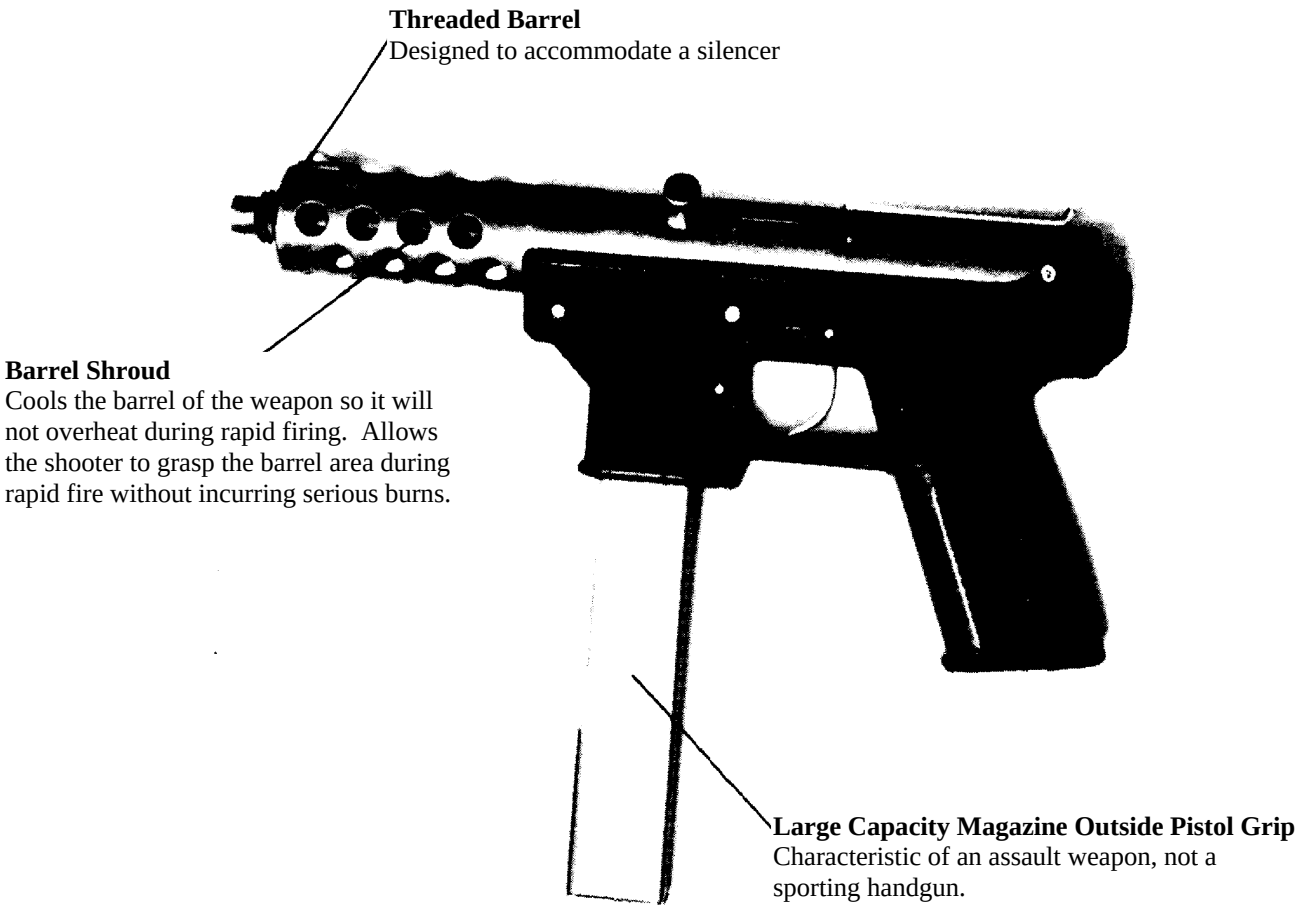
Weapon Category	Military-Style Features (Two or more qualify a firearm as an assault weapon)
Semiautomatic pistols accepting detachable magazines:	1) ammunition magazine that attaches outside the pistol grip 2) threaded barrel capable of accepting a barrel extender, flash hider, forward handgrip, or silencer 3) heat shroud attached to or encircling the barrel 4) weight of more than 50 ounces unloaded 5) semiautomatic version of a fully automatic weapon
Semiautomatic rifles accepting detachable magazines:	1) folding or telescoping stock 2) pistol grip that protrudes beneath the firing action 3) bayonet mount 4) flash hider or threaded barrel designed to accommodate one 5) grenade launcher
Semiautomatic shotguns:	1) folding or telescoping stock 2) pistol grip that protrudes beneath the firing action 3) fixed magazine capacity over 5 rounds 4) ability to accept a detachable ammunition magazine

2.2. Large Capacity Magazines

In addition, the ban prohibits most ammunition feeding devices holding more than 10 rounds of ammunition (referred to hereafter as large capacity magazines, or LCMs).[4] Most notably, this limits the capacity of detachable ammunition magazines for semiautomatic firearms. Though often overlooked in media coverage of the law, this provision impacted a larger share of the gun market than did the ban on AWs. Approximately 40 percent of the semiautomatic handgun models and a majority of the semiautomatic rifle models being manufactured and advertised prior to the ban were sold with LCMs or had a variation that was sold with an LCM (calculated from Murtz et al., 1994). Still others could accept LCMs made for other firearms and/or by other manufacturers. A national survey of gun owners found that 18% of all civilian-owned firearms and 21% of civilian-owned handguns were equipped with magazines having 10 or more rounds as of 1994 (Cook and Ludwig, 1996, p. 17). The AW provision did not affect most LCM-compatible guns, but the LCM provision limited the capacities of their magazines to 10 rounds.

[4] Technically, the ban prohibits any magazine, belt, drum, feed strip, or similar device that has the capacity to accept more than 10 rounds or ammunition, or which can be readily converted or restored to accept more than 10 rounds of ammunition. The ban exempts attached tubular devices capable of operating only with .22 caliber rimfire (i.e., low velocity) ammunition.

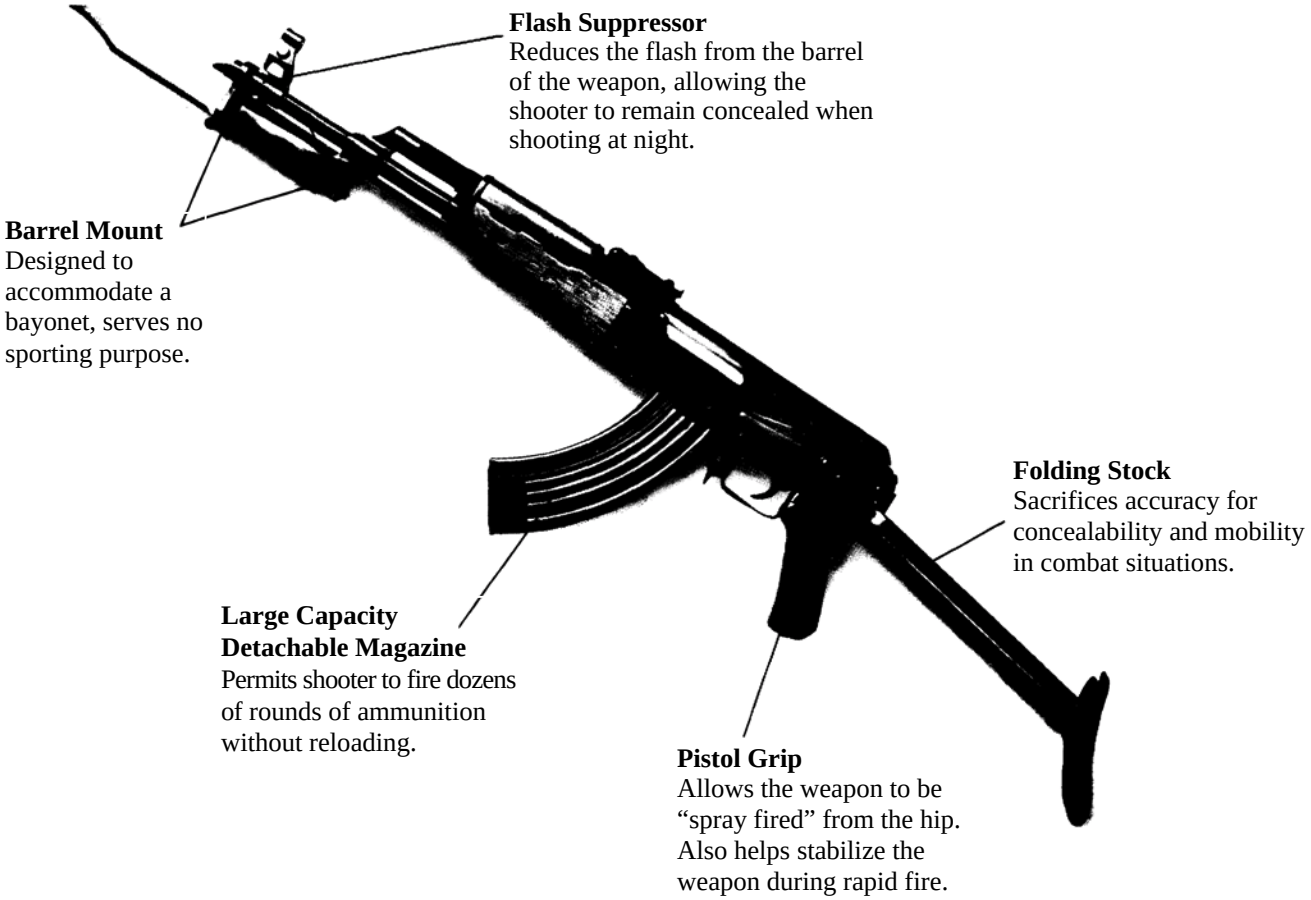
Figure 2-1. Features of Assault Weapons:
The Intratec TEC-9 Assault Pistol



Adapted from exhibit of the Center to Prevent Handgun Violence.

As discussed in later chapters, an LCM is perhaps the most functionally important feature of many AWs. This point is underscored by the AW ban's exemptions for semiautomatic rifles that cannot accept a detachable magazine that holds more than five rounds of ammunition and semiautomatic shotguns that cannot hold more than five rounds in a fixed or detachable magazine. As noted by the U.S. House of Representatives, most prohibited AWs came equipped with magazines holding 30 rounds and could accept magazines holding as many as 50 or 100 rounds (U.S. Department of the Treasury, 1998, p. 14). Also, a 1998 federal executive order (discussed below) banned further importation of foreign semiautomatic rifles capable of accepting LCMs made for military rifles. Accordingly, the magazine ban plays an important role in the logic and interpretations of the analyses presented here.

Figure 2-2. Features of Assault Weapons: The AK-47 Assault Rifle



Adapted from exhibit of the Center to Prevent Handgun Violence.

2.3. Foreign Rifles Accepting Large Capacity Military Magazines

In April of 1998, the Clinton administration broadened the range of the AW ban by prohibiting importation of an additional 58 foreign semiautomatic rifles that were still legal under the 1994 law but that can accept LCMs made for military assault rifles like the AK-47 (U.S. Department of the Treasury, 1998).[5] Figure 2-3 illustrates a few such rifles (hereafter, LCMM rifles) patterned after the banned AK-47 pictured in Figure 2-2. The LCMM rifles in Figure 2-3 do not possess the military-style features incorporated into the AK-47 (such as pistol grips, flash suppressors, and bayonet mounts), but they accept LCMs made for AK-47s.[6]

[5] In the civilian context, AWs are semiautomatic firearms. Many semiautomatic AWs are patterned after military firearms, but the military versions are capable of semiautomatic and fully automatic fire.

[6] Importation of some LCMM rifles, including a number of guns patterned after the AK-47, was halted in 1994 due to trade sanctions against China (U.S. Department of the Treasury, 1998).

Figure 2-3. Foreign Semiautomatic Rifles Capable of Accepting Large Capacity Military Magazines: AK47 Copies Banned by Executive Order in 1998

MISR

ARM

MAK90

WUM 1

Taken from U.S. Department of the Treasury (1998)

2.4. Ban Exemptions

2.4.1. Guns and Magazines Manufactured Prior to the Ban

The ban contains important exemptions. AWs and LCMs manufactured before the effective date of the ban are "grandfathered" and thus legal to own and transfer. Around 1990, there were an estimated 1 million privately owned AWs in the U.S. (about 0.5% of the estimated civilian gun stock) (Cox Newspapers, 1989, p. 1; American Medical Association Council on Scientific Affairs, 1992), though those counts probably did not correspond exactly to the weapons prohibited by the 1994 ban. The leading domestic AW producers manufactured approximately half a million AWs from 1989 through 1993, representing roughly 2.5% of all guns manufactured in the U.S. during that time (see Chapter 5).

We are not aware of any precise estimates of the pre-ban stock of LCMs, but gun owners in the U.S. possessed an estimated 25 million guns that were equipped with LCMs or 10-round magazines in 1994 (Cook and Ludwig, 1996, p. 17), and gun industry sources estimated that, including aftermarket items for repairing and extending magazines, there were at least 25 million LCMs available in the United States as of 1995 (Gun Tests, 1995, p. 30). As discussed in Chapter 7, moreover, an additional 4.8 million pre-ban LCMs were imported into the U.S. from 1994 through 2000 under the grandfathering exemption.

2.4.2. Semiautomatics With Fewer or No Military Features

Although the law bans "copies or duplicates" of the named gun makes and models, federal authorities have emphasized exact copies. Relatively cosmetic changes, such as removing a flash hider or bayonet mount, are sufficient to transform a banned weapon into a legal substitute, and a number of manufacturers now produce modified, legal versions of some of the banned guns (examples are listed in Table 2-1). In general, the AW ban does not apply to semiautomatics possessing no more than one military-style feature listed under the ban's features test provision.[7] For instance, prior to going out of business, Intratec, makers of the banned TEC-9 featured in Figure 2-1, manufactured an AB-10 ("after ban") model that does not have a threaded barrel or a barrel shroud but is identical to the TEC-9 in other respects, including the ability to accept an ammunition magazine outside the pistol grip (Figure 2-4). As shown in the illustration, the AB-10 accepts grandfathered, 32-round magazines made for the TEC-9, but post-ban magazines produced for the AB-10 must be limited to 10 rounds.

[7] Note, however, that firearms imported into the country must still meet the "sporting purposes test" established under the federal Gun Control Act of 1968. In 1989, ATF determined that foreign semiautomatic rifles having any one of a number of named military features (including those listed in the features test of the 1994 AW ban) fail the sporting purposes test and cannot be imported into the country. In 1998, the ability to accept an LCM made for a military rifle was added to the list of disqualifying features. Consequently, it is possible for foreign rifles to pass the features test of the federal AW ban but not meet the sporting purposes test for imports (U.S. Department of the Treasury, 1998).

Another example is the Colt Match Target H-Bar rifle (Figure 2-5), which is a legalized version of the banned AR-15 (see Table 2-1). AR-15 type rifles are civilian weapons patterned after the U.S. military's M-16 rifle and were the assault rifles most commonly used in crime before the ban (Roth and Koper, 1997, Chapter 2). The post-ban version shown in Figure 2-5 (one of several legalized variations on the AR-15) is essentially identical to pre-ban versions of the AR-15 but does not have accessories like a flash hider, threaded barrel, or bayonet lug. The one remaining military feature on the post-ban gun is the pistol grip. This and other post-ban AR-15 type rifles can accept LCMs made for the banned AR15, as well as those made for the U.S. military's M-16. However, post-ban magazines manufactured for these guns must hold fewer than 11 rounds.

The LCMM rifles discussed above constituted another group of legalized AW-type weapons until 1998, when their importation was prohibited by executive order. Finally, the ban includes an appendix that exempts by name several hundred models of rifles and shotguns commonly used in hunting and recreation, 86 of which are semiautomatics. While the exempted semiautomatics generally lack the military-style features common to AWs, many take detachable magazines, and some have the ability to accept LCMs.[8]

2.5. Summary

In the broadest sense, the AW-LCM ban is intended to limit crimes with semiautomatic firearms having large ammunition capacities – which enable shooters to discharge high numbers of shots rapidly – and other features conducive to criminal applications. The gun ban provision targets a relatively small number of weapons based on outward features or accessories that have little to do with the weapons' operation. Removing some or all of these features is sufficient to make the weapons legal. In other respects (e.g., type of firing mechanism, ammunition fired, and the ability to accept a detachable magazine), AWs do not differ from other legal semiautomatic weapons. The LCM provision of the law limits the ammunition capacity of non-banned firearms.

[8] Legislators inserted a number of amendments during the drafting process to broaden the consensus behind the bill (Lennett 1995). Among changes that occurred during drafting were: dropping a requirement to register post-ban sales of the grandfathered guns, dropping a ban on "substantial substitutes" as well as "exact copies" of the banned weapons, shortening the list of named makes and models covered by the ban, adding the appendix list of exempted weapons, and mandating the first impact study of the ban that is discussed below.

Figure 2-4. Post-Ban, Modified Versions of Assault Weapons: The Intratec AB ("After Ban") Model (See Featured Firearm)

Figure 2-5. Post-Ban, Modified Versions of Assault Weapons: The Colt Match Target HBAR Model

3. CRIMINAL USE OF ASSAULT WEAPONS AND LARGE CAPACITY MAGAZINES BEFORE THE BAN

During the 1980s and early 1990s, AWs and other semiautomatic firearms equipped with LCMs were involved in a number of highly publicized mass murder incidents that raised public concern about the accessibility of high powered, military-style weaponry and other guns capable of discharging high numbers of bullets in a short period of time (Cox Newspapers, 1989; Kleck, 1997, pp.124-126,144; Lenett, 1995). In one of the worst mass murders ever committed in the U.S., for example, James Huberty killed 21 persons and wounded 19 others in a San Ysidro, California MacDonald's restaurant on July 18, 1984 using an Uzi carbine, a shotgun, and another semiautomatic handgun. On September 14, 1989, Joseph Wesbecker, armed with an AK-47 rifle, two MAC-11 handguns, and a number of other firearms, killed 7 persons and wounded 15 others at his former workplace in Louisville, Kentucky before taking his own life. Another particularly notorious incident that precipitated much of the recent debate over AWs occurred on January 17, 1989 when Patrick Purdy used a civilian version of the AK-47 military rifle to open fire on a schoolyard in Stockton, California, killing 5 children and wounding 29 persons.

There were additional high profile incidents in which offenders using semiautomatic handguns with LCMs killed and wounded large numbers of persons. Armed with two handguns having LCMs (and reportedly a supply of extra LCMs), a rifle, and a shotgun, George Hennard killed 22 people and wounded another 23 in Killeen, Texas in October 1991. In a December 1993 incident, a gunman named Colin Ferguson, armed with a handgun and LCMs, opened fire on commuters on a Long Island train, killing 5 and wounding 17.

Indeed, AWs or other semiautomatics with LCMs were involved in 6, or 40%, of 15 mass shooting incidents occurring between 1984 and 1993 in which six or more persons were killed or a total of 12 or more were wounded (Kleck, 1997**,** pp.124-126, 144**).** Early studies of AWs, though sometimes based on limited and potentially unrepresentative data, also suggested that AWs recovered by police were often associated with drug trafficking and organized crime (Cox Newspapers, 1989; also see Roth and Koper, 1997, Chapter 5), fueling a perception that AWs were guns of choice among drug dealers and other particularly violent groups. All of this intensified concern over AWs and other semiautomatics with large ammunition capacities and helped spur the passage of AW bans in California, New Jersey, Connecticut, and Hawaii between 1989 and 1993, as well as the 1989 federal import ban on selected semiautomatic rifles. Maryland also passed AW legislation in 1994, just a few months prior to the passage of the 1994 federal AW ban.[9]

Looking at the nation's gun crime problem more broadly, however, AWs and LCMs were used in only a minority of gun crimes prior to the 1994 federal ban, and AWs were used in a particularly small percentage of gun crimes.

[9] A number of localities around the nation also passed AW bans during this period.

3.1. Criminal Use of Assault Weapons

Numerous studies have examined the use of AWs in crime prior to the federal ban. The definition of AWs varied across the studies and did not always correspond exactly to that of the 1994 law (in part because a number of the studies were done prior to 1994). In general, however, the studies appeared to focus on various semiautomatics with detachable magazines and military-style features. According to these accounts, AWs typically accounted for up to 8% of guns used in crime, depending on the specific AW definition and data source used (e.g., see Beck et al., 1993; Hargarten et al., 1996; Hutson et al., 1994; 1995; McGonigal et al., 1993; New York State Division of Criminal Justice Services, 1994; Roth and Koper, 1997, Chapters 2, 5, 6; Zawitz, 1995). A compilation of 38 sources indicated that AWs accounted for 2% of crime guns on average (Kleck, 1997, pp.112, 141-143).[10]

Similarly, the most common AWs prohibited by the 1994 federal ban accounted for between 1% and 6% of guns used in crime according to most of several national and local data sources examined for this and our prior study (see Chapter 6 and Roth and Koper, 1997, Chapters 5, 6):

- Baltimore (all guns recovered by police, 1992-1993): 2%
- Miami (all guns recovered by police, 1990-1993): 3%
- Milwaukee (guns recovered in murder investigations, 1991-1993): 6%
- Boston (all guns recovered by police, 1991-1993): 2%
- St. Louis (all guns recovered by police, 1991-1993): 1%
- Anchorage, Alaska (guns used in serious crimes, 1987-1993): 4%
- National (guns recovered by police and reported to ATF, 1992-1993): 5%[11]
- National (gun thefts reported to police, 1992-Aug. 1994): 2%
- National (guns used in murders of police, 1992-1994): 7-9%[12]
- National (guns used in mass murders of 4 or more persons, 1992-1994): 4-13%[13]

Although each of the sources cited above has limitations, the estimates consistently show that AWs are used in a small fraction of gun crimes. Even the highest

[10] The source in question contains a total of 48 estimates, but our focus is on those that examined all AWs (including pistols, rifles, and shotguns) as opposed to just assault rifles.

[11] For reasons discussed in Chapter 6, the national ATF estimate likely overestimates the use of AWs in crime. Nonetheless, the ATF estimate lies within the range of other presented estimates.

[12] The minimum estimate is based on AW cases as a percentage of all gun murders of police. The maximum estimate is based on AW cases as a percentage of cases for which at least the gun manufacturer was known. Note that AWs accounted for as many as 16% of gun murders of police in 1994 (Roth and Koper, 1997, Chapter 6; also see Adler et al., 1995).

[13] These statistics are based on a sample of 28 cases found through newspaper reports (Roth and Koper, 1997, Appendix A). One case involved an AW, accounting for 3.6% of all cases and 12.5% of cases in which at least the type of gun (including whether the gun was a handgun, rifle, or shotgun and whether the gun was a semiautomatic) was known. Also see the earlier discussion of AWs and mass shootings at the beginning of this chapter.

estimates, which correspond to particularly rare events such mass murders and police murders, are no higher than 13%. Note also that the majority of AWs used in crime are assault pistols (APs) rather than assault rifles (ARs). Among AWs reported by police to ATF during 1992 and 1993, for example, APs outnumbered ARs by a ratio of 3 to 1 (see Chapter 6).

The relative rarity of AW use in crime can be attributed to a number of factors. Many AWs are long guns, which are used in crime much less often than handguns. Moreover, a number of the banned AWs are foreign weapons that were banned from importation into the U.S. in 1989. Also, AWs are more expensive (see Table 2-1) and more difficult to conceal than the types of handguns that are used most frequently in crime.

3.1.1. A Note on Survey Studies and Assault Weapons

The studies and statistics discussed above were based primarily on police information. Some survey studies have given a different impression, suggesting substantial levels of AW ownership among criminals and otherwise high-risk juvenile and adult populations, particularly urban gang members (Knox et al., 1994; Sheley and Wright, 1993a). A general problem with these studies, however, is that respondents themselves had to define terms like "military-style" and "assault rifle." Consequently, the figures from these studies may lack comparability with those from studies with police data. Further, the figures reported in some studies prompt concerns about exaggeration of AW ownership (perhaps linked to publicity over the AW issue during the early 1990s when a number of these studies were conducted), particularly among juvenile offenders, who have reported ownership levels as high as 35% just for ARs (Sheley and Wright, 1993a).[14]

Even so, most survey evidence on the actual use of AWs suggests that offenders rarely use AWs in crime. In a 1991 national survey of adult state prisoners, for example, 8% of the inmates reported possessing a "military-type" firearm at some point in the past (Beck et al., 1993, p. 19). Yet only 2% of offenders who used a firearm during their conviction offense reported using an AW for that offense (calculated from pp. 18, 33), a figure consistent with the police statistics cited above. Similarly, while 10% of adult inmates and 20% of juvenile inmates in a Virginia survey reported having owned an AR, none of the adult inmates and only 1% of the juvenile inmates reported having carried them at crime scenes (reported in Zawitz, 1995, p. 6). In contrast, 4% to 20% of inmates surveyed in eight jails across rural and urban areas of Illinois and Iowa reported having used an AR in committing crimes (Knox et al., 1994, p. 17). Nevertheless, even assuming the accuracy and honesty of the respondents' reports, it is not clear what

[14] As one example of possible exaggeration of AW ownership, a survey of incarcerated juveniles in New Mexico found that 6% reported having used a "military-style rifle" against others and 2.6% reported that someone else used such a rifle against them. However, less than 1% of guns recovered in a sample of juvenile firearms cases were "military" style guns (New Mexico Criminal Justice Statistical Analysis Center, 1998, pp. 17-19; also see Ruddell and Mays, 2003).

weapons they were counting as ARs, what percentage of their crimes were committed with ARs, or what share of all gun crimes in their respective jurisdictions were linked to their AR uses. Hence, while some surveys suggest that ownership and, to a lesser extent, use of AWs may be fairly common among certain subsets of offenders, the overwhelming weight of evidence from gun recovery and survey studies indicates that AWs are used in a small percentage of gun crimes overall.

3.1.2. Are Assault Weapons More Attractive to Criminal Users Than Other Gun Users?

Although AWs are used in a small percentage of gun crimes, some have argued that AWs are more likely to be used in crime than other guns, i.e., that AWs are more attractive to criminal than lawful gun users due to the weapons' military-style features and their particularly large ammunition magazines. Such arguments are based on data implying that AWs are more common among crime guns than among the general stock of civilian firearms. According to some estimates generated prior to the federal ban, AWs accounted for less than one percent of firearms owned by civilians but up to 11% of guns used in crime, based on firearms reported by police to ATF between 1986 and 1993 (e.g., see Cox Newspapers, 1989; Lennett, 1995). However, these estimates were problematic in a number of respects. As discussed in Chapter 6, ATF statistics are not necessarily representative of the types of guns most commonly recovered by police, and ATF statistics from the late 1980s and early 1990s in particular tended to overstate the prevalence of AWs among crime guns. Further, estimating the percentage of civilian weapons that are AWs is difficult because gun production data are not reported by model, and one must also make assumptions about the rate of attrition among the stock of civilian firearms.

Our own more recent assessment indicates that AWs accounted for about 2.5% of guns produced from 1989 through 1993 (see Chapter 5). Relative to previous estimates, this may signify that AWs accounted for a growing share of civilian firearms in the years just before the ban, though the previous estimates likely did not correspond to the exact list of weapons banned in 1994 and thus may not be entirely comparable to our estimate. At any rate, the 2.5% figure is comparable to most of the AW crime gun estimates listed above; hence, it is not clear that AWs are used disproportionately in most crimes, though AWs still seem to account for a somewhat disproportionate share of guns used in murders and other serious crimes.

Perhaps the best evidence of a criminal preference for AWs comes from a study of young adult handgun buyers in California that found buyers with minor criminal histories (i.e., arrests or misdemeanor convictions that did not disqualify them from purchasing firearms) were more than twice as likely to purchase APs than were buyers with no criminal history (4.6% to 2%, respectively) (Wintemute et al., 1998a). Those with more serious criminal histories were even more likely to purchase APs: 6.6% of those who had been charged with a gun offense bought APs, as did 10% of those who had been charged with two or more serious violent offenses. AP purchasers were also more likely to be arrested subsequent to their purchases than were other gun purchasers.

Among gun buyers with prior charges for violence, for instance, AP buyers were more than twice as likely as other handgun buyers to be charged with any new offense and three times as likely to be charged with a new violent or gun offense. To our knowledge, there have been no comparable studies contrasting AR buyers with other rifle buyers.

3.2. Criminal Use of Large Capacity Magazines

Relative to the AW issue, criminal use of LCMs has received relatively little attention. Yet the overall use of guns with LCMs, which is based on the combined use of AWs and non-banned guns with LCMs, is much greater than the use of AWs alone. Based on data examined for this and a few prior studies, guns with LCMs were used in roughly 14% to 26% of most gun crimes prior to the ban (see Chapter 8; Adler et al., 1995; Koper, 2001; New York Division of Criminal Justice Services, 1994).

- Baltimore (all guns recovered by police, 1993): 14%
- Milwaukee (guns recovered in murder investigations, 1991-1993): 21%
- Anchorage, Alaska (handguns used in serious crimes, 1992-1993): 26%
- New York City (guns recovered in murder investigations, 1993): 16-25%[15]
- Washington, DC (guns recovered from juveniles, 1991-1993): 16%[16]
- National (guns used in murders of police, 1994): 31%-41%[17]

Although based on a small number of studies, this range is generally consistent with national survey estimates indicating approximately 18% of all civilian-owned guns and 21% of civilian-owned handguns were equipped with LCMs as of 1994 (Cook and Ludwig, 1996, p. 17). The exception is that LCMs may have been used disproportionately in murders of police, though such incidents are very rare.

As with AWs and crime guns in general, most crime guns equipped with LCMs are handguns. Two handgun models manufactured with LCMs prior to the ban (the Glock 17 and Ruger P89) were among the 10 crime gun models most frequently recovered by law enforcement and reported to ATF during 1994 (ATF, 1995).

[15] The minimum estimate is based on cases in which discharged firearms were recovered, while the maximum estimate is based on cases in which recovered firearms were positively linked to the case with ballistics evidence (New York Division of Criminal Justice Services, 1994).
[16] Note that Washington, DC prohibits semiautomatic firearms accepting magazines with more than 12 rounds (and handguns in general).
[17] The estimates are based on the sum of cases involving AWs or other guns sold with LCMs (Adler et al., 1995, p.4). The minimum estimate is based on AW-LCM cases as a percentage of all gun murders of police. The maximum estimate is based on AW-LCM cases as a percentage of cases in which the gun model was known.

3.3. Summary

In sum, AWs and LCMs were used in up to a quarter of gun crimes prior to the 1994 AW-LCM ban. By most estimates, AWs were used in less than 6% of gun crimes even before the ban. Some may have perceived their use to be more widespread, however, due to the use of AWs in particularly rare and highly publicized crimes such as mass shootings (and, to a lesser extent, murders of police), survey reports suggesting high levels of AW ownership among some groups of offenders, and evidence that some AWs are more attractive to criminal than lawful gun buyers.

In contrast, guns equipped with LCMs – of which AWs are a subset – are used in roughly 14% to 26% of gun crimes. Accordingly, the LCM ban has greater potential for affecting gun crime. However, it is not clear how often the ability to fire more than 10 shots without reloading (the current magazine capacity limit) affects the outcomes of gun attacks (see Chapter 9). All of this suggests that the ban's impact on gun violence is likely to be small.

4. OVERVIEW OF STUDY DESIGN, HYPOTHESES, AND PRIOR FINDINGS

Section 110104 of the AW-LCM ban directed the Attorney General of the United States to study the ban's impact and report the results to Congress within 30 months of the ban's enactment, a provision which was presumably motivated by a sunset provision in the legislation (section 110105) that will lift the ban in September 2004 unless Congress renews the ban. In accordance with the study requirement, the National Institute of Justice (NIJ) awarded a grant to the Urban Institute to study the ban's short-term (i.e., 1994-1996) effects. The results of that study are available in a number of reports, briefs, and articles written by members of this research team (Koper and Roth, 2001a; 2001b; 2002a; Roth and Koper, 1997; 1999).[18] In order to understand the ban's longer-term effects, NIJ provided additional funding to extend the AW research. In 2002, we delivered an interim report to NIJ based on data extending through at least the late 1990s (Koper and Roth, 2002b). This report is based largely on the 2002 interim report, but with various new and updated analyses extending as far as 2003. It is thus a compilation of analyses conducted between 1998 and 2003. The study periods vary somewhat across the analyses, depending on data availability and the time at which the data were collected.

4.1. Logical Framework for Research on the Ban

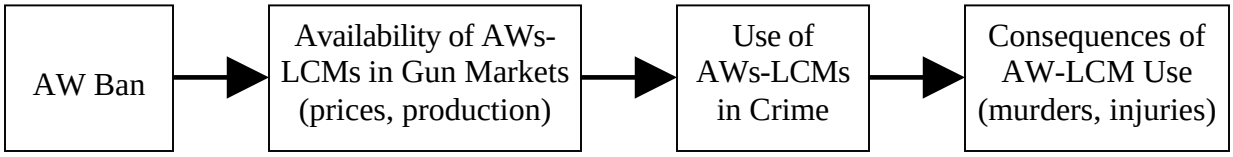
An important rationale for the AW-LCM ban is that AWs and other guns equipped with LCMs are particularly dangerous weapons because they facilitate the rapid firing of high numbers of shots, thereby potentially increasing injuries and deaths from gun violence. Although AWs and LCMs were used in only a modest share of gun crimes before the ban, it is conceivable that a decrease in their use might reduce fatal and non-fatal gunshot victimizations, even if it does not reduce the overall rate of gun crime. (In Chapter 9, we consider in more detail whether forcing offenders to substitute other guns and smaller magazines can reduce gun deaths and injuries.)

It is not clear how quickly such effects might occur, however, because the ban exempted the millions of AWs and LCMs that were manufactured prior to the ban's effective date in September 1994. This was particularly a concern for our first study, which was based on data extending through mid-1996, a period potentially too short to observe any meaningful effects. Consequently, investigation of the ban's effects on gun markets – and, most importantly, how they have affected criminal use of AWs and LCMs – has played a central role in this research. The general logic of our studies, illustrated in Figure 4-1, has been to first assess the law's impact on the availability of AWs and LCMs, examining price and production (or importation) indices in legal markets and relating them to trends in criminal use of AWs and LCMs. In turn, we can relate these market patterns to trends in the types of gun crimes most likely to be affected by changes in the use of AWs and LCMs. However, we cannot make definitive assessments of the

[18] The report to Congress was the Roth and Koper (1997) report.

ban's impact on gun violence until it is clear that the ban has indeed reduced criminal use of AWs and LCMs.

Figure 4-1. Logic Model for Research on the Assault Weapons Ban



4.2. Hypothesized Market Effects

4.2.1. A General Description of Gun Markets

Firearms are distributed in markets commonly referred to as primary and secondary markets. Illicit gun transactions occur in both markets. Primary markets include wholesale and retail transactions by federally-licensed gun dealers, referred to as federal firearm licensees. Licensed dealers are required to, among things, follow federal and state background procedures to verify the eligibility of purchasers, observe any legally required waiting period prior to making transfers, and maintain records of gun acquisitions and dispositions (though records are not required for sales of ammunition magazines).

Despite these restrictions, survey data suggest that as many as 21% of adult gun offenders obtained guns from licensed dealers in the years prior to the ban (Harlow, 2001, p. 6; also see Wright and Rossi, 1986, pp. 183,185). In more recent years, this figure has declined to 14% (Harlow, 2001, p. 6), due likely to the Brady Act, which established a national background check system for purchases from licensed dealers, and reforms of the federal firearms licensing system that have greatly reduced the number of licensed gun dealers (see ATF, 2000; Koper, 2002). Some would-be gun offenders may be legally eligible buyers at the time of their acquisitions, while others may seek out corrupt dealers or use other fraudulent or criminal means to acquire guns from retail dealers (such as recruiting a legally entitled buyer to act as a "straw purchaser" who buys a gun on behalf of a prohibited buyer).

Secondary markets encompass second-hand gun transactions made by non-licensed individuals.[19] Secondary market participants are prohibited from knowingly transferring guns to ineligible purchasers (e.g., convicted felons and drug abusers). However, secondary transfers are not subject to the federal record-keeping and background check requirements placed on licensed dealers, thus making the secondary

[19] Persons who make only occasional sales of firearms are not required to obtain a federal firearms license (ATF, 2000, p. 11).

market almost entirely unregulated and, accordingly, a better source of guns for criminal users.[20] In the secondary market, ineligible buyers may obtain guns from a wide variety of legitimate or illegitimate gun owners: relatives, friends, fences, drug dealers, drug addicts, persons selling at gun shows, or other strangers (e.g., see Wright and Rossi, 1986; Sheley and Wright, 1993a). Of course, ineligible purchasers may also steal guns from licensed gun dealers and private gun owners.

Secondary market prices are generally lower than primary market prices (because the products are used), though the former may vary substantially across a range of gun models, places, circumstances, and actors. For example, street prices of AWs and other guns can be 3 to 6 times higher than legal retail prices in jurisdictions with strict gun controls and lower levels of gun ownership (Cook et al., 1995, p. 72). Nonetheless, experts note that primary and secondary market prices correspond to one another, in that relatively expensive guns in the primary market are also relatively expensive in the secondary market. Moreover, in any given locality, trends in secondary market prices can be expected to track those in the primary market because a rise in primary market prices for new weapons will increase demand for used weapons and therefore increase secondary market prices (Cook et al., 1995, p. 71).

4.2.2. The AW-LCM Ban and Gun Markets

In the long term, we can expect prices of the banned guns and magazines to gradually rise as supplies dwindle. As prices rise, more would-be criminal users of AWs and LCMs will be unable or unwilling to pay the higher prices. Others will be discouraged by the increasing non-monetary costs (i.e., search time) of obtaining the weapons. In addition, rising legal market prices will undermine the incentive for some persons to sell AWs and LCMs to prohibited buyers for higher premiums, thereby bidding some of the weapons away from the channels through which they would otherwise reach criminal users. Finally, some would-be AW and LCM users may become less willing to risk confiscation of their AWs and LCMs as the value of the weapons increases. Therefore, we expect that over time diminishing stocks and rising prices will lead to a reduction in criminal use of AWs and LCMs.[21]

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[20] Some states require that secondary market participants notify authorities about their transactions. Even in these states, however, it is not clear how well these laws are enforced.

[21] We would expect these reductions to be apparent shortly after the price increases (an expectation that, as discussed below, was confirmed in our earlier study) because a sizeable share of guns used in crime are used within one to three years of purchase. Based on analyses of guns recovered by police in 17 cities, ATF (1997, p. 8) estimates that guns less than 3 years old (as measured by the date of first retail sale) comprise between 22% and 43% of guns seized from persons under age 18, between 30% and 54% of guns seized from persons ages 18 to 24, and between 25% and 46% of guns seized from persons over 24. In addition, guns that are one year old or less comprise the largest share of relatively new crime guns (i.e., crime guns less than three years old) (Pierce et al., 1998, p. 11). Similar data are not available for secondary market transactions, but such data would shorten the estimated time from acquisition to criminal use.

However, the expected timing of the market processes is uncertain. We can anticipate that AW and LCM prices will remain relatively stable for as long as the supply of grandfathered weapons is adequate to meet demand. If, in anticipation of the ban, gun manufacturers overestimated the demand for AWs and LCMs and produced too many of them, prices might even fall before eventually rising. Market responses can be complicated further by the continuing production of legal AW substitute models by some gun manufacturers. If potential AW buyers are content with an adequate supply of legal AW-type weapons having fewer military features, it will take longer for the grandfathered AW supply to constrict and for prices to rise. Similarly, predicting LCM price trends is complicated by the overhang of military surplus magazines that can fit civilian weapons (e.g., military M-16 rifle magazines that can be used with AR-15 type rifles) and by the market in reconditioned magazines. The "aftermarket" in gun accessories and magazine extenders that can be used to convert legal guns and magazines into banned ones introduces further complexity to the issue.

4.3. Prior Research on the Ban's Effects

To summarize the findings of our prior study, Congressional debate over the ban triggered pre-ban speculative price increases of upwards of 50% for AWs during 1994, as gun distributors, dealers, and collectors anticipated that the weapons would become valuable collectors' items. Analysis of national and local data on guns recovered by police showed reductions in criminal use of AWs during 1995 and 1996, suggesting that rising prices made the weapons less accessible to criminal users in the short-term aftermath of the ban.

However, the speculative increase in AW prices also prompted a pre-ban boost in AW production; in 1994, AW manufacturers produced more than twice their average volume for the 1989-1993 period. The oversupply of grandfathered AWs, the availability of the AW-type legal substitute models mentioned earlier, and the steady supply of other non-banned semiautomatics appeared to have saturated the legal market, causing advertised prices of AWs to fall to nearly pre-speculation levels by late 1995 or early 1996. This combination of excess supply and reduced prices implied that criminal use of AWs might rise again for some period around 1996, as the large stock of AWs would begin flowing from dealers' and speculators' gun cases to the secondary markets where ineligible purchasers may obtain guns more easily.

We were not able to gather much specific data about market trends for LCMs. However, available data did reveal speculative, pre-ban price increases for LCMs that were comparable to those for AWs (prices for some LCMs continued to climb into 1996), leading us to speculate – incorrectly, as this study will show (see Chapter 8) – that there was some reduction in LCM use after the ban.[22]

[22] To our knowledge, there have been two other studies of changes in AW and LCM use during the post-ban period. One study reported a drop in police recoveries of AWs in Baltimore during the first half of 1995 (Weil and Knox, 1995), while the other found no decline in recoveries of AWs or LCMs in Milwaukee homicide cases as of 1996 (Hargarten et al., 2000). Updated analyses for both of these cities

Determining whether the reduction in AW use (and perhaps LCM use) following the ban had an impact on gun violence was more difficult. The gun murder rate dropped more in 1995 (the first year following the ban) than would have been expected based on preexisting trends, but the short post-ban follow-up period available for the analysis precluded a definitive assessment as to whether the reduction was statistically meaningful (see especially Koper and Roth, 2001a). The reduction was also larger than would be expected from the AW-LCM ban, suggesting that other factors were at work in accelerating the decline. Using a number of national and local data sources, we also examined trends in measures of victims per gun murder incident and wounds per gunshot victim, based on the hypothesis that these measures might be more sensitive to variations in the use of AWs and LCMs. These analyses revealed no ban effects, thus failing to show confirming evidence of the mechanism through which the ban was hypothesized to affect the gun murder rate. However, newly available data presented in subsequent chapters suggest these assessments may have been premature, because any benefits from the decline in AW use were likely offset by steady or rising use of other guns equipped with LCMs, a trend that was not apparent at the time of our earlier study.

We cautioned that the short-term patterns observed in the first study might not provide a reliable guide to longer-term trends and that additional follow-up was warranted. Two key issues to be addressed were whether there had been a rebound in AW use since the 1995-1996 period and, if so, whether that rebound had yet given way to a long-term reduction in AW use. Another key issue was to seek more definitive evidence on short and long-term trends in the availability and criminal use of LCMs. These issues are critical to assessing the effectiveness of the AW-LCM ban, but they also have broader implications for other important policy concerns, namely, the establishment of reasonable timeframes for sunset and evaluation provisions in legislation. In other words, how long is long enough in evaluating policy and setting policy expiration dates?

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are presented in Chapters 6 and 8.

5. MARKET INDICATORS FOR ASSAULT WEAPONS: PRICES AND PRODUCTION

This chapter assesses the ban's impact on the availability of AWs in primary and secondary markets, as measured by trends in AW prices and post-ban production of legal AW substitute models. Understanding these trends is important because they influence the flow of grandfathered weapons to criminals and the availability of non-banned weapons that are close substitutes for banned ones. In the next chapter, we assess the impact of these trends on criminal use of AWs, as approximated by statistics on gun seizures by police. (Subsequent chapters present similar analyses for LCMs.)

Following our previous methods, we compare trends for AWs to trends for various non-banned firearms. The AW analyses generally focus on the most common AWs formerly produced in the U.S., including Intratec and SWD-type APs and AR-15-type ARs produced by Colt and others. In addition, we selected a small number of domestic pistol and rifle models made by Calico and Feather Industries that fail the features test provision of the AW legislation and that were relatively common among crime guns reported by law enforcement agencies to ATF prior to the ban (see Roth and Koper, 1997, Chapter 5). Together, this group of weapons represented over 80% of AWs used in crime and reported to ATF from 1993 through 1996, and the availability of these guns was not affected by legislation or regulations predating the AW-LCM ban.[23] We also examine substitution of legalized, post-ban versions of these weapons, including the Intratec AB-10 and Sport-22, FMJ's PM models (substitutes for the SWD group), Colt Sporters, Calico Liberty models, and others. We generally did not conduct comparative analyses of named foreign AWs (the Uzi, Galil, and AK weapons) because the 1989 federal import ban had already limited their availability, and their legal status was essentially unchanged by the 1994 ban.

The exact gun models and time periods covered vary across the analyses (based on data availability and the time at which data were collected). The details of each analysis are described in the following sections.

5.1. Price Trends for Assault Weapons and Other Firearms

To approximate trends in the prices at which AWs could be purchased throughout the 1990s, we collected annual price data for several APs, ARs, and non-banned comparison firearms from the *Blue Book of Gun Values* (Fjestad, 1990-1999). The *Blue Book* provides national average prices for an extensive list of new and used firearms based on information collected at gun shows and input provided by networks of dealers

[23] The Intratec group includes weapons made by AA Arms. The SWD group contains related models made by Military Armaments Corporation/Ingram and RPB Industries. The AR-15 group contains models made by Colt and copies made by Bushmaster, Olympic Arms, Eagle Arms, SGW Enterprises, Essential Arms, DPMS, and Sendra.

and collectors. The *Blue Book* is utilized widely in the gun industry, though prices in any given locality may differ notably from the averages appearing in the *Blue Book*.

To assess time trends in gun prices, we conducted hedonic price analyses (Berndt, 1990) in which the gun prices were regressed upon a series of year and model indicators. The coefficients for the year indicators show annual changes in the prices of the guns relative to 1994 (the year the ban went into effect), controlling for time-stable differences in the prices of various gun models. Since manufacturers' suggested retail prices (MSRP) were not available for banned AWs during post-ban years, we utilized prices for AWs in 100% condition for all years.[24] For non-banned firearms, we used MSRP.[25] For all models, we divided the gun prices by annual values of the gross domestic product price deflator provided in the December 2001 and 2000 issues of *Economic Indicators* and logged these adjusted prices.

Each model presented below is based on data pooled across a number of firearm models and years, so that observation P_{jt} represents the price of gun model j during year t. We weighted each observation, P_{jt}, based on cumulative estimates of the production of model j from 1985 or 1986 (depending on data availability) through year t using data provided by gun manufacturers to ATF and published by the Violence Policy Center (1999).[26, 27]

[24] Project staff also collected prices of weapons in 80% condition. However, the levels and annual changes of the 80% prices were very highly correlated (0.86 to 0.99) with those of the 100% condition prices. Therefore, we limited the analysis to the 100% prices.

[25] We utilized prices for the base model of each AW and comparison firearm (in contrast to model variations with special features or accessories).

[26] The regression models are based on equal numbers of observations for each gun model. Hence, unweighted regressions would give equal weight to each gun model. This does not seem appropriate, however, because some guns are produced in much larger numbers than are other guns. Weighting the regression models by production estimates should therefore give us a better sense of what one could "typically" expect to pay for a generic gun in each study category (e.g., a generic assault pistol).

[27] Several of the selected weapons began production in 1985 or later. In other cases, available production data extended back to only the mid-1980s. Published production figures for handguns are broken down by type (semiautomatic, revolver) and caliber and thus provide perfect or very good approximations of production for the handgun models examined in this study. Rifle production data, however, are not disaggregated by gun type, caliber, or model. For the ARs under study, the production counts should be reasonable approximations of AR production because most of the rifles made by the companies in question prior to the ban were ARs. The rifles used in the comparison (i.e., non-banned) rifle analysis are made by companies (Sturm Ruger, Remington, and Marlin) that produce numerous semiautomatic and non-semiautomatic rifle models. However, the overall rifle production counts for these companies should provide some indication of differences in the availability of the comparison rifles relative to one another. Because production data were available through only 1997 at the time this particular analysis was conducted (Violence Policy Center, 1999), we used cumulative production through 1997 to weight the 1998 and 1999 observations for the comparison handgun and comparison rifle models. This was not a consideration for AWs since their production ceased in 1994 (note that the AW production figures for 1994 may include some post-ban legal substitute models manufactured after September 13, 1994). Nonetheless, weighting had very little effect on the inferences from either of the comparison gun models.

5.1.1. Assault Pistol Prices

The analysis of AP prices focuses on the Intratec TEC-9/DC-9, TEC-22, SWD M-11/9, and Calico M950 models. Regression results are shown in Table 5-1, while Figure 5-1 graphically depicts the annual trend in prices for the period 1990 through 1999. None of the yearly coefficients in Table 5-1 is statistically significant, thus indicating that average annual AP prices did not change during the 1990s after adjusting for inflation. Although the model is based on a modest number of observations (n=40) that may limit its statistical power (i.e., its ability to detect real effects), the size of the yearly coefficients confirm that prices changed very little from year to year. The largest yearly coefficient is for 1990, and it indicates that AP prices were only 4% higher in 1990 than in 1994.[28]

This stands in contrast to our earlier finding (Roth and Koper, 1997, Chapter 4) that prices for SWD APs may have risen by as much as 47% around the time of the ban. However, the earlier analyses were based on semi-annual or quarterly analyses advertised by gun distributors and were intended to capture short-term fluctuations in price that assumed greater importance in the context of the first AW study, which could examine only short-term ban outcomes. *Blue Book* editions released close in time to the ban (e.g., 1995) also cautioned that prices for some AWs were volatile at that time. This study emphasizes longer-term price trends, which appear to have been more stable.[29]

[28] To interpret the coefficient of each indicator variable in terms of a percentage change in the dependent variable, we exponentiate the coefficient, subtract 1 from the exponentiated value, and multiply the difference by 100.

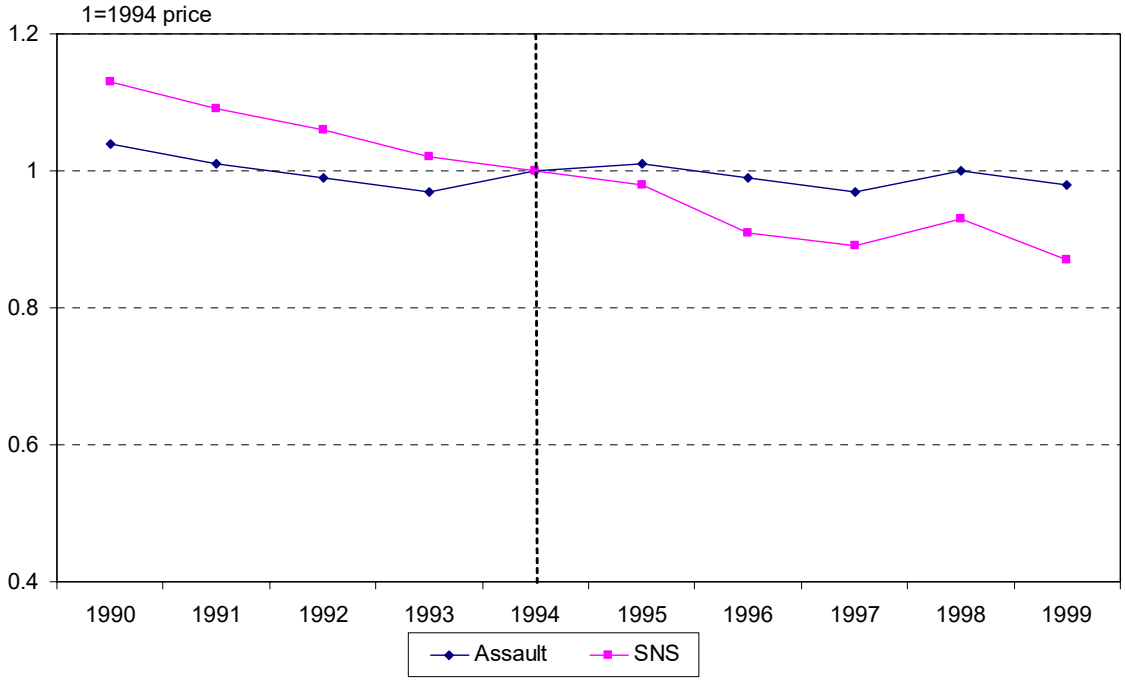
[29] Although the earlier analysis of AP prices focused on the greatest variations observed in semi-annual prices, the results also provide indications that longer-term trends were more stable. Prices in 1993, for example, averaged roughly 73% of the peak prices reached at the time the ban was implemented (i.e., late 1994), while prices in early 1994 and late 1995 averaged about 83% and 79% of the peak prices, respectively. Hence, price variation was much more modest after removing the peak periods around the time of the ban‘s implementation (i.e., late 1994 and early 1995). The wider range of APs used in the current study may also be responsible for some of the differences between the results of this analysis and the prior study.

Table 5-1. Regression of Assault Pistol and Comparison Handgun Prices on Annual Time Indicators, 1990-1999, Controlling for Gun Model

	Assault Pistols (n=40)		Comparison Handguns (n=38)	
	Estimate	**T Value**	**Estimate**	**T Value**
Constant	1.56	26.94***	-0.21	-6.81***
1990	0.04	1.07	0.12	2.07**
1991	0.01	0.30	0.09	1.79*
1992	-0.01	-0.32	0.05	1.30
1993	-0.03	-1.09	0.02	0.48
1995	0.01	0.22	-0.02	-0.48
1996	-0.01	-0.45	-0.09	-2.69***
1997	-0.03	-1.13	-0.11	-3.26***
1998	0.00	-0.10	-0.07	-1.99*
1999	-0.02	-0.58	-0.14	-4.02***
Tec-9	-0.67	-11.95***		
Tec-22	-0.89	-15.59***		
SWD	-0.64	-11.49***		
Davis P32			0.09	3.63***
Davis P380			0.20	8.20***
Lorcin L380			0.29	11.35***
F value	27.79		16.24	
(p value)	<.01		<.01	
Adj. R-square	0.89		0.83	

Time indicators are interpreted relative to 1994. Assault pistol model indicators are interpreted relative to Calico 9mm. Comparison handgun models are interpreted relative to Lorcin .25 caliber.
* Statistically significant at p<=.10.
** Statistically significant at p<=.05.
*** Statistically significant at p<=.01.

Figure 5-1. Annual Price Trends for Assault Pistols and SNS Handguns, 1990-1999



Assault pistol prices basd on TEC9, TEC22, SWD M11/9, and Calico M950. SNS prices based on Davis P32 and P380 and Lorcin L25 and L380.

5.1.2. Comparison Handgun Prices

For comparison, Table 5-1 and Figure 5-1 illustrate price trends for a number of non-banned, cheaply priced, and readily concealable semiautomatic handgun models: the Davis P32 and P380 and the Lorcin L25 and L380. Such guns are often referred to as Saturday night specials (SNS). By a number of accounts, SNS-type guns, and Davis and Lorcin models in particular, are among the guns most frequently used in crime (ATF, 1995; 1997; Kennedy et al., 1996; Wintemute, 1994). Although the differences between APs and SNS handguns (particularly the fact that most SNS handguns do not have LCMs) suggest they are likely to be used by gun consumers with different levels of firearms experience and sophistication, the SNS guns are arguably a good comparison group for APs because both groups of guns are particularly sensitive to criminal demand. Like AP buyers, SNS buyers are more likely than other gun buyers to have criminal histories and to be charged with new offenses, particularly violent or firearm offenses, subsequent to their purchases (Wintemute et al., 1998b).

Prices of SNS handguns dropped notably throughout the 1990s. Prices for SNS handguns were 13% higher in 1990 than in 1994. Prices then dropped another 13% from 1994 to 1999. This suggests that although AP prices remained generally stable throughout the 1990s, they increased relative to prices of other guns commonly used in crime. We say more about this below.

5.1.3. Assault Rifle Prices

To assess trends in prices of ARs, we examined prices for several Colt and Olympic rifle models in the AR-15 class, as well as Calico models M900 and M951 and Feather models AT9 and AT22.[30] Because rifle production data are not disaggregated by weapon type (semiautomatic, bolt action, etc.), caliber, or model, the regressions could only be weighted using overall rifle production counts for each company. For this reason, we calculated the average price of the ARs made by each company for each year and modeled the trends in these average prices over time, weighting by each company's total rifle production.[31]

Results shown in Table 5-2 and Figure 5-2 demonstrate that AR prices rose significantly during 1994 and 1995 before falling back to pre-ban levels in 1996 and remaining there through 1999. Prices rose 16% from 1993 to 1994 and then increased another 13% in 1995 (representing an increase of nearly one third over the 1993 level). Yet by 1996, prices had fallen to levels virtually identical to those before 1994. These patterns are consistent with those we found earlier for the 1992-1996 period (Roth and Koper, 1997, Chapter 4), though the annual price fluctuations shown here were not as dramatic as the quarterly changes shown in the earlier study.

Note, however, that these patterns were not uniform across all of the AR categories. The results of the model were driven largely by the patterns for Colt rifles, which are much more numerous than the other brands. Olympic rifles increased in price throughout the time period, while prices for most Calico and Feather rifles tended to fall throughout the 1990s without necessarily exhibiting spikes around the time of the ban.

[30] Specifically, we tracked prices for the Match Target Lightweight (R6530), Target Government Model (R6551), Competition H-Bar (R6700), and Match Target H-Bar (R6601) models by Colt and the Ultramatch, Service Match, Multimatch M1-1, AR15, and CAR15 models by Olympic Arms. Each of these models has a modified, post-ban version. We utilized prices for the pre-ban configurations during post-ban years.

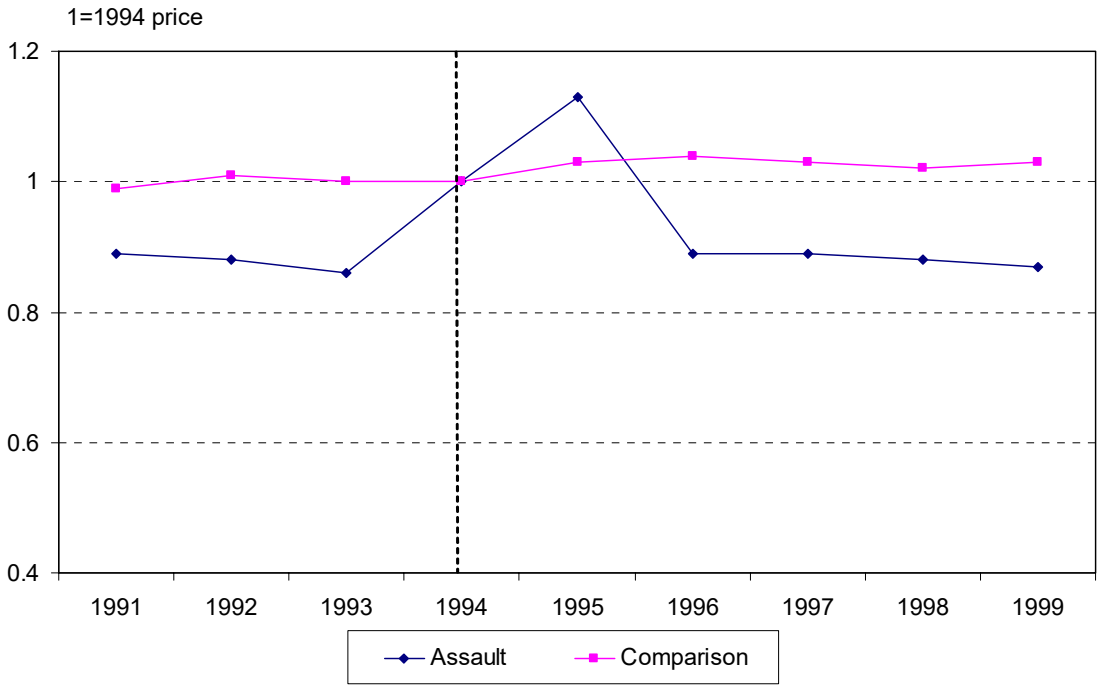
[31] Prices for the different models made by a given manufacturer tended to follow comparable trends, thus strengthening the argument for averaging prices.

Table 5-2. Regression of Assault Rifle and Comparison Semiautomatic Rifle Prices on Annual Time Indicators, 1991-1999, Controlling for Gun Make

	Assault Rifles (n=36)		Comparison Rifles (n=27)	
	Estimate	**T value**	**Estimate**	**T value**
Constant	1.31	21.15***	1.40	76.75***
1991	-0.12	-1.98*	-0.01	-0.21
1992	-0.13	-2.26**	0.01	0.30
1993	-0.15	-2.78**	0	-0.13
1995	0.12	2.47**	0.03	1.08
1996	-0.11	-2.27**	0.04	1.69
1997	-0.11	-2.23**	0.03	1.46
1998	-0.12	-2.47**	0.02	0.91
1999	-0.14	-2.71**	0.03	1.21
Colt (AR-15 type)	1.07	19.93***		
Olympic (AR-15 type)	1.14	16.08***		
Calico	0.43	5.53***		
Ruger			0.26	20.07***
Remington			0.29	21.69***
F statistic	50.52			63.62
(p value)	<.01			<.01
Adj. R-square	0.94			0.96

Time indicators interpreted relative to 1994. Assault rifle makes interpreted relative to Feather.
Comparison rifle makes interpreted relative to Marlin.
* Statistically significant at p<=.10.
** Statistically significant at p<=.05.
*** Statistically significant at p<=.01.

Figure 5-2. Annual Price Trends for Assault Rifles and Comparison Semiautomatic Rifles, 1991-1999



Assault rifle prices based on Colt and Olympic AR-type, Calico, and Feather models. Comparison rifle prices based on selected Remington, Marlin, and Sturm Ruger models.

5.1.4. Comparison Semiautomatic Rifles.

The analysis of comparison rifle prices includes the Remington 7400, Marlin Model 9, and Sturm Ruger Mini-14 and Mini-30 models (the Ruger model prices were averaged for each year). The AW legislation exempted each of these semiautomatic rifles by name, though the exemption does not apply to Mini-14 models with folding stocks (a feature included in the ban's features test). The Ruger models are of particular interest since they are among only four exempted guns that can accept LCMs made for military rifles (U.S. Department of the Treasury, 1998, p. 23), though Ruger produced LCMs only for the Mini-14 model and substituted a 5-round magazine for this gun in 1989 (Fjestad, 2002, pp. 1361-1362). The Marlin model was also manufactured with an LCM prior to 1990 (Fjestad, 2002, p. 917). The Remington model is manufactured with a detachable 4-round magazine.

Prices for these guns remained steady throughout the decade (see Table 5-2 and Figure 5-2). The largest change was a 4% increase (non-significant) in prices in 1996 relative to prices in 1994. Therefore, the rifle price spikes in 1994 and 1995 were specific to assault rifles. However, the steady annual price trends may mask short-term fluctuations that we found

previously (Roth and Koper, 1997, Chapter 4) for some non-banned semiautomatic rifles (including the Ruger Mini-14) during 1994 and early 1995.[32]

5.2. Production Trends for Assault Weapons and Other Firearms

To more fully assess the ban's effects on gun markets, examination of pre and post-ban trends in production of AWs and legal AW substitutes is a useful complement to studying price trends. Our earlier work revealed a spike in AW production during 1994 as the ban was being debated. Post-ban production of legal AW substitutes should reveal additional information about the reaction of gun markets to the ban. If production of these models has fallen off dramatically, it may suggest that the market for AWs has been temporarily saturated and/or that consumers of AWs favor the original AW models that have more military-style features. Stable or rising production levels, on the other hand, may indicate substantial consumer demand for AW substitutes, which would suggest that consumers consider the legal substitute models to be as desirable as the banned models.

5.2.1. Production of Assault Pistols and Other Handguns

Figure 5-3 presents production trends for a number of domestic AP manufacturers from 1985 through 2001 (the most recent year available for data on individual manufacturers).[33] After rising in the early 1990s and surging notably to a peak in 1994, production by these companies dropped off dramatically, falling 80% from 1993-1994 to 1996-1997 and falling another 35% by 1999-2000 (Table 5-3).[34] Makers of Intratec and SWD-type APs continued manufacturing modified versions of their APs for at least a few years following the ban, but at much lower volumes than that at which they produced APs just prior to the ban. Companies like AA Arms and Calico produced very few or no AP-type pistols from 1995 onward, and Intratec – producers of the APs most frequently used in crime – went out of business after 1999.

However, the pattern of rising and then falling production was not entirely unique to APs. Table 5-3 shows that production of all handguns and production of SNS-type pistols both declined sharply in the mid to late 1990s following a peak in 1993. Nonetheless, the trends –

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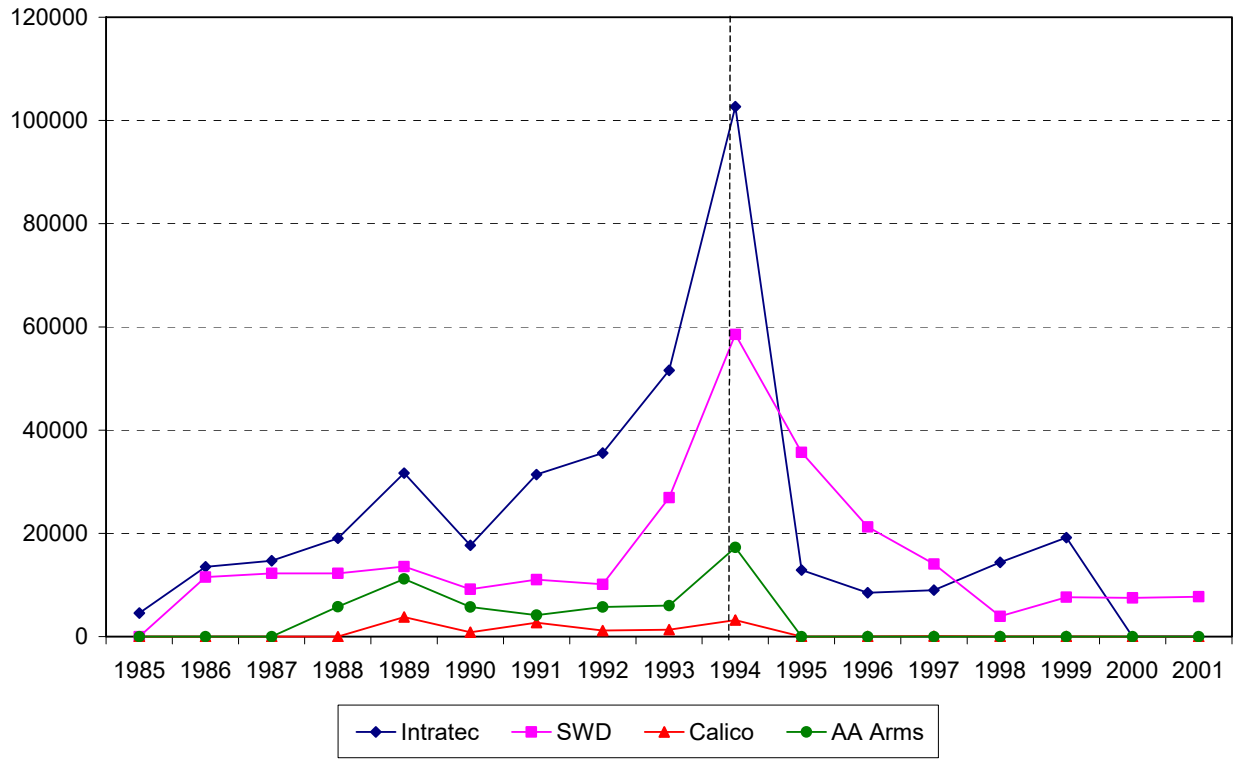
[32] We attributed those short-term fluctuations to pre-ban uncertainty regarding which semiautomatic rifles would be prohibited by the ban. Also note that the prior findings were based on a different set of comparison semiautomatic rifles that included a number of foreign rifles. We concentrated on domestically produced rifles for this updated analysis in order to make more explicit links between rifle price and production trends (data for the latter are available only for domestic firearms).

[33] Production figures for individual manufacturers through 2000 have been compiled by the Violence Policy Center (2002). Year 2001 data are available from ATF via the Internet (see www.atf.treas.gov). National gun production totals through 1998 are also available from ATF (2000, p. A-3).

[34] The assault pistol production figures used here and in the price analysis include 9mm and .22 caliber pistols made by Intratec, 9mm pistols manufactured by AA Arms, all non-.22 caliber pistols manufactured by S.W. Daniels, Wayne Daniels, and Military Armaments Corporation (which together constitute the SWD group), and .22 and 9mm pistols manufactured by Calico. Intratec produces a few non-AW models in .22 and 9mm calibers, so the Intratec figures will overstate production of assault pistols and their legal substitutes to some degree. The comparison, SNS production figures are based on all handguns produced by Lorcin Engineering and Davis Industries.

both peak and decline – were more dramatic for APs than for other handguns. Production of APs rose 69% from 1990-1991 to 1993-1994, while SNS production and overall handgun production each increased 47%. From 1993-1994 to 1996-1997, production of AP-type handguns, SNS models, and all handguns declined 80%, 66%, and 47%, respectively. Further, production of AP-type handguns continued to decline at a faster rate than that of other handguns through the end of the decade.[35]

Figure 5-3. Assault Pistol Production, 1985-2001



[35] Lorcin, a prominent SNS brand that we examined for the price and production analyses, went out of business after 1998. Unlike the situation in the AP market (where, to our knowledge, former AP makers have not been replaced on any large scale), the SNS market appears to have compensated somewhat to offset the loss of Lorcin. The SNS change from 1996-1997 to 1999-2000 is based on examination of a larger group of SNS-type makers, including Lorcin, Davis, Bryco, Phoenix Arms, and Hi-Point. Production among this group declined by 22% from 1996-1997 to 1999-2000, a decline greater than that for total handgun production but less than that for AP-type production.

Table 5-3. Production Trends for Assault Weapons and Other Firearms, 1990-2000*

Firearm Category	% Change 1990/91 to 1993/94	% Change 1993/94 to 1996/97	% Change 1996/97 to 1999/2000
Total Handguns	47%	-47%	-10%
Assault Pistols (or Post-Ban Models)	69%	-80%	-35%
SNS Handguns	47%	-66%	-22%
Total Rifles	22%	8%	18%
Assault Rifles (or Post-Ban Models)	81%	-51%	156%
Comparison Rifles	15%	13%	-16%

* Total handgun and rifle figures include all production by U.S. manufacturers. Assault pistols include Intratec group, SWD group, and Calico models. SNS figures are based on Lorcin Engineering and Davis Industries for changes up through 1996-1997. Because Lorcin went out of business after 1998, the SNS change from 1996-1997 to 1999-2000 is based on a larger group of SNS makers including Lorcin, Davis, Bryco, Phoenix Arms, and Hi-Point. Assault rifles include AR-15 type models by Colt and others. Comparison rifles include Sturm Ruger, Remington, and Marlin.

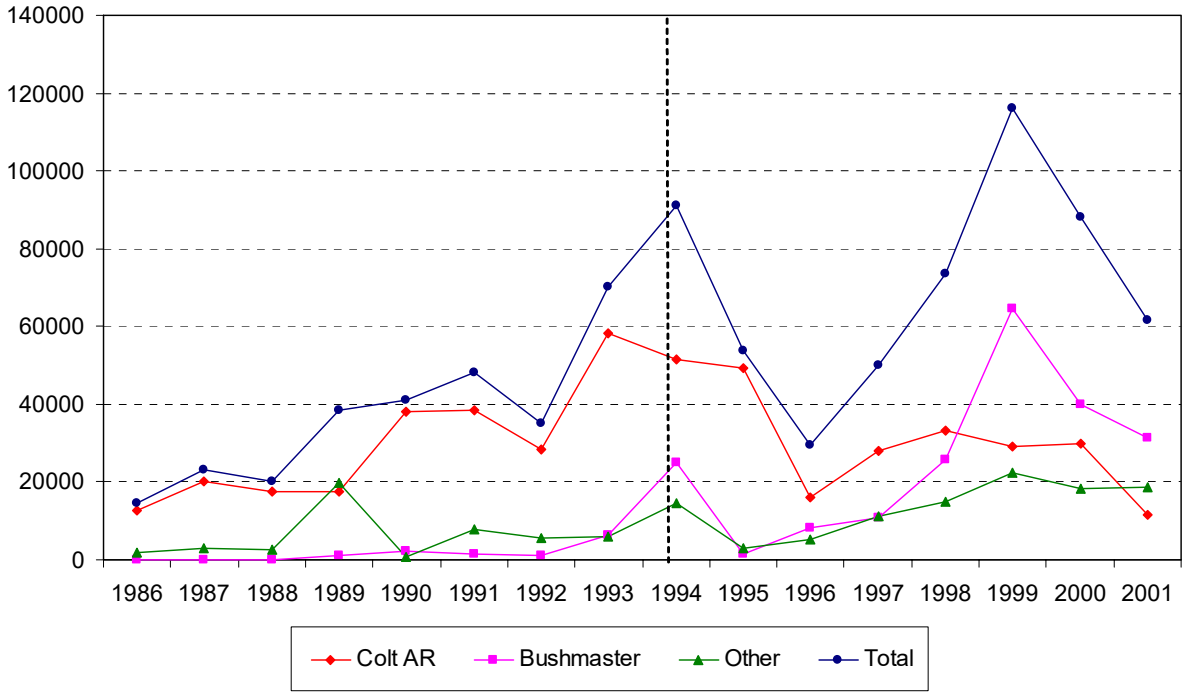
5.2.2. Production of Assault Rifles and Other Rifles

As shown in Figure 5-4, production of AR-15 type rifles surged during the early 1990s, reaching a peak in 1994.[36] AR production during the early 1990s rose almost 4 times faster than total rifle production and over 5 times faster than production of the comparison rifles examined in the price analysis (Table 5-3). Yet, by 1996 and 1997, production of legalized AR-type rifles had fallen by 51%, as production of other rifles continued increasing. AR production trends reversed again during the late 1990s, however, rising over 150%.[37] Total rifle production increased much more modestly during this time (18%), while production of the comparison rifles declined.

[36] Note again that the AR and legalized AR production figures are approximations based on all rifles produced by the companies in question (rifle production data are not available by type, caliber, or model), but it appears that most rifles made by these companies during the study period were AR-type rifles. Also, the figures for the comparison rifle companies (Ruger, Marlin, and Remington) are based on all rifles produced by these companies (the price analysis focused on selected semiautomatic models).

[37] There was also a notable shift in market shares among AR makers, as Bushmaster overtook Colt as the leading producer of AR-15 type rifles (Figure 5-4).

Figure 5-4. Assault Rifle Production, 1986-2001 (AR-15 Type)



Other: Olympic, Eagle/Armalite, DPMS, Essential Arms, Sendra.

5.3. Summary and Interpretations

Below, we offer some interpretations of the patterns found in the price and production analyses, keeping in mind that these analyses were largely descriptive, so causal inferences must be made cautiously. As documented in our earlier study, Congressional debate over the AW-LCM ban triggered speculative price increases for AWs in the months leading up to the ban's enactment. This study's examination of longer-term, annual price trends suggests that this speculative effect was very brief (and perhaps quite variable across jurisdictions) for APs but persisted through 1995 for ARs. This implies that speculators and sophisticated gun collectors (who we suspect played a large role in driving price trends) have more interest in ARs, which tend to be higher in quality and price than APs.

Responding to the speculative price growth, AW manufacturers boosted their production of AWs in 1994. Although total handgun and rifle production were increasing during the early 1990s, the rise in AW production was steeper, and there was a production peak unique to AWs in 1994 (production of other handguns peaked in 1993). It seems that this boost in the supply of grandfathered AWs was sufficient to satisfy speculative demand, thereby restoring national average AP prices to pre-ban levels within a year of the ban and doing the same for AR prices by 1996. AW prices remained stable through the late 1990s, and production of legalized AW-type weapons dropped off

substantially, at least through 1998. This suggests that the supply of grandfathered AWs was sufficient to meet demand through the late 1990s.

However, prices of APs rose relative to other handguns commonly used in crime during the 1990s. Handgun prices and production declined in general during the late 1990s, implying a decrease in demand for APs and other handguns that probably stemmed from the nation's declining crime rates.[38] But the AW ban's restriction of the AP supply, combined with the interest of speculators and collectors in these guns, may have prevented AP prices from falling as did prices for other handguns. The market patterns also suggest that consumers of APs are not as easily satisfied by legalized APs with fewer military-style features; despite the increasing value of APs (in relative terms), post-ban production of legalized APs declined faster than did production of other handguns, and some AP makers went out of business.

Prices of ARs, on the other hand, remained steady during the late 1990s (after the speculative price bubble of 1994-1995) both in absolute terms and relative to other rifles. The failure of AR prices to rise in at least relative terms, as occurred for APs, and the temporary drop in production of AR-type rifles after the ban may signify that the AR market was saturated relative to the AP market for a least a number of years following the ban. However, demand for AR-type rifles later rebounded, as evidenced by the resurgence in production of legalized, AR-type rifles in the late 1990s. In fact, more of these guns were produced in 1999 than in 1994. Unlike AP users, therefore, rifle users appear to be readily substituting the legalized AR-type rifles for the banned ARs, which may be another factor that has kept prices of the latter rifles from rising. All of this suggests that rifle owners, who have a lower prevalence of criminal users than do handgun owners, can more easily substitute rifles with fewer or no military features for the hunting and other sporting purposes that predominate among rifle consumers.

Another relevant factor may have been a surge in the supply of foreign semiautomatic rifles that can accept LCMs for military weapons (the LCMM rifles discussed in Chapter 2) during the early 1990s. Examples of LCMM rifles include legalized versions of banned AK-47, FN-FAL, and Uzi rifles. Importation of LCMM rifles rose from 19,147 in 1991 to 191, 341 in 1993, a nine-fold increase (Department of the Treasury, 1998, p. 34). Due to an embargo on the importation of firearms from China (where many legalized AK-type rifles are produced), imports of LCMM rifles dropped

[38] It seems likely that the rise and fall of handgun production was linked to the rising crime rates of the late 1980s and early 1990s and the falling crime rates of the mid and late 1990s. Self-defense and fear of crime are important motivations for handgun ownership among the general population (e.g., Cook and Ludwig, 1996; McDowall and Loftin, 1983), and the concealability and price of handguns make them the firearms of choice for criminal offenders. It is likely that the peak in 1993 was also linked to the Congressional debate and passage of the Brady Act, which established a background check system for gun purchases from retail dealers. It is widely recognized in the gun industry that the consideration of new gun control legislation tends to increase gun sales.

The decline in production was more pronounced for SNS handguns, whose sales are likely to be particularly sensitive to crime trends. Criminal offenders make disproportionate use of these guns. We can also speculate that they are prominent among guns purchased by low-income citizens desiring guns for protection. In contrast, the poor quality and reliability of these guns make them less popular among more knowledgeable and affluent gun buyers.

back down to 21,261 in 1994. Importation of all foreign LCMM rifles was ended by federal executive order in 1998.

ATF has reported that criminal use of LCMM rifles increased more quickly during the early 1990s than did that of other military-style rifles (U.S. Department of the Treasury, 1998, p. 33; also see Chapter 6). Accordingly, it is possible that the availability of LCMM rifles also helped to depress the prices of domestic ARs and discourage the production of legalized ARs during the 1990s, particularly if criminal users of rifles place a premium on the ability to accept LCMs. It is noteworthy, moreover, that the rebound in domestic production of legalized ARs came on the heels of the 1998 ban on LCMM rifles, perhaps suggesting the LCMM ban increased demand for domestic rifles accepting LCMs.

In sum, this examination of the AW ban's impact on gun prices and production suggests that there has likely been a sustained reduction in criminal use of APs since the ban but not necessarily ARs. Since most AWs used in crime are APs, this should result in an overall decline in AW use. In the following chapter, we examine the accuracy of this prediction.

6. CRIMINAL USE OF ASSAULT WEAPONS AFTER THE BAN

6.1. Measuring Criminal Use of Assault Weapons: A Methodological Note

In this chapter, we examine trends in the use of AWs using a number of national and local data sources on guns recovered by law enforcement agencies (we focus on the domestic AW models discussed at the beginning of the previous chapter). Such data provide the best available indicator of changes over time in the types (and especially the specific makes and models) of guns used in violent crime and possessed and/or carried by criminal and otherwise deviant or high-risk persons. The majority of firearms recovered by police are tied to weapon possession and carrying offenses, while the remainder are linked primarily to violent crimes and narcotics offenses (e.g., see ATF, 1976; 1977; 1997; Brill, 1977). In general, up to a quarter of guns confiscated by police are associated with violent offenses or shots fired incidents (calculated from ATF, 1977, pp. 96-98; 1997; Brill, 1977, pp. 24,71; Shaw, 1994, pp. 63, 65; also see data presented later in this chapter). Other confiscated guns may be found by officers, turned in voluntarily by citizens, or seized by officers for temporary safekeeping in situations that have the potential for violence (e.g., domestic disputes).

Because not all recovered guns are linked to violent crime investigations, we present analyses based on all gun recoveries and gun recoveries linked to violent crimes where appropriate (some of the data sources are based exclusively, or nearly so, on guns linked to violent crimes). However, the fact that a seized gun is not clearly linked to a violent crime does not rule out the possibility that it had been or would have been used in a violent crime. Many offenders carry firearms on a regular basis for protection and to be prepared for criminal opportunities (Sheley and Wright, 1993a; Wright and Rossi, 1986). In addition, many confiscated guns are taken from persons involved in drugs, a group involved disproportionately in violence and illegal gun trafficking (National Institute of Justice, 1995; Sheley and Wright, 1993a). In some instances, criminal users, including those fleeing crime scenes, may have even possessed discarded guns found by patrol officers. For all these reasons, guns recovered by police should serve as a good approximation of the types of guns used in violent crime, even though many are not clearly linked to such crimes.

Two additional caveats should be noted with respect to tracking the use of AWs. First, we can only identify AWs based on banned makes and models. The databases do not contain information about the specific features of firearms, thus precluding any assessment of non-banned gun models that were altered after purchase in ways making them illegal. In this respect, our numbers may understate the use of AWs, but we know of no data source with which to evaluate the commonality of such alterations. Second, one cannot always distinguish pre-ban versions of AWs from post-ban, legalized versions of the same weapons based on weapon make and model information (this occurs when the post-ban version of an AW has the same name as the pre-ban version), a factor which may have caused us to overstate the use of AWs after the ban. This was more of a problem for our assessment of ARs, as will be discussed below.

Finally, we generally emphasize trends in the percentage of crime guns that are AWs in order to control for overall trends in gun violence and gun recoveries. Because gun violence was declining throughout the 1990s, we expected the number of AW recoveries to drop independently of the ban's impact.

6.2. National Analysis of Guns Reported By Police to the Federal Bureau of Alcohol, Tobacco, and Firearms

6.2.1. An Introduction to Gun Tracing Data

In this section, we examine national trends in AW use based on firearm trace requests submitted to ATF by federal, state, and local law enforcement personnel throughout the nation. A gun trace is an investigation that typically tracks a gun from its manufacture to its first point of sale by a licensed dealer. Upon request, ATF traces guns seized by law enforcement as a service to federal, state, and local agencies. In order to initiate a trace on a firearm, the requesting law enforcement agency provides information about the firearm, such as make, model, and serial number.

Although ATF tracing data provide the only available national sample of the types of guns used in crime and otherwise possessed or carried by criminal and high-risk groups, they do have limitations for research purposes. Gun tracing is voluntary, and police in most jurisdictions do not submit trace requests for all, or in some cases any, guns they seize. Crime and tracing data for 1994, for example, suggest that law enforcement agencies requested traces for 27% of gun homicides but only 1% of gun robberies and gun assaults known to police during that year (calculated from ATF, 1995 and Federal Bureau of Investigation, 1995, pp. 13, 18, 26, 29, 31, 32).

The processes by which state and local law enforcement agencies decide to submit guns for tracing are largely unknown, and there are undoubtedly important sources of variation between agencies in different states and localities. For example, agencies may be less likely to submit trace requests in states that maintain their own registers of gun dealers' sales. Knowledge of ATF's tracing capabilities and procedures,[39] as well as participation in federal/state/local law enforcement task forces, are some of the other factors that may affect an agency's tracing practices. Further, these factors are likely to vary over time, a point that is reinforced below.

Therefore, firearms submitted to ATF for tracing may not be representative of the

[39] To illustrate, ATF cannot (or does not) trace military surplus weapons, imported guns without the importer name (generally, pre-1968 guns), stolen guns**,** or guns without a legible serial number (Zawitz 1995). Tracing guns manufactured before 1968 is also difficult because licensed dealers were not required to keep records of their transactions prior to that time. Throughout much of the 1990s, ATF did not generally trace guns older than 5-10 years without special investigative reasons (Kennedy et al., 1996, p. 171). Our data are based on trace requests rather than successful traces, but knowledge of the preceding operational guidelines might have influenced which guns law enforcement agencies chose to trace in some instances.

types of firearms typically seized by police. In general, not much is known about the nature of potential bias in tracing data. In prior studies, however, AWs tended to be more common in tracing data than in more representative samples of guns confiscated by police (Kleck, 1997, pp. 112, 141). This suggests that police have been more likely historically to initiate traces for seized AWs than for other seized guns. Although comparisons across studies are complicated by varying definitions of AWs used in different analyses, studies of guns confiscated by police or used in particular types of crimes generally suggest that AWs accounted for up to 6% of crime guns and about 2% on average prior to the federal AW ban (see Chapter 3 and Kleck, 1997, p. 141), whereas studies of pre-ban tracing data indicated that 8% of traced guns, and sometimes as many as 11%, were AWs (Cox Newspapers, 1989; Lenett, 1995; Zawitz, 1995).

Changes over time in the tracing practices of law enforcement agencies present additional complexities in analyzing tracing data. Due to improvements in the tracing process, ATF promotional efforts, and special initiatives like the Youth Crime Gun Interdiction Initiative (see ATF, 1997; 1999 and more recent reports available via the Internet at www.atf.treas.gov),[40] the utilization of tracing grew substantially throughout the 1990s in jurisdictions that chose to participate (also see ATF, 2000; Roth and Koper, 1997). To illustrate, trace requests to ATF rose from roughly 42,300 in 1991 to 229,500 in 2002 (see Table 6-1 in the next section), an increase of 443%. This growth reflects changes in tracing practices (i.e., changes in the number of agencies submitting trace requests and/or changes in the percentage of recovered guns for which participating agencies requested traces) rather than changes in gun crime; gun homicides, for example, were falling throughout the 1990s (see Table 6-1 in the next section) and were a third lower in 2002 than in 1991.

Therefore, an increase in trace requests for AWs does not necessarily signal a real increase in the use of AWs. Further, examining trends in the percentage of trace requests associated with AWs is also problematic. Because law enforcement agencies were more likely to request traces for AWs than for other guns in years past, we can expect the growth rate in tracing for non-AWs to exceed the growth rate in traces for AWs as gun tracing becomes more comprehensive. Consequently, AWs are likely to decline over time as a share of trace requests due simply to reporting effects, except perhaps during periods when AWs figure prominently in public discourse on crime.[41]

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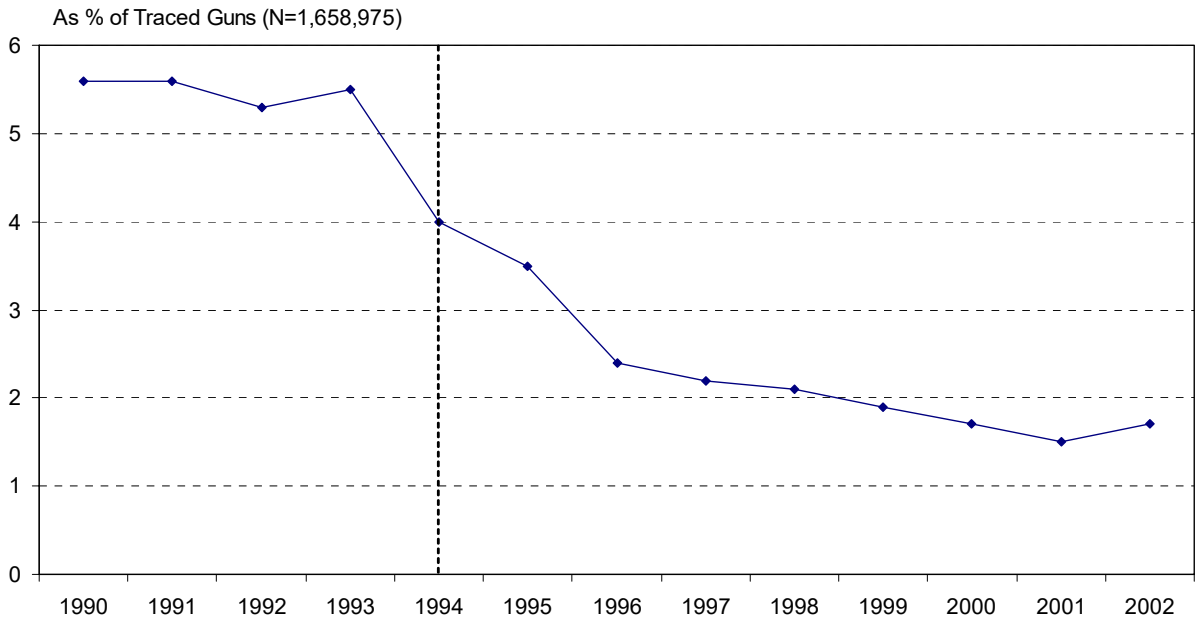
[40] As part of this initiative, police in a few dozen large cities are submitting trace requests to ATF for all guns that they confiscate. The initiative began with 17 cities in 1996 and has since spread to 55 major urban jurisdictions.

[41] To illustrate, assume that a hypothetical police agency recovers 100 guns a year, 2 of which are AWs, and that the agency has a selective tracing policy that results in the submission of trace requests for 20 of the guns, including 1 of the recovered AWs. Under this scenario, the department would be almost three times as likely to request traces for AWs as for other guns. If the department adopted a policy to request traces on all guns (and again recovered 2 AWs and 98 other guns), AW traces would double and traces of other guns would increase by more than 400%. Moreover, AWs would decline from 5% of traced guns to 2% of traced guns due simply to the change in tracing policy.

6.2.2. Traces of Assault Weapons, 1990-2002

Figure 6-1 illustrates the share of all traces that were for AWs from 1990 through 2002. A more detailed assessment of annual changes in traces for AWs and other guns is presented in Table 6-1. Changes in gun murders are also shown in Table 6-1 to emphasize the differences in trends for tracing and gun crime. Below, we summarize key points from the analysis. Due to the instrumentation problems inherent in tracing data, statistical tests are not presented.[42]

Figure 6-1. Police Recoveries of Assault Weapons Reported to ATF (National), 1990-2002



Includes Intratec group, SWD group, AR-15 group, and selected Calico and Feather models.

[42] Nearly 30% of the tracing records lack specific gun model designations (the crucial elements for conducting a trace are the gun make and serial number). For the makes and types of guns likely to be AWs, however, the missing model rate was slightly under 10%. Further, we were able to identity some of the latter weapons as AWs with reasonable confidence based on the makes, types, and calibers alone. Nevertheless, we conducted a supplemental analysis using only those records for which the gun model was identified. The results of that analysis were substantively very similar to those presented below.

Table 6-1. Annual Percentage Changes in Gun Murders and Police Requests to ATF for Traces of Assault Weapons and Other Firearms, 1991-2002 (Number of Traces in Parentheses)

Year	Gun Murders (1)	All Traces (2)	AW Traces* (3)	AP Traces (4)	AR Traces (5)	AW and AW Substitute Traces (6)	Violent Crime Traces (7)	AW Violent Crime Traces (8)	LCMM Rifle Traces** (9)
1991	9%	14% (42281)	14% (2378)	24% (1775)	-6% (603)	14% (2378)	19% (6394)	20% (344)	--
1992	-1%	6% (44992)	1% (2398)	4% (1838)	-7% (560)	1% (2398)	3% (6558)	7% (367)	--
1993	5%	20% (54189)	25% (2994)	20% (2199)	42% (795)	25% (2994)	26% (8248)	41% (516)	252% (183)
1994	-4%	53% (82791)	11% (3337)	23% (2706)	-21% (631)	11% (3337)	22% (10083)	-18% (424)	223% (592)
1995	-10%	-6% (77503)	-19% (2730)	-24% (2051)	8% (679)	-18% (2747)	23% (12439)	-15% (362)	-10% (530)
1996	-9%	66% (128653)	12% (3059)	13% (2309)	10% (750)	17% (3214)	67% (20816)	27% (459)	40% (743)
1997	-7%	42% (183225)	31% (4019)	31% (3017)	34% (1002)	36% (4362)	11% (23147)	13% (519)	24% (925)
1998	-11%	5% (192115)	0% (4014)	-9% (2751)	26% (1263)	7% (4681)	3% (23844)	-22% (404)	33% (1227)
1999	-8%	-2% (188296)	-11% (3581)	-12% (2414)	-8% (1167)	-6% (4406)	3% (24663)	0% (404)	-18% (1003)
2000	1%	-3% (182961)	-11% (3196)	-16% (2027)	0% (1169)	-6% (4143)	-13% (21465)	-25% (305)	-14% (859)
2001	-1%	18% (215282)	1% (3238)	5% (2138)	-6% (1100)	3% (4273)	20% (25822)	6% (322)	-3% (833)
2002	6%	7% (229525)	19% (3839)	4% (2214)	48% (1625)	12% (4765)	20% (30985)	65% (531)	4% (865)

* Based on Intratec group, SWD group, AR-15 group, and Calico and Feather models.

** Foreign semiautomatic rifles accepting large capacity military magazines (banned by executive order in 1998). (Data are not shown for 1991 and 1992 because very few of these guns were traced in those years.)

6.2.2.1. Assault Weapons as a Percentage of Crime Gun Traces

As shown in Figure 6-1, AWs declined from 5.4% of crime gun traces in 1992-1993 to 1.6% in 2001-2002, a decline of 70%. Although this downward trend could be attributable in large part to changes in tracing practices, it is noteworthy that it did not begin until 1994 (the year of the ban); during the pre-ban years, 1990 to 1993, AWs accounted for a steady share of traces despite a 46% increase in total tracing volume. It is also remarkable that about 3,200 AWs were traced in both 2000 and 2001, which is virtually identical to the average number traced during 1993 and 1994 (3,166) even though total traces increased more than 190% during the same period (Table 6-1, columns 2 and 3).[43]

6.2.2.2. Annual Changes in Traces for Assault Weapons and Other Guns

Throughout most of the post-ban period (particularly 1995 to 2001), AW traces either increased less or declined more than total traces (Table 6-1, columns 2 and 3), a pattern that is also consistent with a decline in the use of AWs relative to other guns, though it too may be distorted by changes in tracing practices. This pattern was largely consistent whether analyzing all traces or only traces associated with violent crimes (columns 7 and 8).[44]

The years when total traces declined or were relatively flat are arguably the most informative in the series because they appear to have been less affected by changes in tracing practices. For example, there was a 6% decline in total trace requests from 1994 to 1995 (the years featured in our earlier study) that coincided with a 10% drop in gun murders (Table 6-1, column 1). Therefore, it seems tracing practices were relatively stable (or, conversely, reporting effects were relatively small) from 1994 to 1995. The 19% reduction in AW traces during this same period implies that AW use was declining faster than that of other guns. Furthermore, there were fewer AW traces in 1995 than in 1993, the year prior to the ban. The fact that this occurred during a period when the AW issue was very prominent (and hence police might have been expected to trace more of the AWs they recovered) arguably strengthens the causal inference of a ban effect.[45]

Total traces also declined slightly (2%-3%) in 1999 and 2000. In each of those years, the decline was greater for AWs (11%). Thus, in years when tracing declined overall, AW traces fell 3 to 6 times faster than did total traces. Put another way, AWs fell between 9% and 13% as a percentage of all traces in each of these years.

The general pattern of AW traces increasing less or declining more than those of

[43] These general findings are consistent with those of other tracing analyses conducted by ATF (2003 Congressional Q&A memo provided to the author) and the Brady Center to Prevent Gun Violence (2004).
[44] A caveat is that requests without specific crime type information are often grouped with weapons offenses (ATF, 1999). Therefore, traces associated with violent crimes are likely understated to some degree.
[45] This inference is also supported by our earlier finding that trace requests for AWs declined by only 8% in states that had their own AW bans prior to the federal ban (Roth and Koper, 1997, Chapter 5).

other crime guns was clearly apparent for APs but less consistent for ARs (Table 6-1, columns 4 and 5). For example, AR traces went up 26% in 1998 while total traces went up only 5% and AP traces declined 9%. In 2000, total and AP traces fell 3% and 16%, respectively, but AR traces remained flat. This is consistent with predictions derived from the price and production analyses described above. But note that the post-ban AR counts could be overstated because the data do not distinguish pre-ban from post-ban versions of some popular AR-15 type rifles like the Colt Sporter and Bushmaster XM-15. (Also note that the percentage of traces for ARs did fall from 1.4% in 1992-1993 to 0.6% in 2001-2002.)

More generally, the use of post-ban AW-type weapons (including both legalized APs and ARs) has not been widespread enough to completely offset the apparent decline in the use of banned AWs. Combined traces for banned AWs and AW substitutes (Table 6-1, column 6) also followed the pattern of increasing less or declining more than did total traces throughout most of the period, though the differences were not as pronounced as those between AWs and total traces. In 1999 and 2000, for example, AWs traces dropped 11%, while combined traces for AWs and legal substitutes declined only 6%. Still, the latter figure was greater than the 2%-3% drop for total traces.

Finally, traces of the LCMM rifles banned by executive order in 1998 were generally rising to that point, reaching levels as high as those for AR-15 type rifles (Table 6-1, column 9). Since 1998, however, the number of traces for LCMM rifles has fallen substantially. Despite a 4% increase from 2001 to 2002, the number of LCMM traces in 2002 (865) was 30% lower than the peak number traced in 1998 (1,227). Tentatively, this suggests that the 1998 extension of the ban has been effective in curtailing weapons that offenders may have been substituting for the ARs banned in 1994.

6.2.2.3. Did Use of Assault Weapons Rebound in 2002?

In 2002, tracing volume increased 7%, which closely matched the 6% increase in gun murders for that year. In contrast to the general pattern, AW traces increased by 19%, suggesting a possible rebound in AW use independent of changes in tracing practices, a development that we have predicted elsewhere (Roth and Koper, 1997) based on the boom in AW production leading up to the ban. The disproportionate growth in AW traces was due to ARs, however, so it could partially reflect increasing use of post-ban AR-type rifles (see the discussion above).

Moreover, this pattern could be illusory. With data from the most recent years, it was possible to run a supplementary analysis screening out traces of older weapons (not shown). Focusing on just those guns recovered and traced in the same year for 2000 through 2002 revealed that recoveries of AWs declined in 2001, more so for ARs (16%) than for APs (9%), while total traces increased 1%.[46] Traces for APs and ARs then

[46] The tracing database indicates when guns were recovered and when they were traced. However, the recovery dates were missing for 30% of the records overall and were particularly problematic for years prior to 1998. For this reason, the main analysis is based on request dates. The auxiliary analysis for 2000-

increased in 2002 (1% and 6%, respectively) but by less than total traces (8%). Therefore, the disproportionate growth in AR traces in 2002 shown in Table 6-1 may have been due to tracing of older AWs by newly participating police agencies.

6.2.2.4. Summary of the ATF Gun Tracing Analysis

Complexities arising from recent changes in the use of gun tracing by law enforcement warrant caution in the interpretation of ATF gun tracing data. Notwithstanding, the data suggest that use of AWs in crime, though relatively rare from the start, has been declining. The percentage of gun traces that were for AWs plummeted 70% between 1992-1993 and 2001-2002 (from 5.4% to 1.6%), and this trend did not begin until the year of the AW ban. On a year-to-year basis, AW traces generally increased less or declined by more than other gun traces. Moreover, in years when tracing volume declined – that is, years when changes in reporting practices were least likely to distort the data – traces of AWs fell 3 to 6 times faster than gun traces in general. The drop in AW use seemed most apparent for APs and LCMM rifles (banned in 1998). Inferences were less clear for domestic ARs, but assessment of those guns is complicated by the possible substitution of post-ban legal variations.

6.3. Local Analyses of Guns Recovered By Police

Due to concerns over the validity of national ATF tracing data for investigating the types of guns used in crime, we sought to confirm the preceding findings using local data on guns recovered by police. To this end, we examined data from half a dozen localities and time periods.

- All guns recovered by the Baltimore Police Department from 1992 to 2000 (N=33,933)
- All guns recovered by the Metro-Dade Police Department (Miami and Dade County, Florida) from 1990 to 2000 (N=39,456)
- All guns recovered by the St. Louis Police Department from 1992 to 2003 (N=34,143)
- All guns recovered by the Boston Police Department (as approximated by trace requests submitted by the Department to ATF) from 1991 to 1993 and 2000 to 2002 (N=4,617)[47]

2002 focuses on guns both recovered and traced in the same year because it is likely that some guns recovered in 2002 had not yet been traced by the spring of 2003 when this database was created. Using only guns recovered and traced in the same year should mitigate this bias.

[47] The Boston Police Department has been tracing guns comprehensively since 1991 (Kennedy et al., 1996). However, we encountered difficulties in identifying Boston Police Department traces for several years in the mid-1990s. For this reason, we chose to contrast the 1991 to 1993 period with the 2000 to 2002 period.

- Guns recovered during murder investigations in Milwaukee County from 1991 to 1998 (N=592)[48]
- Guns linked to serious crimes in Anchorage and other parts of Alaska and submitted to state firearm examiners for evidentiary testing from 1987 to 2000 (N=900)[49]

The selection of these particular locations and samples reflects data availability.[50] The locations were not selected randomly, and some of the samples are small for conducting trend analysis of relatively rare events (i.e., AW recoveries). Accordingly, we must use caution in generalizing the results to other places. However, the data sources reflect a wide geographic range and cover post-ban periods extending through at least the latter 1990s (and typically through the year 2000 or beyond). To the extent that the results are similar across these jurisdictions, therefore, we can have more confidence that they reflect national patterns.

In each jurisdiction, we examined pre-post changes in recoveries of AWs (focusing on the domestic AW group defined earlier) and substitution of post-ban AW models for the banned models. Where possible, we conducted separate analyses of all AW recoveries and those linked specifically to violent crimes.[51] We also differentiated between AP and AR trends using the larger databases from Baltimore, Miami, and St. Louis. But since most of these databases do not extend more than two years beyond 1998, we do not present analyses specifically for LCMM rifles.

Key summary results are summarized in Table 6-2, while more detailed results from each site appear at the end of the chapter in Tables 6-3 through 6-6 and Figures 6-2 through 6-6.[52] The number of AW recoveries declined by 28% to 82% across these

[48] The data are described in reports from the Medical College of Wisconsin (Hargarten et al., 1996; 2000) and include guns used in the murders and other guns recovered at the crime scenes. Guns are recovered in approximately one-third of Milwaukee homicide cases.

[49] The data include guns submitted by federal, state, and local agencies throughout the state. Roughly half come from the Anchorage area. Guns submitted by police to the state lab are most typically guns that were used in major crimes against persons (e.g. murder, attempted murder, assault, robbery).

[50] We contacted at least 20 police departments and crime labs in the course of our data search, focusing much of our attention on police departments participating in ATF's Youth Crime Gun Interdiction Initiative (YCGII) (ATF, 1997; 1999). Departments participating in the YCGII submit data to ATF on all guns that they recover. Though the YCGII did not begin until 1996 (well after the implementation of the AW ban), we suspected that these departments would be among those most likely to have electronically-stored gun data potentially extending back in time to before the ban. Unfortunately, most of these departments either did not have their gun data in electronic format or could not provide data for other reasons (e.g., resource constraints). In the course of our first AW study (Roth and Koper, 1997), we contacted many other police departments that also did not have adequate data for the study.

[51] All of the Milwaukee and Anchorage analyses were limited to guns involved in murders or other serious crimes. Despite evidence of a decline, AW recoveries linked to violence were too rare in Boston to conduct valid test statistics.

[52] We omitted guns recovered in 1994 from both the pre and post-ban counts because the speculative price increases for AWs that occurred in 1994 (see previous section and Roth and Koper, 1997, Chapter 4) raise questions about the precise timing of the ban's impact on AW use during that year, thereby clouding the designation of the intervention point. This is particularly a concern for the Baltimore analysis due to a

locations and time periods, but the discussion below focuses on changes in AWs as a share of crime guns in order to control for general trends in gun crime and gun seizures. Prior to the ban, AWs ranged from about 1% of guns linked to violent crimes in St. Louis to nearly 6% of guns recovered in Milwaukee murder cases.[53]

AWs dropped as share of crime guns in all jurisdictions after the ban. Reductions ranged from a low of 17% in Milwaukee (based on guns linked to homicides) to a high of 72% in Boston (based on all crime guns) but were generally between 32% and 40%.[54, 55] A decline in the use of AWs relative to other guns was generally apparent whether examining all AW recoveries or just those linked to violent crimes.[56] An exception was in St. Louis, where

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state AP ban that took effect a few months prior to the federal AW ban.

[53] These figures should be treated as approximations of the prevalence of AWs. On the one hand, the numbers may understate the prevalence of AWs to a small degree because they are based on only the domestic AW group defined earlier. Based on analysis of national ATF gun tracing data, we estimated previously that the domestic AW group accounts for 82% of AWs used in crime (Roth and Koper, 1997, Chapter 5). To further test the reliability of this assessment, we investigated the prevalence of all banned AW models among guns recovered in Baltimore using an ATF list of all guns defined as AWs under the 1994 Crime Act criteria (118 model and caliber combinations). We chose the Baltimore database because it provides a complete inventory of guns recovered by police in that city during the study period and, having been maintained by crime lab personnel, is particularly thorough with regard to make and model identifications. Though there was some ambiguity in classifying a small number of AK-type semiautomatic rifles (there are many civilian variations of the AK-47 rifle, some of which were legal under the 1994 legislation), our examination suggested that the domestic AW group accounted for approximately 90% of the AWs recovered in Baltimore. (In addition, including all AWs had virtually no effect on the pre-post changes in AW use in Baltimore.) But as discussed previously, the counts could also overstate AW use to some degree because imprecision in the identification of gun models in some data sources may have resulted in some legalized firearms being counted as banned AWs.

[54] The AW counts for Miami also include Interdynamics KG9 and KG99 models. These models were produced during the early 1980s and were forerunners to the Intratec models (ATF restricted the KG9 during the early 1980s because it could be converted too easily to fully automatic fire). These weapons were very rare or non-existent in most of the local data sources, but they were more common in Miami, where Interdynamics was formerly based. Including these guns increased the AW count in Miami by about 9% but did not affect pre-post changes in AW recoveries.

[55] State AW legislation passed in Maryland and Massachusetts could have had some impact on AW trends in Baltimore and Boston, respectively. Maryland implemented an AP ban, similar in coverage to the federal AW ban, in June 1994 (Maryland has also required background checks for retail sales of a broader list of state-defined AWs since 1989), and Massachusetts implemented additional legislation on federally-defined AWs in late 1998. The timing and scope of these laws make them largely redundant with the federal ban, so they should not unduly complicate inferences from the analysis. However, Maryland forbids additional transfers of grandfathered APs, and Massachusetts has imposed additional requirements for possession and transfer of LCMs and guns accepting LCMs. Both states also have enhanced penalties for certain crimes involving APs, LCMs, and/or guns accepting LCMs. Hence, the ban on AWs was arguably strengthened in Baltimore and Boston, relative to the other jurisdictions under study. This does not appear to have affected trends in AW use in Baltimore, which were very similar to those found in the other study sites. However, use of AWs and combined use of AWs and post-ban AW substitutes declined more in Boston than in any other study site. Although the trends in Boston could reflect ongoing, post-2000 reductions in use of AWs and similar weapons (Boston was one of the only study sites from which we obtained post-2000 data), it is possible that the Massachusetts legislation was also a contributing factor.

[56] There may be some inconsistency across jurisdictions in the identification of guns associated with violent crimes. In Miami, for example, 28% of the guns had an offense code equal to "other/not listed," and this percentage was notably higher for the later years of the data series.

Table 6-2. Pre-Post Changes in Assault Weapons As a Share of Recovered Crime Guns For Selected Localities and Time Periods: Summary Results (Total Number of Assault Weapons for Pre and Post Periods in Parentheses) [a]

Locality and Time Period	AWs	AWs (Linked to Violence)	APs	ARs	AWs and Post-Ban Substitutes
Baltimore (all recoveries) pre=1992-1993, post=1995-2000	-34%*** (425)	-41%** (75)	-35%*** (383)	-24% (42)	-29%*** (444)
Miami-Dade (all recoveries) pre=1990-1993, post=1995-2000	-32%*** (733)	-39%*** (101)	-40%*** (611)	37%* (115)	-30%*** (746)
St. Louis (all recoveries) pre=1992-1993, post=1995-2003	-32%*** (306)	1% (28)	-34%*** (274)	10% (32)	-24%** (328)
Boston (all recoveries) pre=1991-1993, post=2000-2002	-72%*** (71)	N/A	N/A	N/A	-60%*** (76)
Milwaukee (recoveries in murder cases) pre=1991-1993, post=1995-1998	N/A	-17% (28)	N/A	N/A	2% (31)
Anchorage, AK (recoveries in serious crimes) pre=1987-1993, post=1995-2000	N/A	-40% (24)	N/A	N/A	-40% (24)

a. Based on Intratec group, SWD group, AR-15 group, and Calico and Feather models. See the text for additional details about each sample and Tables 6-3 through 6-6 for more detailed results from each locality.

* Statistically significant change at chi-square p level < .1

** Statistically significant change at chi-square p level < .05

*** Statistically significant change at chi-square p level < .01

AWs declined as share of all guns but not of guns linked to violent crimes, though the latter test was based on rather small samples.

These reductions were not due to any obvious pre-ban trends (see Figures 6-2 through 6-6 at the end of the chapter). On the contrary, AW recoveries reached a peak in most of these jurisdictions during 1993 or 1994 (Boston, which is not shown in the graphs due to missing years, was an exception). We tested changes in AW prevalence using simple chi-square tests since there were no observable pre-existing time trends in the data. Due to the small number of AWs in some of these samples, these changes were not all statistically significant. Nonetheless, the uniformity of the results is highly suggestive, especially when one considers the consistency of these results with those found in the national ATF tracing analysis.

The changes in Tables 6-2 through 6-6 reflect the average decline in recoveries of AWs during the post-ban period in each locality. However, some of these figures may understate reductions to date. In several of the localities, the prevalence of AWs among crime guns was at, or close to, its lowest mark during the most recent year analyzed (see Figures 6-2 through 6-6 at the end of the chapter), suggesting that AW use continues to decline. In Miami, for example, AWs accounted for 1.7% of crime guns for the whole 1995 to 2000 period but had fallen to 1% by 2000. Further, the largest AW decline was recorded in Boston, one of two cities for which data extended beyond the year 2000 (however, this was not the case in St. Louis, the other locality with post-2000 data).

Breakouts of APs and ARs in Baltimore, Miami, and St. Louis show that the decline in AW recoveries was due largely to APs, which accounted for the majority of AWs in these and almost all of the other localities (the exception was Anchorage, where crimes with rifles were more common, as a share of gun crimes, than in the other sites). Pre-post changes in recoveries of the domestic AR group weapons, which accounted for less than 1% of crime guns in Baltimore, Miami, and St. Louis, were inconsistent. AR recoveries declined after the ban in Baltimore but increased in St. Louis and Miami. As discussed previously, however, the AR figures may partly reflect the substitution of post-ban, legalized versions of these rifles, thus overstating post-ban use of the banned configurations. Further, trends for these particular rifles may not be indicative of those for the full range of banned rifles, including the various foreign rifles banned by the 1994 law and the import restrictions of 1989 and 1998 (e.g., see the ATF gun tracing analysis of LCMM rifles).[57]

[57] As discussed in the last chapter, our research design focused on common AWs that were likely to be most affected by the 1994 ban as opposed to earlier regulations (namely, the 1989 import ban) or other events (e.g., company closings or model discontinuations prior to 1994). However, an auxiliary analysis with the Baltimore data revealed a statistically meaningful drop in recoveries of all ARs covered by the 1994 legislation (not including the LCMM rifles) that was larger than that found for just the domestic group ARs discussed in the text. Similarly, an expanded AR analysis in Miami showed that total AR recoveries declined after the ban, in contrast to the increase found for the domestic group ARs. (Even after expanding the analysis, ARs still accounted for no more than 0.64% of crime guns before the ban in both locations. As with the domestic AR group, there are complexities in identifying banned versus non-banned versions of some of the other ARs, so these numbers are approximations.) Consequently, a more nuanced view of AR trends may be that AR use is declining overall, but this decline may be due largely to the 1989 import

Finally, the overall decline in AW use was only partially offset by substitution of the post-ban legalized models. Even if the post-ban models are counted as AWs, the share of crime guns that were AWs still fell 24% to 60% across most jurisdictions. The exception was Milwaukee where recoveries of a few post-ban models negated the drop in banned models in a small sample of guns recovered during murder investigations.[58]

6.4. Summary

Consistent with predictions derived from the analysis of market indicators in Chapter 5, analyses of national ATF gun tracing data and local databases on guns recovered by police in several localities have been largely consistent in showing that criminal use of AWs, while accounting for no more than 6% of gun crimes even before the ban, declined after 1994, independently of trends in gun crime. In various places and times from the late 1990s through 2003, AWs typically fell by one-third or more as a share of guns used in crime.[59, 60] Some of the most recent, post-2000 data suggest

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restrictions that predated the AW ban. It is not yet clear that there has been a decline in the most common ARs prohibited exclusively by the 1994 ban.

[58] This was not true when focusing on just those guns that were used in the incident as opposed to all guns recovered during the investigations. However, the samples of AWs identified as murder weapons were too small for valid statistical tests of pre-post changes.

[59] These findings are also supported by prior research in which we found that reported thefts of AWs declined 7% in absolute terms and 14% as a fraction of stolen guns in the early period following the ban (i.e., late 1994 through early 1996) (Koper and Roth, 2002a, p. 21). We conducted that analysis to account for the possibility that an increase in thefts of AWs might have offset the effect of rising AW prices on the availability of AWs to criminals. Because crimes with AWs appear to have declined after the ban, the theft analysis is not as central to the arguments in this paper.

[60] National surveys of state prisoners conducted by the federal Bureau of Justice Statistics show an increase from 1991 to 1997 in the percentage of prisoners who reported having used an AW (Beck et al., 1993; Harlow, 2001). The 1991 survey (discussed in Chapter 3) found that 2% of violent gun offenders had carried or used an AW in the offense for which they were sentenced (calculated from Beck et al. 1993, pp. 18,33). The comparable figure from the 1997 survey was nearly 7% (Harlow, 2001, pp.3, 7).

Although these figures appear contrary to the patterns shown by gun recovery data, there are ambiguities in the survey findings that warrant caution in such an interpretation. First, the definition of an AW (and most likely the respondents' interpretation of this term) was broader in the 1997 survey. For the 1991 survey, respondents were asked about prior ownership and use of a "…military-type weapon, such as an Uzi, AK-47, AR-15, or M-16" (Beck et al., 1993, p. 18), all of which are ARs or have AR variations. The 1997 survey project defined AWs to "…include the Uzi, TEC-9, and the MAC-10 for handguns, the AR-15 and AK-47 for rifles, and the 'Street Sweeper' for shotguns" (Harlow, 2001, p. 2). (Survey codebooks available from the Inter-University Consortium for Political and Social Research also show that the 1997 survey provided more detail and elaboration about AWs and their features than did the 1991 survey, including separate definitions of APs, ARs, and assault shotguns.)

A second consideration is that many of the respondents in the 1997 survey were probably reporting criminal activity prior to or just around the time of the ban. Violent offenders participating in the survey, for example, had been incarcerated nearly six years on average at the time they were interviewed (Bureau of Justice Statistics, 2000, p. 55). Consequently, the increase in reported AW use may reflect an upward trend in the use of AWs from the 1980s through the early to mid 1990s, as well as a growing recognition of these weapons (and a greater tendency to report owning or using them) stemming from publicity about the AW issue during the early 1990s.

Finally, we might view the 1997 estimate skeptically because it is somewhat higher than that from most other sources. Nevertheless, it is within the range of estimates discussed earlier and could reflect a

reductions as high as 70%.[61] This trend has been driven primarily by a decline in the use of APs, which account for a majority of AWs used in crime. AR trends have been more varied and complicated by the substitution of post-ban guns that are very similar to some banned ARs. More generally, however, the substitution of post-ban AW-type models with fewer military features has only partially offset the decline in banned AWs.

These findings raise questions as to the whereabouts of surplus AWs, particularly APs, produced just prior to the ban. Presumably, many are in the hands of collectors and speculators holding them for their novelty and value.[62] Even criminal possessors may be more sensitive to the value of their AWs and less likely to use them for risk of losing them to police.

Finally, it is worth noting the ban has not completely eliminated the use of AWs, and, despite large relative reductions, the share of gun crimes involving AWs is similar to that before the ban. Based on year 2000 or more recent data, the most common AWs continue to be used in up to 1.7% of gun crimes.

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somewhat higher use of AWs among the subset of offenders who are most active and/or dangerous; recall that the highest estimate of AW use among the sources examined in this chapter came from a sample of guns recovered during murder investigations in Milwaukee (also see the discussion of offender surveys and AWs in Chapter 3).

[61] Developing a national estimate of the number of AW crimes prevented by the ban is complicated by the range of estimates of AW use and changes therein derived from different data sources. Tentatively, nonetheless, it appears the ban prevents a few thousand crimes with AWs annually. For example, using 2% as the best estimate of the share of gun crimes involving AWs prior to the ban (see Chapter 3) and 40% as a reasonable estimate of the post-ban drop in this figure implies that almost 2,900 murders, robberies, and assaults with AWs were prevented in 2002 (this assumes that 1.2% of the roughly 358,000 gun murders, gun robberies, and gun assaults reported to police in 2002 [see the *Uniform Crime Reports*] involved AWs but that 2% would have involved AWs had the ban not been in effect). Even if this estimate is accurate, however, it does not mean the ban prevented 2,900 gun crimes in 2002; indeed, the preceding calculation assumes that offenders prevented from using AWs committed their crimes using other guns. Whether forcing such weapon substitution can reduce the number of persons wounded or killed in gun crimes is considered in more detail in Chapter 9.

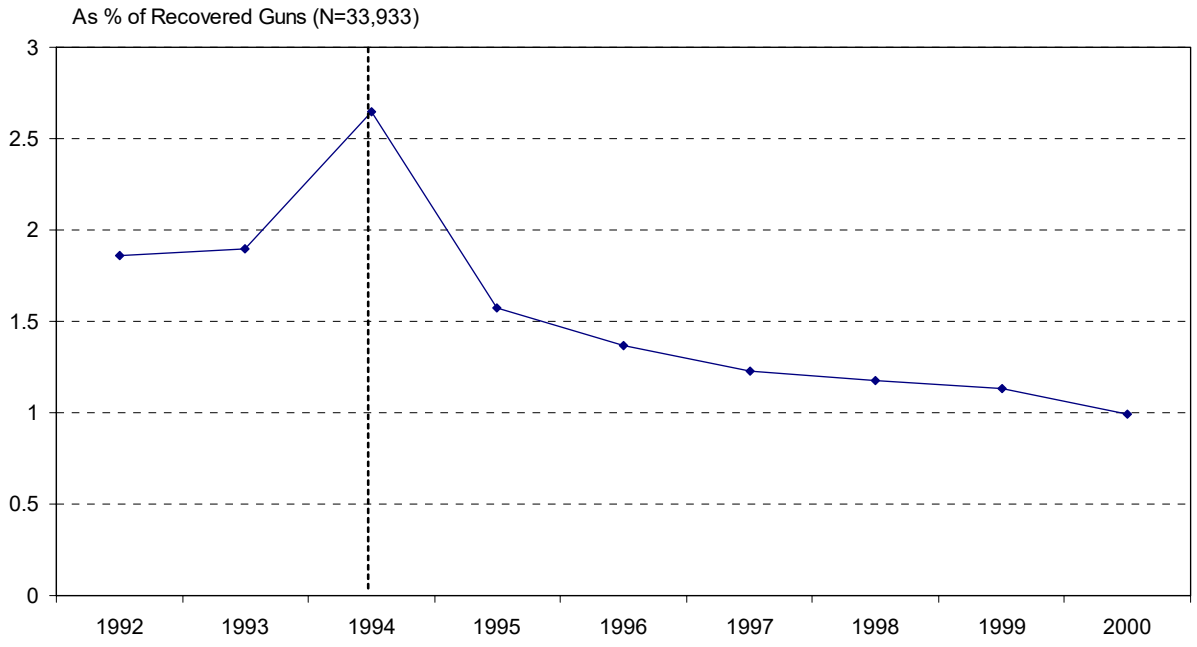
[62] The 1997 national survey of state prisoners discussed in footnote 60 found that nearly 49% of AW offenders obtained their gun from a “street” or illegal source, in contrast to 36% to 42% for other gun users (Harlow, 2001, p. 9). This could be another sign that AWs have become harder to acquire since the ban, but the data cannot be used to make an assessment over time.

Table 6-3. Trends in Police Recoveries of Domestic Assault Weapons in Baltimore, 1992-2000 [a]

	Pre-Ban Period	Post-Ban Period	Change
A. All Recoveries	Jan. 1992-Dec. 1993	Jan. 1995-Dec. 2000	
Total AWs	135	290	
Annual Mean	67.5	48.33	-28%
AW's as % of Guns	1.88%	1.25%	-34%**
APs	123	260	
Annual Mean	61.5	43.33	-30%
APs as % of Guns	1.71%	1.12%	-35%**
ARs	12	30	
Annual Mean	6	5	-17%
ARs as % of Guns	0.17%	0.13%	-24%
Total AWs and Substitutes	135	309	
Annual Mean	67.5	51.5	-24%
AWs/Subs as % of Guns	1.88%	1.33%	-29%**
B. Recoveries Linked to Violent Crimes [b]			
Total AWs	28	47	
Annual Mean	14	7.83	-44%
AWs as % of Violent Crime Guns	2.1%	1.24%	-41%*

a. Domestic assault weapons include Intratec group, SWD group, AR-15 group, and Calico and Feather models.
b. Murders, assaults, and robberies
* Chi-square p level < .05 (changes in percentages of guns that were AWs/APs/ARs/AW-subs were tested for statistical significance).
** Chi-square p level < .01 (changes in percentages of guns that were AWs/APs/ARs/AW-subs were tested for statistical significance).

Figure 6-2. Police Recoveries of Assault Weapons in Baltimore, 1992-2000



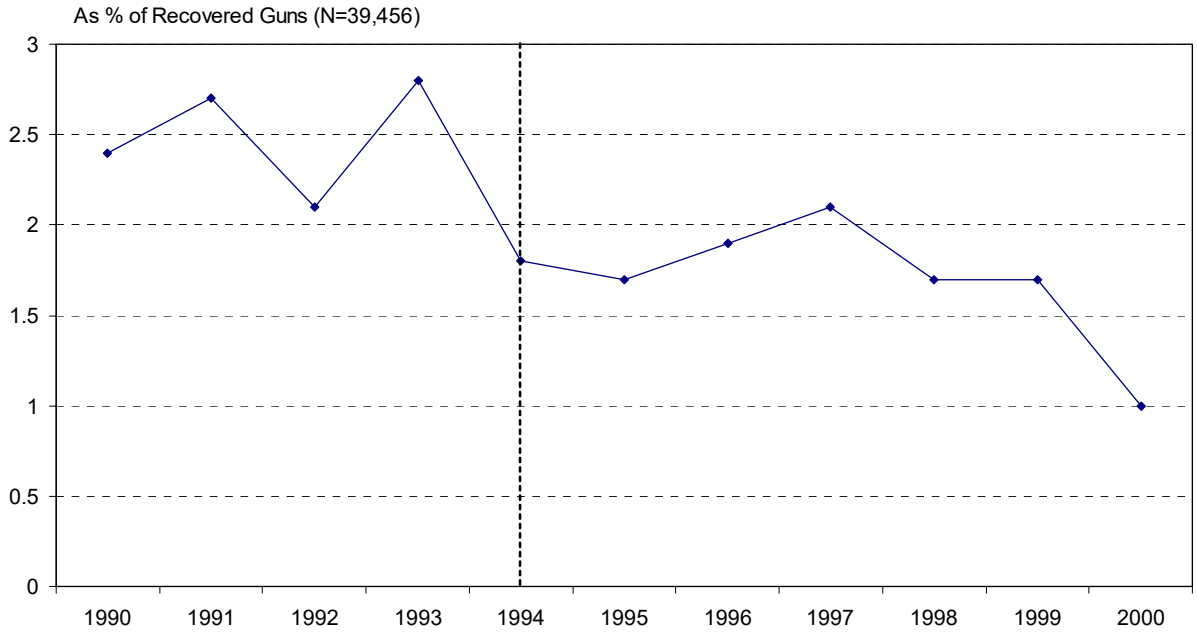
Includes Intratec group, SWD group, AR-15 group, and selected Calico and Feather models.

Table 6-4. Trends in Police Recoveries of Domestic Assault Weapons in Miami (Metro-Dade), 1990-2000 [a]

	Pre-Ban Period	Post-Ban Period	Change
A. All Recoveries	Jan. 1990-Dec. 1993	Jan. 1995-Dec. 2000	
Total AWs	403	330	
Annual Mean	100.75	55	-45%
AW's as % of Guns	2.53%	1.71%	-32%***
APs	355	256	
Annual Mean	88.75	42.67	-52%
APs as % of Guns	2.23%	1.33%	-40%***
ARs	43	72	
Annual Mean	10.75	12	12%
ARs as % of Guns	0.27%	0.37%	37%*
Total AWs and Substitutes	403	343	
Annual Mean	100.75	57.17	-43%
AWs/Subs as % of Guns	2.53%	1.78%	-30%***
B. Recoveries Linked to Violent Crimes [b]			
Total AWs	69	32	
Annual Mean	17.25	5.33	-69%
AWs as % of Violent Crime Guns	2.28%	1.39%	-39%**

a. Domestic assault weapons include Intratec group, SWD group, AR-15 group, and Calico and Feather models.
b. Murders, assaults, and robberies
* Chi-square p level < .1 (changes in percentages of guns that were AWs/APs/ARs/AW-subs were tested for statistical significance)
** Chi-square p level < .05 (changes in percentages of guns that were AWs/APs/ARs/AW-subs were tested for statistical significance)
*** Chi-square p level <.01 (changes in percentages of guns that were AWs/APs/ARs/AW-subs were tested for statistical significance)

Figure 6-3. Police Recoveries of Assault Weapons in Miami (Metro-Dade), 1990-2000



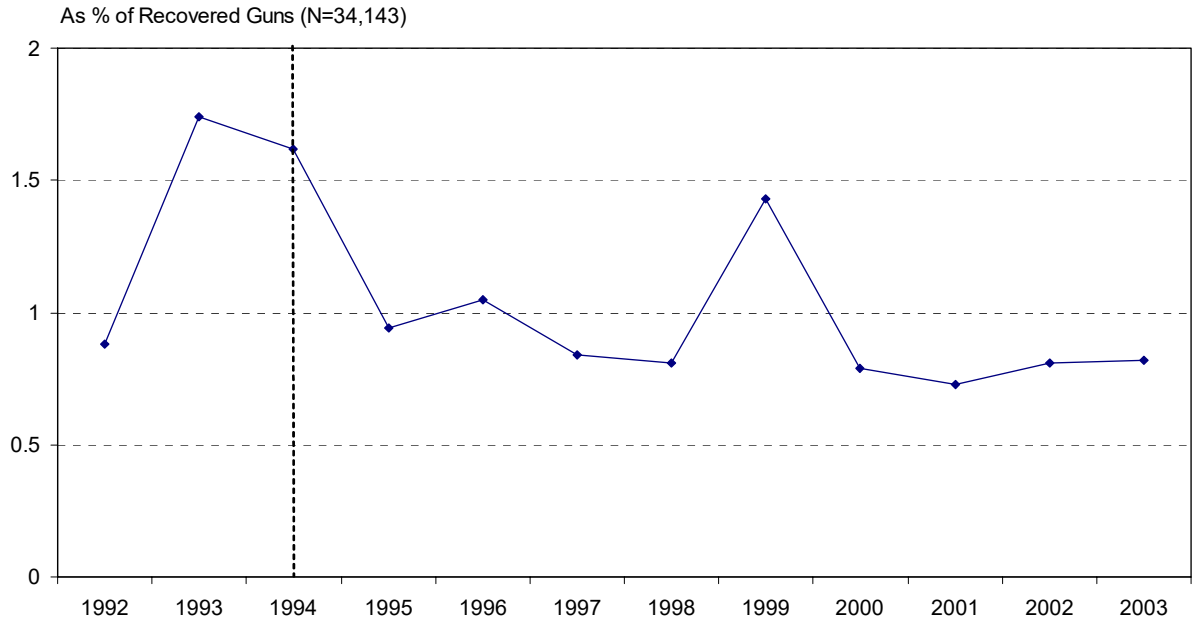
Includes Intratec group, SWD group, AR-15 group, and selected Calico and Feather models.

Table 6-5. Trends in Police Recoveries of Domestic Assault Weapons in St. Louis, 1992-2003 [a]

	Pre-Ban Period	Post-Ban Period	Change
A. All Recoveries	Jan. 1992-Dec. 1993	Jan. 1995-Dec. 2003	
Total AWs	94	212	
Annual Mean	47	23.56	-50%
AW's as % of Guns	1.33%	0.91%	-32%**
APs	87	187	
Annual Mean	43.5	20.78	-52%
APs as % of Guns	1.23%	0.81%	-34%**
ARs	7	25	
Annual Mean	3.5	2.78	-21%
ARs as % of Guns	0.1%	0.11%	10%
Total AWs and Substitutes	94	234	
Annual Mean	47	26	-45%
AWs/Subs as % of Guns	1.33%	1.01%	-24%*
B. Recoveries Linked to Violent Crimes [b]			
Total AWs	8	20	
Annual Mean	4	2.2	-45%
AWs as % of Violent Crime Guns	0.8%	0.81%	1%

a. Domestic assault weapons include Intratec group, SWD group, AR-15 group, and Calico and Feather models.
b. Murders, assaults, and robberies
* Chi-square p level < .05 (changes in percentages of guns that were AWs/APs/ARs/AW-subs were tested for statistical significance)
** Chi-square p level <.01 (changes in percentages of guns that were AWs/APs/ARs/AW-subs were tested for statistical significance)

Figure 6-4. Police Recoveries of Assault Weapons in St. Louis, 1992-2003



Includes Intratec group, SWD group, AR-15 group, and selected Calico and Feather models.

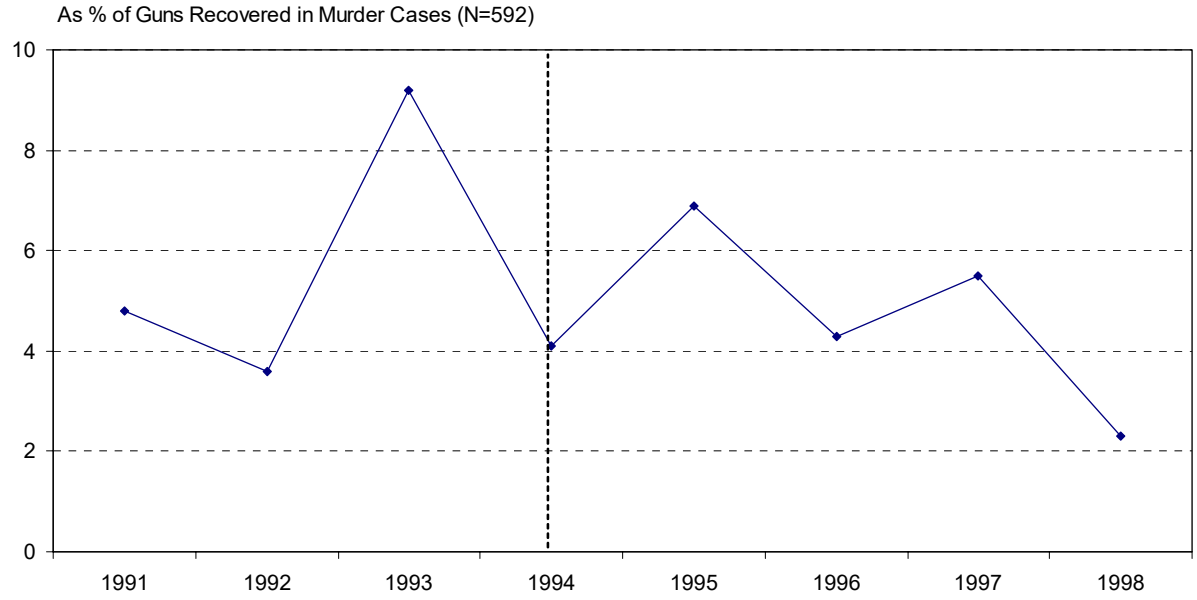
Table 6-6. Trends in Police Recoveries of Domestic Assault Weapons in Boston, Milwaukee, and Anchorage (Alaska) [a]

	Pre-Ban Period	Post-Ban Period	Change
Boston	Jan. 1991-Dec. 1993	Jan. 2000-Dec. 2002	
(All Gun Traces)			
AWs	60	11	
Annual Mean	20	3.7	-82%
AWs as % of Guns	2.16%	0.6%	-72%*
AWs and Substitutes	60	16	
Annual Mean	20	5.3	-74%
AWs/Subs as % of Guns	2.16%	0.87%	-60%*
Milwaukee	Jan. 1991-Dec. 1993	Jan. 1995-Dec. 1998	
(Guns Recovered in Murder Cases)			
AWs	15	13	
Annual Mean	5	3.25	-35%
AWs as % of Guns	5.91%	4.91%	-17%
AWs and Substitutes	15	16	
Annual Mean	5	4	-20%
AWs/Subs as % of Guns	5.91%	6.04%	2%
Anchorage	Jan. 1987-Dec. 1993	Jan. 1995-Dec. 2000	
(Guns Tested for Evidence)			
AWs	16	8	
Annual Mean	2.29	1.33	-42%
AW's as % of Guns	3.57%	2.13%	-40%
AWs and Substitutes	N/A	N/A	

a. Domestic assault weapons include Intratec group, SWD group, AR-15 group, and Calico and Feather models.

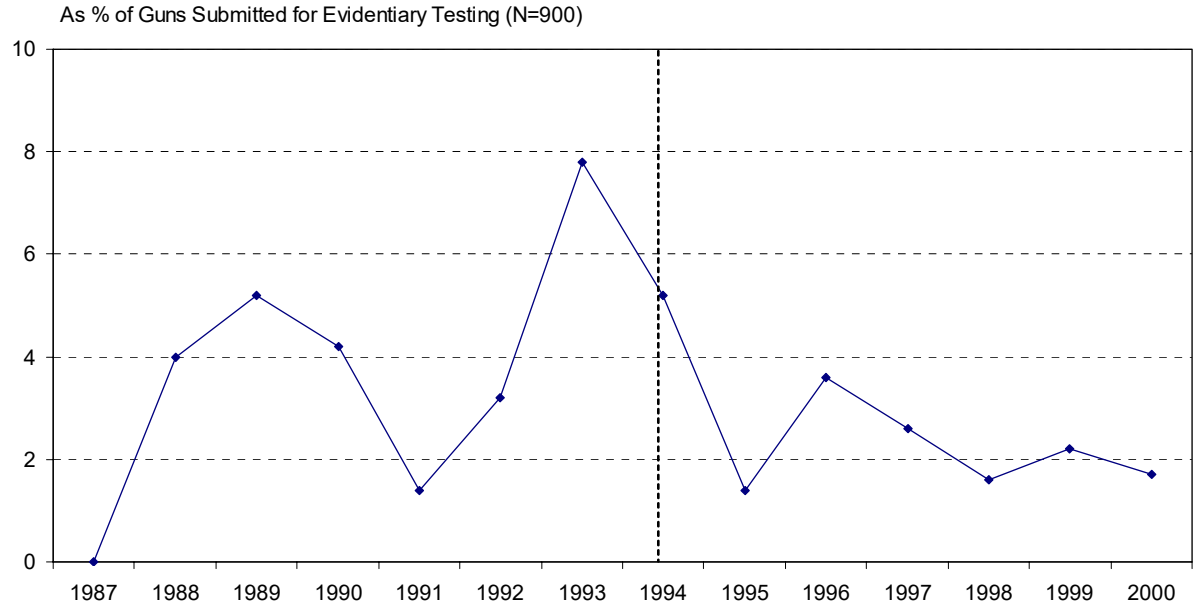
* Chi-square p level < .01 (changes in percentages of guns that were AWs/AW-subs were tested for statistical significance)

Figure 6-5. Assault Weapons Recovered in Milwaukee County Murder Cases, 1991-1998



Includes Intratec group, SWD group, AR-15 group, and selected Calico and Feather models.

Figure 6-6. Police Recoveries of Assault Weapons in Anchorage (Alaska), 1987-2000



Includes Intratec group, SWD group, AR-15 group, and selected Calico and Feather models.

7. MARKET INDICATORS FOR LARGE CAPACITY MAGAZINES: PRICES AND IMPORTATION

The previous chapters examined the AW-LCM ban's impact on the availability and criminal use of AWs. In this chapter and the next, we consider the impact of the ban's much broader prohibition on LCMs made for numerous banned and non-banned firearms. We begin by studying market indicators. Our earlier study of LCM prices for a few gun models revealed that prices rose substantially during 1994 and into 1995 (Roth and Koper, 1997, Chapter 4). Prices of some LCMs remained high into 1996, while others returned to pre-ban levels or oscillated more unpredictably. The price increases may have reduced LCM use at least temporarily in the short-term aftermath of the ban, but we could not confirm this in our prior investigation.

7.1. Price Trends for Large Capacity Magazines

For this study, we sought to approximate longer term trends in the prices at which users could purchase banned LCMs throughout the country. To that end, we analyzed quarterly data on the prices of LCMs advertised by eleven gun and magazine distributors in *Shotgun News*, a national gun industry publication, from April 1992 to December 1998.[63] Those prices are available to any gun dealer, and primary market retailers generally re-sell within 15% of the distributors' prices.[64] The distributors were chosen during the course of the first AW study (Roth and Koper, 1997) based on the frequency with which they advertised during the April 1992 to June 1996 period. For each quarterly period, project staff coded prices for one issue from a randomly selected month. We generally used the first issue of each selected month based on a preliminary, informal assessment suggesting that the selected distributors advertised more frequently in those issues. In a few instances, first-of-month issues were unavailable to us or provided too few observations, so we substituted other issues.[65] Also, we were unable to obtain *Shotgun News* issues for the last two quarters of 1996. However, we aggregated the data annually to study price trends, and the omission of those quarters did not appear to affect the results (this is explained further below).

We ascertained trends in LCM prices by conducting hedonic price analyses,

[63] The *Blue Book of Gun Values*, which served as the data source for the AW price analysis, does not contain ammunition magazine prices.

[64] According to gun market experts, retail prices track wholesale prices quite closely (Cook et al., 1995, p. 71). Retail prices to eligible purchasers generally exceed wholesale (or original-purchase) prices by 3% to 5% in the large chain stores, by about 15% in independent dealerships, and by about 10% at gun shows (where overhead costs are lower).

[65] The decision to focus on first-of-month issues was made prior to data collection for price analysis update. For the earlier study (Roth and Koper, 1997), project staff coded data for one or more randomly selected issues of every month of the April 1992 to June 1996 period. For this analysis, we utilized data from only the first-of-month issues selected at random during the prior study. If multiple first-of-month issues were available for a given quarter, we selected one at random or based on the number of recorded advertisements. If no first-of-month issue was available for a given quarter, we selected another issue at random from among those coded during the first study.

similar to those described in the AW price analysis (Chapter 5), in which we regressed inflation-adjusted LCM prices (logged) on several predictors: magazine capacity (logged), gun make (for which the LCM was made), year of the advertisement, and distributor. We cannot account fully for the meaning of significant distributor effects. They may represent unmeasured quality differentials in the merchandise of different distributors, or they may represent other differences in stock volume or selling or service practices between the distributors.[66] We included the distributor indicators when they proved to be significant predictors of advertised price. In addition, we focused on LCMs made for several of the most common LCM-compatible handguns and rifles, rather than try to model the differences in LCM prices between the several hundred miscellaneous makes and models of firearms that were captured in the data. Finally, for both the handgun and rifle models, we created and tested seasonal indicator variables to determine if their incorporation would affect the coefficient for 1996 (the year with winter/spring data only), but they proved to be statistically insignificant and are not shown in the results below.[67]

7.1.1. Large Capacity Magazines for Handguns

The handgun LCM analysis tracks the prices of LCMs made for Intratec and Cobray (i.e., SWD) APs and non-banned semiautomatic pistols made by Smith and Wesson, Glock, Sturm Ruger, Sig-Sauer, Taurus, and Beretta (each of the manufacturers in the former group produces numerous models capable of accepting LCMs). In general, LCMs with greater magazine capacities commanded higher prices, and there were significant price differentials between LCMs made for different guns and sold by different distributors (see Table 7-1). Not surprisingly, LCMs made for Glock handguns were most expensive, followed by those made for Beretta and Sig-Sauer firearms.

Turning to the time trend indicators (see Table 7-1 and Figure 7-1), prices for these magazines increased nearly 50% from 1993 to 1994, and they rose another 56% in 1995. Prices declined somewhat, though not steadily, from 1996 to 1998. Nevertheless, prices in 1998 remained 22% higher than prices in 1994 and nearly 80% higher than those in 1993.

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[66] For example, one possible difference between the distributors may have been the extent to which they sold magazines made of different materials (e.g., steel, aluminum, etc.) or generic magazines manufactured by companies other than the companies manufacturing the firearms for which the magazines were made. For example, there were indications in the data that 3% of the handgun LCMs and 10% of the AR-15 and Mini-14 rifle LCMs used in the analyses (described below) were generic magazines. We did not control for these characteristic, however, because such information was often unclear from the advertisements and was not recorded consistently by coders.

[67] Project staff coded all LCM advertisements by the selected distributors. Therefore, the data are inherently weighted. However, the weights are based on the frequency with which the different LCMs were advertised (i.e., the LCMs that were advertised most frequently have the greatest weight in the models) rather than by production volume.

Table 7-1. Regression of Handgun and Rifle Large Capacity Magazine Prices on Annual Time Indicators, 1992-1998, Controlling for Gun Makes/Models and Distributors

	Handgun LCMs (n=1,277)		Rifle LCMs (n=674)	
	Estimate	**T value**	**Estimate**	**T value**
Constant	-1.79	-12.74***	-4.10	-19.12***
1992	-0.19	-2.11**	-0.48	-4.20***
1993	-0.38	-6.00***	-0.55	-6.14***
1995	0.44	6.88***	-0.25	-2.64***
1996	0.29	4.05***	-0.12	-0.93
1997	0.36	6.33***	-0.31	-3.68***
1998	0.20	3.51***	-0.44	-5.19***
Rounds (logged)	0.26	5.73***	0.84	15.08***
Cobray	-0.36	-4.15***		
Glock	0.41	8.15***		
Intratec	-0.40	-4.18***		
Ruger	-0.42	-7.79***		
Smith&Wesson	-0.08	-1.71*		
Sig-Sauer	0	-0.09		
Taurus	-0.31	-6.10***		
AK-type			-0.25	-3.15***
Colt AR-15			0.14	1.68*
Ruger Mini-14			-0.08	-0.92
Distributor 1	-0.72	-16.38***	-0.35	-5.15***
Distributor 2	-0.15	-0.97	-0.83	-5.24***
Distributor 3	-0.16	-3.93***	0.19	2.69***
Distributor 4	-0.55	-5.72***	0.16	0.80
Distributor 5	-0.07	-1.79*	-0.18	-2.65***
Distributor 6	-0.53	-1.23	-0.12	-0.32
Distributor 7	-1.59	-3.70***	-0.10	-0.91
Distributor 8			0.14	0.70
Distributor 9	-0.91	-12.52***	-0.48	-4.00***
F statistic	58.76		21.22	
(p value)	<.0001		<.0001	
Adj. R-square	0.51		0.38	

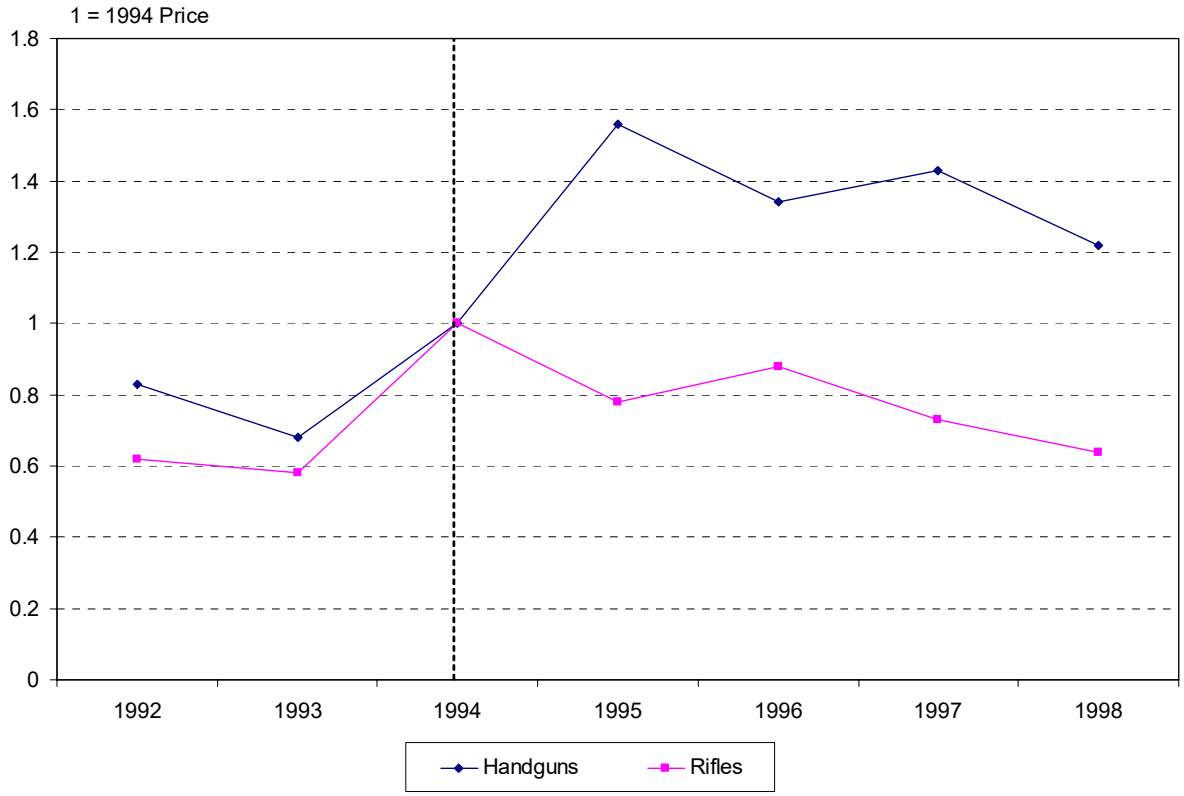
Year indicators are interpreted relative to 1994, and distributors are interpreted relative to distributor 10. Handgun makes are relative to Beretta and rifle models are relative to SKS.

* Statistically significant at p<=.10.

** Statistically significant at p<=.05.

*** Statistically significant at p<=.01.

Figure 7-1. Annual Price Trends for Large Capacity Magazines, 1992-1998



Based on 1,277 sampled ads for LCMs fitting models of 8 handgun makers and 674 sampled ads for LCMs fitting 4 rifle model groups.

7.1.2. Large Capacity Magazines for Rifles

We approximated trends in the prices of LCMs for rifles by modeling the prices of LCMs manufactured for AR-15, Mini-14, SKS,[68] and AK-type rifle models (including various non-banned AK-type models). As in the handgun LCM model, larger LCMs drew higher prices, and there were several significant model and distributor effects. AR-15 magazines tended to have the highest prices, and magazines for AK-type models had the lowest prices (Table 7-1).

Like their handgun counterparts, prices for rifle LCMs increased over 40% from 1993 to 1994, as the ban was debated and implemented (see Table 7-1 and Figure 7-1). However, prices declined over 20% in 1995. Following a rebound in 1996, prices moved downward again during 1997 and 1998. Prices in 1998 were over one third lower than the peak prices of 1994 and were comparable to pre-ban prices in 1992 and 1993.

[68] The SKS is a very popular imported rifle (there are Russian and Chinese versions) that was not covered by either the 1989 AR import ban or the 1994 AW ban. However, importation of SKS rifles from China was discontinued in 1994 due to trade restrictions.

7.2. Post-Ban Importation of Large Capacity Magazines

ATF does not collect (or at least does not publicize) statistics on production of LCMs. Therefore, we cannot clearly document pre-ban production trends. Nevertheless, it seems likely that gun and magazine manufacturers boosted their production of LCMs during the debate over the ban, just as AW makers increased production of AWs. Regardless, gun industry sources estimated that there were 25 million LCMs available as of 1995 (including aftermarket items for repairing magazines or converting them to LCMs) (Gun Tests, 1995, p. 30).

Moreover, the supply of LCMs continued to grow even after the ban due to importation of foreign LCMs that were manufactured prior to the ban (and thus grandfathered by the LCM legislation), according to ATF importation data.[69] As shown in Table 7-2, nearly 4.8 million LCMs were imported for commercial sale (as opposed to law enforcement uses) from 1994 through 2000, with the largest number (nearly 3.7 million) arriving in 1999.[70] During this period, furthermore, importers received permission to import a total of 47.2 million LCMs; consequently, an additional 42 million LCMs may have arrived after 2000 or still be on the way, based on just those approved through 2000.[71, 72]

To put this in perspective, gun owners in the U.S. possessed 25 million firearms that were equipped with magazines holding 10 or more rounds as of 1994 (Cook and Ludwig, 1996, p. 17). Therefore, the 4.7 million LCMs imported in the U.S. from 1994 through 2000 could conceivably replenish 19% of the LCMs that were owned at the time of the ban. The 47.2 million approved during this period could supply nearly 2 additional LCMs for all guns that were so equipped as of 1994.

7.3. Summary and Interpretations

Prices of LCMs for handguns rose significantly around the time of the ban and, despite some decline from their peak levels in 1995, remained significantly higher than pre-ban prices through at least 1998. The increase in LCM prices for rifles proved to be more temporary, with prices returning to roughly pre-ban levels by 1998.[73]

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[69] To import LCMs into the country, importers must certify that the magazines were made prior to the ban. (The law requires companies to mark post-ban LCMs with serial numbers.) As a practical matter, however, it is hard for U.S. authorities to know for certain whether imported LCMs were produced prior to the ban.

[70] The data do not distinguish between handgun and rifle magazines or the specific models for which the LCMs were made. But note that roughly two-thirds of the LCMs imported from 1994 through 2000 had capacities between 11 and 19 rounds, a range that covers almost all handgun LCMs as well as many rifle LCMs. It seems most likely that the remaining LCMs (those with capacities of 20 or more rounds) were primarily for rifles.

[71] The statistics in Table 7-2 do not include belt devices used for machine guns.

[72] A caveat to the number of approved LCMs is that importers may overstate the number of LCMs they have available to give themselves leeway to import additional LCMs, should they become available.

[73] A caveat is that we did not examine prices of smaller magazines, so the price trends described here may not have been entirely unique to LCMs. Yet it seems likely that these trends reflect the unique impact of the ban on the market for LCMs.

Table 7-2. Large Capacity Magazines Imported into the United States or Approved For Importation for Commercial Sale, 1994-2000

Year	Imported	Approved
1994	67,063	77,666
1995	3,776	2,066,228
1996	280,425	2,795,173
1997	99,972	1,889,773
1998	337,172	20,814,574
1999	3,663,619	13,291,593
2000	346,416	6,272,876
Total	*4,798,443*	*47,207,883*

Source: Firearms and Explosives Imports Branch, Bureau of Alcohol, Tobacco, Firearms, and Explosives. Counts do not include "links" (belt devices) or imports for law enforcement purposes.

The drop in rifle LCM prices between 1994 and 1998 may have due to the simultaneous importation of approximately 788,400 grandfathered LCMs, most of which appear to have been rifle magazines (based on the fact that nearly two-thirds had capacities over 19 rounds), as well as the availability of U.S. military surplus LCMs that fit rifles like the AR-15 and Mini-14. We can also speculate that demand for LCMs is not as great among rifle consumers, who are less likely to acquire their guns for defensive or criminal purposes.

The pre-ban supply of handgun LCMs may have been more constricted than the supply of rifle LCMs for at least a few years following the ban, based on prices from 1994 to 1998. Although there were an estimated 25 million LCMs available in the U.S. as of 1995, some major handgun manufacturers (including Ruger, Sig Sauer, and Glock) had or were close to running out of new LCMs by that time (Gun Tests, 1995, p. 30). Yet the frequency of advertisements for handgun LCMs during 1997 and 1998, as well as the drop in prices from their 1995 peak, suggests that the supply had not become particularly low. In 1998, for example, the selected distributors posted a combined total of 92 LCM ads per issue (some of which may have been for the same make, model, and capacity combinations) for just the handguns that we incorporated into our model.[74] Perhaps the

[74] Project staff found substantially more advertisements per issue for 1997 and 1998 than for earlier years. For the LCMs studied in the handgun analysis, staff recorded an average of 412 LCM advertisements per year (103 per issue) during 1997 and 1998. For 1992-1996, staff recorded an average of about 100 ads per year (25 per issue) for the same LCMs. A similar but smaller differential existed in the volume of ads for the LCMs used in the rifle analysis. The increase in LCM ads over time may reflect changes in supply and

demand for enhanced firepower among handgun consumers, who are more likely to acquire guns for crime or defense against crime, was also a factor (and perhaps a large one) putting a premium on handgun LCMs.

Although we might hypothesize that high prices depressed use of handguns with LCMs for at least a few years after the ban, a qualification to this prediction is that LCM use may be less sensitive to prices than is use of AWs because LCMs are much less expensive than the firearms they complement and therefore account for a smaller fraction of users' income (e.g., see Friedman, 1962). To illustrate, TEC-9 APs typically cost $260 at retail during 1992 and 1993, while LCMs for the TEC-9, ranging in capacity from 30 to 36 rounds, averaged $16.50 in *Shotgun News* advertisements (and probably $19 or less at retail) during the same period. So, for example, a doubling of both gun and LCM prices would likely have a much greater impact on purchases of TEC-9 pistols than purchases of LCMs for the TEC-9. Users willing and able to pay for a gun that accepts an LCM are most likely willing and able to pay for an LCM to use with the gun.

Moreover, the LCM supply was enhanced considerably by a surge in LCM imports that occurred after the period of our price analysis. During 1999 and 2000, an additional 4 million grandfathered LCMs were imported into the U.S., over two-thirds of which had capacities of 11-19 rounds, a range that covers almost all handgun LCMs (as well as many rifle LCMs). This may have driven prices down further after 1998.

In sum, market indicators yield conflicting signs on the availability of LCMs. It is perhaps too early to expect a reduction in crimes with LCMs, considering that tens of millions of grandfathered LCMs were available at the time of the ban, an additional 4.8 million – enough to replenish one-fifth of those owned by civilians – were imported from 1994 through 2000, and that the elasticity of demand for LCMs may be more limited than that of firearms. And if the additional 42 million foreign LCMs approved for importation become available, there may not be a reduction in crimes with LCMs anytime in the near future.

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demand for LCMs during the study period, as well as product shifts by distributors and perhaps changes in ad formats (e.g., ads during the early period may have been more likely to list magazines by handgun model without listing the exact capacity of each magazine, in which case coders would have been more likely to miss some LCMs during the early period). Because the data collection effort for the early period was part of a larger effort that involved coding prices in *Shotgun News* for LCMs and numerous banned and non-banned firearms, it is also possible that coders were more likely to miss LCM ads during that period due to random factors like fatigue or time constraints.

8. CRIMINAL USE OF LARGE CAPACITY MAGAZINES AFTER THE BAN

Assessing trends in criminal use of LCMs is difficult. There is no national data source on crime guns equipped with LCMs (ATF national tracing data do not include information about magazines recovered with traced firearms), and, based on our contacts with numerous police departments over the course of this study and the first AW study, it seems that even those police departments that maintain electronic databases on recovered firearms do not typically record the capacity of the magazines with which the guns are equipped.[75,76] Indeed, we were unable to acquire sufficient data to examine LCM use for the first AW study (Roth and Koper, 1997).

For the current study, we obtained four data sources with which to investigate trends in criminal use of LCMs. Three of the databases utilized in the AW analysis – those from Baltimore, Milwaukee, and Anchorage – contained information about the magazines recovered with the guns (see the descriptions of these databases in Chapter 6). Using updated versions of these databases, we examined all LCM recoveries in Baltimore from 1993 through 2003, recoveries of LCMs in Milwaukee murder cases from 1991 to 2001, and recoveries of LCMs linked to serious crimes in Anchorage (and other parts of Alaska) from 1992 through 2002.[77] In addition, we studied records of guns and magazines submitted to the Jefferson Regional Forensics Lab in Louisville, Kentucky from 1996 through 2000. This lab of the Kentucky State Police services law enforcement agencies throughout roughly half of Kentucky, but most guns submitted to the lab are from the Louisville area. Guns examined at the lab are most typically those associated with serious crimes such as murders, robberies, and assaults.

The LCM analyses and findings were not as uniform across locations as were those for AWs. Therefore, we discuss each site separately. As in the AW analysis, we emphasize changes in the percentage of guns equipped with LCMs to control for overall trends in gun crime and gun recoveries. Because gun crime was falling during the latter 1990s, we anticipated that the number of guns recovered with LCMs might decline independently of the ban's impact. (Hereafter, we refer to guns equipped with LCMs as LCM guns.)

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[75] For the pre-ban period, one can usually infer magazine capacity based on the firearm model. For post-ban recoveries, this is more problematic because gun models capable of accepting LCMs may have been equipped with grandfathered LCMs or with post-ban magazines designed to fit the same gun but holding fewer rounds.

[76] As for the AW analysis in Chapter 6, we utilize police data to examine trends in criminal use of LCMs. The reader is referred to the general discussion of police gun seizure data in Chapter 6.

[77] Findings presented in our 2002 interim report (Koper and Roth, 2002b) indicated that LCM use had not declined as of the late 1990s. Therefore, we sought to update the LCM analyses where possible for this version of the report.

8.1. Baltimore

In Baltimore, about 14% of guns recovered by police were LCM guns in 1993. This figure remained relatively stable for a few years after the ban but had dropped notably by 2002 and 2003 (Figure 8-1). For the entire post-ban period (1995-2003), recoveries of LCM guns were down 8% relative to those of guns with smaller magazines (Table 8-1, panel A), a change of borderline statistical significance. Focusing on the most recent years, however, LCM gun recoveries were 24% lower in 2002 and 2003 than during the year prior to the ban, a difference that was clearly significant (Table 8-1, panel B).[78,79,80] This change was attributable to a 36% drop in LCM handguns (Table 8-1, panel C). LCM rifles actually increased 36% as a share of crime guns, although they still accounted for no more than 3% in 2002 and 2003 (Table 8-1, panel D).[81]

Yet there was no decline in recoveries of LCM guns used in violent crimes (i.e., murders, shootings, robberies, and other assaults). After the ban, the percentage of violent crime guns with LCMs generally oscillated in a range consistent with the pre-ban level (14%) and hit peaks of roughly 16% to 17% in 1996 and 2003 (Figure 8-1).[82] Whether comparing the pre-ban period to the entire post-ban period (1995-2003) or the most recent years (2002-2003), there was no meaningful decline in LCM recoveries linked to violent crimes (Table 8-2, panels A and B).[83] Neither violent uses of LCM

[78] Data on handgun magazines were also available for 1992. An auxiliary analysis of those data did not change the substantive inferences described in the text.

[79] The Maryland AP ban enacted in June 1994 also prohibited ammunition magazines holding over 20 rounds and did not permit additional sales or transfers of such magazines manufactured prior to the ban. This ban, as well as the Maryland and federal bans on AWs that account for many of the guns with magazines over 20 rounds, may have contributed to the downward trend in LCMs in Baltimore, but only 2% of the guns recovered in Baltimore from 1993 to 2000 were equipped with such magazines.

[80] All comparisons of 1993 to 2002-2003 in the Baltimore data are based on information from the months of January through November of each year. At the time we received these data, information was not yet available for December 2003, and preliminary analysis revealed that guns with LCMs were somewhat less likely to be recovered in December than in other months for years prior to 2003. Nevertheless, utilizing the December data for 1993 and 2002 did not change the substantive inferences. We did not remove December data from the comparisons of 1993 and the full post-ban period because those comparisons seemed less likely to be influenced by the absence of one month of data.

[81] This increase may have been due largely to a general increase in rifle seizures. LCM rifles actually dropped as a percentage of all rifle recoveries from 1993 to 2002-2003, suggesting that recoveries of LCM rifles were increasing less than recoveries of other rifles.

[82] For 1996, 45% of all records and 24% of those linked to violent crimes had missing data for magazine capacity (due to temporary changes in operational procedures in the Baltimore crime lab). For other years, missing data rates were no more than 6%. Based on those cases for which data were available, the share of guns with LCMs in 1996 was comparable to that in other years, particularly when examining all gun recoveries. At any rate, the analyses focusing on 1993, 2002, and 2003 reinforce the findings of those that include the 1996 data.

[83] The ammunition capacity code in the Baltimore data usually reflected the full capacity of the magazine and weapon, but sometimes reflected the capacity of the magazine only. (For instance, a semiautomatic with a 10-round magazine and the ability to accept one additional round in the chamber might have been coded as having a capacity of 10 or 11.) Informal assessment suggested that capacity was more likely to reflect the exact capacity of the magazine in the early years of the database and more likely to reflect the full capacity of the gun and magazine in later years. For the main runs presented in the text and tables, guns were counted as having LCMs if the coded capacity was greater than 11 rounds. This ensured that LCMs were not overestimated, but it potentially understated LCM prevalence, particularly for the earlier

handguns or LCM rifles had declined appreciably by 2002-2003 (Table 8-2, panels C and D). Hence, the general decline in LCM recoveries may reflect differences in the availability and use of LCMs among less serious offenders, changes in police practices,[84] or other factors.

Figure 8-1. Police Recoveries of Guns Equipped With Large Capacity Magazines in Baltimore, 1993-2003

years. However, coding the guns as LCM weapons based on a threshold of 10 (i.e., a coded capacity over 10 rounds) in 1993 and a threshold of 11 (i.e., a coded capacity over 11 rounds) for 2002-2003 did not change the inferences of the violent crime analysis. Further, this coding increased the pre-ban prevalence of LCMs by very little (about 4% in relative terms).

[84] During the late 1990s, for example, Baltimore police put greater emphasis on detecting illegal gun carrying (this statement is based on prior research and interviews the author has done in Baltimore as well as the discussion in Center to Prevent Handgun Violence, 1998). One can hypothesize that this effort reduced the fraction of recovered guns with LCMs because illegal gun carriers are probably more likely to carry smaller, more concealable handguns that are less likely to have LCMs.

Table 8-1. Trends in All Police Recoveries of Firearms Equipped With Large Capacity Magazines, Baltimore, 1993-2003

	Pre-Ban Period	Post-Ban Period	Change
A. All LCM Guns	Jan.-Dec. 1993	Jan. 1995-Nov. 2003	
Total	473	3703	
Annual Mean	473	445.86 [a]	-6%
LCM Guns as % of All Guns	13.51%	12.38%	-8%*
B. All LCM Guns	Jan.-Nov. 1993	Jan.-Nov. 2002-2003	
Total	430	626	
Annual Mean	430	313	-27%
LCM Guns as % of All Guns	13.47%	10.3%	-24%***
C. LCM Handguns	Jan.-Nov. 1993	Jan.-Nov. 2002-2003	
Total	359	440	
Annual Mean	359	220	-39%
LCM Handguns as % of All Guns	11.25%	7.24%	-36%***
D. LCM Rifles	Jan.-Nov. 1993	Jan.-Nov. 2002-2003	
LCM Rifles	71	183	
Annual Mean	71	91.5	29%
LCM Rifles as % of All Guns	2.22%	3.01%	36%**

a. Annual average calculated without 1996 and 2003 (to correct for missing months or missing magazine data).
* Chi-square p level < .10 (changes in percentages of guns equipped with LCMs were tested for statistical significance)
** Chi-square p level <.05 (changes in percentages of guns equipped with LCMs were tested for statistical significance)
** Chi-square p level < .01 (changes in percentages of guns equipped with LCMs were tested for statistical significance)

Table 8-2. Trends in Police Recoveries of Firearms Equipped With Large Capacity Magazines in Violent Crime Cases, Baltimore, 1993-2003

	Pre-Ban Period	Post-Ban Period	Change [a]
A. All LCM Guns	Jan.-Dec. 1993	Jan. 1995-Nov. 2003	
Total	87	711	
Annual Mean	87	81.86 [b]	-6%
LCM Guns as % of All Guns	14.01%	14.44%	3%
B. All LCM Guns	Jan.-Nov. 1993	Jan.-Nov. 2002-2003	
Total	79	104	
Annual Mean	79	52	-34%
LCM Guns as % of All Guns	13.96%	13.65%	-2%
C. LCM Handguns	Jan.-Nov. 1993	Jan.-Nov. 2002-2003	
Total	62	81	
Annual Mean	62	40.5	-35%
LCM Handguns as % of All Guns	10.95%	10.63%	-3%
D. LCM Rifles	Jan.-Nov. 1993	Jan.-Nov. 2002-2003	
LCM Rifles	17	23	
Annual Mean	17	11.5	-32%
LCM Rifles as % of All Guns	3%	3.02%	1%

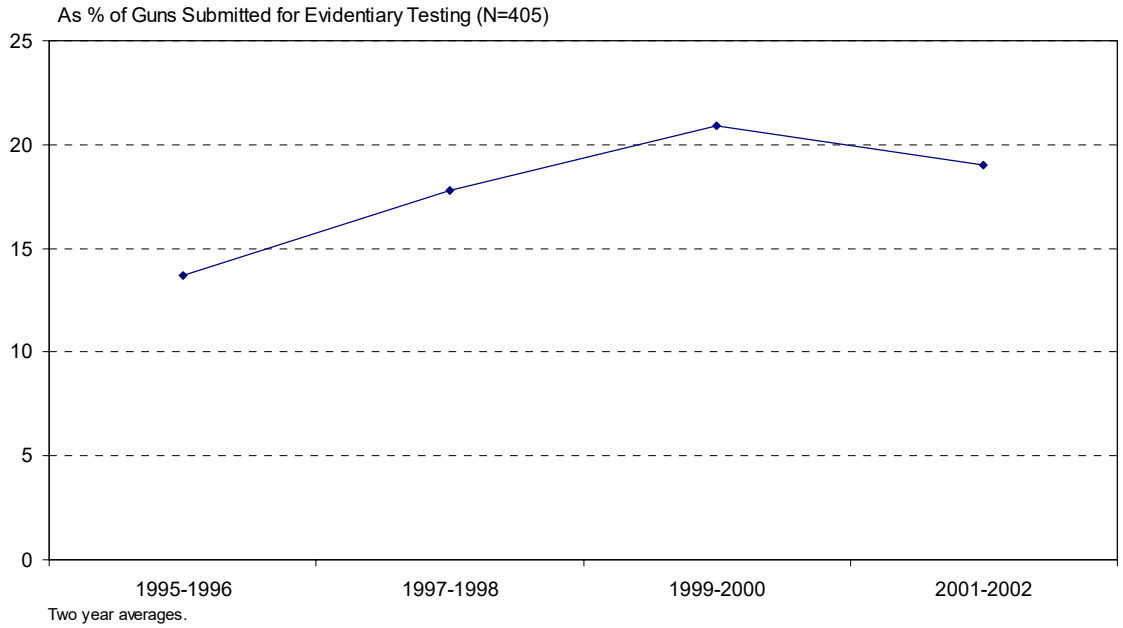
a. Changes in the percentages of guns with LCMs were statistically insignificant in chi-square tests.
b. Annual average calculated without 1996 and 2003 (to correct for missing months or missing magazine data).

8.2. Anchorage

In the Alaska database, magazine capacity was recorded only for guns recovered during the post-ban years, 1995 through 2002. However, we estimated pre-ban use of LCM handguns by identifying handgun models inspected during 1992 and 1993 that were manufactured with LCMs prior to the ban.[85] This permitted an assessment of pre-post changes in the use of LCM handguns.

As shown in Figure 8-2 (also see Table 8-3, panel A), LCM guns rose from 14.5% of crime guns in 1995-1996 to 24% in 2000-2001 (we present two-year averages because the sample are relatively small, particularly for the most recent years) and averaged about 20% for the entire post-ban period. LCM handguns drove much of this trend, but LCM rifles also increased from about 3% of crime guns in 1995-96 to 11% in 2000-2001.

Figure 8-2. Police Recoveries of Guns Equipped With Large Capacity Magazines in Anchorage (Alaska), 1995-2002



[85] To make these determinations, we consulted gun catalogs such as the *Blue Book of Gun Values* and *Guns Illustrated.*

Table 8-3. Trends in Police Recoveries of Firearms Equipped With Large Capacity Magazines in Violent Crime Cases, Anchorage (Alaska), 1992-2002 [a]

	Pre-Ban Period	Post-Ban Period	Change [b]
A. All LCM Guns	N/A	Jan. 1995-Dec. 2002	
Total		80	
Annual Mean		10	N/A
LCM Guns as % of All Guns		19.75%	N/A
B. LCM Handguns	Jan. 1992-Dec. 1993	Jan. 1995-Dec. 2002	
Total	17	57	
Annual Mean	8.5	7.13	-16%
LCM Handguns as % All Handguns	26.15%	22.35%	-15%
C. LCM Handguns	Jan. 1992-Dec. 1993	Jan. 2001-Dec. 2002	
Total	17	10	
Annual Mean	8.5	5	-41%
LCM Handguns as % of All Handguns	26.15%	19.23%	-26%

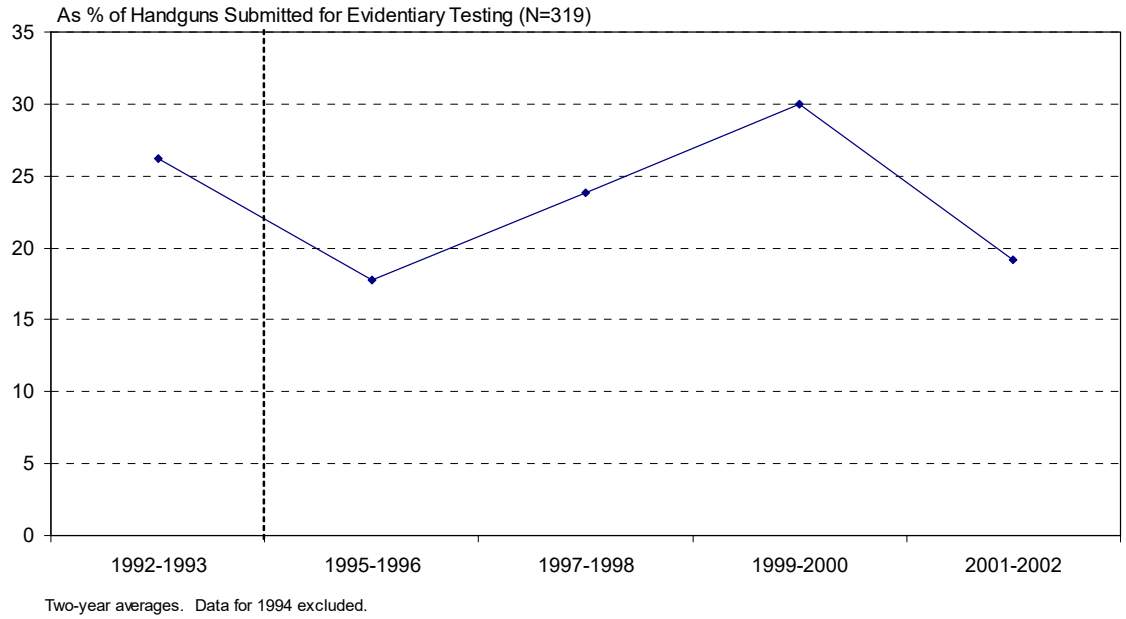
a. Based on guns submitted to State Police for evidentiary testing.
b. Changes in the percentages of guns equipped with LCMs were statistically insignificant in chi-square tests.

Investigation of pre-post changes for handguns revealed an inconsistent pattern (Figure 8-3). LCM handguns dropped initially after the ban, declining from 26% of handguns in 1992-1993 to 18% in 1995-1996. However, they rebounded after 1996, reaching a peak of 30% of handguns in 1999-2000 before declining to 19% in 2001-2002.

For the entire post-ban period, the share of handguns with LCMs was about 15% lower than in the pre-ban period (Table 8-3, panel B). By the two most recent post-ban years (2001-2002), LCM use had dropped 26% from the pre-ban years (Table 8-3, panel C). These changes were not statistically significant, but the samples of LCM handguns were rather small for rigorous statistical testing. Even so, it seems premature to conclude

that there has been a lasting reduction in LCM use in Alaska. LCM use in 2001-2002 was somewhat higher than that immediately following the ban in 1995-1996, after which there was a substantial rebound. Considering the inconsistency of post-ban patterns, further follow-up seems warranted before making definitive conclusions about LCM use in Alaska.

Figure 8-3. Police Recoveries of Handguns Equipped With Large Capacity Magazines in Anchorage (Alaska), 1992-2002



8.3. Milwaukee

LCM guns accounted for 21% of guns recovered in Milwaukee murder investigations from 1991 to 1993 (Table 8-4, panel A). Following the ban, this figure rose until reaching a plateau of over 36% in 1997 and 1998 (Figure 8-4). On average, the share of guns with LCMs grew 55% from 1991-1993 to 1995-1998, a trend that was driven by LCM handguns (Table 8-4, panels A and B).[86] LCM rifles held steady at between 4% and 5% of the guns (Table 8-4, panel C).

We also analyzed a preliminary database on 48 guns used in murders during 2000 and 2001 (unlike the 1991-1998 database, this database did not include information on other guns recovered during the murder investigations). About 11% of these guns were LCM guns, as compared to 19% of guns used in murders from 1991 to 1993 (analyses not shown). However, nearly a quarter of the 2000-2001 records were missing information on magazine capacity.[87] Examination of the types and models of guns with

[86] LCM guns also increased as share of guns that were used in the murders (the full sample results discussed in the text include all guns recovered during the investigations).
[87] Magazine capacity was missing for less than 4% of the records in earlier years.

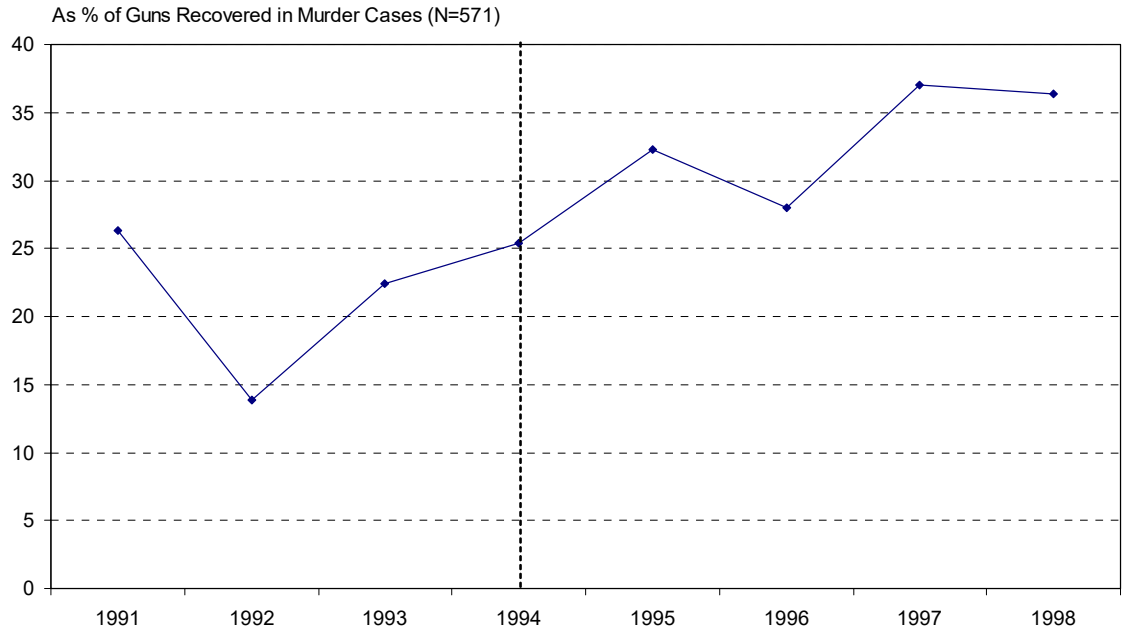
unidentified magazines suggested that as many as 17% of guns used in murders during 2000 and 2001 may have been LCM guns (based on all those that either had LCMs, were models sold with LCMs prior to the ban, or were unidentified semiautomatics). While this still suggests a drop in LCM use from the peak levels of the late 1990s (26% of guns used in murders from 1995 to 1998 had LCMs), it is not clear that LCM use has declined significantly below pre-ban levels.

Table 8-4. Trends in Police Recoveries of Firearms Equipped With Large Capacity Magazines in Murder Cases, Milwaukee County, 1991-1998

	Pre-Ban Period	Post-Ban Period	Change
A. All LCM Guns	Jan. 1991-Dec. 1993	Jan. 1995-Dec. 1998	
Total	51	83	
Annual Mean	17	20.75	22%
LCM Guns as % of All Guns	20.9%	32.42%	55%*
B. LCM Handguns	Jan. 1991-Dec. 1993	Jan. 1995-Dec. 1998	
Total	40	71	
Annual Mean	13.33	17.75	33%
LCM Handguns as % of All Guns	16.39%	27.73%	69%*
C. LCM Rifles	Jan. 1991-Dec. 1993	Jan. 1995-Dec. 1998	
Total	11	12	
Annual Mean	3.67	3	-18%
LCM Rifles as % of All Guns	4.51%	4.69%	4%

* Chi-square p level < .01 (changes in percentages of guns equipped with LCMs were tested for statistical significance)

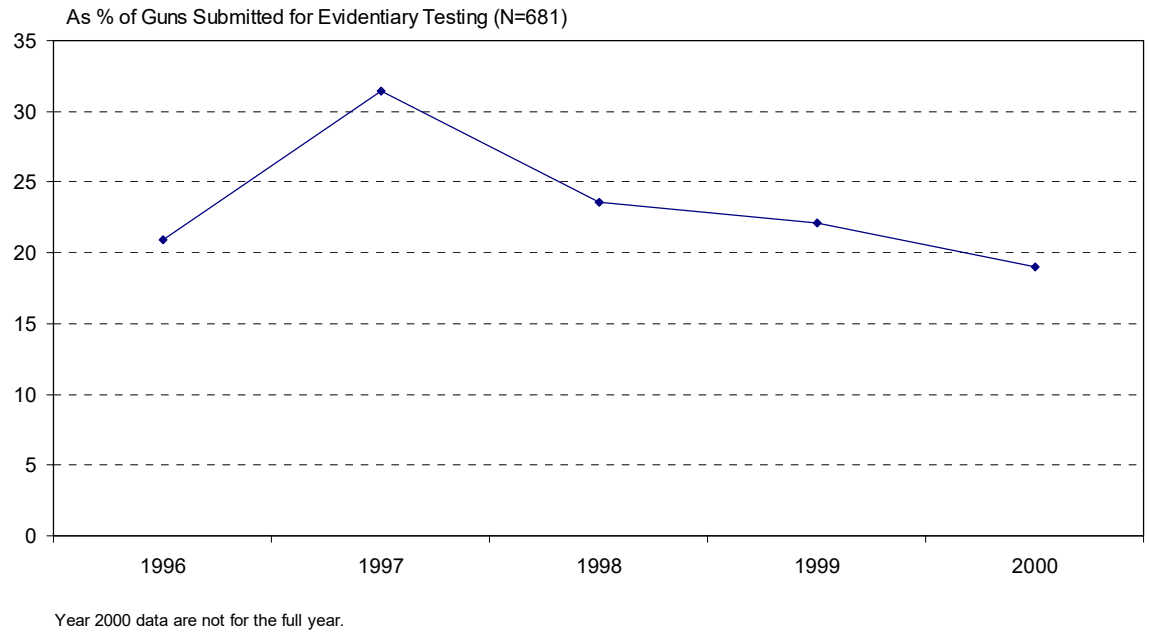
Figure 8-4. Recoveries of Guns Equipped With Large Capacity Magazines in Milwaukee County Murder Cases, 1991-1998



8.4. Louisville

The Louisville LCM data are all post-ban (1996-2000), so we cannot make pre-post comparisons. Nonetheless, the share of crime guns with LCMs in Louisville (24%) was within the range of that observed in the other cities during this period. And similar to post-ban trends in the other sites, LCM recoveries peaked in 1997 before leveling off and remaining steady through the year 2000 (Figure 8-5). LCM rifles dropped 21% as a share of crime guns between 1996 and 2000 (analyses not shown), but there were few in the database, and they never accounted for more than 6.2% of guns in any year.

Figure 8-5. Police Recoveries of Guns Equipped With Large Capacity Magazines in Louisville (Kentucky), 1996-2000



8.5. Summary

Despite a doubling of handgun LCM prices between 1993 and 1995 and a 40% increase in rifle LCM prices from 1993 to 1994, criminal use of LCMs was rising or steady through at least the latter 1990s, based on police recovery data from four jurisdictions studied in this chapter. These findings are also consistent with an earlier study finding no decline in seizures of LCM guns from juveniles in Washington, DC in the year after the ban (Koper, 2001).[88] Post-2000 data, though more limited and inconsistent, suggest that LCM use may be dropping from peak levels of the late 1990s but provide no definitive evidence of a drop below pre-ban levels.[89] These trends have been driven primarily by LCM handguns, which are used in crime roughly three times as

[88] From 1991 to 1993, 16.4% of guns recovered from juveniles in Washington, DC had LCMs (14.2% had LCMs in 1993). In 1995, this percentage increased to 17.1%. We did not present these findings in this chapter because the data were limited to guns recovered from juveniles, the post-ban data series was very short, and the gun markets supplying DC and Baltimore are likely to have much overlap (Maryland is a leading supplier of guns to DC – see ATF, 1997; 1999).

[89] We reran selected key analyses with the Baltimore, Milwaukee, and Louisville data after excluding .22 caliber guns, some of which could have been equipped with attached tubular magazines that are exempted from the LCM ban, and obtained results consistent with those reported in the text. It was possible to identify these exempted magazines in the Anchorage data. When they were removed from Anchorage's LCM count, the general pattern in use of banned LCMs was similar to that presented in the main 1995-2002 analysis: guns with banned LCMs rose, reaching a peak of 21% of crime guns in 1999-2000, before declining slightly to 19% in 2001-2002.

often as LCM rifles. Nonetheless, there has been no consistent reduction in the use of LCM rifles either.

The observed patterns are likely due to several factors: a hangover from pre-ban growth in the production and marketing of LCM guns (Cook and Ludwig, 1997, pp. 5-6; Wintemute, 1996);[90] the low cost of LCMs relative to the firearms they complement, which seems to make LCM use less sensitive to prices than is firearm use;[91] the utility that gun users, particularly handgun users, attach to LCMs; a plentiful supply of grandfathered LCMs, likely enhanced by a pre-ban surge in production (though this has not been documented) and the importation of millions of foreign LCMs since the ban;[92] thefts of LCM firearms (see Roth and Koper, 1997, Chapter 4); or some combination of these factors.[93] However, it is worth noting that our analysis did not reveal an upswing in use of LCM guns following the surge of LCM importation in 1999 (see the previous chapter). It remains to be seen whether recent imports will have a demonstrable effect on patterns of LCM use.

Finally, we must be cautious in generalizing these results to the nation because they are based on a small number of non-randomly selected jurisdictions. Nonetheless, the consistent failure to find clear evidence of a pre-post drop in LCM use across these geographically diverse locations strengthens the inference that the findings are indicative of a national pattern.

[90] To illustrate this trend, 38% of handguns acquired by gun owners during 1993 and 1994 were equipped with magazines holding 10 or more rounds, whereas only 14% of handguns acquired before 1993 were so equipped (Cook and Ludwig, 1997, pp. 5-6).

[91] Although elevated post-ban prices did not suppress use of LCMs, a more subtle point is that LCM use rose in most of these locations between 1995 and 1998, as LCM prices were falling from their peak levels of 1994-1995. Therefore, LCM use may have some sensitivity to price trends.

[92] However, we do not have the necessary data to determine if LCMs used in crime after the ban were acquired before or after the ban.

[93] In light of these considerations, it is conceivable that the ban slowed the rate of growth in LCM use, accelerated it temporarily (due to a pre-ban production boom), or had no effect. We do not have the data necessary to examine this issue rigorously. Moreover, the issue might be regarded as somewhat superfluous; the more critical point would seem to be that nearly a decade after the ban, LCM use has still not declined demonstrably below pre-ban levels.

9. THE CONSEQUENCES OF CRIMES WITH ASSAULT WEAPONS AND LARGE CAPACITY MAGAZINES

One of the primary considerations motivating passage of the ban on AWs and LCMs was a concern over the perceived dangerousness of these guns and magazines. In principal, semiautomatic weapons with LCMs enable offenders to fire high numbers of shots rapidly, thereby potentially increasing both the number of person wounded per gunfire incident (including both intended targets and innocent bystanders) and the number of gunshot victims suffering multiple wounds, both of which would increase deaths and injuries from gun violence. Ban advocates also argued that the banned AWs possessed additional features conducive to criminal applications.

The findings of the previous chapters suggest that it is premature to make definitive assessments of the ban's impact on gun violence. Although criminal use of AWs has declined since the ban, this reduction was offset through at least the late 1990s by steady or rising use of other guns equipped with LCMs. As argued previously, the LCM ban has greater potential for reducing gun deaths and injuries than does the AW ban. Guns with LCMs – of which AWs are only a subset – were used in up to 25% of gun crimes before the ban, whereas AWs were used in no more than 8% (Chapter 3). Furthermore, an LCM is arguably the most important feature of an AW. Hence, use of guns with LCMs is probably more consequential than use of guns with other military-style features, such as flash hiders, folding rifle stocks, threaded barrels for attaching a silencers, and so on.[94]

This is not to say that reducing use of AWs will have no effect on gun crime; a decline in the use of AWs does imply fewer crimes with guns having particularly large magazines (20 or more rounds) and other military-style features that could facilitate some crimes. However, it seems that any such effects would be outweighed, or at least

[94] While it is conceivable that changing features of AWs other than their magazines might prevent some gunshot victimizations, available data provide little if any empirical basis for judging the likely size of such effects. Speculatively, some of the most beneficial weapon redesigns may be the removal of folding stocks and pistol grips from rifles. It is plausible that some offenders who cannot obtain rifles with folding stocks (which make the guns more concealable) might switch to handguns, which are more concealable but generally cause less severe wounds (e.g. see DiMaio, 1985). However, such substitution patterns cannot be predicted with certainty. Police gun databases rarely have information sufficiently detailed to make assessments of changes over time in the use of weapons with specific features like folding stocks. Based on informal assessments, there was no consistent pattern in post-ban use of rifles (as a share of crime guns) in the local databases examined in the prior chapters (also see the specific comments on LCM rifles in the previous chapters).

Pistol grips enhance the ability of shooters to maintain control of a rifle during rapid, "spray and pray" firing (e.g., see Violence Policy Center, 2003). (Heat shrouds and forward handgrips on APs serve the same function.) While this feature may prove useful in military contexts (e.g., firefights among groups at 100 meters or less – see data of the U.S. Army's Operations Research Office as cited in Violence Policy Center, 2003), it is unknown whether civilian attacks with semiautomatic rifles having pistol grips claim more victims per attack than do those with other semiautomatic rifles. At any rate, most post-ban AR-type rifles still have pistol grips. Further, the ban does not count a stock thumbhole grip, which serves the same function as a pistol grip (e.g., see the illustration of LCMM rifles in Chapter 2), as an AR feature.

obscured, by the wider effects of LCM use, which themselves are likely to be small at best, as we argue below.[95]

Because offenders can substitute non-banned guns and small magazines for banned AWs and LCMs, there is not a clear rationale for expecting the ban to reduce assaults and robberies with guns.[96] But by forcing AW and LCM offenders to substitute non-AWs with small magazines, the ban might reduce the number of shots fired per gun attack, thereby reducing both victims shot per gunfire incident and gunshot victims sustaining multiple wounds. In the following sections, we consider the evidence linking high-capacity semiautomatics and AWs to gun violence and briefly examine recent trends in lethal and injurious gun violence.

9.1. The Spread of Semiautomatic Weaponry and Trends in Lethal and Injurious Gun Violence Prior to the Ban

Nationally, semiautomatic handguns grew from 28% of handgun production in 1973 to 80% in 1993 (Zawitz, 1995, p. 3). Most of this growth occurred from the late 1980s onward, during which time the gun industry also increased marketing and production of semiautomatics with LCMs (Wintemute, 1996). Likewise, semiautomatics grew as a percentage of crime guns (Koper, 1995; 1997), implying an increase in the average firing rate and ammunition capacity of guns used in crime.[97]

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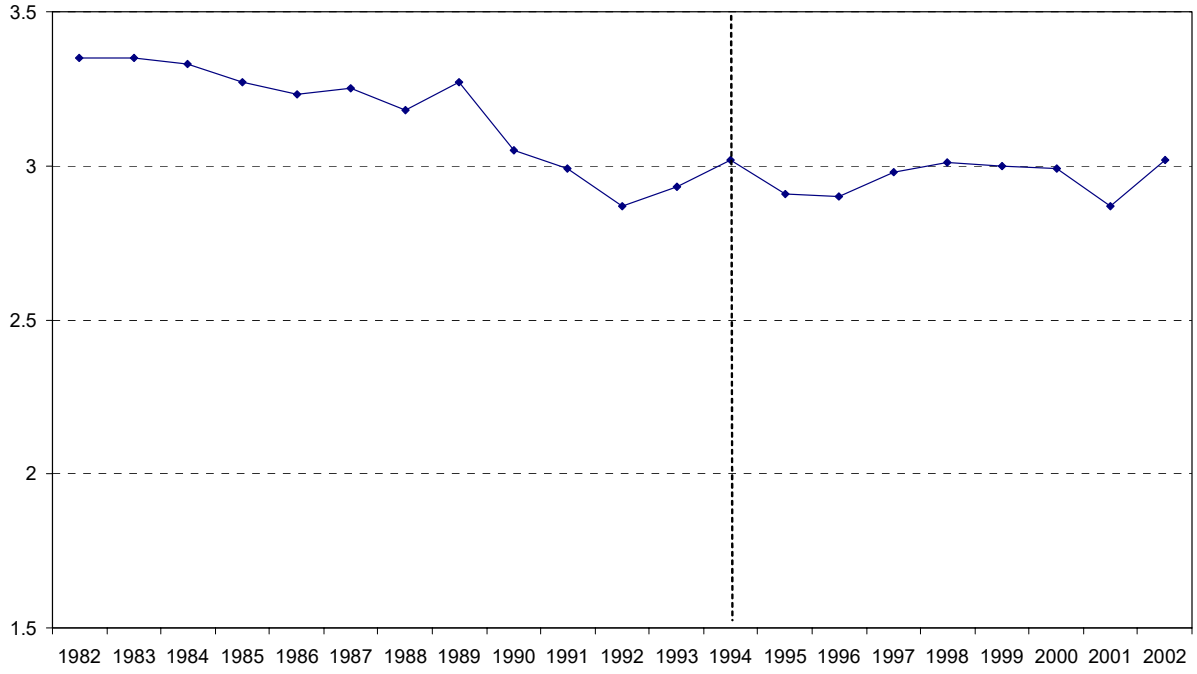
[95] On a related note, a few studies suggest that state-level AW bans have not reduced crime (Koper and Roth, 2001a; Lott, 2003). This could be construed as evidence that the federal AW ban will not reduce gunshot victimizations without reducing LCM use because the state bans tested in those studies, as written at the time, either lacked LCM bans or had LCM provisions that were less restrictive than that of the federal ban. (New Jersey's 1990 AW ban prohibited magazines holding more than 15 rounds. AP bans passed by Maryland and Hawaii prohibited magazines holding more than 20 rounds and pistol magazines holding more than 10 rounds, respectively, but these provisions did not take effect until just a few months prior to the federal ban.) However, it is hard to draw definitive conclusions from these studies for a number of reasons, perhaps the most salient of which are the following: there is little evidence on how state AW bans affect the availability and use of AWs (the impact of these laws is likely undermined to some degree by the influx of AWs from other states, a problem that was probably more pronounced prior to the federal ban when the state laws were most relevant); studies have not always examined the effects of these laws on gun homicides and shootings, the crimes that are arguably most likely to be affected by AW bans (see discussion in the main text); and the state AW bans that were passed prior to the federal ban (those in California, New Jersey, Hawaii, Connecticut, and Maryland) were in effect for only three months to five years (two years or less in most cases) before the imposition of the federal ban, after which they became largely redundant with the federal legislation and their effects more difficult to predict and estimate.

[96] One might hypothesize that the firepower provided by AWs and other semiautomatics with LCMs emboldens some offenders to engage in aggressive behaviors that prompt more shooting incidents. On the other hand, these weapons might also prevent some acts of violence by intimidating adversaries, thus discouraging attacks or resistance. We suspect that firepower does influence perceptions, considering that many police departments have upgraded their weaponry in recent years – often adopting semiautomatics with LCMs – because their officers felt outgunned by offenders. However, hypotheses about gun types and offender behavior are very speculative, and, pending additional research on such issues, it seems prudent to focus on indicators with stronger theoretical and empirical foundations.

[97] Revolvers, the most common type of non-semiautomatic handgun, typically hold only 5 or 6 rounds (and sometimes up to 9). Semiautomatic pistols, in contrast, hold ammunition in detachable magazines that, prior to the ban, typically held 5 to 17 bullets and sometimes upwards of 30 (Murtz et al., 1994).

The impact of this trend is debatable. Although the gun homicide rate rose considerably during the late 1980s and early 1990s (Bureau of Justice Statistics, 1994, p. 13), the percentage of violent gun crimes resulting in death was declining (see Figure 9-1 and the related discussion in section 9.3). Similarly, the percentage of victims killed or wounded in handgun discharge incidents declined from 27% during the 1979-1987 period to 25% for the 1987-1992 period (calculated from Rand, 1990, p. 5; 1994, p. 2) as semiautomatics were becoming more common crime weapons.[98] On the other hand, an increasing percentage of gunshot victims died from 1992 to 1995 according to hospital data (Cherry et al., 1998), a trend that could have been caused in part by a higher number of gunshot victims with multiple wounds (also see McGonigal et al., 1993). Most notably, the case fatality rate for assaultive gunshot cases involving 15 to 24-year-old males rose from 15.9% in late 1993 to 17.5% in early 1995 (p. 56).

Figure 9-1. Percentage of Violent Gun Crimes Resulting in Death (National), 1982-2002



Based on gun homicides, gun robberies, and gun assaults reported in the Uniform Crime Reports and Supplemental Homicide Reports.

[98] A related point is that there was a general upward trend in the average number of shots fired by offenders in gunfights with New York City police from the late 1980s through 1992 (calculated from Goehl, 1993, p. 51). However, the average was no higher during this time than during many years of the early 1980s and 1970s.

Some researchers have inferred links between the growing use of semiautomatics in crime and the rise of both gun homicides and bystander shootings in a number of cities during the late 1980s and early 1990s (Block and Block, 1993; McGonigal et al., 1993; Sherman et al., 1989; Webster et al., 1992). A study in Washington, DC, for example, reported increases in wounds per gunshot victim and gunshot patient mortality during the 1980s that coincided with a reported increase in the percentage of crime guns that were semiautomatics (Webster et al., 1992).

Nevertheless, changes in offender behavior, coupled with other changes in crime guns (e.g., growing use of large caliber handguns – see Caruso et al., 1999; Koper, 1995; 1997; Wintemute, 1996), may have been key factors driving such trends. Washington, DC, for example, was experiencing an exploding crack epidemic at the time of the aforementioned study, and this may have raised the percentage of gun attacks in which offenders had a clear intention to injure or kill their victims. Moreover, studies that attempted to make more explicit links between the use of semiautomatic firearms and trends in lethal gun violence via time series analysis failed to produce convincing evidence of such links (Koper, 1995; 1997). However, none of the preceding research related specific trends in the use of AWs or LCMs to trends in lethal gun violence.

9.2. Shots Fired in Gun Attacks and the Effects of Weaponry on Attack Outcomes

The evidence most directly relevant to the potential of the AW-LCM ban to reduce gun deaths and injuries comes from studies examining shots fired in gun attacks and/or the outcomes of attacks involving different types of guns. Unfortunately, such evidence is very sparse.

As a general point, the faster firing rate and larger ammunition capacities of semiautomatics, especially those equipped with LCMs, have the potential to affect the outcomes of many gun attacks because gun offenders are not particularly good shooters. Offenders wounded their victims in no more than 29% of gunfire incidents according to national, pre-ban estimates (computed from Rand, 1994, p. 2; also see estimates presented later in this chapter). Similarly, a study of handgun assaults in one city revealed a 31% hit rate per shot, based on the sum totals of all shots fired and wounds inflicted (Reedy and Koper, 2003, p. 154). Other studies have yielded hit rates per shot ranging from 8% in gunfights with police (Goehl, 1993, p. 8) to 50% in mass murders (Kleck, 1997, p. 144). Even police officers, who are presumably certified and regularly re-certified as proficient marksman and who are almost certainly better shooters than are average gun offenders, hit their targets with only 22% to 39% of their shots (Kleck, 1991, p. 163; Goehl, 1993). Therefore, the ability to deliver more shots rapidly should raise the likelihood that offenders hit their targets, not to mention innocent bystanders.[99]

[99] However, some argue that this capability is offset to some degree by the effects of recoil on shooter aim, the limited number of shots fired in most criminal attacks (see below), and the fact that criminals using non-semiautomatics or semiautomatics with small magazines usually have the time and ability to deliver multiple shots if desired (Kleck, 1991, pp. 78-79).

A few studies have compared attacks with semiautomatics, sometimes specifically those with LCMs (including AWs), to other gun assaults in terms of shots fired, persons hit, and wounds inflicted (see Tables 9-1 and 9-2). The most comprehensive of these studies examined police reports of attacks with semiautomatic pistols and revolvers in Jersey City, New Jersey from 1992 through 1996 (Reedy and Koper, 2003), finding that use of pistols resulted in more shots fired and higher numbers of gunshot victims (Table 9-1), though not more gunshot wounds per victim (Table 9-2).[100] Results implied there would have been 9.4% fewer gunshot victims overall had semiautomatics not been used in any of the attacks. Similarly, studies of gun murders in Philadelphia (see McGonigal et al., 1993 in Table 9-1) and a number of smaller cities in Pennsylvania, Ohio, and Iowa (see Richmond et al., 2003 in Table 9-2) found that attacks with semiautomatics resulted in more shots fired and gunshot wounds per victim. An exception is that the differential in shots fired between pistol and revolver cases in Philadelphia during 1990 did not exist for cases that occurred in 1985, when semiautomatics and revolvers had been fired an average of 1.6 and 1.9 times, respectively. It is not clear whether the increase in shots fired for pistol cases from 1985 to 1990 was due to changes in offender behavior, changes in the design or quality of pistols (especially an increase in the use of models with LCMs – see Wintemute, 1996), the larger sample for 1990, or other factors.

[100] But unlike other studies that have examined wounds per victim (see Table 9-2), this study relied on police reports of wounds inflicted rather than medical reports, which are likely to be more accurate.

Table 9-1. Shots Fired and Victims Hit in Gunfire Attacks By Type of Gun and Magazine

Data Source	Measure	Outcome
Gun attacks with semiautomatic pistols and revolvers, Jersey City, 1992-1996 [a]	Shots Fired	Avg. = 3.2 – 3.7 (n=165 pistol cases) * Avg. = 2.3 – 2.6 (n=71 revolver cases) *
Gun homicides with semiautomatic pistols and revolvers, Philadelphia, 1985 and 1990 [b]	Shots Fired	Avg. = 1.6 (n=21 pistol cases, 1985) Avg. = 1.9 (n=57 revolver cases, 1985) Avg. = 2.7 (n=95 pistol cases, 1990) Avg. = 2.1 (n=108 revolver cases, 1990)
Gun attacks with semiautomatic pistols and revolvers, Jersey City, 1992-1996 [a]	Victims Hit	Avg. = 1.15 (n=95 pistol cases) * Avg. = 1.0 (n=40 revolver cases) *
Mass shootings with AWs, semiautomatics having LCMs, or other guns, 6+ dead or 12+ shot, United States, 1984-1993 [c]	Victims Hit	Avg. = 29 (n=6 AW/LCM cases) Avg. = 13 (n=9 non-AW/LCM cases)
Self-reported gunfire attacks by state prisoners with AWs, other semiautomatics, and non-semiautomatic firearms, United States, 1997 or earlier [d]	% of Attacks With Victims Hit	19.5% (n=72 AW or machine gun cases) 22.3% (n=419 non-AW, semiautomatic cases) 23.3% (n=608 non-AW, non-semiautomatic cases)

a. Reedy and Koper (2003)
b. McGonigal et al. (1993)
c. Figures calculated by Koper and Roth (2001a) based on data presented by Kleck (1997, p. 144)
d. Calculated from Harlow (2001, p. 11). (Sample sizes are based on unpublished information provided by the author of the survey report.)
* Pistol/revolver differences statistically significant at $p<.05$ (only Reedy and Koper [2003] and Harlow [2001] tested for statistically significant differences). The shots fired ranges in Reedy and Koper are based on minimum and maximum estimates.

Table 9-2. Gunshot Wounds Per Victim By Type of Gun and Magazine

Data Source	Measure	Outcome
Gun attacks with semiautomatic pistols and revolvers, Jersey City, 1992-1996 [a]	Gunshot Wounds	Avg. = 1.4 (n=107 pistol victims) Avg. = 1.5 (n=40 revolver victims)
Gun homicides with semiautomatic pistols and revolvers, Iowa City (IA), Youngstown (OH), and Bethlehem (PA), 1994-1998 [b]	Gunshot Wounds	Avg. = 4.5 total (n=212 pistol victims)* Avg. = 2.9 entry Avg. = 2.0 total (n=63 revolver victims)* Avg. = 1.5 entry
Gun homicides with assault weapons (AWs), guns having large capacity magazines (LCMs), and other firearms, Milwaukee, 1992-1995 [c]	Gunshot Wounds	Avg. = 3.23 (n=30 LCM victims) ** Avg. = 3.14 (n=7 AW victims) Avg. = 2.08 (n=102 non-AW/LCM victims)**

a. Reedy and Koper (2003)
b. Richmond et al. (2003)
c. Roth and Koper (1997, Chapter 6)
* Pistol/revolver differences statistically significant at $p<.01$.
** The basic comparison between LCM victims and non-AW/LCM victims was moderately significant ($p<.10$) with a one-tailed test. Regression results (with a slightly modified sample) revealed a difference significant at $p=.05$ (two-tailed test). Note that the non-LCM group included a few cases involving non-banned LCMs (.22 caliber attached tubular devices).

Also, a national survey of state prisoners found that, contrary to expectations, offenders who reported firing on victims with AWs and other semiautomatics were no more likely to report having killed or injured victims than were other gun offenders who reported firing on victims (Table 9-1). However, the measurement of guns used and attack outcomes were arguably less precise in this study, which was based on offender self-reports, than in other studies utilizing police and medical reports.[101]

Attacks with AWs or other guns with LCMs may be particularly lethal and injurious, based on very limited evidence. In mass shooting incidents (defined as those in which at least 6 persons were killed or at least 12 were wounded) that occurred during the decade preceding the ban, offenders using AWs and other semiautomatics with LCMs (sometimes in addition to other guns) claimed an average of 29 victims in comparison to an average of 13 victims for other cases (Table 9-1). (But also see the study discussed in the preceding paragraph in regards to victims hit in AW cases.)

Further, a study of Milwaukee homicide victims from 1992 through 1995 revealed that those killed with AWs were shot 3.14 times on average, while those killed with any

[101] See the discussion of self-reports and AW use in Chapter 3.

gun having an LCM were shot 3.23 times on average (Table 9-2). In contrast, victims shot with guns having small magazines had only 2.1 wounds on average. If such a wound differential can be generalized to other gun attacks – if, that is, both fatal and non-fatal LCM gunshot victims are generally hit one or more extra times – then LCM use could have a considerable effect on the number of gunshot victims who die. To illustrate, the fatality rate among gunshot victims in Jersey City during the 1990s was 63% higher for those shot twice than for those shot once (26% to 16%) (Koper and Roth, 2001a; 2001b). Likewise, fatality rates are 61% higher for patients with multiple chest wounds than for patients with a single chest wound (49% to 30.5%), based on a Washington, DC study (Webster et al., 1992, p. 696).

Similar conclusions can also be inferred indirectly from the types of crimes involving LCM guns. To illustrate, handguns associated with gunshot victimizations in Baltimore (see the description of the Baltimore gun and magazine data in the preceding chapter) are 20% to 50% more likely to have LCMs than are handguns associated with other violent crimes, controlling for weapon caliber (Table 9-3). This difference may be due to higher numbers of shots and hits in crimes committed with LCMs, although it is also possible that offenders using LCMs are more likely to fire on victims. But controlling for gunfire, guns used in shootings are 17% to 26% more likely to have LCMs than guns used in gunfire cases resulting in no wounded victims (perhaps reflecting higher numbers of shots fired and victims hit in LCM cases), and guns linked to murders are 8% to 17% more likely to have LCMs than guns linked to non-fatal gunshot victimizations (perhaps indicating higher numbers of shots fired and wounds per victim in LCM cases).[102] These differences are not all statistically significant, but the pattern is consistent. And as discussed in Chapter 3, AWs account for a larger share of guns used in mass murders and murders of police, crimes for which weapons with greater firepower would seem particularly useful.

[102] Cases with and without gunfire and gunshot victims were approximated based on offense codes contained in the gun seizure data (some gunfire cases not resulting in wounded victims may not have been identified as such, and it is possible that some homicides were not committed with the guns recovered during the investigations). In order to control for caliber effects, we focused on 9mm and .38 caliber handguns. Over 80% of the LCM handguns linked to violent crimes were 9mm handguns. Since all (or virtually all) 9mm handguns are semiautomatics, we also selected .38 caliber guns, which are close to 9mm in size and consist almost entirely of revolvers and derringers.

The disproportionate involvement of LCM handguns in injury and death cases is greatest in the comparisons including both 9mm and .38 caliber handguns. This may reflect a greater differential in average ammunition capacity between LCM handguns and revolvers/derringers than between LCM handguns and other semiautomatics. The differential in fatal and non-fatal gunshot victims may also be due to caliber effects; 9mm is generally a more powerful caliber than .38 based on measures like kinetic energy or relative stopping power (e.g., see DiMaio, 1985, p. 140; Warner 1995, p. 223; Wintemute, 1996, p. 1751).

Table 9-3. Probabilities That Handguns Associated With Murders, Non-Fatal Shootings, and Other Violent Crimes Were Equipped With Large Capacity Magazines in Baltimore, 1993-2000

Handgun Sample	% With LCM	% Difference (#2 Relative to #1)
A. Handguns Used in Violent Crimes With and Without Gunshot Injury		
1) 9mm and .38: violence, no gunshot victims	23.21%	
2) 9mm and .38: violence with gunshot victims	34.87%	50%*
1) 9mm: violence, no gunshot victims	52.92%	
2) 9mm: violence with gunshot victims	63.24%	20%*
B. Handguns Used in Gunfire Cases With and Without Gunshot Injury		
1) 9mm and .38: gunfire, no gunshot victims	27.66%	
2) 9mm and .38: gunfire with gunshot victims	34.87%	26%
1) 9mm: gunfire, no gunshot victims	54.17%	
2) 9mm: gunfire with gunshot victims	63.24%	17%
C. Handguns Used in Fatal Versus Non-Fatal Gunshot Victimizations		
1) 9mm and .38: non-fatal gunshot victims	32.58%	
2) 9mm and .38: homicides	38.18%	17%
1) 9mm: non-fatal gunshot victims	61.14%	
2) 9mm: homicides	66.04%	8%

* Statistically significant difference at p<.01 (chi-square).

The findings of the preceding studies are subject to numerous caveats. There were few if any attempts to control for characteristics of the actors or situations that might have influenced weapon choices and/or attack outcomes.[103] Weapons data were typically missing for substantial percentages of cases. Further, many of the comparisons in the tables were not tested for statistical significance (see the notes to Tables 9-1 and 9-2).[104]

Tentatively, nonetheless, the evidence suggests more often than not that attacks with semiautomatics, particularly those equipped with LCMs, result in more shots fired, leading to both more injuries and injuries of greater severity. Perhaps the faster firing rate and larger ammunition capacities afforded by these weapons prompt some offenders to fire more frequently (i.e., encouraging what some police and military persons refer to as a "spray and pray" mentality). But this still begs the question of whether a 10-round limit on magazine capacity will affect the outcomes of enough gun attacks to measurably reduce gun injuries and deaths.

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[103] In terms of offender characteristics, recall from Chapter 3 that AP buyers are more likely than other gun buyers to have criminal histories and commit subsequent crimes. This does not seem to apply, however, to the broader class of semiautomatic users: handgun buyers with and without criminal histories tend to buy pistols in virtually the same proportions (Wintemute et al., 1998b), and youthful gun offenders using pistols and revolvers have very comparable criminal histories (Sheley and Wright, 1993b, p. 381). Further, semiautomatic users, including many of those using AWs, show no greater propensity to shoot at victims than do other gun offenders (Harlow, 2001, p. 11; Reedy and Koper, 2003). Other potential confounders to the comparisons in Tables 9-1 and 9-2 might include shooter age and skill, the nature of the circumstances (e.g., whether the shooting was an execution-style shooting), the health of the victim(s), the type of location (e.g., indoor or outdoor location), the distance between the shooter and intended victim(s), the presence of multiple persons who could have been shot intentionally or accidentally (as bystanders), and (in the mass shooting incidents) the use of multiple firearms.

[104] Tables 9-1 and 9-2 present the strongest evidence from the available studies. However, there are additional findings from these studies and others that, while weaker, are relevant. Based on gun model information available for a subset of cases in the Jersey City study, there were 12 gunfire cases involving guns manufactured with LCMs before the ban (7 of which resulted in wounded victims) and 94 gunfire cases involving revolvers or semiautomatic models without LCMs. Comparisons of these cases produced results similar to those of the main analysis: shot fired estimates ranged from 2.83 to 3.25 for the LCM cases and 2.22 to 2.6 for the non-LCM cases; 1.14 victims were wounded on average in the LCM gunshot cases and 1.06 in the non-LCM gunshot cases; and LCM gunshot victims had 1.14 wound on average, which, contrary to expectations, was less than the 1.47 average for other gunshot victims.

The compilation of mass shooting incidents cited in Table 9-1 had tentative shots fired estimates for 3 of the AW-LCM cases and 4 of the other cases. The AW-LCM cases averaged 93 shots per incident, a figure two and a half times greater than the 36.5 shot average for the other cases.

Finally, another study of firearm mass murders found that the average number of victims killed (tallies did not include others wounded) was 6 in AW cases and 4.5 in other cases (Roth and Koper, 1997, Appendix A). Only 2 of the 52 cases studied clearly involved AWs (or very similar guns). However, the make and model of the firearm were available for only eight cases, so additional incidents may have involved LCMs; in fact, at least 35% of the cases involved unidentified semiautomatics. (For those cases in which at least the gun type and firing action were known, semiautomatics outnumbered non-semiautomatics by 6 to 1, perhaps suggesting that semiautomatics are used disproportionately in mass murders.)

9.2.1. Will a 10-Round Magazine Limit Reduce Gunshot Victimizations?

Specific data on shots fired in gun attacks are quite fragmentary and often inferred indirectly, but they suggest that relatively few attacks involve more than 10 shots fired.[105] Based on national data compiled by the FBI, for example, there were only about 19 gun murder incidents a year involving four or more victims from 1976 through 1995 (for a total of 375) (Fox and Levin, 1998, p. 435) and only about one a year involving six or more victims from 1976 through 1992 (for a total of 17) (Kleck, 1997, p. 126). Similarly, gun murder victims are shot two to three times on average according to a number of sources (see Table 9-2 and Koper and Roth, 2001a), and a study at a Washington, DC trauma center reported that only 8% of all gunshot victims treated from 1988 through 1990 had five or more wounds (Webster et al., 1992, p. 696).

However, counts of victims hit or wounds inflicted provide only a lower bound estimate of the number of shots fired in an attack, which could be considerably higher in light of the low hit rates in gunfire incidents (see above).[106] The few available studies on shots fired show that assailants fire less than four shots on average (see sources in Table 9-1 and Goehl, 1993), a number well within the 10-round magazine limit imposed by the AW-LCM ban, but these studies have not usually presented the full distribution of shots fired for all cases, so it is usually unclear how many cases, if any, involved more than 10 shots.

An exception is the aforementioned study of handgun murders and assaults in Jersey City (Reedy and Koper, 2003). Focusing on cases for which at least the type of handgun (semiautomatic, revolver, derringer) could be determined, 2.5% of the gunfire cases involved more than 10 shots.[107] These incidents – all of which involved pistols – had a 100% injury rate and accounted for 4.7% of all gunshot victims in the sample (see Figure 9-2). Offenders fired a total of 83 shots in these cases, wounding 7 victims, only 1 of whom was wounded more than once. Overall, therefore, attackers fired over 8 shots

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[105] Although the focus of the discussion is on attacks with more than 10 shots fired, a gun user with a post-ban 10-round magazine can attain a firing capacity of 11 shots with many semiautomatics by loading one bullet into the chamber before loading the magazine.

[106] As a dramatic example, consider the heavily publicized case of Amadou Diallo, who was shot to death by four New York City police officers just a few years ago. The officers in this case fired upon Diallo 41 times but hit him with only 19 shots (a 46% hit rate), despite his being confined in a vestibule. Two of the officers reportedly fired until they had emptied their 16-round magazines, a reaction that may not be uncommon in such high-stress situations. In official statistics, this case will appear as having only one victim.

[107] The shots fired estimates were based on reported gunshot injuries, physical evidence (for example, shell casings found at the scene), and the accounts of witnesses and actors. The 2.5% figure is based on minimum estimates of shots fired. Using maximum estimates, 3% of the gunfire incidents involved more than 10 shots (Reedy and Koper, 2003, p. 154).

A caveat to these figures is that the federal LCM ban was in effect for much of the study period (which spanned January 1992 to November 1996), and a New Jersey ban on magazines with more than 15 rounds predated the study period. It is thus conceivable that these laws reduced attacks with LCM guns and attacks with more than 10 shots fired, though it seems unlikely that the federal ban had any such effect (see the analyses of LCM use presented in the previous chapter). Approximately 1% of the gunfire incidents involved more than 15 shots.

for every wound inflicted, suggesting that perhaps fewer persons would have been wounded had the offenders not been able to fire as often.[108]

Figure 9-2. Attacks With More Than 10 Shots Fired

Jersey City Handgun Attacks, 1992-1996

- **2.5% - 3% of gunfire incidents involved 11+ shots**
  - **3.6% - 4.2% of semiauto pistol attacks**
- **100% injury rate**
- **Produced 4.7% of all gunshot wound victims**
- **8.3 shots per gunshot wound**

Based on data reported by Reedy and Koper (2003). Injury statistics based on the 2.5% of cases involving 11+ shots by minimum estimate.

Caution is warranted in generalizing from these results because they are based on a very small number of incidents (6) from one sample in one city. Further, it is not known if the offenders in these cases had LCMs (gun model and magazine information was very limited); they may have emptied small magazines, reloaded, and continued firing. But subject to these caveats, the findings suggest that the ability to deliver more than 10 shots without reloading may be instrumental in a small but non-trivial percentage of gunshot victimizations.

On the other hand, the Jersey City study also implies that eliminating AWs and LCMs might only reduce gunshot victimizations by up to 5%. And even this estimate is probably overly optimistic because the LCM ban cannot be expected to prevent all incidents with more than 10 shots. Consequently, any effects from the ban (should it be extended) are likely to be smaller and perhaps quite difficult to detect with standard statistical methods (see Koper and Roth, 2001a), especially in the near future, if recent patterns of LCM use continue.

9.3. Post-Ban Trends in Lethal and Injurious Gun Violence

Having established some basis for believing the AW-LCM ban could have at least a small effect on lethal and injurious gun violence, is there any evidence of such an effect to date? Gun homicides plummeted from approximately 16,300 in 1994 to 10,100 in 1999, a reduction of about 38% (see the Federal Bureau of Investigation's *Uniform Crime*

[108] These figures are based on a supplemental analysis not contained in the published study. We thank Darin Reedy for this analysis.

Reports). Likewise, non-fatal, assaultive gunshot injuries treated in hospitals nationwide declined one-third, from about 68,400 to under 46,400, between 1994 and 1998 (Gotsch et al., 2001, pp. 23-24). Experts believe numerous factors contributed to the recent drop in these and other crimes, including changing drug markets, a strong economy, better policing, and higher incarceration rates, among others (Blumstein and Wallman, 2000). Attributing the decline in gun murders and shootings to the AW-LCM ban is problematic, however, considering that crimes with LCMs appear to have been steady or rising since the ban. For this reason, we do not undertake a rigorous investigation of the ban's effects on gun violence.[109]

But a more casual assessment shows that gun crimes since the ban have been no less likely to cause death or injury than those before the ban, contrary to what we might expect if crimes with AWs and LCMs had both declined. For instance, the percentage of violent gun crimes resulting in death has been very stable since 1990 according to national statistics on crimes reported to police (see Figure 9-1 in section 9.1).[110] In fact, the percentage of gun crimes resulting in death during 2001 and 2002 (2.94%) was slightly higher than that during 1992 and 1993 (2.9%).

Similarly, neither medical nor criminological data sources have shown any post-ban reduction in the percentage of crime-related gunshot victims who die. If anything, this percentage has been higher since the ban, a pattern that could be linked in part to more multiple wound victimizations stemming from elevated levels of LCM use. According to medical examiners' reports and hospitalization estimates, about 20% of gunshot victims died nationwide in 1993 (Gotsch et al., 2001). This figure rose to 23% in 1996, before declining to 21% in 1998 (Figure 9-3).[111] Estimates derived from the Uniform Crime Reports and the Bureau of Justice Statistics' annual National Crime Victimization Survey follow a similar pattern from 1992 to 1999 (although the ratio of fatal to non-fatal cases is much higher in these data than that in the medical data) and also show a considerable increase in the percentage of gunshot victims who died in 2000 and 2001 (Figure 9-3).[112] Of course, changes in offender behavior or other changes in crime

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[109] In our prior study (Koper and Roth 2001a; Roth and Koper, 1997, Chapter 6), we estimated that gun murders were about 7% lower than expected in 1995 (the first year after the ban), adjusting for pre-existing trends. However, the very limited post-ban data available for that study precluded a definitive judgment as to whether this drop was statistically meaningful (see especially Koper and Roth, 2001a). Furthermore, that analysis was based on the assumption that crimes with both AWs and LCMs had dropped in the short-term aftermath of the ban, an assumption called into question by the findings of this study. It is now more difficult to credit the ban with any of the drop in gun murders in 1995 or anytime since. We did not update the gun murder analysis because interpreting the results would be unavoidably ambiguous. Such an investigation will be more productive after demonstrating that the ban has reduced crimes with both AWs and LCMs.

[110] The decline in this figure during the 1980s was likely due in part to changes in police reporting of aggravated assaults in recent decades (Blumstein, 2000). The ratio of gun murders to gun robberies rose during the 1980s, then declined and remained relatively flat during the 1990s.

[111] Combining homicide data from 1999 with non-fatal gunshot estimates for 2000 suggests that about 20% of gunshot victimizations resulted in death during 1999 and 2000 (Simon et al., 2002).

[112] The SHR/NCVS estimates should be interpreted cautiously because the NCVS appears to undercount non-fatal gunshot wound cases by as much as two-thirds relative to police data, most likely because it fails to represent adequately the types of people most likely to be victims of serious crime (i.e., young urban males who engage in deviant lifestyles) (Cook, 1985). Indeed, the rate of death among gunshot victims

weaponry (such as an increase in shootings with large caliber handguns) may have influenced these trends. Yet is worth noting that multiple wound shootings were elevated over pre-ban levels during 1995 and 1996 in four of five localities examined during our first AW study, though most of the differences were not statistically significant (Table 9-4, panels B through E).

Another potential indicator of ban effects is the percentage of gunfire incidents resulting in fatal or non-fatal gunshot victimizations. If attacks with AWs and LCMs result in more shots fired and victims hit than attacks with other guns and magazines, we might expect a decline in crimes with AWs and LCMs to reduce the share of gunfire incidents resulting in victims wounded or killed. Measured nationally with UCR and NCVS data, this indicator was relatively stable at around 30% from 1992 to 1997, before rising to about 40% from 1998 through 2000 (Figure 9-4).[113] Along similar lines, multiple victim gun homicides remained at relatively high levels through at least 1998, based on the national average of victims killed per gun murder incident (Table 9-4, panel A).[114]

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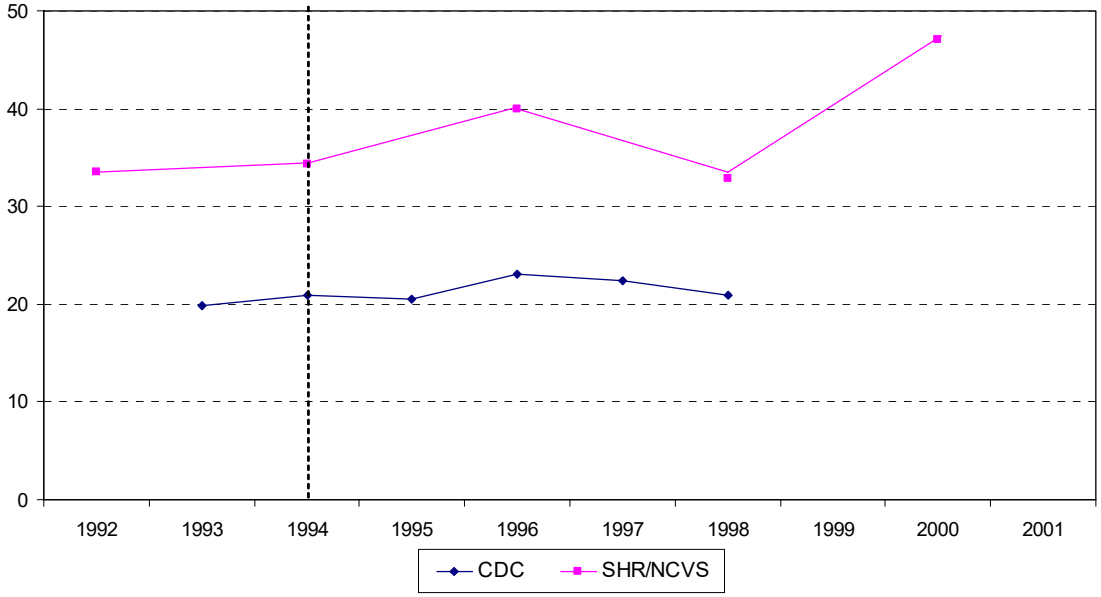
appears much higher in the SHR/NCVS series than in data compiled from medical examiners and hospitals (see the CDC series in Figure 9-3). But if these biases are relatively consistent over time, the data may still provide useful insights into trends over time.

[113] The NCVS estimates are based on a compilation of 1992-2002 data recently produced by the Inter-University Consortium for Political and Social Research (ICPSR study 3691). In 2002, only 9% of non-fatal gunfire incidents resulted in gunshot victimizations. This implies a hit rate for 2002 that was below pre-ban levels, even after incorporating gun homicide cases into the estimate. However, the 2002 NCVS estimate deviates quite substantially from earlier years, for which the average hit rate in non-fatal gunfire incidents was 24% (and the estimate for 2001 was 20%). Therefore, we did not include the 2002 data in our analysis. We used two-year averages in Figures 9-3 and 9-4 because the annual NCVS estimates are based on very small samples of gunfire incidents. The 2002 sample was especially small, so it seems prudent to wait for more data to become available before drawing conclusions about hit rates since 2001.

[114] We thank David Huffer for this analysis.

Figure 9-3. Percentage of Gunshot Victimizations Resulting in Death (National), 1992-2001



SHR/NCVS series based on two-year averages from the Supplemental Homicide Reports and National Crime Victimization Survey. CDC series based on homicide and hospitalization data from the Centers for Disease Control (reported by Gotsch et al. 2001).

Table 9-4. Short-Term, Post-Ban Changes in the Lethality and Injuriousness of Gun Violence: National and Local Indicators, 1994-1998 [a]

Measure and Location	Pre-Ban Period	Post-Ban Period	Change
A. Victims Per Gun Homicide Incident (National)	Jan. 1986-Sept. 1994 1.05 (N=106,668)	Oct. 1994-Dec. 1998 1.06 (N=47,511)	1%**
B. Wounds per Gun Homicide Victim: Milwaukee County	Jan. 1992-Aug. 1994 2.28 (N=282)	Sept. 1994-Dec. 1995 2.52 (N=136)	11%
C. Wounds Per Gun Homicide Victim: Seattle (King County)	Jan. 1992-Aug. 1994 2.08 (N=184)	Sept. 1994-Jun. 1996 2.46 (N=91)	18%
D. Wounds Per Gunshot Victim: Jersey City (NJ)	Jan. 1992-Aug. 94 1.42 (N=125)	Sept. 1994-Jun. 1996 1.39 (N=137)	-2%
E. % of Gun Homicide Victims With Multiple Wounds: San Diego County	Jan. 1992-Aug. 1994 41% (N=445)	Sept. 1994-Jun. 1996 43% (N=223)	5%
F. % of Non-Fatal Gunshot Victims With Multiple Wounds: Boston	Jan. 1992-Aug. 1994 18% (N=584)	Sept. 1994-Dec. 1995 24% (N=244)	33%*

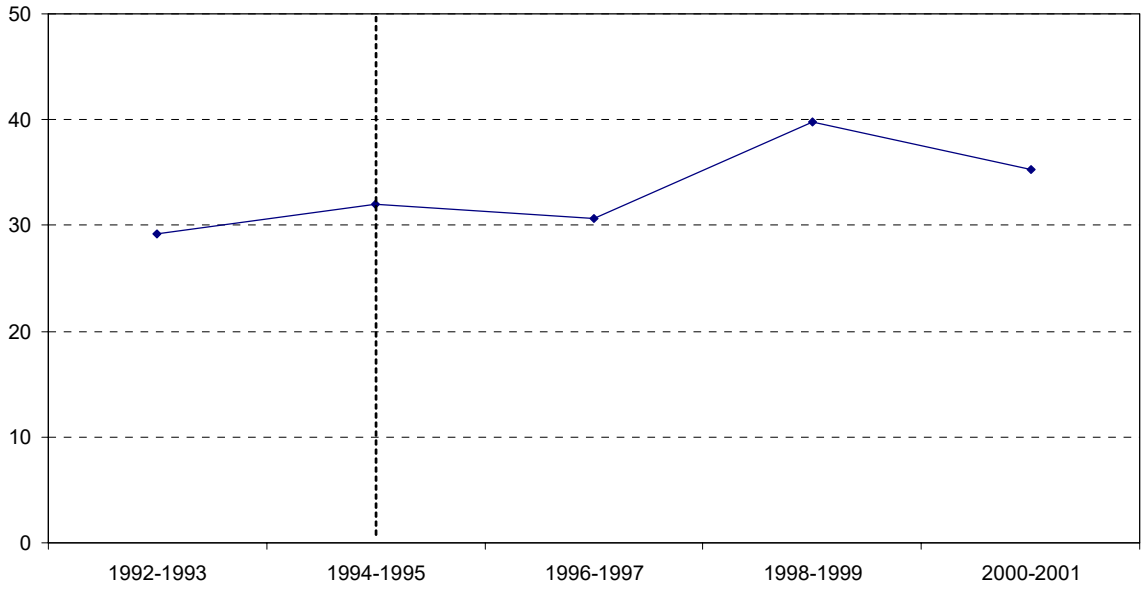
a. National victims per incident figures based on unpublished update of analysis reported in Roth and Koper (1997, Chapter 5). Gunshot wound data are taken from Roth and Koper (1997, Chapter 6) and Koper and Roth (2001a). Wound data are based on medical examiners' reports (Milwaukee, Seattle, San Diego), hospitalization data (Boston), and police reports (Jersey City).

* Chi-square p level < .1.

** T-test p level < .01.

If anything, therefore, gun attacks appear to have been more lethal and injurious since the ban. Perhaps elevated LCM use has contributed to this pattern. But if this is true, then the reverse would also be true – a reduction in crimes with LCMs, should the ban be extended, would reduce injuries and deaths from gun violence.

Figure 9-4. Percentage of Gunfire Cases Resulting in Gunshot Victimizations (National), 1992-2001



Based on two-year averages from the Supplemental Homicide Reports and National Crime Victimization Survey.

9.4. Summary

Although the ban has been successful in reducing crimes with AWs, any benefits from this reduction are likely to have been outweighed by steady or rising use of non-banned semiautomatics with LCMs, which are used in crime much more frequently than AWs. Therefore, we cannot clearly credit the ban with any of the nation's recent drop in gun violence. And, indeed, there has been no discernible reduction in the lethality and injuriousness of gun violence, based on indicators like the percentage of gun crimes resulting in death or the share of gunfire incidents resulting in injury, as we might have expected had the ban reduced crimes with both AWs and LCMs.

However, the grandfathering provision of the AW-LCM ban guaranteed that the effects of this law would occur only gradually over time. Those effects are still unfolding and may not be fully felt for several years into the future, particularly if foreign, pre-ban LCMs continue to be imported into the U.S. in large numbers. It is thus premature to make definitive assessments of the ban's impact on gun violence.

Having said this, the ban's impact on gun violence is likely to be small at best, and perhaps too small for reliable measurement. AWs were used in no more than 8% of gun crimes even before the ban. Guns with LCMs are used in up to a quarter of gun crimes, but it is not clear how often the outcomes of gun attacks depend on the ability to fire more than 10 shots (the current limit on magazine capacity) without reloading.

Nonetheless, reducing crimes with AWs and especially LCMs could have non-trivial effects on gunshot victimizations. As a general matter, hit rates tend to be low in gunfire incidents, so having more shots to fire rapidly can increase the likelihood that offenders hit their targets, and perhaps bystanders as well. While not entirely consistent, the few available studies contrasting attacks with different types of guns and magazines generally suggest that attacks with semiautomatics – including AWs and other semiautomatics with LCMs – result in more shots fired, persons wounded, and wounds per victim than do other gun attacks. Further, a study of handgun attacks in one city found that about 3% of gunfire incidents involved more than 10 shots fired, and those cases accounted for nearly 5% of gunshot victims. However, the evidence on these matters is too limited (both in volume and quality) to make firm projections of the ban's impact, should it be reauthorized.

10. LOOKING TO THE FUTURE: RESEARCH RECOMMENDATIONS AND SPECULATION ABOUT THE CONSEQUENCES OF REAUTHORIZING, MODIFYING, OR LIFTING THE ASSAULT WEAPONS BAN

In this chapter, we discuss future lines of inquiry that would be informative whether or not the AW-LCM ban is renewed in September 2004. We then offer some brief thoughts about the possible consequences of reauthorizing the ban, modifying it, or allowing it to expire.

10.1. Research Recommendations and Data Requirements

10.1.1. An Agenda for Assault Weapons Research and Recommendations for Data Collection by Law Enforcement

The effects of the AW-LCM ban have yet to be fully realized; therefore, we recommend continued study of trends in the availability and criminal use of AWs and LCMs. Even if the ban is lifted, longer-term study of crimes with AWs and LCMs will inform future assessment of the consequences of these policy shifts and improve understanding of the responses of gun markets to gun legislation more generally.[115]

Developing better data on crimes with LCMs is especially important. To this end, we urge police departments and their affiliated crime labs to record information about magazines recovered with crime guns. Further, we recommend that ATF integrate ammunition magazine data into its national gun tracing system and encourage reporting of magazine data by police departments that trace firearms.

As better data on LCM use become available, more research is warranted on the impacts of AW and LCM trends (which may go up or down depending on the ban's fate) on gun murders and shootings, as well as levels of death and injury per gun crime. Indicators of the latter, such as victims per gunfire incident and wounds per gunshot victim, are useful complementary outcome measures because they reflect the mechanisms through which use of AWs and LCMs is hypothesized to affect gun deaths and injuries.[116] Other potentially promising lines of inquiry might relate AW and LCM use to mass murders and murders of police, crimes that are very rare but appear more likely to involve AWs (and perhaps LCMs) and to disproportionately affect public perceptions.[117]

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[115] Establishing time series data on primary and secondary market prices and production or importation of various guns and magazines of policy interest could provide benefits for policy researchers. Like similar statistical series maintained for illegal drugs, such price and production series would be valuable instruments for monitoring effects of policy changes and other influences on markets for various weapons.

[116] However, more research is needed on the full range of factors that cause variation in these indicators over time and between places.

[117] Studying these crimes poses a number of challenges, including modeling of rare events, establishing the reliability and validity of methods for measuring the frequency and characteristics of mass murders (such as through media searchers; see Duwe, 2000, Roth and Koper, 1997, Appendix A), and controlling for factors like the use of bullet-proof vests by police.

Finally, statistical studies relating AW and LCM use to trends in gun violence should include statistical power analysis to ensure that estimated models have sufficient ability to detect small effects, an issue that has been problematic in some of our prior time series research on the ban (Koper and Roth, 2001a) and is applicable more generally to the study of modest, incremental policy changes.

Research on aggregate trends should be complemented by more incident-based studies that contrast the dynamics and outcomes of attacks with different types of guns and magazines, while controlling for relevant characteristics of the actors and situations. Such studies would refine predictions of the change in gun deaths and injuries that would follow reductions in attacks with AWs and LCMs. For instance, how many homicides and injuries involving AWs and LCMs could be prevented if offenders were forced to substitute other guns and magazines? In what percentage of gun attacks does the ability to fire more than ten rounds without reloading affect the number of wounded victims or determine the difference between a fatal and non-fatal attack? Do other AW features (such as flash hiders and pistol grips on rifles) have demonstrable effects on the outcomes of gun attacks? Studies of gun attacks could draw upon police incident reports, forensic examinations of recovered guns and magazines, and medical and law enforcement data on wounded victims.

10.1.2. Studying the Implementation and Market Impacts of Gun Control

More broadly, this study reiterates the importance of examining the implementation of gun policies and the workings of gun markets, considerations that have been largely absent from prior research on gun control. Typical methods of evaluating gun policies involve statistical comparisons of total or gun crime rates between places and/or time periods with and without different gun control provisions. Without complimentary implementation and market measures, such studies have a "black box" quality and may lead to misleading conclusions. For example, a time series study of gun murder rates before and after the AW-LCM ban might find that the ban has not reduced gun murders. Yet the interpretation of such a finding would be ambiguous, absent market or implementation measures. Reducing attacks with AWs and LCMs may in fact have no more than a trivial impact on gun deaths and injuries, but any such impact cannot be realized or adequately assessed until the availability and use of the banned guns and magazines decline appreciably. Additionally, it may take many years for the effects of modest, incremental policy changes to be fully felt, a reality that both researchers and policy makers should heed. Similar implementation concerns apply to the evaluation of various gun control policies, ranging from gun bans to enhanced sentences for gun offenders.

Our studies of the AW ban have shown that the reaction of manufacturers, dealers, and consumers to gun control policies can have substantial effects on demand and supply for affected weapons both before and after a law's implementation. It is important to study these factors because they affect the timing and form of a law's impact

on the availability of weapons to criminals and, by extension, the law's impact on gun violence.

10.2. Potential Consequences of Reauthorizing, Modifying, or Lifting the Assault Weapons Ban

10.2.1. Potential Consequences of Reauthorizing the Ban As Is

Should it be renewed, the ban might reduce gunshot victimizations. This effect is likely to be small at best and possibly too small for reliable measurement. A 5% reduction in gunshot victimizations is perhaps a reasonable upper bound estimate of the ban's potential impact (based on the only available estimate of gunshot victimizations resulting from attacks in which more than 10 shots were fired), but the actual impact is likely to be smaller and may not be fully realized for many years into the future, particularly if pre-ban LCMs continue to be imported into the U.S. from abroad. Just as the restrictions imposed by the ban are modest – they are essentially limits on weapon accessories like LCMs, flash hiders, threaded barrels, and the like – so too are the potential benefits.[118] In time, the ban may be seen as an effective prevention measure that stopped further spread of weaponry considered to be particularly dangerous (in a manner similar to federal restrictions on fully automatic weapons). But that conclusion will be contingent on further research validating the dangers of AWs and LCMs.

10.2.2. Potential Consequences of Modifying the Ban

We have not examined the specifics of legislative proposals to modify the AW ban. However, we offer a few general comments about the possible consequences of such efforts, particularly as they relate to expanding the range of the ban as some have advocated (Halstead, 2003, pp. 11-12).

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[118] But note that although the ban's impact on gunshot victimizations would be small in percentage terms and unlikely to have much effect on the public's fear of crime, it could conceivably prevent hundreds of gunshot victimizations annually and produce notable cost savings in medical care alone. To help place this in perspective, there were about 10,200 gun homicides and 48,600 non-fatal, assault-related shootings in 2000 (see the FBI's *Uniform Crime Reports* for the gun homicide estimate and Simon et al. [2002] for the estimate of non-fatal shootings). Reducing these crimes by 1% would have thus prevented 588 gunshot victimizations in 2000 (we assume the ban did not actually produce such benefits because the reduction in AW use as of 2000 was outweighed by steady or rising levels of LCM use). This may seem insubstantial compared to the 342,000 murders, assaults, and robberies committed with guns in 2000 (see the *Uniform Crime Reports*). Yet, gunshot victimizations are particularly costly crimes. Setting aside the less tangible costs of lost lives and human suffering, the lifetime medical costs of assault-related gunshot injuries (fatal and non-fatal) were estimated to be about $18,600 per injury in 1994 (Cook et al., 1999). Therefore, the lifetime costs of 588 gun homicides and shootings would be nearly $11 million in 1994 dollars (the net medical costs could be lower for reasons discussed by Cook and Ludwig [2000] but, on the other hand, this estimate does not consider other governmental and private costs that Cook and Ludwig attribute to gun violence). This implies that small reductions in gunshot victimizations sustained over many years could produce considerable long-term savings for society. We do not wish to push this point too far, however, considering the uncertainty regarding the ban's potential impact.

Gun markets react strongly merely to debates over gun legislation. Indeed, debate over the AW ban's original passage triggered spikes upwards of 50% in gun distributors' advertised AW prices (Roth and Koper, 1997, Chapter 4). In turn, this prompted a surge in AW production in 1994 (Chapter 5). Therefore, it seems likely that discussion of broadening the AW ban to additional firearms would raise prices and production of the weapons under discussion. (Such market reactions may already be underway in response to existing proposals to expand the ban, but we have not investigated this issue.) Heightened production levels could saturate the market for the weapons in question, depressing prices and delaying desired reductions in crimes with the weapons, as appears to have happened with banned ARs.

Mandating further design changes in the outward features of semiautomatic weapons (e.g., banning weapons having any military-style features) may not produce benefits beyond those of the current ban. As noted throughout this report, the most important feature of military-style weapons may be their ability to accept LCMs, and this feature has been addressed by the LCM ban and the LCMM rifle ban. Whether changing other features of military-style firearms will produce measurable benefits is unknown.

Finally, curbing importation of pre-ban LCMs should help reduce crimes with LCMs and possibly gunshot victimizations. Crimes with LCMs may not decline substantially for quite some time if millions of LCMs continue to be imported into the U.S.

10.2.3. Potential Consequences of Lifting the Ban

If the ban is lifted, it is likely that gun and magazine manufacturers will reintroduce AW models and LCMs, perhaps in substantial numbers.[119] In addition, AWs grandfathered under the 1994 law may lose value and novelty, prompting some of their lawful owners to sell them in secondary markets, where they may reach criminal users. Any resulting increase in crimes with AWs and LCMs might increase gunshot victimizations, though this effect could be difficult to discern statistically.

It is also possible, and perhaps probable, that new AWs and LCMs will eventually be used to commit mass murder. Mass murders garner much media attention, particularly when they involve AWs (Duwe, 2000). The notoriety likely to accompany mass murders if committed with AWs and LCMs, especially after these guns and magazines have been deregulated, could have a considerable negative impact on public perceptions, an effect that would almost certainly be intensified if such crimes were committed by terrorists operating in the U.S.

[119] Note, however, that foreign semiautomatic rifles with military features, including the LCMM rifles and several rifles prohibited by the 1994 ban, would still be restricted by executive orders passed in 1989 and 1998. Those orders stem from the sporting purposes test of the Gun Control Act of 1968.

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EXHIBIT "8"

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Large-Capacity Magazines and the Casualty Counts in Mass Shootings: The Plausibility of Linkages

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Abstract

Do bans on large-capacity magazines (LCMs) for semiautomatic firearms have significant potential for reducing the number of deaths and injuries in mass shootings? The most common rationale for an effect of LCM use is that they allow mass killers to fire many rounds without reloading. LCMs are known to have been used in less than one third of 1% of mass shootings. News accounts of 23 shootings in which more than six people were killed or wounded and LCMs were known to have been used, occurring in the United States in 1994–2013, were examined. There was only one incident in which the shooter may have been stopped by bystander intervention when he tried to reload. In all of these 23 incidents, the shooter possessed either multiple guns or multiple magazines, meaning that the shooter, even if denied LCMs, could have continued firing without significant interruption by either switching loaded guns or changing smaller loaded magazines with only a 2- to 4-seconds delay for each magazine change. Finally, the data indicate that mass shooters maintain such slow rates of fire that the time needed to reload would not increase the time between shots and thus the time available for prospective victims to escape.

Keywords

mass shootings, gun control, large-capacity magazines

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Introduction—Mass Shootings and Large-Capacity Magazines (LCMs)

There have been at least 23 shootings in which more than six victims were shot and one or more LCMs were known to have been used in the United States in the period 1994–2013. One of the most common political responses to mass shootings has been to propose new gun control measures, commonly focusing on "assault weapons" and LCMs. LCMs are detachable ammunition magazines used in semiautomatic firearms that are capable of holding more than a specified number (most commonly 10 or 15) rounds. For example, the 1994 federal assault weapons ban prohibited both (a) certain kinds of guns defined as assault weapons and (b) magazines able to hold more than 10 rounds (Koper, 2004). At least eight states and the District of Columbia similarly ban magazines with a large capacity, and still other states are considering bills to enact such restrictions (Brady Campaign to Prevent Gun Violence, 2013).

Theory—The Rationale for LCM Bans

When supporters of bans on LCMs provide an explicit rationale for these measures, they stress the potential for such restrictions to reduce the death toll in mass shootings. And indeed there is a statistical association between LCM use and the casualty count in mass shootings (Koper, 2004), though it is unknown whether this reflects an effect of LCM use or is merely a spurious association reflecting the offender's stronger intention to harm many people. If there is a causal effect, how would it operate? Does possession of LCMs somehow enable aggressors to shoot more victims, above and beyond the ability conferred by the use of semiautomatic guns equipped with smaller capacity detachable magazines? (A semiautomatic firearm is a gun that fires a single shot for each pull of the gun's trigger, but automatically causes a fresh round to be loaded into the gun's firing chamber.)

Possession of LCMs is largely irrelevant to ordinary gun crimes, that is, those with fewer victims than mass shootings, because it is extremely rare that the offenders in such attacks fire more rounds than can be fired from guns with ordinary ammunition capacities. For example, only 2.5% of handgun crimes in Jersey City, NJ, in 1992–1996 involved over 10 rounds being fired (Reedy & Koper, 2003, p. 154). Even among those crimes in which semiautomatic pistols were used, and some of the shooters were therefore likely to possess magazines holding more than 10 rounds, only 3.6% of the incidents involved over 10 rounds fired. Thus, if LCMs have any effect on the outcomes of violent crimes, it is more likely to be found among mass shootings with many victims, which involve unusually large numbers of rounds being fired.

Koper (2004) noted that "one of the primary considerations motivating passage of the ban on [LCMs]" was the belief that

> semiautomatic weapons with LCMs enable offenders to fire high numbers of shots rapidly, thereby potentially increasing both the number of persons wounded per gunfire incident . . . and the number of gunshot victims suffering multiple wounds, both of which would increase deaths and injuries from gun violence. (p. 80)

This summary was as much a rationale for restricting semiautomatic guns as it was for limits on magazine capacity, but Koper also concluded that "an LCM is arguably the most important feature of an AW. Hence, use of guns with LCMs is probably more consequential than use of guns with other military-style features" (p. 80). He then went on: "By forcing AW and LCM offenders to substitute non-AWs with small magazines, the ban might reduce the number of shots fired per gun, thereby reducing both victims shot per gunfire incident and gunshot victims sustaining multiple wounds" (p. 81).

It is reasonable to expect fewer people shot if fewer rounds were fired, but Koper did not explain why, for example, the use of three 10-round magazines would result in fewer shots fired than if a 30-round magazine were used. After all, three 10-round magazines and one 30-round magazine both contain 30 cartridges and thus allow 30 shots to be fired. Semiautomatic guns do not fire any faster when they have a larger magazine inserted in them than when they have a smaller magazine, nor is the lethality of any one shot affected by the size of the magazine from which it came. A limit on the number of cartridges that the shooter could fit into any *one* magazine would not limit the total number of rounds of ammunition that a would-be mass shooter could bring to the scene of their crime, or even the total number loaded into multiple detachable magazines.

The main difference between a 30-round magazine and three 10-round magazines, however, is that a shooter equipped with three 10-round magazines would have to change magazines twice in order to fire 30 rounds, while a shooter with a 30-round magazine would not have to change magazines at all. This presumably is what Koper (2004) meant when he wrote that "semiautomatic weapons with LCMs enable offenders to fire high numbers of shots rapidly" (p. 80).

Thus, it could be the *additional magazine changes* necessitated by the use of smaller magazines that might reduce the number of people hurt in mass shootings. Advocates of LCM bans argue that, if LCMs were not available, would-be mass murderers would shoot fewer people because they *would have to reload more often* due to the more limited capacities of the magazines that would then be legally available. A spokesperson for the Violence Policy Center (2011), for example, argued that "High-capacity ammunition magazines facilitate mass shootings by giving attackers the ability to fire numerous rounds without reloading."

It is not, however, self-evident why this should be so. Skilled shooters can change detachable magazines in 2 seconds or less, and even relatively unskilled persons can, with minimal practice, do so in 4 seconds (for a demonstration, see the video at https://www.youtube.com/watch?v=ZRCjY-GtROY, which shows a 2-seconds magazine change by an experienced shooter). Certainly, additional magazine changes do not increase the time needed to fire a given number of rounds by much.

Why, then, might inducing more magazine changes reduce casualty counts? Two explanations have been offered. First, during an additional interval when the shooter was forced to change magazines, *bystanders might tackle the shooter and prevent any further shooting*. Bystanders are presumably more willing to tackle a shooter while the shooter was reloading because it would be safer to do so—a shooter armed with only

one loaded gun would not be able to shoot those seeking to intervene during the effort to reload. A shooter equipped only with smaller capacity magazines would have to change magazines sooner and would therefore presumably shoot fewer people before he was tackled by the bystanders.

Second, additional magazine changes could extend the time interval between some of the shots, thereby *allowing more prospective victims to safely escape the scene* than otherwise would have been the case had the possession of LCMs enabled the shooter to reload less often.

These scenarios are plausible as logical possibilities, but have they actually occurred in the past often enough for it to be plausible that they would happen with some nonnegligible frequency in the future? If the past is any guide to the future, the credibility of any expectation of future benefits from LCM restrictions would rely heavily on how often these scenarios have actually played out in past mass shootings. This research is intended to test the plausibility of these possible causal linkages between LCM use and the casualty counts of mass shootings by closely examining the relevant details of such crimes. In particular, it was intended to estimate the share of mass shootings in which LCM use could plausibly have affected the casualty count.

Prior Research on LCMs

No one has actually tested whether mass shooters with LCMs fire more rounds than those without LCMs. We only have evidence indirectly bearing on this issue. Koper reported data showing that there are more *gunshot wound victims* in incidents in which the offender used an LCM (Koper, 2004, p. 86). The meaning of this statistical association, however, is unclear since one would expect it to exist even if LCM use had no causal effect on either the number of shots fired or the number of victims shot. The association is at least partly spurious if the deadliness of the shooter's intentions affects both his selection of weaponry (including magazines) and the number of shots he fires or persons he wounds.

It is a virtual tautology that the deadliness of the shooter's intentions affects the number of people hurt, unless one is prepared to assert that there is no relationship whatsoever between violent intentions and outcomes. While it is certainly true that outcomes do not match intentions perfectly, it is unlikely that there is no correlation at all.

The deadliness of a would-be mass shooter's intentions, however, is also likely to affect preparations for the shooting, such as accumulating many rounds of ammunition, acquiring multiple guns and multiple magazines, and selecting larger magazines rather than smaller ones. Accounts of mass shootings with high death tolls routinely describe the shooters making elaborate plans for their crimes, well in advance of the attacks, and stockpiling weaponry and ammunition (e.g., see Office of the State's Attorney 2013, regarding the Sandy Creek elementary school shootings; *Washington Post* "Pa. Killer had Prepared for 'Long Siege,'" October 4, 2006, regarding the Amish school killings in Lancaster, PA; Virginia Tech Review Panel, 2007, especially pp. 25–26, regarding the shootings at Virginia Tech; "Before gunfire, hints of bad

news," *New York Times* August 27, 2012, regarding the Aurora Colorado movie theater shootings). In short, people who intend to shoot many people are not only more likely to end up doing so but also prepare for doing so by acquiring equipment that they believe is better suited to this task.

The most direct indication that the intentions of mass shooters are more deadly than those of the average gun aggressor, aside from the number of casualties inflicted itself, is the percentage of wounded victims who were killed rather than nonfatally wounded. The data gathered for the present study indicate that in 23 LCM-involved mass shooting incidents, a total of 197 gunshot victims were killed and 298 were nonfatally wounded, for a fatality rate of 40.0%. In contrast, Cook (1985, p. 96) reported that police reports on general samples of shootings indicated that about only 15% of those wounded by gunshot were killed. Thus, the lethality of gunshot wounds inflicted by mass shooters is about 2.7 times as high as for shootings in general. Any one shot fired from a gun equipped with a larger capacity magazine is no more deadly or accurate than one fired from a gun with a smaller capacity magazine, so it is implausible that LCMs affect this fatality rate (deaths/persons wounded) by enabling shooters to more accurately hit vital areas of a victim's body where wounds are more likely to be fatal. Indeed, if those who suggest that shooters with LCMs fire faster than other shooters are correct, accuracy would be worse in LCM-involved shootings.

Thus, it is more likely that the high fatality rate in mass shootings is a product of the aggressor's stronger intentions to shoot more people, though it could also be partly a product of the greater use of rifles and shotguns in mass shootings (25 of the 66 guns used in these incidents [38%] of known gun type were rifles or shotguns; in comparison, only 8% of all U.S. gun homicides in 2014 were committed with rifles or shotguns—U.S. Federal Bureau of Investigation [FBI], 2015). This too could be an indication of greater shooter lethality, since rifles and shotguns are, on average, more lethal than handguns (Kleck, 1984). In sum, mass shooters appear to have more lethal intentions as aggressors, apart from any advantages they may gain from use of LCMs.

There is therefore sound reason to question whether a simple bivariate association between LCM use and number of shots fired, or victims wounded, in a mass shooting reflects a causal effect of LCM use. Unfortunately, there is no known way to directly measure the lethality of shooters' intentions at the time of their shootings, so we cannot simply statistically control for lethality of intentions in order to isolate the effect of LCM use. On the other hand, it would become more plausible to conclude that LCM use made its own contribution to the casualty count of shootings, above and beyond the effects of the apparently more lethal intentions of their users, if there was some evidence that either (a) significant numbers of mass shootings were disrupted by bystanders intervening when the shooters attempted to reload detachable magazines or (b) magazine changes increase the time intervals between shots fired, thus potentially allowing more prospective victims to escape to safety. This article provides a close examination of the details of mass shootings so as to cast light on these and related issues.

Method

Definition of Eligible Incidents

We tried to identify, as comprehensively as possible, all mass shootings that occurred in the United States in the 20-year period from 1994 through 2013 inclusive and that were known to have involved an LCM. An LCM was defined as a magazine holding more than 10 rounds of ammunition. A mass shooting was defined as one in which more than six people were shot, either fatally or nonfatally, in a single incident. Any specific numerical cutoff is necessarily somewhat arbitrary, but some are less arbitrary than others. The six-victim cutoff was used because an offender could shoot as many as six persons using a typical old-fashioned six-shot revolver of the sort that has been around since the 19th century, and our goal was to identify all incidents in which it was plausible that use of an LCM (always used in connection with modern semiautomatic firearms) affected the number of casualties. It is less likely that LCMs affect the casualty count in incidents in which few people were shot, and generally fewer rounds were fired, since the rationale for banning LCMs is that they permit shooters to fire many rounds without reloading, and thereby kill or injure more victims (Koper, 2004). Thus, had the numerical cutoff been set lower, the sample of incidents would have included more cases in which LCM use was unlikely to have affected the number of victims. In that way, we have intentionally biased the sample in favor of the hypothesis that LCM use causes a higher casualty count.

We partly relied on a list compiled by the staff of the Violence Policy Center (2015) to identify LCM-involved mass shootings. Because this organization advocates bans on LCMs (Violence Policy Center, 2011), we are confident its staff were well motivated to compile as comprehensive a list as possible so as to better document the need to restrict magazine capacities. Our search of NewsBank and the other compilations of mass shootings that we cite (see Data Sources section) did not uncover any additional qualifying incidents. It is nevertheless logically impossible to know for certain that all qualifying incidents were included.

We did not employ the oft-used definition of "mass murder" as a homicide in which four or more victims were killed, because most of these involve just four to six victims (Duwe, 2007), which could therefore have involved as few as six rounds fired, a number that shooters using even ordinary revolvers are capable of firing without reloading. LCMs obviously cannot help shooters who fire no more rounds than could be fired without LCMs, so the inclusion of "nonaffectable" cases with only four to six victims would dilute the sample, reducing the percentage of sample incidents in which an LCM might have affected the number of casualties. Further, had we studied only homicides with four or more dead victims, drawn from the FBI's Supplementary Homicide Reports (SHR), we would have missed cases in which huge numbers of people were shot, and huge numbers of rounds were fired, but three or fewer of the victims died. For example, in one widely publicized shooting carried out in Los Angeles on February 28, 1997, two bank robbers shot a total of 18 people—surely a mass shooting by any reasonable standard (Table 1). Yet, because none of the people they shot died, this incident would not qualify as a mass murder (or even murder of

Table 1. Mass Shootings in Which Shooter(s) Used Magazines With a Capacity Over 10 Rounds, United States, 1994–2013.[a]

Shooter(s)	Date	Number of Shooters	Number of Guns	Number of Magazines	Capacity of Largest Magazine	Shooter(s) Reloaded?	Number of Shots Fired	Seconds Per Shot	Number Killed	Number Nonfatally Wounded
Dean Allen Mellberg	June 20, 1994	1	2	4	70	?	43–56	<6	4	23
Larry Phillips, Jr., and Emil Matasareanu	February 28, 1997	2	6	9+	100	Yes	1,101	2.40	0	18
Mitchell Johnson and Andrew Golden	March 24, 1998	2	13	3	30	?	30	?	5	11
Kip Kinkel	May 21, 1998	1	3	3+	50	Yes	51	?	2	15
Dylan Klebold and Eric Harris	April 20, 1999	2	4	16	52	Yes	188	15.64	13	21
Larry Gene Ashbrook	September 15, 1999	1	2	6	15	Yes	>100	6.00	7	7
Byran Koji Uyesugi	November 2, 1999	1	1	3	15	?	10	180.0	7	0
Michael McDermott	December 26, 2000	1	3	4+	30	Yes	37	10.54	7	0
Terry Ratzmann	March 12, 2005	1	1	3	15?	Yes	22	<2.7	7	4
Seung-Hui Cho	April 16, 2007	1	2	19	15	Yes	174	53.79	32	23
Robert Hawkins	December 5, 2007	1	1	2	30	?	>30	12.00	8	5
Steven Kazmierczak	February 14, 2008	1	4	6+	33	Yes	56	5.36	5	21
Jiverley Wong	April 3, 2009	1	2	3	30	Yes	99	?	13	4
George Sodini	August 4, 2009	1	4	3+	30	?	50	?	3	9
Nidal Hasan	November 5, 2009	1	2	15	30	Yes	214	?	13	38
Timothy Hendron	January 7, 2010	1	4	3+	Probable LCM	?	115	c. 18	3	5
Omar Thorton	August 3, 2010	1	2	4	17	?	19	9.47	8	2
Jared Loughner	January 8, 2011	1	1	4	33	No[b]	31	0.45	6	13
Eduardo Sancion	September 6, 2011	1	3	3	30	Yes	60+	1.42	4	14
James Holmes	July 20, 2012	1	4	4	100	Yes	76	4.74	12	58
Michael Page	August 5, 2012	1	1	3	19	Yes	33+	?	6	3
Andrew Engeldinger	September 27, 2012	1	1	2	15	Yes	46+	16.3	6	2
Adam Lanza	December 14, 2012	1	4	12+	30	Yes	154+	1.56	26	2

Note. Details of these incidents and citations to news accounts used as sources may be found in the appendix to an extended version of this article, with the same title, on the Social Science Research Network, at http://papers.ssrn.com/sol3/papers.cfm?abstract_id=2700166. LCM = large-capacity magazine; c = circa, i.e. approximately; ? = unknown.
[a]Number of guns is the number in the shooter's immediate possession, not necessarily the number fired. Number of magazines is the number of detachable magazines in the shooter's immediate possession. The number includes magazines in loaded semiautomatic firearms. "Seconds per shot" is the average time interval between shots through the period of shooting. [b]Shooter was prevented from reloading a defective magazine by bystanders tackling him.

any kind). Exclusion of such incidents would bias the sample against the proposition that LCM use increases the number of victims by excluding incidents with large numbers of victims.

We also excluded shootings in which more than six persons were shot over the entire course of the incident, but the shootings occurred in multiple locations with no more than six people shot in any one of the locations, and substantial periods of time intervened between episodes of shooting. An example is the series of killings committed by Rodrick Dantzler on July 7, 2011. He killed seven people and wounded two others, but did so in three different locations over a 5-hr period, shooting no more than four people in any one of the locations. Since shooters in these types of incidents have ample time to reload between sets of shots even without LCMs, use of an LCM is less likely to be relevant to the casualty counts than in a mass shooting as defined herein.

It is not possible to compare shootings involving LCMs with shootings not involving LCMs, because no source of information on shooting incidents, whether news media reports or police offense reports, systematically establishes which shootings did *not* involve LCMs. Thus, it is impossible to distinguish (a) shootings in which the perpetrator did not use an LCM from (b) shootings in which the perpetrator *did* use an LCM, but this fact was not mentioned in the account of the incident. Consequently, we are necessarily limited to describing incidents that were affirmatively identified as involving LCMs. In any case, since our purpose was to establish how often LCM use affects casualty counts in mass shootings, even if we could identify incidents that definitely did not involve LCMs, they would be irrelevant to this narrow purpose because they are obviously cases in which LCM use could not have affected casualty counts.

Data Sources

We relied on news stories to identify mass shootings and get information on their details. Relying on news outlets has obvious limits, since some mass shootings get little news coverage beyond a few stories by news outlets near the shooting location, and it is possible that none of the writers of these few stories used even one of the common words and phrases we used in our database searches. Further, even multiple news accounts of widely reported incidents may not include crucial details of the incidents, especially the number of shots fired and the duration of the shooting. Also, early news accounts of shootings are sometimes inaccurate in their details (Huff-Corzine, Corzine, Jarvis, Tetzlaff-Bemiller, Weller, & Landon, 2014), so we consulted later stories on a given incident (often pertaining to the trial of the shooter) in addition to early ones. Excluding the early news stories, we found that reported details of mass shootings were extremely consistent across stories. Fortunately, the known biases of news coverage of crime mostly work in favor of our goal of covering shootings in which many shots were fired, since news coverage is biased in favor of reporting incidents with larger numbers of victims (Duwe, 2000).

The alternative of using police reports was not feasible because such reports are not publicly available for a large share of homicides. Relying on the FBI's SHR would be

even worse than news accounts for our purposes, because this source says nothing about the number of rounds fired, number of guns used, details about the guns used (beyond whether they were handguns, rifles, or shotguns), number of magazines used, or the capacity of magazines used for *any* homicide incidents, whereas news stories provide such information for many mass shootings. These same deficiencies apply to data from the FBI's National Incident-based Reporting System, which have the additional disadvantage of covering only part of the nation.

A variety of sources were used to identify eligible incidents. First, as previously noted, we consulted "Mass Shootings in the United States Involving High-Capacity Ammunition Magazines," a fact sheet compiled by the Violence Policy Center, available online at http://www.vpc.org/fact_sht/VPCshootinglist.pdf. This source only covers incidents known to involve magazines with a capacity of 10 or more rounds.

Second, we searched the NewsBank Infoweb online database which covers hundreds of print, broadcast, and online news outlets, including newspapers, news magazines, transcripts of television news programs, and online-only news providers, in every state in the nation. We searched for articles whose text (including headlines) included any of the following phrases: "mass shooting," "massacre," mass murder, "shooting spree," or "rampage" for the 20-year period from January 1, 1994, through December 31, 2013.

Third, we consulted the following existing compilations of mass shootings, mass murders, and "active shooter incidents" (and the sources they cited) to identify potentially relevant shooting incidents:

- "US Mass Shootings, 1982–2012: Data from Mother Jones' (2013) Investigation," created by the staff of *Mother Jones* magazine, available online at http://www.motherjones.com/politics/2012/12/mass-shootings-mother-jones-full-data. This source only covers incidents in public places with four or more dead, and therefore misses those with many victims shot but three or fewer of them fatally as well as incidents occurring in private places. It also includes some spree shootings in which only a few victims were shot in any one location.
- "Analysis of Recent Mass Shootings" (September 2013), compiled by Mayors Against Illegal Guns, and available online at http://www.demandaction.org/detail/2013-09-updated-analysis-of-recent-mass-shootings. This covers incidents only for January 2009 to September 2013, and only those with four or more dead victims, thereby excluding those with many victims shot, but three or fewer shot fatally.
- Bjelopera, Bagalman, Caldwell, Finklea, and McCallion (March 18, 2013). *Public Mass Shootings in the United States: Selected Implications for Federal Public Health and Safety Policy*. Washington, DC: Congressional Research Service. This source only covers incidents occurring in public places and with four or more deaths, thereby excluding cases with many victims shot but three or fewer fatally as well as those occurring in private places.
- Citizens Crime Commission of New York City. "Mass Shooting Incidents in America (1984–2012)," at http://www.nycrimecommission.org/mass-shoot

ing-incidents-america.php, accessed January 15, 2014. This source covers shootings with four or more persons killed, with a magazine capable of holding more than 10 rounds. It excludes cases with no known use of LCMs, and incidents with many victims shot but three or fewer killed.

Notwithstanding the use of these multiple sources, we cannot be certain of achieving absolutely complete coverage of all LCM-involved mass shootings. Most of the sources rely, directly or indirectly, on news media accounts of the incidents, and some of these shootings received little coverage beyond local news outlets and perhaps an Associated Press state wire service story. The fewer news stories reporting an incident, the more likely it is that there were no stories containing any of the commonly used phrases for which we searched. The mass shootings most likely to receive little news coverage are those with fewer than four victims killed. Most of the lightly covered incidents we discovered also involved fewer than 10 victims shot, fatally or nonfatally.

On the other hand, it is unlikely that we missed many large-scale shootings, because these are likely to be well covered by multiple news outlets. Since those we missed are likely to involve fewer victims, it is also less likely that an LCM was needed for shooting as many people as were shot in these incidents. Omission of these cases, therefore, biases the sample in favor of the hypothesis that LCMs affect casualty counts.

As a check on the completeness of coverage of our methods, we used the FBI's SHRs data to identify all SHR-covered U.S. homicides that involved more than six dead victims and the use of firearms (not just those involving LCMs). These SHR data sets cover about 90% of U.S. homicides. For the period 1994–2013, we identified 17 qualifying incidents in the SHR data sets. We then checked to see if our search methods would have identified these cases. We found that searches of the NewsBank database alone identified all 17 of these incidents. Thus, shootings with many dead victims clearly are completely covered by the news media.

Once eligible incidents were identified, we searched through news accounts for details related to whether the use of LCMs could have influenced the casualty counts. Specifically, we searched for (1) the number of magazines in the shooter's immediate possession, (2) the capacity of the largest magazine, (3) the number of guns in the shooter's immediate possession during the incident, (4) the types of guns possessed, (5) whether the shooter reloaded during the incident, (6) the number of rounds fired, (7) the duration of the shooting from the first shot fired to the last, and (8) whether anyone intervened to stop the shooter.

Findings

How many mass shootings were known to have been committed using LCMs? We identified 23 total incidents in which more than six people were shot at a single time and place in the United States from 1994 through 2013 and that were known to involve use of any magazines with capacities over 10 rounds. Table 1 summarizes key details of the LCM-involved mass shootings relevant to the issues addressed in this article.

What fraction of all mass shootings are known to involve LCMs? There is no comprehensive listing of all mass shootings available for the entire 1994–2013 period, but the most extensive one currently available is the one at the Shootingtracker.com website, which only began its coverage in 2013. For 2013, this database identified 31 incidents in which more than six victims were supposedly killed or injured. This source includes deaths or injuries of perpetrators in their counts of ''victim'' deaths and injuries and also counts as victims' persons who were shot at, but not hit. Correcting these flaws eliminated six of the incidents as mass shootings, while another three incidents were spree shootings. Eliminating these nine ineligible incidents left 22 genuine mass shootings. The Shootingtracker database itself does not record LCM use, but examination of news media accounts indicated that none of these 22 incidents in 2013 were known to involve use of an LCM. For 2013, the Violence Policy Center (2015) identified just one shooting with more than six victims killed or injured that involved an LCM, but this incident was a spree shooting in which eight people were shot in three different widely spaced locations, with no more than three shot in any one of the locations (the June 7, 2013, incident in Santa Monica, CA). Thus, there apparently were zero mass shootings in 2013 known to involve LCMs.

To put these numbers in perspective, for the United States as a whole in 2013, there were an estimated 14,196 people killed in murders and nonnegligent manslaughters (MNNM) involving any weapon types, 9,795 of them killed with firearms (U.S. FBI, 2014b). There were an estimated 13,349 mnnm incidents,[1] of which just 3 involved more than six dead victims, 12,675 involved a single dead victim, and 13,346 involved six or fewer dead victims (U.S. Department of Justice Federal Bureau of Investigation, 2015). The 22 qualifying shooting incidents identified by Shooting Tracker as involving more than six victims therefore accounted for less than one sixth of 1% of homicide incidents and victims killed in those incidents claimed less than one tenth of 1% of homicide victims.

One might speculate that there were significant numbers of mass shootings in which LCMs were used, but not a single news account mentioned the LCM use. The use of LCMs has been a major focus of gun control advocacy groups and national news outlets since at least 1989, when a Stockton California schoolyard shooting lead to the nation's first state-level assault weapons ban (Kleck, 1997, chap. 4). In this light, it seems unlikely that LCM use in a mass shooting would go completely unreported in all news accounts, but it cannot be ruled out as a logical possibility. It is, however, irrelevant to our analyses unless shootings with unmentioned LCM use are systematically different from those that explicitly mentioned LCM use—a speculation we cannot test.

LCMs are sometimes defined as magazines holding over 10 rounds, sometimes as those holding over 15 rounds (Koper, 2004). For our entire 20-year study period of 1994–2013, 23 mass shootings were known to involve LCMs using the more inclusive cutoff of 10 rounds, that is, at least one round was fired during the incident from a gun equipped with a magazine capable of holding more than 10 rounds. Using the more stringent cutoff of more than 15 rounds, 20 incidents were known to involve LCMs.

Thus, LCM-involved mass shootings are known to have occurred an average of once per year in the United States over this 20-year period.

How often have bystanders intervened while a mass shooter was trying to reload? How many times people have disrupted a mass shooting while the shooter was trying to load a detachable magazine into a semiautomatic gun? Note that it is irrelevant whether interveners have stopped a shooter while trying to reload some other type of gun, using other kinds of magazines, since we are addressing the potential significance of restrictions on the capacity of detachable magazines that are used only with semiautomatic firearms. Thus, bystander intervention directed at shooters using other types of guns that take much longer to reload than a semiautomatic gun using detachable magazines could not provide any guidance as to the likelihood of bystander intervention when the shooter was using a semiautomatic gun equipped with detachable magazines that can be reloaded very quickly. Prospective interveners would presumably be more likely to tackle a shooter who took a long time to reload than one who took only 2- to 4-s to do so. Likewise, bystander interventions that occurred at a time when the shooter was *not* reloading (e.g., when he was struggling with a defective gun or magazine) are irrelevant, since that kind of bystander intervention could occur regardless of what kinds of magazines or firearms the shooter was using. It is the need to reload detachable magazines sooner and more often that differentiates shooters using smaller detachable magazines from those using larger ones.

For the period 1994–2013 inclusive, we identified three mass shooting incidents (with or without LCM use) in which it was claimed that interveners disrupted the shooting by tackling the shooter while he was trying to reload. In only one of the three cases, however, did interveners actually tackle the shooter while he may have been reloading a semiautomatic firearm. In one of the incidents, the weapon in question was a shotgun that had to be reloaded by inserting one shotshell at a time into the weapon (*Knoxville News Sentinel* "Takedown of Alleged Shooter Recounted" July 29, 2008, regarding a shooting in Knoxville, TN on July 27, 2008), and so the incident is irrelevant to the effects of detachable LCMs. In another incident, occurring in Springfield, OR, on May 21, 1998, the shooter, Kip Kinkel, was using a semiautomatic gun, and he was tackled by bystanders, but not while he was reloading. After exhausting the ammunition in one gun, the shooter started firing another loaded gun, one of the three firearms he had with him. The first intervener was shot in the hand in the course of wresting this still-loaded gun away from the shooter (*The (Portland) Oregonian*, May 23, 1998).

The final case occurred in Tucson, AZ, on January 8, 2011. This is the shooting in which a man named Jared Loughner attempted to assassinate Representative Gabrielle Giffords. The shooter was using a semiautomatic firearm and was tackled by bystanders, purportedly while trying to reload a detachable magazine. Even in this case, however, there were important uncertainties. According to one news account, one bystander "grabbed a full magazine" that the shooter dropped, and two others helped subdue him (Associated Press, January 9, 2011). It is not, however, clear whether this bystander intervention was facilitated because (1) the shooter was reloading or

because (2) the shooter stopping firing when his gun or magazine failed to function properly. Eyewitness testimony, including that of the interveners, was inconsistent as to exactly why or how the intervention transpired in the Giffords shooting. One intervener insisted that he was sure the shooter had exhausted the ammunition in the first magazine (and thus was about to reload) because he saw the gun's slide locked back—a condition he believed could only occur with this particular firearm after the last round is fired. In fact, this can also happen when the gun jams, that is, fails to chamber the next round (Morrill, 2014; Salzgeber, 2014).

Complicating matters further, the *New York Times* reported that the spring on the second magazine was broken, presumably rendering it incapable of functioning. Their story's headline and text characterized this mechanical failure as "perhaps the only fortunate event of the day" (*New York Times* "A Single, Terrifying Moment: Shots, Scuffle, Some Luck," January 10, 2011, p. A1). If the *New York Times* account was accurate, the shooter would not have been able to continue shooting with that magazine even if no one had stopped him from loading it into his gun. Detachable magazines of any size can malfunction, which would at least temporarily stop a prospective mass shooter from firing, and thereby provide an opportunity for bystanders to stop the shooter. It is possible that the bystander intervention in the Tucson case could have occurred regardless of what size magazines the shooter possessed, since a shooter struggling with a defective small-capacity magazine would be just as vulnerable to disruption as one struggling with a defective LCM. Thus, it remains unclear whether the shooter was reloading a functioning magazine when the bystanders tackled him.

The real significance of LCM use in the Gabrielle Giffords shooting is that the first magazine that the shooter used had a capacity of 33 rounds, and the shooter fired 31 times before being tackled. Had he possessed only a 15-round magazine, and bystanders were willing to intervene when the shooter either reloaded or struggled with a defective magazine, he would have been able to fire at most 16 rounds (including one in the firing chamber)—15 fewer than the 31 he actually fired before he was stopped, for whatever reason. Consequently, instead of the 19 people he shot (6 fatally, 13 nonfatally), it would be reasonable to estimate that he would have shot only about half as many victims. Thus, the absence of an LCM might have prevented three killings and six or seven nonfatal gunshot woundings in this incident.

The bystander intervention in the Giffords shooting was, however, unique, and occurred only because there were extraordinarily courageous and quick-thinking bystanders willing and able to tackle the shooter. Over a 20-year period in the United States, the Tucson incident appears to be the only known instance of a mass shooter using a semiautomatic firearm and detachable magazines in which the shooter was stopped by bystanders while the shooter may have been trying to reload such a magazine. All other mass shootings have instead stopped only when the shooter chose to stop and left the scene, the shooter committed suicide, or armed police arrived and forced the shooter to stop (see U.S. FBI, 2014a).

The use of multiple guns and multiple magazines. Restrictions on LCMs obviously could not have affected mass shootings in which no LCMs were used, so it is just those that

Table 2. Summary of Key Characteristics of Mass Shootings (>6 Shot) With Large-Capacity Magazines, United States, 1994–2013.

Key Characteristics of the Incidents	Mass Shootings With Magazines Over 10 Rounds ($n = 23$)			Mass Shootings With Magazines Over 15 Rounds ($n = 20$)		
	Yes	No	Not Reported	Yes	No	Not Reported
Multiple guns	17 (74/74%)	6	0	15 (75/75%)	5	0
Multiple magazines	23 (100/100%)	0	0	20 (100/100%)	0	0
Both multiple guns and multiple magazines	17 (74/74%)	6	0	15 (75/75%)	5	0
Either multiple guns or multiple magazines	23 (100/100%)	0	0	20 (100/100%)	0	0
Shooter reloaded	14 (88/61%)	2	7	12 (86/60%)	2	6

Note. First number in parentheses after each frequency is the percentage of incidents with nonmissing information that had the indicated attribute. The second number in parentheses is the percentage of all incidents, including those for which the relevant information was missing, that had the indicated attribute.

involved LCMs that are relevant to judging the benefits that might have accrued had LCMs been unavailable at the beginning of the study period. As previously noted, there is considerable evidence that people who commit large-scale shootings, unlike most ordinary aggressors, devote considerable advance planning to their crimes. Part of their preparations entails cumulating multiple guns, multiple magazines, and many rounds of ammunition. The significance of this is that, in cases where the shooter has more than one loaded gun, he can continue firing, without significant pause, even without LCMs, simply by switching to a loaded gun. Alternatively, if he has multiple small magazines rather than LCMs, the shooter can continue firing many rounds with only a 2- to 4-s pause between shots for switching magazines.

Table 2 displays how often LCM-involved mass shootings involved shooters using either multiple guns or multiple magazines. Of 23 such incidents using the "more-than-10-rounds" criterion, the shooters possessed more than one gun in 17 incidents (74%), leaving six cases in which it was known that the shooter possessed just one gun. Of 20 incidents using the more-than-15-rounds criterion, the shooters possessed more than one gun in 15 incidents (75%), leaving five cases in which it was known that the shooter possessed just one gun.

Of 23 mass shootings with LCMs (>10 rounds), offenders were known to possess multiple detachable magazines in all 23 incidents (100%). Likewise, of the 20 mass shootings with magazines holding over 15 rounds, all 20 involved shooters with multiple magazines.

The average number of magazines in the immediate possession of offenders in incidents in which magazines with a capacity greater than 10 were possessed was at least 5.78 (Table 1). These offenders could have continued firing, even if they had possessed only one gun, with only the interruptions of 2–4 s that it would take for each magazine change.

In sum, there were no mass shootings in the United States in 1994–2013 known to have involved LCMs in which the shooter did not possess either multiple guns or multiple detachable magazines. In all mass shootings in which the shooters were known to have possessed one or more LCMs, the shooters could have either continued firing many rounds without any interruption at all simply by switching loaded guns or could have fired many rounds with only very brief interruptions of 2–4 s to change detachable magazines.

The offenders in LCM-involved mass shootings were also known to have *reloaded* during 14 of the 23 (61%) incidents with magazine holding over 10 rounds. The shooters were known to have *not* reloaded in another 2 of these 20 incidents, and it could not be determined if they reloaded in the remaining seven incidents. Thus, even if the shooters had been denied LCMs, we know that most of them definitely would have been able to reload smaller detachable magazines without interference from bystanders since they in fact did change magazines. The fact that this percentage is less than 100% should not, however, be interpreted to mean that the shooters were *unable* to reload in the other nine incidents. It is possible that the shooters could also have reloaded in many of these nine shootings, but chose not to do so, or did not need to do so in order to fire all the rounds they wanted to fire. This is consistent with the fact that there has been at most only one mass shooting in 20 years in which reloading a semiautomatic firearm might have been blocked by bystanders intervening and thereby stopping the shooter from doing all the shooting he wanted to do. All we know is that in two incidents, the shooter did not reload, and news accounts of seven other incidents did not mention whether the offender reloaded.

Do more magazine changes allow more prospective victims to escape? An alternative rationale for why limiting aggressors to smaller magazines would result in fewer casualties in mass shootings is that the increased number of magazine changes necessitated by use of smaller magazines would create additional pauses in the shooting, allowing more potential victims to escape than would otherwise escape. For example, a story in the *Hartford Courant* about the Sandy Hook elementary school killings in 2012 was headlined "Shooter Paused, and Six Escaped," the text asserting that as many as six children may have survived because the shooter paused to reload (December 23, 2012). The author of the story, however, went on to concede that this was just a speculation by an unnamed source, and that it was also possible that some children simply escaped when the killer was shooting other children. There was no reliable evidence that the pauses were due to the shooter reloading, rather than his guns jamming or the shooter simply choosing to pause his shooting while his gun was still loaded.

The plausibility of the "victims escape" rationale depends on the average rates of fire that shooters in mass shootings typically maintain. If they fire very fast, the 2–4 s it takes to change box-type detachable magazines could produce a slowing of the rate of fire that the shooters otherwise would have maintained without the magazine changes, increasing the average time between rounds fired and potentially allowing more victims to escape during the between-shot intervals. On the other hand, if mass

Table 3. Known Rates of Fire in Mass Shootings, 1994–2013.

Date of Incident	Shots Fired[a]	Time of Firing (Minutes)[a]	Average Shots Per Minute	Average Seconds Per Shot	Number of Guns
June 20, 1994	>50	c. 5	>10	<6.0	2
February 28, 1997	1,101	44	25	2.4	4
April 20, 1999	188	49	3.8	15.8	4
September 15, 1999	>100	10	>10.0	<6.0	2
September 2, 1999	10	<30	>0.33	<180.0	1
May 24, 2000	c. 7	<90	>0.08	<771.4	1
September 22, 2000	9+	<10	>0.9	<66.7	1
December 26, 2000	37	5–8 (6.5)	5.7	10.5	3
February 5, 2001	25–30 (27.5)	8–15 (11.5)	2.4	25.1	4
March 5, 2001	c. 24	6	c. 4.0	c. 15.0	1
March12, 2005	22	<1	>22.0	<2.7	1
March 21, 2005	45	9	5.0	12.0	3
March 25, 2006	9+	c. 5	>1.6	<33.3	2
October 2, 2006	17–18 (17.5)	c. 2	c. 8.75	c. 6.9	2
April 16, 2007	c. 174	156	c. 1.11	c. 53.8	2
October 7, 2007	30	c. 1	c. 30.0	c. 2.0	3
December 5, 2007	>30	c. 6	>5.0	<12.0	1
February 14, 2008	56	5	11.1	5.4	4
January 7, 2010	115	30	3.8	15.7	4
August 3, 2010	19	3	6.3	9.5	2
January 8, 2011	31	0.25	125	0.48	1
September 6, 2011	60+	1.42	42.3+	1.4	3
July 20, 2012	76	c. 6	12.7	4.74	4
September 27, 2012	46+	14	>3.3	<18.3	1
December 14, 2012	154+	4	38.5+	1.6	3

Note. c = circa.
[a]Where a range was provided in news accounts, the midpoint of the range (shown in parentheses) of shots fired or time of firing was used in rate-of-fire computations.

shooters fire their guns with the average interval between shots lasting *more* than 2–4 s, the pauses due to additional magazine changes would be no longer than the pauses the shooter typically took between shots even when not reloading. In that case, there would be no more opportunity for potential victims to escape than there would have been without the additional magazine changes.

Table 3 displays data on rates of fire for LCM-involved mass shootings in 1994–2013. Information on both the duration of the firing and the number of rounds fired was available for 17 of the 23 incidents shown in Table 1 plus another 8 mass shootings for which the necessary information was available but that did not involve any known LCM use. Reliable information on duration of fire may well be unavailable from any source for many mass shootings. There are rarely audio recordings that would provide precise information on the duration of fire (as there were in the 2012 Aurora Colorado movie

theater shooting), so eyewitness estimates are usually the basis for establishing this. On the other hand, there is often quite reliable information on the number of rounds fired, since semiautomatic firearms eject an empty shell casing after each round is fired. When shooters use such guns, crime scene investigators can (absent removal of the evidence by the offender or souvenir hunters) establish the number of rounds fired by counting cartridge casings recovered at the scene.

Average rate of fire was computed as the average number of seconds between shots. In the 25 incidents for which average rates of fire could be determined, shooters never maintained an average rate of fire anywhere as fast as that at which their firearms were capable of firing. Shooters firing as fast as the gun allows can easily fire three rounds per second with a typical semiautomatic firearm, that is, with only about one third of a second between rounds. In only three incidents were mass shooters known to have averaged less than *2* s between rounds. This is no more than one sixth of the maximum rate of fire of which semiautomatic guns are capable (see Table 3, incidents occurring on January 8, 2011, September 6, 2011, and December 14, 2012). This means that taking 2 s to reload a detachable magazine would not have slowed the shooters' average rate of fire at all in 22 of the 25 incidents for which rate of fire could be established and would have only slightly slowed the rate in the remaining three incidents.

It cannot be assumed, however, that in the three incidents in which usually high rates of fire were maintained, use of smaller magazines would have slowed the rate of fire due to a need to change magazines more often. Shooters possessed multiple guns in two of these three relatively rapid fire incidents (those occurring on September 6, 2011 and December 13, 2012), which means that, rather than needing to change magazines to continue shooting, the aggressors could simply have switched guns, from one firearm emptied of rounds to another loaded firearm, without pausing in their shooting at all. Over the 20-year study period, there was just one LCM-involved mass shooting incident in the United States in which a shooter maintained an average rate of fire with less than 2 s elapsing between shots, *and* possessed only a single gun—the shooting involving Jared Loughner (on January 8, 2011), who was stopped from further shooting when he was tackled by bystanders.

In sum, in nearly all LCM-involved mass shootings, the time it takes to reload a detachable magazine is no greater than the average time between shots that the shooter takes anyway when not reloading. Consequently, there is no affirmative evidence that reloading detachable magazines slows mass shooters' rates of fire, and thus no affirmative evidence that the number of victims who could escape the killers due to additional pauses in the shooting is increased by the shooter's need to change magazines.

Conclusions

In light of the foregoing information, it is unlikely that the larger number of rounds fired in the average LCM-linked mass shooting found by Koper (2004) was in any sense caused by the use of LCMs. In all but one of such cases in the period from 1994 through 2013, there was nothing impossible or even difficult about the shooter firing

equally large numbers of rounds even if he had possessed only smaller capacity magazines, since the same number of rounds could easily have been fired with smaller detachable magazines of the sort that would remain legally available under LCM bans. Instead, the larger number of rounds fired by LCM-using shooters is more likely to reflect the more lethal intentions prevailing among such shooters, just as their planned use of multiple guns and multiple magazines, and the unusually high fatality rate (deaths over total woundings) of their attacks are outward indications of a desire to shoot many people. Unfortunately, there are no known methods for reliably measuring the lethality of shooters' intentions independent of the outcomes of their crimes, making it impossible to statistically control for this factor in a multivariate statistical analysis and thereby isolate the effects of LCM use.

One cannot prove a negative, and it is possible that mass shooters in the future might be different from those in the past, and that would-be mass shooters, unlike those of the past, would not obtain multiple guns or multiple smaller capacity magazines as substitutes for LCMs. One might also speculate that incidents that did *not* end up with many shooting victims turned out that way because the shooter did *not* use an LCM. At this point, however, there is little sound affirmative empirical basis for expecting that fewer people would be killed or injured if LCM bans were enacted.

Focusing gun control efforts on mass shootings makes sense from a political standpoint, since support for gun control is elevated following highly publicized gun crimes. Such efforts, however, are less sensible for purposes of reducing the death toll from gun violence, especially if they focus on technologies rarely used in gun crime as a whole. Controls aimed at reducing ordinary forms of firearm violence, such as shootings with just one or a few victims, are more likely to have large impacts on the aggregate gun violence death toll for the simple reason that nearly all victims of gun violence are hurt in incidents with a small number of victims. For example, less than 1% of U.S. homicide incidents in 2013 involved more than two victims killed (U.S. Department of Justice Federal Bureau of Investigation, 2015).

Most types of gun control focus on preventing more dangerous people from acquiring, possessing, or using *any* type of gun, and therefore have potential to prevent a wide array of gun crimes. A prime example is a law requiring background checks on persons seeking to buy guns. Gun laws with a background check component, such owner license and purchase permit laws, have been found to be potentially effective in reducing homicide (Kleck & Patterson, 1993, p. 274). There is already a federal law requiring background checks, but it only applies to purchases from licensed gun dealers. Extending these checks to cover private gun transfers—that is, implementing a federal universal background check (Kleck, 1991, pp. 433–435)—is far more likely to prevent significant numbers of gun crimes than measures aimed at rarely used gun technologies like LCMs and extremely rare types of violent incidents like mass shootings.

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Supplementary Material

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Note

1. Supplementary Homicide Reports (SHR) data for 2013 indicate that there were an average of 1.063 victims per SHR-covered homicide incident, implying 13,349 incidents.

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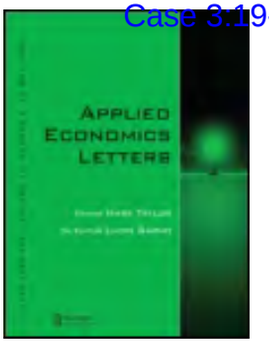
Gary Kleck is the Emeritus David J. Bordua Professor of Criminology and Criminal Justice at Florida State University, having retired after 38 years at FSU. He has won the Michael J. Hindelang Award for Point Blank, testified to Congress and state legislatures on gun control, and served on numerous national task forces and panels. He is currently completing a book, with Brion Sever, on the effects of legal punishment on crime.

EXHIBIT "9"



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An examination of the effects of concealed weapons laws and assault weapons bans on state-level murder rates

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The purpose of the present study is to determine the effects of state-level assault weapons bans and concealed weapons laws on state-level murder rates. Using data for the period 1980 to 2009 and controlling for state and year fixed effects, the results of the present study suggest that states with restrictions on the carrying of concealed weapons had higher gun-related murder rates than other states. It was also found that assault weapons bans did not significantly affect murder rates at the state level. These results suggest that restrictive concealed weapons laws may cause an increase in gun-related murders at the state level. The results of this study are consistent with some prior research in this area, most notably Lott and Mustard (1997).

Keywords: gun control; assault weapons; concealed weapons

JEL Classification: K14

I. Introduction

On 14 December 2012, a young man carrying a Bushmaster XM15-E2S (Bushmaster Firearms, Madison, NC, USA) semi-automatic rifle shot his way into an elementary school in Newtown, Connecticut, killing 26 people, 20 of whom were children. Since a semi-automatic weapon was used in the commission of this crime, there have been debates both in Congress and in various state legislatures regarding the potential enactment of assault weapons bans. One of the measures that were considered at the Federal level was a revival of the 1994 Federal assault weapons ban, which expired in 2004. This firearms ban was part of the Violent Crime Control and Law Enforcement Act of 1994, and this act outlawed semi-automatic weapons and prohibited large capacity magazines that held more than 10 rounds of ammunition.

Regarding state-level bans, no state had an assault weapons ban before 1989. Then, in that year, California enacted the first state-level ban on assault weapons. Several states followed suit, and shortly thereafter Connecticut, Hawaii and New Jersey enacted their own bans. In 1994, the Federal ban was enacted, thus rendering state laws moot. After the Federal ban expired in 2004, several states enacted their own bans once again.

Of course, there are many other types of gun control measures, both at the state and Federal level. One state-level gun control measure that was very common years ago but, in recent years, has become much less prevalent is the restrictive concealed carry weapons (CCW) law. These laws concern how permits are issued to individuals who want to carry concealed weapons, primarily handguns. There are four broad types of CCW laws. The first is unrestricted; individuals in these states do not need a permit to carry a concealed handgun. For years, the only state that had no CCW restrictions was Vermont. The next type of CCW law is a 'shall issue' law. In a 'shall issue' state, a permit is required to carry a concealed weapon, but state and local

authorities must issue a permit to any qualified applicant who requests one. This type of CCW law is not very restrictive. The third type of law is 'may issue'. In a 'may issue' state, local and state authorities can deny requests for concealed carry permits, even requests are from qualified applicants. This type of CCW law is considered restrictive. Finally, there some states that do not allow private citizens to carry concealed weapons. These states are known as 'no issue' or prohibited states. It is important to note that these four categories of CCW laws are rather broad, and not all states within a given category are equally restrictive. These laws vary in restrictiveness depending upon how states interpret and enforce their CCW statutes. In addition, some cities and counties have more restrictive concealed weapons laws than their home states.

In the present study, panel data controlling for both state and year fixed effects will be used to determine if state-level CCW laws and assault weapons bans had any effects on gun-related murder rates. Given that these laws are well-defined at the state level, and given that many states have altered these laws over the past 30 years, an analysis of the effects of CCW laws and assault weapons bans would be much more informative than an analysis of other types of gun control measures that few states have ever enacted and laws for which there has been little change over the past 30 years.

II. Literature Review

Although there have been numerous studies on the topic of gun control (Kwon *et al*., 1997; Kleck and Hogan, 1999; Miller *et al.*, 2002; Moorhouse and Wanner, 2006), research on assault weapons bans and CCW laws have been more limited. One of the few studies that examined assault weapons bans was Koper and Roth (2001). Using state-level data from 1970 to 1995, the authors found that the Federal ban had little to no effect on homicide rates associated with firearms and on gunshot wounds per victim.

Regarding CCW laws, Lott and Mustard (1997) found that states with 'shall issue' concealed weapons laws had lower crime rates than states with more restrictive gun laws. They found that 'shall issue' laws resulted in a 7.65% drop in murders and a 5% drop in rapes. Their research suggests that individuals would be less likely to commit crimes if they knew that many others may be carrying concealed weapons.

Other research on CCW laws have yielded mixed results. Three papers that corroborated the findings of Lott and Mustard (1997) were Bronars and Lott (1998), Bartley and Cohen (1998) and Moody (2001). Studies that contradicted the findings of Lott of Mustard include Ludwig (1998), Dezhbakhsh and Rubin (1998) and Donohue (2003).

The present study differs from this prior research in several ways. First, data for the period 1980 to 2009 is examined; this is one of the longest time periods examined in any research on assault weapons bans or CCW laws. Second, the gun-related murder rate is used as the dependent variable. The use of this crime rate is important because most other studies looked at violent crime rates or homicide rates. Violent crime rate data is not disaggregated into gun-related violent crime and non gun violent crime, and homicides include justifiable killings and state-sanctioned killings; hence, an analysis using these types of crime rates may result in spurious conclusions.

III. Empirical Technique and Data

In order to determine if concealed weapons laws and assault weapons bans had statistically-significant effects on gun-related murder rates, a fixed effects model that controls for both state-level and year effects is used. The dependent variable used was the state-level gun-related murder rate. The gun-related murder rate is the crime rate most affected by gun control measures, and hence is the most appropriate crime rate to use in an analysis of the effectiveness of gun control measures.

Regarding the explanatory variables, dummy variables for assault weapons bans and restrictive CCW laws were included in the regression model. For the CCW dummy variable, if a state prohibits concealed weapons or if it is 'may issue', then it is assumed to be restrictive and is denoted by a value of one. For the assault weapons dummy variable, if a state has an assault weapons law, then it is denoted by a one. Although the contents of these statutes may differ quite substantially between states, for the purposes of this study, it is assumed that states with these laws restrict firearm possession in some way. Finally, a dummy variable that equals one for the period 1994 to 2004 is included in order to control for the Federal assault weapons ban.

In addition to the gun control measures, it is assumed that murder rates are dependent upon state demographics and various other state-level socioeconomic factors. These control variables were selected based on their use in prior research.

State-level data on gun-related murder rates were obtained from the *Supplementary Homicide Reports* which are compiled by the United States Department of Justice. The murder rate is in terms of murders per 100 000 persons. Information on state-level assault weapons bans and CCW laws were obtained from Ludwig and Cook (2003), the Legal Community Against Violence, the National Rifle Association and the United States Bureau of Alcohol, Tobacco, Firearms and Explosives. All other state-level data were obtained from relevant Census Bureau reports.

IV. Results and Concluding Remarks

Results are presented on Table 1. The CCW dummy variable is significant and positive, but the assault weapons ban is insignificant. Given that the average gun-related murder rate over the period in question was 3.44, the results of the present study indicate that states with more restrictive CCW laws had gun-related murder rates that were 10% higher. In addition, the Federal assault weapons ban is significant and positive, indicating that murder rates were 19.3% higher when the Federal ban was in effect. These results corroborate the findings of Lott and Mustard (1997). These results suggest that, even after controlling for unobservable state and year fixed effects, limiting the ability to carry concealed weapons may cause murder rates to increase. There may, however, be other explanations for these results. Laws may be ineffective due to loopholes and exemptions. The most violent states may also have the toughest gun control measures. Further research is warranted in this area.

Table 1. Fixed effects regression gun-related murder rate

Constant	−3.02
	(–3.20)***
Assault weapons ban	−0.29
	(–1.57)
Federal assault weapons ban	0.66
	(2.42)**
Restrictive concealed carry laws	0.365
	(3.74)***
Proportion of population that is white	0.172
	(1.76)*
Proportion of population that is rural	1.93
	(3.97)***
Real per capita median income	0.00021
	(6.03)***
Proportion of population with college degree	−1.367
	(–1.20)
Unemployment rate	3.397
	(1.34)
Proportion of population >18 and <25	11.45
	(2.27)**
Proportion of population >24 and <35	−2.876
	(–0.91)
Per capita alcohol consumption	0.688
	(4.05)***

Notes: $R^2 = 0.797$.
Test statistics in parentheses.
* 5% < *p*-value < 10%; ** 1% < *p*-value < 5%; *** *p*-value < 1%.

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EXHIBIT "10"

JOHN R. LOTT, JR.

MORE GUNS LESS CRIME

THIRD EDITION

UNDERSTANDING **CRIME** AND **GUN CONTROL** LAWS

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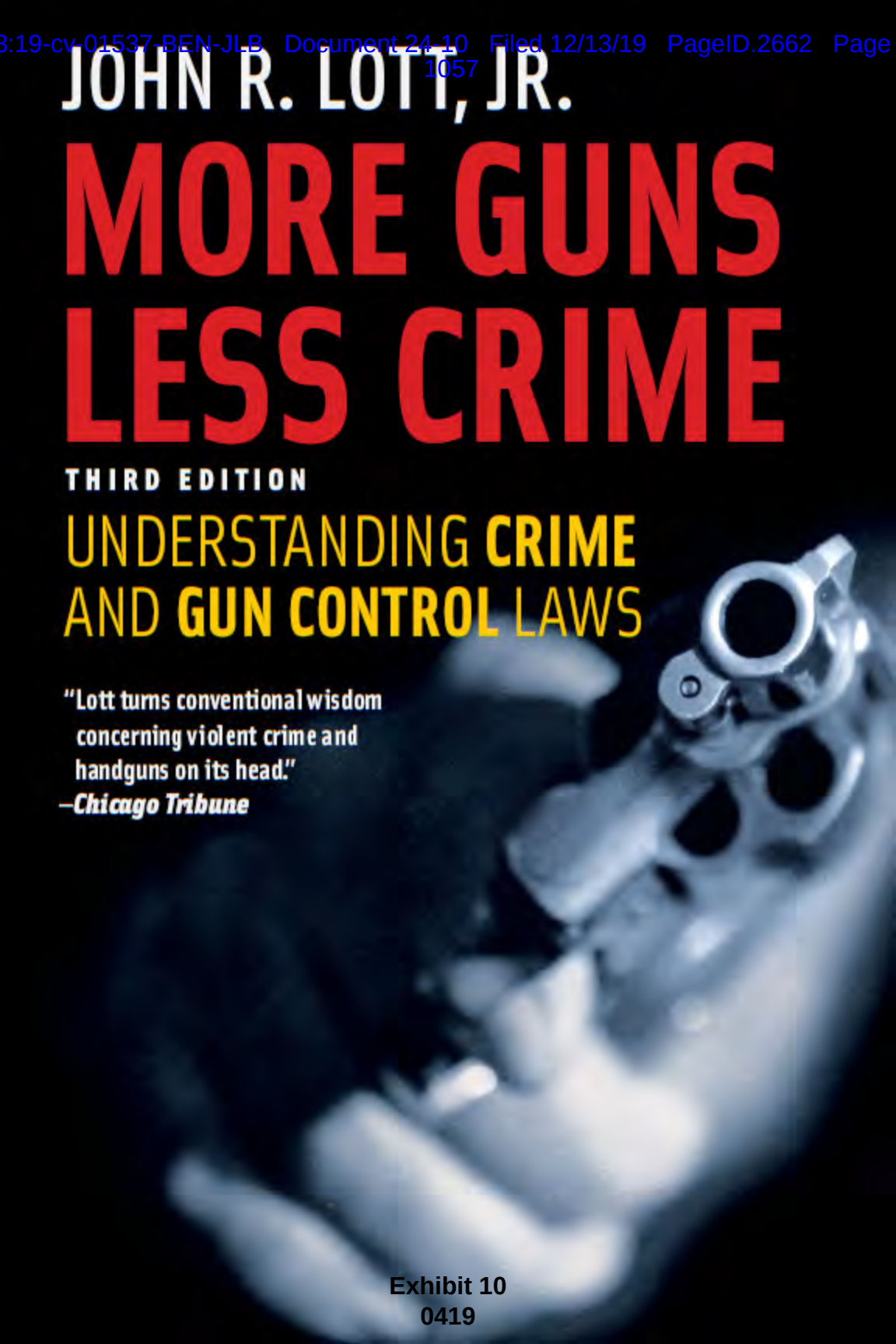

More Guns, Less Crime

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UNDERSTANDING **CRIME** *AND* **GUN-CONTROL** *LAWS*

THIRD EDITION

JOHN R. **LOTT,** JR.

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CONTENTS

PREFACE TO THE THIRD EDITION

Ten years have passed since the last edition of this book. Much has happened in those years. Now thirty-nine states have right-to-carry laws, a huge increase from the eighteen states with them when David Mustard and I first examined the relationship between such laws and crime. That original research in this book covered the sixteen years from 1977 through 1992. The second edition extended it by four years and covered twenty-eight states. Now, this new edition covers thirty-nine states and twenty-nine years from 1977 through 2005. That is a lot of crime data to study, especially with so many more states having adopted the law during the time.

By now, dozens of academics have published studies on right-to-carry laws using national data. These studies have either confirmed the beneficial link between gun ownership and crime or at least not found any indication that ownership *increases* crime. Not too surprisingly, depending on the precise methods used and the exact data set, the results have varied. Some claim no effect from these laws, but not a single refereed study finds the opposite result, that right-to-carry laws have a bad effect on crime.

Unfortunately, even normally level-headed scholars can get very emotional debating guns. Perhaps I am naive, but I have continued to be amazed by the great lengths people can go to attack others and to distort research. I had no idea of

the political intensity of the gun issue when I entered into this hornet's nest.

Right-to-carry laws are still the focus of this new edition, but I will address questions about some new laws as well. One of them is the Castle Doctrine, which states that it is not necessary for a would-be victim to retreat as far as possible before using a gun defensively. The federal assault weapons ban will also be studied. The federal assault weapons ban started in 1994 but ended in 2004. Rarely do we get a chance to look at the impact of gun laws when they are first passed and then when they are eliminated.

Not only have laws changed, but also the Supreme Court has ruled on important gun rights cases. A case of possible historic significance was the Supreme Court striking down the DC handgun ban in June 2008. The handgun ban in Chicago is currently being challenged in the courts, and it is possible that the Supreme Court will review that case also. Since the DC ruling, there has been a lot of renewed interest in the impact of gun bans on crime and suicide rates, and those same questions will be brought up again when deciding whether the Second Amendment applies to states as well as to areas controlled by the federal government, such DC.

The legal questions will also now focus on how much the government can regulate gun ownership and on the ability to carry guns. The courts will turn from the simple legal question of whether governments at any level can ban guns to more complicated questions of what specific regulations are to be allowed.

Since the second edition came out in 2000, I have continued working on gun-control issues. The research that John Whitley and I did on gun storage laws was published in the *Journal of Law and Economics* and is extended and updated in the new chapter. I have also updated previous research on multiple victim public shootings, work originally done together with Bill Landes. My previous work on gun show regulations and assault weapons has also been extended.

I have had a lot of help on these different projects. Research assistants have been extremely helpful with the very large data sets used in these studies. I need to thank Brian Blasé, James Knowles, and Maxim Lott for putting the new data together.

Finally, I would like to thank academics at George Mason University Law School, Chapman University Law School, the University of Florida, and the University of Miami for their helpful comments on the entirely new chapter added at the end of this book. I would also like to thank six anonymous referees for their comments.

PREFACE TO THE SECOND EDITION

The debate set off by this book was quite astonishing to me. Despite attacks early on when my paper was published in the *Journal of Legal Studies,* I was still rather unprepared for the publicity generated by the book in 1998. This expanded edition not only discusses the ensuing political debate and responds to the various criticisms, but also extends the data set to cover additional years. Replicating the results over additional years is important, so as to verify the original research. The new extended and broadened data set has also allowed me to study new gun laws, ranging from safe-storage provisions to one-gun-a-month purchase rules. It has also allowed me to extend my study of the Brady law and its impact to its first three years. Other extensions of the data set include entirely new city-level statistics, which made it possible to account more fully for policing policies.

Since I finished writing the first edition of this book in 1997, I have continued working on many related gun and crime issues. A new section of the book draws on continued research that I am conducting with numerous talented coauthors: William Landes on multiple-victim public shootings, John Whitley on safe-storage gun laws, and Kevin Cremin on police policies. Other work was published in the May 1998 *American Economic Review* under the title "Criminal Deterrence, Geographic Spillovers, and the Right to Carry

Concealed Handguns," coauthored with Stephen Bronars. Also, an article of mine, "The Concealed Handgun Debate," was published in the January 1998 issue of the *Journal of Legal Studies*.

I am grateful for the many opportunities to present my new research in a variety of academic forums and for the many useful comments that I have received. The research on guns and crime has been presented at (a partial listing) Arizona State University, Auburn University, the University of Chicago, Claremont Graduate School, the University of Houston, the University of Illinois, the University of Kansas, the University of Miami, New York University, the University of Oklahoma, the University of Southern California, Rice University, the University of Texas at Austin, the University of Texas at Dallas, the University of Virginia, the College of William and Mary, and Yeshiva University School of Law, as well as at the "Economics of Law Enforcement" Conference at Harvard Law School, the Association of American Law Schools meetings, the American Economic Association meetings, the American Society of Criminology meetings, the Midwestern Economic Association meetings, the National Lawyers Conference, the Southern Economic Association meetings, and the Western Economic Association meetings. Other presentations have been made at such places as the Chicago Crime Commission, the Kansas Koch Crime Commission, the American Enterprise Institute, and the Heritage Foundation.

Finally, I must thank the Yale Law School, where I am a senior research scholar, for providing me with the opportunity to write the new material that has been added to the book. I must also especially thank George Priest, who made this opportunity possible. The input of my wife and sons has been extremely important, and its importance has only been exceeded by their tolerance in putting up with the long working hours required to finish this revision.

PREFACE TO THE FIRST EDITION

Does allowing people to own or carry guns deter violent crime? Or does it simply cause more citizens to harm each other? Using the most comprehensive data set on crime yet assembled, this book examines the relationship between gun laws, arrest and conviction rates, the socioeconomic and demographic compositions of counties and states, and different rates of violent crime and property crime. The efficacy of the Brady Law, concealed-handgun laws, waiting periods, and background checks is evaluated for the first time using nationwide, county-level data.

The book begins with a description of the arguments for and against gun control and of how the claims should be tested. A large portion of the existing research is critically reviewed. Several chapters then empirically examine what facts influence the crime rate and answer the questions posed above. Finally, I respond to the political and academic attacks leveled against the original version of my work, which was published in the January 1997 issue of the *Journal of Legal Studies*.

I would like to thank my wife, Gertrud Fremling, for patiently reading and commenting on many early drafts of this book, and my four children for sitting through more dinnertime conversations on the topics covered here than anyone should be forced to endure. David Mustard also assisted me in collecting the data for the original article, which serves as the basis for some

of the discussions in chapters 4 and 5. Ongoing research with Steve Bronars and William Landes has contributed to this book. Maxim Lott provided valuable research assistance with the polling data.

For their comments on different portions of the work included in this book, I would like to thank Gary Becker, Steve Bronars, Clayton Cramer, Ed Glaeser, Hide Ichimura, Jon Karpoff, C. B. Kates, Gary Kleck, David Kopel, William Landes, Wally Mullin, Derek Neal, Dan Polsby, Robert Reed, Tom Smith, seminar participants at the University of Chicago (the Economics and Legal Organization, the Rational Choice, and Divinity School workshops), Harvard University, Yale University, Stanford University, Northwestern University, Emory University, Fordham University, Valparaiso University, the American Law and Economics Association Meetings, the American Society of Criminology, the Western Economic Association Meetings, and the Cato Institute. I also benefited from presentations at the annual conventions of the Illinois Police Association and the National Association of Treasury Agents. Further, I would like to express my appreciation to the John M. Olin Law and Economics Program at the University of Chicago Law School for its generous funding (a topic dealt with at length in chapter 7).

1 Introduction

American culture is a gun culture—not merely in the sense that in 2009 about 124 million people lived in households that owned a total of about 270 million guns,[1] but in the broader sense that guns pervade our debates on crime and are constantly present in movies and the news. How many times have we read about shootings, or how many times have we heard about tragic accidental gun deaths—bad guys shooting innocent victims, bad guys shooting each other in drug wars, shots fired in self-defense, police shootings of criminals, let alone shooting in wars? We are inundated by images through the television and the press. Our kids are fascinated by computer war games and toy guns.

So we're obsessed with guns. But the big question is: What do we really know? How many times have most of us actually used a gun or seen a gun being used? How many of us have ever seen somebody in real life threatening somebody else with a gun, witnessed a shooting, or seen people defend themselves by displaying or firing guns?

The truth is that most of us have very little firsthand experience with using guns as weapons. Even the vast majority of police officers have never exchanged shots with a suspect.[2] Most of us receive our images of guns and their use through television, film, and newspapers.

Unfortunately, the images from the screen and the newspapers are often unrepresentative

or biased because of the sensationalism and exaggeration typically employed to sell news and entertainment. A couple of instances of news reporting are especially instructive in illustrating this bias. In a highly publicized incident, a Dallas man became the first Texas resident charged with using a permitted concealed weapon in a fatal shooting.[3] Only long after the initial wave of publicity did the press report that the person had been savagely beaten and in fear for his life before firing the gun. In another case a Japanese student was shot on his way to a Halloween party in Louisiana in 1992. It made international headlines and showed how defensive gun use can go tragically wrong.[4] However, this incident was a rare event: in the entire United States during a year, only about 30 people are accidentally killed by private citizens who mistakenly believe the victim to be an intruder.[5] By comparison, police accidentally kill as many as 330 innocent individuals annually.[6] In neither the Louisiana case nor the Texas case did the courts find the shooting to be criminal.

While news stories sometimes chronicle the defensive uses of guns, such discussions are rare compared to those depicting violent crime committed with guns. Since in many defensive cases a handgun is simply brandished, and no one is harmed, many defensive uses are never even reported to the police. I believe that this underreporting of defensive gun use is large, and this belief has been confirmed by the many stories I received from people across the country after the publicity broke on my original study. On the roughly one hundred radio talk shows on which I discussed that study, many people called in to say that they believed having a gun to defend themselves with had saved their lives. For instance, on a Philadelphia radio station, a New Jersey woman told how two men simultaneously had tried to open both front doors of the car she was in. When she brandished her gun and yelled, the men backed away and fled. Given the stringent gun-control laws in New Jersey, the woman said she never thought seriously of reporting the attempted attack to the police.

Similarly, while I was on a trip to testify before the Nebraska Senate, John Haxby—a television newsman for the CBS affiliate in Omaha—privately revealed to me a frightening experience that he had faced in the summer of 1995 while visiting in Arizona. At about 10 A.M., while riding in a car with his brother at the wheel, they stopped for a red light. A man appeared wielding a "butcher's knife" and opened the passenger door, but just as he was lunging towards John, the attacker suddenly turned and ran away. As John turned to his brother, he saw that his brother was holding a hand-

gun. His brother was one of many who had acquired permits under the concealed-handgun law passed in Arizona the previous year.

Philip Van Cleave, a former reserve deputy sheriff in Texas, wrote me, "Are criminals afraid of a law-abiding citizen with a gun? You bet. Most cases of a criminal being scared off by an armed citizen are probably not reported. But I have seen a criminal who was so frightened of an armed, seventy-year-old woman that in his panic to get away, he turned and ran right into a wall! (He was busy trying to kick down her door, when she opened a curtain and pointed a gun at him.)"

Such stories are not limited to the United States. On February 3, 1996, outside a bar in Texcoco, Mexico (a city thirty miles east of Mexico City), a woman used a gun to stop a man from raping her. When the man lunged at the woman, "ripping her clothes and trying to rape her," she pulled a .22-caliber pistol from her purse and shot her attacker once in the chest, killing him.[7] The case generated much attention in Mexico when a judge initially refused to dismiss murder charges against the woman because she was viewed as being responsible for the attempted rape, having "enticed" the attacker "by having a drink with him at the bar."[8]

A national survey that I conducted during 2002 indicates that about 95 percent of the time that people use guns defensively, they merely have to brandish a weapon to break off an attack. Such stories are not hard to find: pizza deliverymen defend themselves against robbers, carjackings are thwarted, robberies at automatic teller machines are prevented, and numerous armed robberies on the streets and in stores are foiled,[9] though these do not receive the national coverage of other gun crimes.[10] Yet the cases covered by the news media are hardly typical; most encounters reported involve a shooting that ends in a fatality.[11]

A typical dramatic news story involved an Atlanta woman who prevented a carjacking and the kidnapping of her child; she was forced to shoot her assailant:

> A College Park woman shot and killed an armed man she says was trying to carjack her van with her and her 1-year-old daughter inside, police said Monday. . . .
>
> Jackson told police that the gunman accosted her as she drove into the parking lot of an apartment complex on Camp Creek Parkway. She had planned to watch a broadcast of the Evander Holyfield–Mike Tyson fight with friends at the complex.

> She fired after the man pointed a revolver at her and ordered her to "move over," she told police. She offered to take her daughter and give up the van, but the man refused, police said.
>
> "She was pleading with the guy to let her take the baby and leave the van, but he blocked the door," said College Park Detective Reed Pollard. "She was protecting herself and the baby."
>
> Jackson, who told police she bought the .44-caliber handgun in September after her home was burglarized, said she fired several shots from the gun, which she kept concealed in a canvas bag beside her car seat. "She didn't try to remove it," Pollard said. "She just fired."[12]

Although the mother saved herself and her baby by her quick actions, it was a risky situation that might have ended differently. Even though there was no police officer to help protect her or her child, defending herself was not necessarily the only alternative. She could have behaved passively, and the criminal might have changed his mind and simply taken the van, letting the mother and child go. Even if he had taken the child, he might later have let the baby go unharmed. Indeed, some conventional wisdom claims that the best approach is not to resist an attack. According to a recent *Los Angeles Times* article, "'active compliance' is the surest way to survive a robbery. Victims who engage in active resistance . . . have the best odds of hanging on to their property. Unfortunately, they also have much better odds of winding up dead."[13]

Yet the evidence suggests that the College Park woman probably engaged in the correct action. While resistance is generally associated with higher probabilities of serious injury to the victim, not all types of resistance are equally risky. By examining the data provided from 1979 to 1987 by the Department of Justice's National Crime Victimization Survey,[14] Lawrence Southwick, confirming earlier estimates by Gary Kleck, found that the probability of serious injury from an attack is 2.5 times greater for women offering no resistance than for women resisting with a gun. In contrast, the probability of women being seriously injured was almost 4 times greater when resisting without a gun than when resisting with a gun. In other words, the best advice is to resist with a gun, but if no gun is available, it is better to offer no resistance than to fight.[15]

Men also fare better with guns, but the benefits are substantially smaller. Behaving passively is 1.4 times more likely to result in serious injury than resisting with a gun. Male victims, like females, also run the greatest risk when they resist without a gun, yet the difference is again much smaller:

resistance without a gun is only 1.5 times as likely to result in serious injury than resistance with a gun. The much smaller difference for men reflects the fact that a gun produces a smaller change in a man's ability to defend himself than it does for a woman.

Although usually skewed toward the dramatic, news stories do shed light on how criminals think. Anecdotes about criminals who choose victims whom they perceive as weak are the most typical. While "weak" victims are frequently women and the elderly, this is not always the case. For example, in a taped conversation with police investigators reported in the *Cincinnati Enquirer* (October 9, 1996, p. B2), Darnell "Bubba" Lowery described how he and Walter "Fatman" Raglin robbed and murdered musician Michael Bany on December 29, 1995:

> Mr. Lowery said on the tape that he and Walter "Fatman" Raglin, who is also charged with aggravated robbery and aggravated murder and is on trial in another courtroom, had planned to rob a cab driver or a "dope boy."
>
> He said he gave his gun and bullets to Mr. Raglin. They decided against robbing a cab driver or drug dealer because both sometimes carried guns, he said.
>
> Instead, they saw a man walking across the parking lot with some kind of musical instrument. He said as he looked out for police, Mr. Raglin approached the man and asked for money.
>
> After getting the money, Mr. Raglin asked if the man's car was a stick or an automatic shift. Then Mr. Raglin shot the man.

Criminals are motivated by self-preservation, and handguns can therefore be a deterrent. The potential defensive nature of guns is further evidenced by the different rates of so-called "hot burglaries," where a resident is at home when a criminal strikes.[16] In Canada and Britain, both with tough gun-control laws, almost half of all burglaries are "hot burglaries." In contrast, the United States, with fewer restrictions, has a "hot burglary" rate of only 13 percent. Criminals are not just behaving differently by accident. Convicted American felons reveal in surveys that they are much more worried about armed victims than about running into the police.[17] The fear of potentially armed victims causes American burglars to spend more time than their foreign counterparts "casing" a house to ensure that nobody is home. Felons frequently comment in these interviews that they avoid late-night burglaries because "that's the way to get shot."[18]

To an economist such as myself, the notion of deterrence—which causes criminals to avoid cab drivers, "dope boys," or homes where the residents

are in—is not too surprising. We see the same basic relationships in all other areas of life: when the price of apples rises relative to that of oranges, people buy fewer apples and more oranges. To the non-economist, it may appear cold to make this comparison, but just as grocery shoppers switch to cheaper types of produce, criminals switch to attacking more vulnerable prey. Economists call this, appropriately enough, "the substitution effect."

Deterrence matters not only to those who actively take defensive actions. People who defend themselves may indirectly benefit other citizens. In the Cincinnati murder case just described, cab drivers and drug dealers who carry guns produce a benefit for cab drivers and drug dealers without guns. In the example involving "hot burglaries," homeowners who defend themselves make burglars generally wary of breaking into homes. These spillover effects are frequently referred to as "third-party effects" or "external benefits." In both cases criminals cannot know in advance who is armed.

The case for allowing concealed handguns—as opposed to openly carried handguns—relies on this argument. When guns are concealed, criminals are unable to tell whether the victim is armed before striking, which raises the risk to criminals of committing many types of crimes. On the other hand, with "open-carry" handgun laws, a potential victim's defensive ability is readily identified, which makes it easier for criminals to choose the more vulnerable prey. In interviews with felony prisoners in ten state correctional systems, 56 percent claimed that they would not attack a potential victim who was known to be armed. Indeed, the criminals in states with high civilian gun ownership were the most worried about encountering armed victims.[19]

Other examples suggest that more than just common crimes may be prevented by law-abiding citizens carrying concealed handguns. Referring to the July, 1984, massacre at a San Ysidro, California, McDonald's restaurant, Israeli criminologist Abraham Tennenbaum described

> what occurred at a [crowded venue in] Jerusalem some weeks before the California McDonald's massacre: three terrorists who attempted to machine-gun the throng managed to kill only one victim before being shot down by handgun-carrying Israelis. Presented to the press the next day, the surviving terrorist complained that his group had not realized that Israeli civilians were armed. The terrorists had planned to machine-gun a succession of crowd spots, thinking that they would be able to escape before the police or army could arrive to deal with them.[20]

On March 13, 1997, seven young seventh- and eighth-grade Israeli girls were shot to death by a Jordanian soldier while visiting Jordan's so-called Island of Peace. Reportedly, the Israelis had "complied with Jordanian requests to leave their weapons behind when they entered the border enclave. Otherwise, they might have been able to stop the shooting, several parents said."[21]

Obviously, arming citizens has not stopped terrorism in Israel; however, terrorists have responded to the relatively greater cost of shooting in public places by resorting to more bombings. This is exactly what the substitution effect discussed above would predict. Is Israel better off with bombings instead of mass public shootings? That is not completely clear, although one might point out that if the terrorists previously chose shooting attacks rather than bombings but now can only be effective by using bombs, their actions are limited in a way that should make terrorist attacks less effective (even if only slightly).[22]

Substitutability means that the most obvious explanations may not always be correct. For example, when the February 23, 1997, shooting at the Empire State Building left one person dead and six injured, it was not New York's gun laws but Florida's—where the gun was sold—that came under attack. New York City Mayor Rudolph W. Giuliani immediately called for national gun-licensing laws.[23] While it is possible that even stricter gun-sale regulations in Florida might have prevented this and other shootings, we might ask, Why did the gunman travel to New York rather than remain in Florida to do the shooting? And could someone intent on committing the crime and willing to travel to Florida still have gotten a gun illegally some other way? It is important to study whether states that adopt concealed-handgun laws similar to those in Israel experience the same virtual elimination of mass public shootings. Such states may also run the risk that would-be attackers will substitute bombings for shootings, though there is the same potential downside to successfully banning guns. The question still boils down to an empirical one: Which policy will save the largest number of lives?

The Numbers Debate and Crime

Unfortunately, the debate over crime involves many commonly accepted "facts" that simply are not true. For example, take the claim that individuals are frequently killed by people they know.[24] As shown in table 1.1, according to the FBI's *Uniform Crime Reports*, 58 percent of the country's murders

Table 1.1 Murderers and victims: relationship and characteristics

	Percent of cases involving the relationship	Percent of victims	Percent of offenders
Relationship		—	—
Family	18%		
Acquaintance (non-friend and friend)	40		
Stranger	13		
Unknown	30		
Total	101		
Race			
Black		38%	33%
White		54	42
Hispanic		2	2
Other		5	4
Unknown		1	19
Total		100	100
Sex			
Female		29	9
Male		71	72
Unknown		0	19
Total		100	100

Source: U.S. Dept. of Justice, FBI staff, *Uniform Crime Reports,* (Washington, DC: U.S. Govt. Printing Office, 1992
Note: Nonfriend acquaintances include drug pushers and buyers, gang members, prostitutes and their clients, bar customers, gamblers, cab drivers killed by their customers, neighbors, other nonfriend acquaintances, and friends. The total equals more than 100 percent because of rounding. The average age of victims was 33; that of offenders was 30.

were committed either by family members (18 percent) or by those who "knew" the victims (40 percent). Although the victims' relationship to their attackers could not be determined in 30 percent of the cases, 13 percent of all murders were committed by complete strangers.[25]

Surely the impression created by these numbers has been that most victims are murdered by close acquaintances. Yet this is far from the truth. In interpreting the numbers, one must understand how these classifications are made. In this case, "murderers who know their victims" is a very broad category. A huge but not clearly determined portion of this category includes rival gang members who know each other.[26] In larger urban areas, where most murders occur, the majority of murders are due to gang-related turf wars over drugs.

The Chicago Police Department, which keeps unusually detailed numbers on these crimes, finds that just 5 percent of all murders in the city from 1990 to 1995 were committed by nonfamily friends, neighbors, or roommates.[27] This is clearly important in understanding crime. The list of nonfriend acquaintance murderers is filled with cases in which the relationships would not be regarded by most people as particularly close: for

example, relationships between drug pushers and buyers, gang members, prostitutes and their clients, bar customers, gamblers, and cabdrivers killed by their customers.

While I do not wish to downplay domestic violence, most people do not envision gang members or drug buyers and pushers killing each other when they hear that 58 percent of murder victims were either relatives or acquaintances of their murderers.[28] If family members are included, no more than 17 percent of all murders in Chicago for 1990–95 involved family members, friends, neighbors, or roommates.[29] While the total number of murders in Chicago grew from 395 in 1965 to 814 in 1995, the number involving family members, friends, neighbors, or roommates remained virtually unchanged. What has grown is the number of murders by nonfriend acquaintances, strangers, identified gangs, and persons unknown.[30]

Few murderers could be classified as previously law-abiding citizens. In the largest seventy-five counties in the United States in 1988, over 89 percent of adult murderers had criminal records as adults.[31] Evidence for Boston, the one city where reliable data have been collected, shows that, from 1990 to 1994, 76 percent of juvenile murder victims and 77 percent of juveniles who murdered other juveniles had prior criminal arraignments.[32]

Claims of the large number of murders committed against acquaintances also create a misleading fear of those we know. To put it bluntly, criminals are not typical citizens. As is well known, young males from their mid-teens to mid-thirties commit a disproportionate share of crime,[33] but even this categorization can be substantially narrowed. We know that criminals tend to have low IQs as well as atypical personalities.

For example, delinquents generally tend to be more "assertive, unafraid, aggressive, unconventional, extroverted, and poorly socialized," while nondeliquents are "self-controlled, concerned about their relations with others, willing to be guided by social standards, and rich in internal feelings like insecurity, helplessness, love (or lack of love), and anxiety."[34] Other evidence indicates that criminals tend to be more impulsive and put relatively little weight on future events.[35] Finally, we cannot ignore the unfortunate fact that crime (particularly violent crime, and especially murder) is disproportionately committed against blacks by blacks.[36]

The news media also play an important role in shaping what we perceive as the greatest threats to our safety. Because we live in such a national news market, we learn very quickly about tragedies in other parts of the country.[37] As a result, some events appear to be much more common than they actually are. For instance, children are much less likely to be accidentally

killed by guns (particularly handguns) than most people think. Consider the following numbers: In 2006 there were a total of 642 accidental firearm deaths in the entire country. A relatively small portion of these involved children under age ten: 13 deaths involved children up to four years of age and 18 more deaths involved five- to nine-year-olds.[38] In comparison, 1,305 children died in motor-vehicle crashes and another 392 died when they were struck by motor vehicles, 651 died from drowning, and 348 were killed by fire and burns. Almost three times as many children drown in bathtubs each year than die from all types of firearm accidents.

Of course, any child's death is tragic, and it offers little consolation to point out that common fixtures in life from pools to heaters result in even more deaths. Yet the very rules that seek to save lives can result in more deaths. For example, banning swimming pools would help prevent drowning, and banning bicycles would eliminate bicycling accidents, but if fewer people exercise, life spans will be shortened. Heaters may start fires, but they also keep people from getting sick and from freezing to death. So whether we want to allow pools or space heaters depends not only on whether some people may end up being harmed, but also on whether more people are helped than hurt.

Similar trade-offs exist for gun-control issues, such as gun locks. As former president Clinton argued many times, "We protect aspirin bottles in this country better than we protect guns from accidents by children."[39] Yet gun locks require that guns be unloaded, and a locked, unloaded gun does not offer ready protection from intruders.[40] The debate is not simply over whether one wants to save lives or not. Rather, it involves the question of how many of these two hundred accidental gun deaths would have been avoided under different rules versus the extent to which such rules would have reduced the ability to defend against criminals. Without looking at data, one can only guess the net effects.[41] Unfortunately, despite the best intentions, evidence indicates that child-resistant bottle caps actually have resulted in "3,500 additional poisonings of children under age 5 annually from [aspirin-related drugs] . . . [as] consumers have been lulled into a less-safety-conscious mode of behavior by the existence of safety caps."[42] If President Clinton had been aware of such research, he surely wouldn't have referred to aspirin bottles when telling us how to deal with guns.[43]

Another common argument made in favor of banning guns involves the number of people who die from guns each year: there were 17,034 homicides and 18,169 suicides in 1992 alone.[44] Yet, just because a law is passed to ban guns, it does not automatically follow that the total number of deaths

will decline. Given the large stock of guns in the country and given the difficulties the government faces in preventing other illegal items, such as drugs, from entering the country, it is quite doubtful that the government would be able to eliminate most guns. This raises the important question: Would the law primarily reduce the number of guns held by law-abiding citizens? How would such a law alter the relative balance of power between criminals and law-abiding citizens?

Suppose it were indeed possible to remove all guns. Other problems are still likely to arise. Perhaps successfully removing guns would discourage murders and other crimes because criminals would find knives and clubs poor alternatives. But on the other hand it would be easier for criminals to prey on the weakest citizens, who would find it more difficult to defend themselves. Suicide raises other questions. It is simply not sufficient to point to the number of people who kill themselves with guns. The debate must be over what substitute methods are available and whether they appear sufficiently less attractive. Even evidence about the "success rate" of different methods of suicide is not enough, because we need to ask why people choose the method that they do. If people who were more intent than others on successfully killing themselves previously chose guns, forcing them to use other methods might raise the reported "success rate" for these other methods. Broader concerns for the general public also arise. For example, even if we banned many of the obvious ways of committing suicide, many methods exist that we could never really control. And these substitute methods might endanger others in ways that shootings do not. For example, deliberately crashing one's car, throwing oneself in front of a train, or jumping off a building.

This book attempts to measure this trade-off for guns. Our primary questions are the following: Will allowing citizens to carry concealed handguns mean that otherwise law-abiding people will harm each other? Will the threat of self-defense by citizens armed with guns primarily deter criminals? Without a doubt, both "bad" and "good" uses of guns occur. The question isn't really whether both occur; it is, rather: Which is more important? In general, do concealed handguns save or cost lives? Even a devoted believer in deterrence cannot answer this question without examining the data, because these two different effects clearly exist, and they work in opposite directions.

To some, however, the logic is fairly straightforward. Philip Cook argues that "if you introduce a gun into a violent encounter, it increases the chance that someone will die."[45] A large number of murders may arise from un-

intentional fits of rage that are quickly regretted, and simply keeping guns out of people's reach would prevent deaths.[46] Others point to the horrible public shootings that occur not just in the United States but in recent years around the world, from the two worst high school shootings in Germany to Mumbai, India, where 163 people were killed.

The survey evidence of defensive gun use weighs importantly in this debate. At the lowest end of these estimates, the U.S. Department of Justice's National Crime Victimization Survey reports that each year there are "only" 110,000 defensive uses of guns during assaults, robberies, and household burglaries.[47] Other national polls weight regions by population and thus have the advantage, unlike the National Crime Victimization Survey, of not relying too heavily on data from urban areas.[48] These national polls should also produce more honest answers, since a law-enforcement agency is not asking the questions.[49] They imply much higher defensive use rates. Fifteen national polls, including those by organizations such as the *Los Angeles Times*, Gallup, and Peter Hart Research Associates, imply that there are 760,000 defensive handgun uses to 3.6 million defensive uses of any type of gun per year.[50] Yet even if these estimates are wrong by a very large factor, they still suggest that defensive gun use is extremely common.

Some evidence on whether concealed-handgun laws will lead to increased crimes is readily available. Between October 1, 1987, when Florida's "concealed-carry" law took effect, and the end of 1996, over 380,000 licenses had been issued, and only 72 had been revoked because of crimes committed by license holders (most of which did not involve the permitted gun).[51] A statewide breakdown on the nature of those crimes is not available, but Dade County records indicate that four crimes involving a permitted handgun took place there between September 1987 and August 1992, and none of those cases resulted in injury.[52] Similarly, Multnomah County, Oregon, issued 11,140 permits over the period from January 1990 to October 1994; only five permit holders were involved in shootings, three of which were considered justified by grand juries. Of the other two cases, one involved a shooting in a domestic dispute, and the other involved an accident that occurred while a gun was being unloaded; neither resulted in a fatality.[53]

In Virginia, "Not a single Virginia permit-holder has been involved in violent crime."[54] In the first year following the enactment of concealed-carry legislation in Texas, more than 114,000 licenses were issued, and only 17 have so far been revoked by the Department of Public Safety (reasons not specified).[55] After Nevada's first year, "Law enforcement officials throughout

the state could not document one case of a fatality that resulted from irresponsible gun use by someone who obtained a permit under the new law."[56] Speaking for the Kentucky Chiefs of Police Association, Lt. Col. Bill Dorsey, Covington assistant police chief, concluded that after the law had been in effect for nine months, "We haven't seen any cases where a [concealed-carry] permit holder has committed an offense with a firearm,"[57] In North Carolina, "Permit-holding gun owners have not had a single permit revoked as a result of use of a gun in a crime."[58] Similarly, for South Carolina, "Only one person who has received a pistol permit since 1989 has been indicted on a felony charge, a comparison of permit and circuit court records shows. That charge, . . . for allegedly transferring stolen property last year, was dropped by prosecutors after evidence failed to support the charge."[59]

During state legislative hearings on concealed-handgun laws, the most commonly raised concerns involved fears that armed citizens would attack each other in the heat of the moment following car accidents or accidentally shoot a police officer. The evidence shows that such fears are unfounded: although thirty-one states had so-called nondiscretionary concealed-handgun laws when this book was first written, some of them decades old, there existed only one recorded incident of a permitted, concealed handgun being used in a shooting following a traffic accident, and that involved self-defense.[60] No permit holder has ever shot a police officer, and there have been cases where permit holders have used their guns to save officers' lives.

Let us return to the fundamental issue of self-protection. For many people, the ultimate concern boils down to protection from violence. Unfortunately, our legal system cannot provide people with all the protection that they desire, and yet individuals are often prevented from defending themselves. A particularly tragic event occurred in 1996 in Baltimore:

> Less than a year ago, James Edward Scott shot and wounded an intruder in the back yard of his West Baltimore home, and according to neighbors, authorities took away his gun.
>
> Tuesday night, someone apparently broke into his three-story row house again. But this time the 83-year-old Scott didn't have his .22-caliber rifle, and police said he was strangled when he confronted the burglar.
>
> "If he would have had the gun, he would be OK," said one neighbor who declined to give his name, fearing retribution from the attacker, who had not been arrested as of yesterday. . . .

> Neighbors said burglars repeatedly broke into Scott's home. Ruses [a neighbor] said Scott often talked about "the people who would harass him because he worked out back by himself."[61]

Others find themselves in a position in which either they no longer report attacks to the police when they have used a gun to defend themselves, or they no longer carry guns for self-defense. Josie Cash learned this lesson the hard way, though charges against her were ultimately dropped. "The Rockford [Illinois] woman used her gun to scare off muggers who tried to take her pizza delivery money. But when she reported the incident to police, they filed felony charges against her for carrying a concealed weapon."[62]

A well-known story involved Alan Berg, a liberal Denver talk-show host who took great delight in provoking and insulting those he disagreed with. Berg attempted to obtain a permit after receiving death threats from white supremacists, but the police first attempted to talk him out of applying and then ultimately rejected his request. Shortly after his request was denied, Berg was murdered by members of the Aryan Nations.[63]

As a Chicago cabdriver told me, "What good is a police officer going to do me if you pulled a knife or a gun on me right now?"[64] Nor are rural, low-crime areas immune from these concerns. Illinois State Representative Terry Deering (Democrat) noted that "we live in areas where if we have a state trooper on duty at any given time in a whole county, we feel very fortunate. Some counties in downstate rural Illinois don't even have 24-hour police protection."[65] The police cannot feasibly protect everybody all the time, and perhaps because of this, police officers are typically sympathetic to law-abiding citizens who own guns.[66]

Mail-in surveys are seldom accurate, because only those who feel intensely about an issue are likely to respond, but they provide the best information that we have on police officers' views. A 2005 mail survey of twenty-two thousand chiefs of police and sheriffs conducted by the National Association of Chiefs of Police found that 92 percent believed that law-abiding citizens should continue to be able to purchase guns for self-defense.[67] Sixty percent thought that a national concealed-handgun permit law will "reduce rates of violent crime." The Southern States Police Benevolent Association surveyed its eleven thousand members during June of 1993 (36 percent responded) and reported similar findings: 96 percent of those who responded agreed with the statement, "People should have the right to own a gun for self-protection," and 71 percent did not believe that stricter

handgun laws would reduce the number of violent crimes.[68] A national reader survey conducted in 1991 by *Law Enforcement Technology* magazine found that 76 percent of street officers and 59 percent of managerial officers agreed that all trained, responsible adults should be able to obtain handgun-carry permits.[69] By similarly overwhelming percentages, these officers and police chiefs rejected claims that the Brady law would lower the crime rate.

The passage of concealed-handgun laws has also caused former opponents in law enforcement to change their positions. Recently in Texas, "vocal opponent" Harris County District Attorney John Holmes admitted, "I'm eating a lot of crow on this issue. It's not something I necessarily like to do, but I'm doing it on this."[70] Soon after the implementation of the Florida law, the president and the executive director of the Florida Chiefs of Police and the head of the Florida Sheriff's Association all admitted that they had changed their views on the subject. They also admitted that despite their best efforts to document problems arising from the law, they have been unable to do so.[71] The experience in Kentucky has been similar; as Campbell County Sheriff John Dunn says, "I have changed my opinion of this [program]. Frankly, I anticipated a certain type of people applying to carry firearms, people I would be uncomfortable with being able to carry a concealed weapon. That has not been the case. These are all just everyday citizens who feel they need some protection."[72]

Support among rank-and-file police officers and the general population for the right of individuals to carry guns for self-protection is even higher than it is among police chiefs. A national poll by the Lawrence Research group (September 21—28, 1996) found that by a margin of 69 to 28 percent, registered voters favor "a law allowing law-abiding citizens to be issued a permit to carry a firearm for personal protection outside their home."[73] A recent national polling by the Zogby International (July 2009) appears even more supportive of at least allowing some law-abiding citizens to carry concealed handguns. They found that 83 percent supported "laws that allow residents to carry firearms to protect themselves," while only 11 opposed them.[74] Perhaps just as telling, a 2008 Gallup poll found that the percent of people who favor a ban on handguns had fallen to a fifty-year low.[75]

A National Opinion Research Center poll also provides some insights into who supports tighter restrictions on gun ownership; it claims that "the less educated and those who haven't been threatened with a gun are most supportive of gun control."[76] If this is true, it appears that those most supportive of restrictions also tend to be those least directly threatened by crime.[77]

State legislators also acknowledge the inability of the police to be always available, even in the most public places, by voting to allow themselves unusually broad rights to carry concealed handguns. During the 1996 legislative session, for example, Georgia "state legislators quietly gave themselves and a few top officials the right to carry concealed guns to places most residents can't: schools, churches, political rallies, and even the Capitol."[78] Even local prosecutors in California strenuously objected to restrictions on their rights to carry concealed handguns.[79]

Although people with concealed handgun permits must generally view the police as offering insufficient protection, it is difficult to discern any pattern of political orientation among celebrities who have concealed-handgun permits: Bill Cosby, Cybill Shepherd, Howard Stern, Donald Trump, Arthur O. Sulzberger (chairman of the *New York Times*), union bosses, Laurence Rockefeller, Tom Selleck, and Robert De Niro. The reasons these people gave on their applications for permits were similar. Laurence Rockefeller's reason was that he carries "large sums of money"; Arthur Sulzberger wrote that he carries "large sums of money, securities, etc."; and William Buckley listed "protection of personal property when traveling in and about the city" as his reason.[80] Some made their decision to carry a gun after being victims of crime.[81]

And when the *Denver Post* asked Sen. Ben Nighthorse Campbell (R-Colo.) "how it looks for a senator to be packing heat," he responded, "You'd be surprised how many senators have guns." Campbell said that "he needed the gun back in the days when he exhibited his Native American jewelry and traveled long distances between craft shows."[82]

Emotion, Rationality, and Deterrence

In 1995 two children, ten and eleven years old, dropped a five-year-old boy from the fourteenth floor of a vacant Chicago Housing Authority apartment.[83] The reason? The five-year-old refused to steal candy for them. Or consider the case of Vincent Drost, a promising musician in the process of composing a symphony, who was stabbed to death immediately after making a call from a pay telephone to his girlfriend. The reason? According to the newspapers, "His five teenage attackers told police they wanted to have some fun and simply wanted 'to do' somebody."[84] It is not difficult to find crimes such as "the fatal beating of a school teacher" described as "extremely wicked, shockingly evil." The defense attorney in this crime described the act as one of "insane jealousy."[85]

The notion of "irrational" crime is enshrined by forty-seven states that recognize insanity defenses.[86] Criminal law recognizes that emotions can overwhelm our normal judgments in other ways.[87] For example, under the Model Penal Code, intentional homicide results in the penalty for manslaughter when it "is committed under the influence of extreme mental or emotional disturbance for which there is reasonable explanation or excuse."[88] These mitigating factors are often discussed in terms of the "heat of passion" or "cooling time," the latter phrase referring to "the interval in which 'blood' can be expected 'to cool'" or the time required for "reason to reassert itself."[89] Another related distinction is drawn between first- and second-degree murder: "The deliberate killer is guilty of first-degree murder; the impulsive killer is not."[90] In practice, the distinction between these two grades appears to rely less on premeditation than on whether the act was done without emotion or "in cold blood," "as is the case [when] someone who kills for money . . . displays calculation and greed."[91]

Some academics go beyond these cases or laws to make more general claims about the motives behind crime. Thomas Carroll, an associate professor of sociology at the University of Missouri at Kansas City, states that "murder is an irrational act, [and] we don't have explanations for irrational behavior."[92] From this he draws the conclusion that "there's really no statistical explanation" for what causes murder rates to fluctuate. Do criminals respond to disincentives? Or are emotions and attitudes the determining factors in crime? If violent acts occur merely because of random emotions, stronger penalties would only reduce crime to the extent that the people least able to control such violent feelings can be imprisoned.

There are obvious difficulties with taking this argument against deterrence to its extreme. For example, as long as "even a handful" of criminals respond to deterrence, increasing penalties will reduce crime. Higher probabilities of arrest or conviction as well as longer prison terms might then possibly "pay" for themselves. As the cases in the previous section have illustrated, criminal decisions—from when to break into a residence, whom to attack, or whether to attack people by using guns or bombs—appear difficult to explain without reference to deterrence. Some researchers try to draw a distinction between crimes that they view as "more rational," like robbery and burglary, and others, such as murder. If such a distinction is valid, one might argue that deterrence would then at least be effective for the more "rational" crimes.

Yet even if we assume that most criminals are largely irrational, deterrence issues raise some tough questions about human nature, questions

that are at the heart of very different views of crime and how to combat it. Still it is important to draw a distinction between "irrational" behavior and the notion that deterrence doesn't matter. One doesn't necessarily imply the other. For instance, some people may hold strange, unfathomable objectives, but this does not mean that they cannot be discouraged from doing things that bring increasingly undesirable consequences. While we may not solve the deeper mysteries of how the human mind works, I hope that the following uncontroversial example can help show how deterrence works.

Suppose that a hypothetical Mr. Smith is passed over for promotion. He kept a stiff upper lip at work, but after he gets home, he kicks his dog. Now this might appear entirely irrational: the dog did not misbehave. Obviously, Mr. Smith got angry at his boss, but he took it out on his poor dog instead. Could we conclude that he is an emotional, irrational individual not responding to incentives? Hardly. The reason that he did not respond forcefully to his boss is probably that he feared the consequences. Expressing his anger at the boss might have resulted in being fired or passed up for future promotions. An alternative way to vent his frustration would have been to kick his co-workers or throw things around the office. But again, Mr. Smith chose not to engage in such behavior because of the likely consequences for his job and possible assault charges. In economic terms, the costs are too high. He managed to bottle up his anger until he gets home and kicks his dog. The dog is a "low-cost" victim.

Here lies the perplexity: the whole act may be viewed as highly irrational—after all, Mr. Smith doesn't truly accomplish anything. But still he tries to minimize the bad consequences of venting his anger. Perhaps we could label Mr. Smith's behavior as "semirational," a mixture of seemingly senseless emotion and rational behavior at the same time.

What about changing the set of punishments in the example above? What if Mr. Smith had a "killer dog," that bit anyone who abused it (equivalent to arming potential victims)? Or what if Mr. Smith were likely to be arrested and convicted for animal abuse? Several scenarios are plausible. First, he might have found another victim, perhaps a family member, to hit or kick. Or he might have modified his outwardly aggressive acts by merely yelling at family and neighbors or demolishing something. Or he might have repressed his anger—either by bottling up his frustration or finding some nonviolent substitute, such as watching a video, to help him forget the day's events.

Responding to disincentives is by no means limited to "rational" humans. Economists have produced a large number of studies that investigate whether animals take the costs of doing things into account.[93] Animal subjects have included both rats and pigeons, and the typical experiment measures the amount of some desired treat or standard laboratory food or fluid that is consumed in relation to the number of times the animal must push a lever to get the item. Other experiments alter the amount of the item received for a given number of lever pushes. These experiments have been tried in many different contexts. For example, does an animal's willingness to work for special treats, such as root beer or cherry cola, depend upon the existence of unlimited supplies of water or standard laboratory food? These experiments consistently show that as the "cost" of obtaining the food increases, the animal obtains less food. In economic terms, "Demand curves are downward sloping."

As for human beings, a large economics literature exists that overwhelmingly demonstrates that people commit fewer crimes if criminal penalties are more severe or more certain. Whether we consider the number of airliners hijacked in the 1970s,[94] evasion of the military draft,[95] or international data on violent and property crimes,[96] stiffer penalties or higher probabilities of conviction result in fewer violations of the law. Sociologists are more cautious, but the National Research Council of the U.S. National Academy of Sciences established the Panel on Research on Deterrent and Incapacitative Effects in 1978 to evaluate the many academic studies of deterrence. The panel concluded as follows: "Taken as a whole, the evidence consistently finds a negative association between crime rates and the risks of apprehension, conviction or imprisonment. . . . the evidence certainly favors a proposition supporting deterrence more than it favors one asserting that deterrence is absent."[97]

This debate on incentives and how people respond to them arises repeatedly in many different contexts. Take gun-buyback programs. Surely the intention of such programs is good, but why should we believe that they will greatly influence the number of guns on the street? True, the guns purchased are removed from circulation, and these programs may help to stigmatize gun ownership. Yet if they continue, one effect of such programs will be to increase the return to buying a gun. The price that a person is willing to pay for a gun today increases as the price for which it can be sold rises. In the extreme case, if the price offered in these gun-buyback programs ever became sufficiently high, people would simply buy guns

in order to sell them through these programs. This would hardly distress gun manufacturers. Empirical work on this question reveals no impact on crime from these programs.[98]

Introspection can go only so far. Ultimately, the issue of whether sanctions or other costs deter criminals can be decided only empirically. To what extent will concealed-handgun laws or gun-control laws raise these costs? To what extent will criminals be deterred by these costs? In chapter 2 we will consider how to test these questions.

An Overview

This book offers a critical review of the existing evidence on gun control and crime. The primary focus will be on whether gun laws save or cost lives.

To answer these questions I use a wide array of data. For instance, I have employed polls that allow us to track how gun ownership has changed over time in different states, as well as the massive FBI yearly crime rate data for all 3,054 U.S. counties from 1977 to 1992. I use additional, more recently available data for 1993 and 1994 later to check my results. Over the last decade, gun ownership has been growing for virtually all demographic groups, though the fastest growing group of gun owners is Republican women, thirty to forty-four years of age, who live in rural areas. National crime rates have been falling at the same time as gun ownership has been rising. Likewise, states experiencing the greatest reductions in crime are also the ones with the fastest growing percentages of gun ownership.

Overall, my conclusion is that criminals as a group tend to behave rationally—when crime becomes more difficult, less crime is committed. Higher arrest and conviction rates dramatically reduce crime. Criminals also move out of jurisdictions in which criminal deterrence increases. Yet criminals respond to more than just the actions taken by the police and the courts. Citizens can take private actions that also deter crime. Allowing citizens to carry concealed handguns reduces violent crimes, and the reductions coincide very closely with the number of concealed-handgun permits issued. Mass shootings in public places are reduced when law-abiding citizens are allowed to carry concealed handguns.

Not all crime categories showed reductions, however. Allowing concealed handguns might cause small increases in larceny and auto theft. When potential victims are able to arm themselves, some criminals turn away from crimes like robbery that require direct attacks and turn instead

to such crimes as auto theft, where the probability of direct contact with victims is small.

There were other surprises as well. While the support for the strictest gun-control laws is usually strongest in large cities, the largest drops in violent crime from legalized concealed handguns occurred in the most urban counties with the greatest populations and the highest crime rates. Given the limited resources available to law enforcement and our desire to spend those resources wisely to reduce crime, the results of my studies have implications for where police should concentrate their efforts. For example, I found that increasing arrest rates in the most crime-prone areas led to the greatest reductions in crime. Comparisons can also be made across different methods of fighting crime. Of all the methods studied so far by economists, the carrying of concealed handguns appears to be the most cost-effective method for reducing crime. Accident and suicide rates were unaltered by the presence of concealed handguns.

Guns also appear to be the great equalizer among the sexes. Murder rates decline when either more women or more men carry concealed handguns, but the effect is especially pronounced for women. One additional woman carrying a concealed handgun reduces the murder rate for women by about 3–4 times more than one additional man carrying a concealed handgun reduces the murder rate for men. This occurs because allowing a woman to defend herself changes her ability to defend herself much more than it would for a man. After all, men are usually bigger and stronger.

While some evidence indicates that increased penalties for using a gun in the commission of a crime reduce crime, the effect is small. Furthermore, I find no crime-reduction benefits from state-mandated waiting periods and background checks before allowing people to purchase guns. At the federal level, the Brady law has proven to be no more effective. Surprisingly, there is also little benefit from training requirements or age restrictions for concealed-handgun permits.

2 How to Test the Effects of Gun Control

The Existing Literature

Despite intense feelings on both sides of the gun debate, I believe everyone is at heart motivated by the same concerns: Will gun control increase or decrease the number of lives lost? Will these laws improve or degrade the quality of life when it comes to violent crime? The common fears we all share with regard to murders, rapes, robberies, and aggravated assaults motivate this discussion. Even those who debate the meaning of the Constitution's Second Amendment cannot help but be influenced by the answers to these questions.[1]

Anecdotal evidence is undoubtedly useful in understanding the issues at hand, but it has definite limits in developing public policy. Good arguments exist on both sides, and neither side has a monopoly on stories of tragedies that might have been avoided if the law had only been different. One side presents the details of a loved one senselessly murdered in a massacre like the April 2007 Virginia Tech shooting, where thirty-two people were killed. The other side points to an attack during a service at the New Life Church in Colorado with seven thousand people attending—an attack that was stopped by a concealed-carry permit holder.

Surveys have filled many important gaps in our knowledge; nevertheless, they suffer from many inherent problems. For example, how ac-

curately can a person judge whether the presence of a gun actually saved her life or whether it really prevented a criminal from attacking? Might people's policy preferences influence how they answer the pollster's questions? Other serious concerns arise with survey data. Does a criminal who is thwarted from committing one particular crime merely substitute another victim or another type of crime? Or might this general deterrence raise the costs of these undesirable activities enough so that some criminals stop committing crimes? Survey data just has not been able to answer such questions.

To study these issues more effectively, academics have turned to statistics on crime. Depending on what one counts as academic research, there are at least two hundred studies on gun control. The existing work falls into two categories, using either "time-series" or "cross-sectional" data. Time-series data deal with one particular area (a city, county, or state) over many years; cross-sectional data look across many different geographic areas within the same year. The vast majority of gun-control studies that examine time-series data present a comparison of the average murder rates before and after the change in laws; those that examine cross-sectional data compare murder rates across places with and without certain laws. Unfortunately, these studies make no attempt to relate fluctuations in crime rates to changing law-enforcement factors like arrest or conviction rates, prison-sentence lengths, or other obvious variables.

Both time-series and cross-sectional analyses have their limitations. Let us first examine the cross-sectional studies. Suppose, as happens to be true, that areas with the highest crime rates are the ones that most frequently adopt the most stringent gun-control laws. Even if restrictions on guns were to lower the crime rates, it might appear otherwise. Suppose crime rates were lowered, but not by enough to reach the level of rates in low-crime areas that did not adopt the laws. In that case, looking across areas would make it appear that stricter gun control produced higher crime. Would this be proof that stricter gun control caused higher crime? Hardly. Ideally, one should examine how the high-crime areas that adopted the controls changed over time—not only relative to their past levels but also relative to areas without the controls. Economists refer to this as an "endogeneity" problem. The adoption of the policy is a reaction (that is, "endogenous") to other events, in this case crime.[2] To correctly estimate the impact of a law on crime, one must be able to distinguish and isolate the influence of crime on the adoption of the law.

For time-series data, other problems arise. For example, while the ideal study accounts for other factors that may help explain changing crime rates,

a pure time-series study complicates such a task. Many potential causes of crime might fluctuate in any one jurisdiction over time, and it is very difficult to know which one of those changes might be responsible for the shifting crime rate. If two or more events occur at the same time in a particular jurisdiction, examining only that jurisdiction will not help us distinguish which event was responsible for the change in crime. Evidence is usually much stronger if a law changes in many different places at different times, and one can see whether similar crime patterns exist before and after such changes.

The solution to these problems is to combine both time-series and cross-sectional evidence and then allow separate variables, so that each year the national or regional changes in crime rates can be separated out and distinguished from any local deviations.[3] For example, crime may have fallen nationally between 1991 and 1992, but this study is able to identify whether there exists an additional decline over and above that national drop for states that have adopted concealed-handgun laws. I also use a set of measures that control for the average differences in crime rates across places even after demographic, income, and other factors have been accounted for. No previous gun-control studies had taken this approach when the first edition of this book was written.

The largest cross-sectional gun-control study examined 170 cities in 1980.[4] While this study controlled for many differences across cities, no variables were used to deal with issues of deterrence (such as arrest or conviction rates or prison-sentence lengths). It also suffered from the bias discussed above that these cross-sectional studies face in showing a positive relationship between gun control and crime.

The time-series work on gun control that has been most heavily cited by the media was done by three criminologists at the University of Maryland who looked at five different counties (one at a time) from three different states (three counties from Florida, one county from Mississippi, and one from Oregon) from 1973 to 1992 (though a different time period was used for Miami).[5] While this study has received a great deal of media attention, it suffers from serious problems. Even though these concealed-handgun laws were state laws, the authors say that they were primarily interested in studying the effect in urban areas. Yet they do not explain how they chose the particular counties used in their study. For example, why examine Tampa but not Fort Lauderdale, or Jacksonville but not Orlando? Like most previous studies, their research does not account for any other variables that might also help explain the crime rates.

Some cross-sectional studies have taken a different approach and used

the types of statistical techniques found in medical case studies. Possibly the best known paper was done by Arthur Kellermann and his many coauthors,[6] who purport to show that "keeping a gun in the home was strongly and independently associated with an increased risk of homicide."[7] The claim is that the gun will be more likely to kill someone the gun owner knows than the criminal. The data for this test consists of a "case sample" (444 homicides that occurred in the victim's homes in three counties) and a "control" group (388 "matched" individuals who lived near the deceased and were the same sex and race as well as the same age range). After information was obtained from relatives of the homicide victim or the control subjects regarding such things as whether they owned a gun or had a drug or alcohol problem, these authors attempted to see if the probability of a homicide was correlated with the ownership of a gun.

There are many problems with Kellermann et al.'s paper that undercut the misleading impression that victims were killed by the gun in the home. For example, they fail to report that in only 8 of these 444 homicide cases could it be established that the "gun involved had been kept in the home."[8] Counting only the deaths from defensive gun use also ignores the much larger number of effective defensive gun uses that don't require that the gun be fired. Indeed, in less than one out of every thousand defensive gun uses is the attacker killed. More important, the question posed by the authors cannot be tested properly using their chosen methodology because of the endogeneity problem discussed earlier with respect to cross-sectional data.

To demonstrate this, suppose that the same statistical method—with a matching control group—was used to do an analogous study on the efficacy of hospital care. Assume that we collected data just as these authors did; that is, we got a list of all the people who died in a particular county over the period of a year, and we asked their relatives whether they had been admitted to a hospital during the previous year. We would also put together a control sample with people of similar ages, sex, race, and neighborhoods, and ask these men and women whether they had been in a hospital during the past year. My bet is that we would find a very strong positive relationship between those who spent time in hospitals and those who died, quite probably a stronger relationship than in Kellermann's study on homicides and gun ownership. If so, would we take that as evidence that hospitals kill people? I would hope not. We would understand that, although our methods controlled for age, sex, race, and neighborhood, the people who had visited a hospital during the past year and the people in the "control" sample who did not visit a hospital were really not the same

types of people. The difference is pretty obvious: those hospitalized were undoubtedly sick, and thus it should come as no surprise that they would face a higher probability of dying.

The relationship between homicides and gun ownership is no different. The finding that those who are more likely to own guns suffer a higher homicide rate makes us ask, Why were they more likely to own guns? Could it be that they were at greater risk of being attacked? Is it possible that this difference arose because of a higher rate of illegal activities among those in the case study group than among those in the control group? Owning a gun could lower the probability of attack but still leave it higher than the probability faced by those who never felt the need to buy a gun to begin with. The fact that all or virtually all the homicide victims were killed by weapons brought into their homes by intruders makes this all the more plausible.

Unfortunately, the case study method was not designed for studying these types of social issues. Compare these endogeneity concerns with a laboratory experiment to test the effectiveness of a new drug. Some patients with the disease are provided with the drug, while others are given a placebo. The random assignment of who gets the drug and who receives the placebo is extremely important. A comparable approach to the link between homicide and guns would have researchers randomly place guns inside certain households and also randomly determine in which households guns would be forbidden. Who receives a gun would not be determined by other factors that might themselves be related to whether a person faces a high probability of being killed.

So how does one solve this causation problem? Think for a moment about the preceding hospital example. One approach would be to examine a change in something like the cost of going to hospitals. For example, if the cost of going to hospitals fell, one could see whether some people who would otherwise not have gone to the hospital would now seek help there. As we observed an increase in the number of people going to hospitals, we could then check to see whether this was associated with an increase or decrease in the number of deaths. By examining changes in hospital care prices, we could see what happens to people who now choose to go to the hospital and who were otherwise similar in terms of characteristics that would determine their probability of living.

Obviously, despite these concerns over previous work, only statistical evidence can reveal the net effect of gun laws on crimes and accidental deaths. The laws being studied here range from those that allow concealed-handgun permits to those demanding waiting periods or setting mandatory

minimum sentences for using a gun in the commission of a crime. Instead of just examining how crime changes in a particular city or state, I analyze the first systematic national evidence for all 3,054 counties in the United States over the sixteen years from 1977 to 1992 and ask whether these rules saved or cost lives. I attempt to control for a change in the price people face in defending themselves by looking at the change in the laws regarding the carrying of concealed handguns. I will also use the data to examine why certain states have adopted concealed-handgun laws while others have not.

This book is the first to study the questions of deterrence using these data. While many recent studies employ proxies for deterrence—such as police expenditures or general levels of imprisonment—I am able to use arrest rates by type of crime and also, for a subset of the data, conviction rates and sentence lengths by type of crime.[9] I also attempt to analyze a question noted but not empirically addressed in this literature: the concern over causality related to increases in both handgun use and crime rates. Do higher crime rates lead to increased handgun ownership or the reverse? The issue is more complicated than simply whether carrying concealed firearms reduces murders, because questions arise about whether criminals might substitute one type of crime for another as well as the extent to which accidental handgun deaths might increase.

The Impact of Concealed Handguns on Crime

Many economic studies have found evidence broadly consistent with the deterrent effect of punishment.[10] The notion is that the expected penalty affects the prospective criminal's desire to commit a crime. Expectations about the penalty include the probabilities of arrest and conviction, and the length of the prison sentence. It is reasonable to disentangle the probability of arrest from the probability of conviction, since accused individuals appear to suffer large reputational penalties simply from being arrested.[11] Likewise, conviction also imposes many different penalties (for example, lost licenses, lost voting rights, further reductions in earnings, and so on) even if the criminal is never sentenced to prison.[12]

While these points are well understood, the net effect of concealed-handgun laws is ambiguous and awaits testing that controls for other factors influencing the returns to crime. The first difficulty involves the availability of detailed county-level data on a variety of crimes in 3,054 counties during the period from 1977 to 1992. Unfortunately, for the time period we are studying, the FBI's *Uniform Crime Reports* include arrest-rate data but not conviction

rates or prison sentences. While I make use of the arrest-rate information, I include a separate variable for each county to account for the different average crime rates each county faces,[13] which admittedly constitutes a rather imperfect way to control for cross-county differences such as expected penalties.

Fortunately, however, alternative variables are available to help us measure changes in legal regimes that affect the crime rate. One such method is to use another crime category to explain the changes in the crime rate being studied. Ideally, one would pick a crime rate that moves with the crime rate being studied (presumably because of changes in the legal system or other social conditions that affect crime), but is unrelated to changes in laws regulating the right to carry firearms. Additional motivations for controlling other crime rates include James Q. Wilson's and George Kelling's "broken window" effect, where less serious crimes left undeterred will lead to more serious ones.[14] Finally, after telephoning law-enforcement officials in all fifty states, I was able to collect time-series, county-level conviction rates and mean prison-sentence lengths for three states (Arizona, Oregon, and Washington).

The FBI crime reports include seven categories of crime: murder and non-negligent manslaughter, rape, aggravated assault, robbery, auto theft, burglary, and larceny.[15] Two additional summary categories were included: violent crimes (including murder, rape, aggravated assault, and robbery) and property crimes (including auto theft, burglary, and larceny). Although they are widely reported measures in the press, these broader categories are somewhat problematic in that all crimes are given the same weight (for example, one murder equals one aggravated assault).

The most serious crimes also make up only a very small portion of this index and account for very little of the variation in the total number of violent crimes across counties (see table 2.1). For example, the average county has about eight murders, and counties differ from this number by an average of twelve murders. Obviously, the number of murders cannot be less than zero; the average difference is greater than the average simply because while 46 percent of the counties had no murders in 1992, some counties had a very large number of murders (forty-one counties had more than a hundred murders, and two counties had over one thousand murders). In comparison, the average county experienced 619 violent crimes, and counties differ from this amount by an average of 935. Not only does the murder rate contribute just a little more than 1 percent to the total number of violent crimes, but the average difference in murders across counties also explains just a little more than 1 percent of the differences in violent crimes across counties.

Table 2.1 The most common crimes and the variation in their prevalence across counties (1992)

	Average number of crimes	Percent of crime category	Dispersion	Percent of variation in general category due to each crime	Number of counties
Violent crime	619.1		934.50		2,853
Murder	7.8	1.3%	11.60	1.2%	2,954
Rape	35.4	5.7%	48.96	5.2%	2,853
Robbery	224.8	36.3%	380.70	40.7%	2,954
Aggravated assault	367.5	59.4%	534.80	57.2%	2,954
Property crime	4,078.2		5,672		2,954
Auto theft	533.9	13.1%	868	15.3%	2,954
Burglary	969.1	23.8%	1,331	23.4%	2,954
Larceny	2,575.2	63.1%	3.516	62.0%	2,954

Note: Dispersion provides a measure of variation for each crime category; it is a measure of the average difference between the overall average and each county's number of crimes. The total of the percents for specific crimes in the violent-crime category does not equal 100 percent because not all counties report consistent measures of rape. Other differences are due to rounding errors.

Even the narrower categories are somewhat broad for our purposes. For example, robbery includes not only street robberies, which seem the most likely to be affected by concealed-handgun laws, but also bank robberies, too. And for the bank robberies the additional return to permitting citizens to be armed would appear to be small because of the presence of armed guards.[16] Likewise, larceny involves crimes of "stealth," which includes those committed by pickpockets, purse snatchers, shoplifters, and bike thieves, and such crimes as theft from buildings, coin machines, and motor vehicles. However, while most of these fit the categories in which concealed-handgun laws are likely to do little to discourage criminals, pickpockets do come into direct contact with their victims.

This aggregation of crime categories makes it difficult to isolate crimes that might be deterred by increased handgun ownership and crimes that might be increasing as a result of a substitution effect. Generally, the crimes most likely to be deterred by concealed-handgun laws are those involving direct contact between the victim and the criminal, especially when they occur in places where victims otherwise would not be allowed to carry firearms. Aggravated assault, murder, robbery, and rape are both confrontational and likely to occur where guns were not previously allowed.

In contrast, crimes like auto theft of unattended cars seem unlikely to be deterred by gun ownership. While larceny is more debatable, in general—to the extent that these crimes actually involve "stealth"—the probability that victims will notice the crime being committed seems low, and thus the opportunities to use a gun are relatively rare. The effect on burglary is am-

biguous from a theoretical standpoint. It is true that if nondiscretionary laws cause more people to own a guns, burglars will face greater risks when breaking into houses, and this should reduce the number of burglaries. However, if some of those who already own guns now obtain right-to-carry permits, the relative cost of crimes like armed street robbery and certain other types of robberies (where an armed patron may be present) should rise relative to that for burglary or residential robbery. This may cause some criminals to engage in burglaries instead of armed street robbery. Indeed, a recent Texas poll suggests that such substitution may be substantial: 97 percent of first-time applicants for concealed-handgun permits already owned a handgun.[17]

Previous concealed-handgun studies that rely on state-level data suffer from an important potential problem: they ignore the heterogeneity within states.[18] From my telephone conversations with many law-enforcement officials, it has become very clear that there was a large variation across counties within a state in terms of how freely gun permits were granted to residents prior to the adoption of nondiscretionary right-to-carry laws.[19] All those I talked to strongly indicated that the most populous counties had previously adopted by far the most restrictive practices in issuing permits. The implication for existing studies is that simply using state-level data rather than county data will bias the results against finding any impact from passing right-to-carry provisions. Those counties that were unaffected by the law must be separated from those counties where the change could be quite dramatic. Even cross-sectional city data will not solve this problem, because without time-series data it is impossible to determine the impact of a change in the law for a particular city.[20]

There are two ways of handling this problem. First, for the national sample, one can see whether the passage of nondiscretionary right-to-carry laws produces systematically different effects in the high- and low-population counties. Second, for three states—Arizona, Oregon, and Pennsylvania—I acquired time-series data on the number of right-to-carry permits for each county. The normal difficulty with using data on the number of permits involves the question of causality: Do more permits make crimes more costly, or do higher crime rates lead to more permits? The change in the number of permits before and after the change in the state laws allows us to rank the counties on the basis of how restrictive they had actually been in issuing permits prior to the change in the law. Of course there is still the question of why the state concealed-handgun law changed, but since we are dealing with county-level rather than state-level data, we benefit from the fact that those counties with the most restrictive

policies regarding permits were also the most likely to have the new laws imposed upon them by the state.

Using county-level data also has another important advantage in that both crime and arrest rates vary widely within states. In fact, as indicated in table 2.2, the variation in both crime rates and arrest rates across states is almost always smaller than the average within-state variation across counties. With the exception of the rates for robbery, the variation in crime rates

Table 2.2 Comparing the variation in crime rates across states and across counties within states from 1977 to 1992

	Percent of variation across states relative to the average variation within states
Crime rates per 100,000 population	
Violent-crime rate	111%
Murder rate	75
Murder rate with guns (from 1982 to 1991)	61
Rape rate	69
Aggravated-assault rate	83
Robbery rate	166
Property-crime rate	66
Auto theft rate	74
Burglary rate	69
Larceny rate	61
Arrest rates (number of arrests divided by number of offenses)*	
Violent crimes	21
Murder	21
Rape	17
Robbery	21
Aggravated assault	32
Property crime	18
Burglary	23
Larceny	15
Auto theft	15
Truncating arrest rates to be no greater than one	
Violent crime	44
Murder	30
Rape	34
Robbery	25
Aggravated assault	41
Property crimes	43
Burglary	33
Larceny	46
Auto theft	31

Note: The percents are computed as the standard deviation of state means divided by the average within-state standard deviations across counties.

*Because of multiple arrests for a crime and because of the lags between the time when a crime occurs and the time an arrest takes place, the arrest rate for counties and states can be greater than one. This is much more likely to occur for counties than for states.

across states is from 61 to 83 percent of their average variation within states. (The difference in violent-crime rates arises because robberies make up such a large fraction of the total crimes in this category.) For arrest rates, the numbers are much more dramatic; the variation across states is as small as 15 percent of the average of the variation within states.

These results imply that it is no more accurate to view all the counties in the typical state as a homogenous unit than it is to view all the states in the United States as a homogenous unit. For example, when a state's arrest rate rises, it may make a big difference whether that increase is taking place in the most or least crime-prone counties. Widely differing estimates of the deterrent effect of increasing a state's average arrest rate may be made, depending on which types of counties are experiencing the changes in arrest rates and depending on how sensitive the crime rates are to arrest-rate changes in those particular counties. Aggregating these data may thus make it more difficult to discern the true relationship between deterrence and crime.

Another way of illustrating the differences between state and county data is simply to compare the counties with the highest and lowest crime rates to the states with the highest and lowest rates. Tables 2.3 and 2.4 list

Table 2.3 Murder rates: state and county variation in the states with the ten highest and ten lowest murder rates (1992)

States ranked by level of murder rate (10 highest; 10 lowest)	Murder rate per 100,000	County with highest murder rate	Highest county murder rate per 100,000	Number of counties with zero murder rate
Louisiana (1)	15.3	Orleans	57	5
New York (2)	13.2	Kings	28	13
Texas (3)	12.7	Delta	64	116
California (4)	12.66	Los Angeles	21	8
Maryland (5)	12.1	Baltimore	46	4
Illinois (6)	11.21	St. Clair	31	67
Arkansas (7)	10.8	Chicot	53	19
Georgia (8)	10.7	Taliaferro	224	62
North Carolina (9)	10.4	Graham	56	16
South Carolina (10)	10.35	Jasper	32	4
Nebraska (41)	3.2	Pierce	13	72
Utah (42)	2.99	Kane	20	15
Massachusetts (43)	2.97	Suffolk	12	2
Montana (44)	2.22	Meager	55	32
North Dakota (45)	1.9	Golden Valley	53	44
Maine (46)	1.7	Washington	5.5	7
New Hampshire (47)	1.5	Carroll	5.5	5
Iowa (48)	1.1	Wayne	14	71
Vermont (49)	0.7	Chittenden	2.2	9
South Dakota (50)	0.6	Bon Homme	14	49

Table 2.4 Rape rates: state and county variation in the states with the ten highest and ten lowest rape rates (1992)

States ranked by level of rape rate (10 highest; 10 lowest)	Rape rate per 100,000	County with highest rape rate	Highest county rape rate per 100,000	County with lowest rape rate	Lowest county rape rate per 100,000
Alaska (1)	98	North Slope	473	Matanuska-Susitina	14
Delaware (2)	86	Sussex	118	New Castle	74
Michigan (3)	79	Branch	198	Keweenaw	0
Washington (4)	71	Ferry	237	Garfield	0
South Carolina (5)	59	Dillon	97	2 counties	0
Nevada (6)	55	Washoe	82	5 counties	0
Florida (7)	53.7	Putnam	178	3 counties	0
Texas (8)	53.5	Rains	130	70 counties	0
Oregon (9)	53	Multnomah	95	3 counties	0
South Dakota (10)	50	Pennington	136	24 counties	0
Mississippi (41)	29	Harrison	108	11 counties	0
Pennsylvania (42)	27.4	Fulton	85	2 counties	0
Connecticut (43)	26.8	New Haven	38	Windham	1
Wisconsin (44)	26.4	Menominee	98	10 counties	0
North Dakota (45)	25	Morton	81	33 counties	0
Maine (46)	23	Franklin	41	Sagadahoc	0
West Virginia (47)	22	Cabell	99	8 counties	0
Montana (48)	21	Mineral	179	24 counties	0
Iowa (49)	13	Buchanan	62	40 counties	0
Vermont (50)	12	Chittenden	47	Orange	0

the ten safest and ten most dangerous states by murder and rape rates, along with those same crime rates for the safest and most dangerous counties in each state. (When rates were zero in more than one county, the number of counties is given.) Two conclusions are clear from these tables. First, even the states with the highest murder and rape rates have counties with no murders or rapes, and these counties in the most dangerous states are much safer than the safest states, according to the average state crime rates for the safest states. Second, while the counties with the highest murder rates tend to be well-known places like Orleans (New Orleans, Louisiana), Kings (Brooklyn, N.Y.), Los Angeles, and Baltimore, there are a few relatively small, rural counties that, for very short periods of time, garner the top spots in a state. The reverse is not true, however: counties with the lowest murder rates are always small, rural ones.

The two exceptions to this general situation are the two states with the highest rape rates: Alaska and Delaware. Alaska, possibly because of the imbalance of men and women in the population, has high rape rates over the entire state.[21] Even Matanuska-Susitina, which is the Alaskan borough

with the lowest rape rate, has a higher rape rate than either Iowa or Vermont. Delaware, which has a very narrow range between the highest and lowest county rape rates, is another exception. However, at least part of the reason for a nonzero rape rate in New Castle county (although this doesn't explain the overall high rape rate in the state) is that Delaware has only three counties, each with a relatively large population, and these large numbers virtually guarantee that some rapes take place.

Perhaps the relatively small across-state variation, as compared to within-state variations, is not so surprising, as states tend to average out differences when they include both rural and urban areas. Yet when coupled with the preceding discussion on the differing effects of concealed-handgun provisions on different counties in the same state, these numbers strongly imply that it is risky to assume that states are homogenous units with respect either to how crimes are punished or how the laws that affect gun usage are changed. Unfortunately, this emphasis on state-level data pervades the entire crime literature, which focuses on state- or city-level data and fails to recognize the differences between rural and urban counties.

However, using county-level data has some drawbacks. Because of the low crime rates in many low-population counties, it is quite common to find huge variations in the arrest and conviction rates from year to year. These variations arise both because the year in which the offense occurs frequently differs from the year in which the arrests and/or convictions occur, and because an offense may involve more than one offender. Unfortunately, the FBI data set allows us neither to link the years in which offenses and arrests occurred nor to link offenders with a particular crime. In counties where only a couple of murders occur annually, arrests or convictions can be many times higher than the number of offenses in a year. This data problem appears especially noticeable for counties with few people and for crimes that are relatively infrequent, like murder and rape.

One partial solution is to limit the sample to counties with large populations. Counties with a large number of crimes have a significantly smoother flow of arrests and convictions relative to offenses. An alternative solution is to take a moving average of the arrest or conviction rates over several years, though this reduces the length of the usable sample period, depending on how many years are used to compute this average. Furthermore, the moving-average solution does nothing to alleviate the effect of multiple suspects being arrested for a single crime.

Another concern is that otherwise law-abiding citizens may have carried concealed handguns even before it was legal to do so.[22] If nondiscretion-

ary laws do not alter the total number of concealed handguns carried by otherwise law-abiding citizens, but merely legalize their previous actions, passing these laws seems unlikely to affect crime rates. The only real effect from making concealed handguns legal could arise from people being more willing to use them to defend themselves, though this might also imply that they would be more likely to make mistakes in using them.

It is also possible that concealed-firearm laws both make individuals safer and increase crime rates at the same time. As Sam Peltzman has pointed out in the context of automobile safety regulations, increasing safety may lead drivers to offset these gains by taking more risks as they drive.[23] Indeed, recent studies indicate that drivers in cars equipped with air bags drive more recklessly and get into accidents at sufficiently higher rates to offset the life-saving effect of air bags for the driver and actually increase the total risk of death for others.[24] The same thing is possible with regard to crime. For example, allowing citizens to carry concealed firearms may encourage them to risk entering more dangerous neighborhoods or to begin traveling during times they previously avoided:

> Martha Hayden, a Dallas saleswoman, said the right-to-carry law introduced in Texas this year has turned her life around.
>
> She was pistol-whipped by a thief outside her home in 1993, suffering 300 stitches to the head, and said she was "terrified" of even taking out the garbage after the attack.
>
> But now she packs a .357 Smith and Wesson. "It gives me a sense of security; it allows you to get on with your life," she said.[25]

Staying inside her house may have reduced Ms. Hayden's probability of being assaulted again, but since her decision to engage in these riskier activities is a voluntary one, she at least believes that this is an acceptable risk. Likewise, society as a whole might be better off even if crime rates were to rise as a result of concealed-handgun laws.

Finally, we must also address the issues of why certain states adopted concealed-handgun laws and whether higher offense rates result in lower arrest rates. To the extent that states adopted the laws because crime was rising, econometric estimates that fail to account for this relationship will underpredict the drop in crime and perhaps improperly blame some of the higher crime rates on the measures taken to help solve the problem, such as increasing the police force. To explain this problem differently, crime rates may have risen even though concealed-handgun laws were passed, but the rates might have risen even higher if the laws had not been passed.

Likewise, if the laws were adopted when crime rates were falling, the bias would be in the opposite direction. None of the previous gun-control studies deal with this type of potential bias.[26]

The basic problem is one of causation. Does the change in the laws alter the crime rate, or does the change in the crime rate alter the law? Do higher crime rates lower the arrest rate or the reverse? Does the arrest rate really drive the changes in crime rates? And are any errors in measuring crime driving the relationship between crime and arrest rates? Fortunately, we can deal with these potential biases by using well-known techniques that let us see what relationships, if any still exist after we try to explain the arrest rates and the adoption of these laws. For example, we can see how arrest rates change in response to changes in crime rates and then examine to what extent the unexplained portion of the arrest rates helps explain the crime rate. We will find that accounting for these concerns actually strengthens the general initial findings. My general approach, however, is to examine first how concealed-handgun laws and crime rates, as well as arrest rates and crime rates, tend to move in comparison to one another before we try to deal with more complicated relationships.

3 Gun Ownership, Gun Laws, and the Data on Crime

Who Owns Guns?

Before studying what determines the crime rate, let's look at what types of people own guns and how this has been changing over time.

Information on gun ownership rates is difficult to obtain, and the only way to overcome this problem is to rely on surveys. The largest, most extensive polls are the exit polls conducted during the general elections every two years. Only these surveys interview enough people to get a useful estimate of gun ownership in individual states. Presidential election polls for 1988 and 1996 included a question on whether a person owned a gun, as well as information on the person's age, sex, race, income, place of residence, and political views. In 2004, a question on whether a person's family owned a gun was included. The available 1992, 2000, and 2008 survey data did not include a question on gun ownership.

Using the individual respondent data in the 1988 CBS News General Election Exit Poll and the 1996 Voter News Service National General Election Exit Poll, we can construct a very detailed description of the people who own guns. The Voter News Service poll collected data for a consortium of national news bureaus (CNN, CBS, ABC, NBC, Fox, and AP). I will soon discuss an exit poll survey from the 2004 presidential elec-

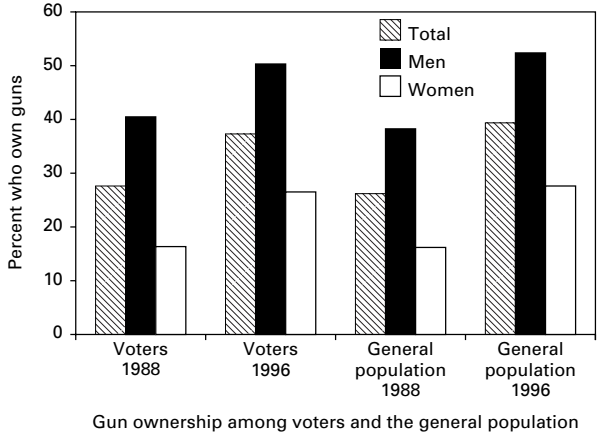


Figure 3.1. Percent of women and men who owned guns in 1988 and 1996: examining both voters and the general population

tion, but the gun ownership question is sufficiently different that I want to treat the 2004 survery separately.

What stands out immediately is the large increase in the number of people identifying themselves as gun owners (see figure 3.1). In 1988, 27.4 percent of voters owned guns.[1] By 1996, the number of voters owning guns had risen to 37 percent. In general, the percentages of voters and the general population who appear to own guns are extremely similar; among the general population, gun ownership rose from 26 to 39 percent,[2] which represented 76 million adults in 1996. Perhaps in retrospect, given all the news media discussions about high crime rates in the last couple of decades, this increase is not very surprising. Just as spending on private security has grown dramatically—reaching $82 billion in 1996, more than twice the amount spent in 1980 (even after taking into account inflation)—more people have been obtaining guns.[3] The large rise in gun sales that took place immediately before the Brady law went into effect in 1994 accounts for some of the increase.[4]

Three points must be made about these numbers. First, the form of the question changed somewhat between these two years. In 1988 people were asked, "Are you any of the following? (Check as many as apply)," and the list included "Gun Owner." In 1996 respondents were asked to record "yes" or "no" to the question, "Are you a gun owner?" This difference may have accounted for part, though not all, of the change.[5] Second, Tom Smith, director of the General Social Survey, told me he guessed that voters might own guns "by up to 5 percent more" than nonvoters, though this was difficult to know

for sure because in polls of the general population, over 60 percent of respondents claim to have voted, but we know that only around 50 percent did vote.[6] Given the size of the error in the General Social Survey regarding the percentage of those surveyed who were actual voters, it is nevertheless possible that nonvoters own guns by a few percentage points more than voters.[7]

Finally, there is strong reason to believe that women greatly underreport gun ownership. The most dramatic evidence of this arises from a comparison of the ownership rates for married men and married women. If the issue is whether women have immediate access to a gun in their house when they are threatened with a crime, it is the presence of a gun that is relevant, not ownership. For example, the 1988 poll data show that 20 percent of married women acknowledged owning a gun, which doesn't come close to the 47 percent figure reported for married men. Obviously, some women interpret this poll question literally regarding personal ownership as opposed to family ownership. If married women were assumed to own guns at the same rate as married men, the gun ownership rate in 1988 would increase from 27 to 36 percent.[8] Unfortunately, the 1996 data do not allow such a comparison, though presumably a similar effect is also occurring. The estimates reported in the figures do not attempt to adjust for these three considerations.

The other finding that stands out is that while some types of people are more likely than others to own guns, large numbers of people in all groups own guns. Almost one in four voters who identify themselves as liberals and almost one in three Democrats own a gun (see figure 3.2). The most typical gun owner may be a rural, white male, middle-aged or older, who is a conservative Republican earning between $30,000 and $75,000. Women, however, experienced the greatest growth in gun ownership during this eight-year period, with an increase of over 70 percent: between the years 1988 and 1996, women went from owning guns at 41 percent of the rate of men to over 53 percent.

High-income people are also more likely to own guns. In 1996, people earning over $100,000 per year were 7 percentage points more likely to own guns than those making less than $15,000. The gap between those earning $30,000 to $75,000 and those making less than $15,000 was over 10 percentage points. These differences in gun ownership between high- and low-income people changed little between the two polls.

When comparing these poll results with the information shown in table 1.1 on murder victims' and offenders' race, the poll results imply that, at least for blacks and whites, gun ownership does not explain why blacks have higher murder rates. For example, while white gun ownership exceeds that

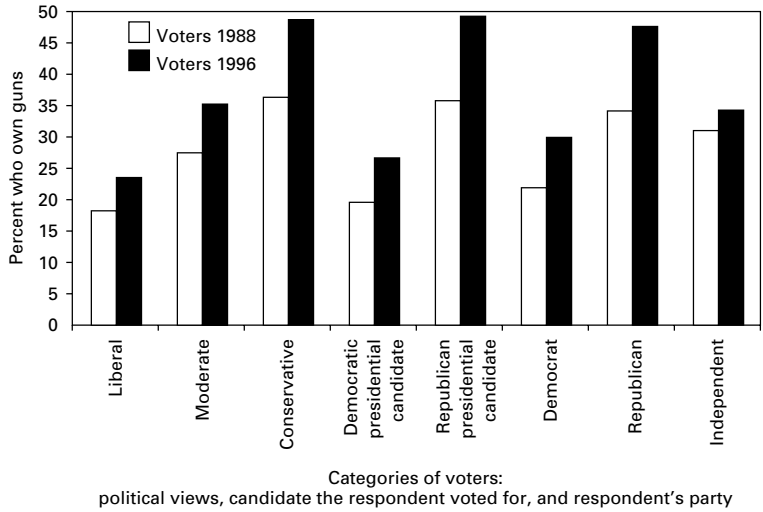


Figure 3.2. Percent of different groups of voters who owned guns in 1988 and 1996

for blacks by about 40 percent in 1996 (see figure 3.3), and the vast majority of violent crimes are committed against members of the offender's own racial group, blacks are 4.6 times more likely to be murdered and 5.1 times more likely to be offenders than are whites. Even if blacks underreported their gun ownership rate, it still couldn't explain the gap in crime rates. Even a 100 percent gun ownership rate among blacks would still leave a gap in gun ownership that is smaller than the gap in crime rates.

The polls also indicate that families that included union members tended to own guns at relatively high and more quickly growing rates (see figure 3.3). While the income categories in these polls varied across the two years, it is clear that gun ownership increased across all ranges of income. In fact, of the categories examined, only one experienced declines in gun ownership—people living in urban areas with a population of over 500,000 (see figure 3.4). Not too surprisingly, while rural areas have the highest gun ownership rates and the lowest crime rates, cities with more than 500,000 people have the lowest gun ownership rates and the highest crime rates (for example, in 1993 cities with over 500,000 people had murder rates that were over 60 percent higher than the rates in cities with populations between 50,000 and 500,000).

For a subset of the relatively large states, the polls include enough respondents to provide a fairly accurate description of gun ownership even at the state level, as shown in table 3.1. The 1988 survey was extensive enough

to provide us with over 1,000 respondents for twenty-one states, and over 770 respondents for three other states. The 1996 survey was less extensive, with only fourteen of the states surveyed having at least 100 respondents. Since these fourteen states were relatively more urban, they tended to have lower gun ownership rates than the nation as a whole.

The polls show that the increase in gun ownership was nationwide and not limited to any particular group. Of the fourteen states with enough respondents to make state-level comparisons, thirteen states had more people owning guns in 1996 than 1988. Six states each had over a million

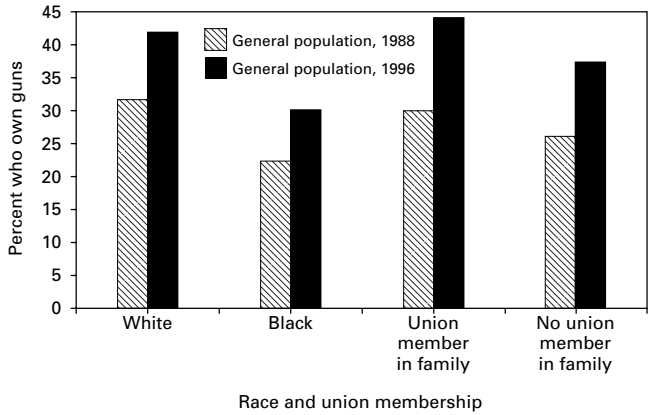


Figure 3.3. Percent of people by race and by union membership who own guns

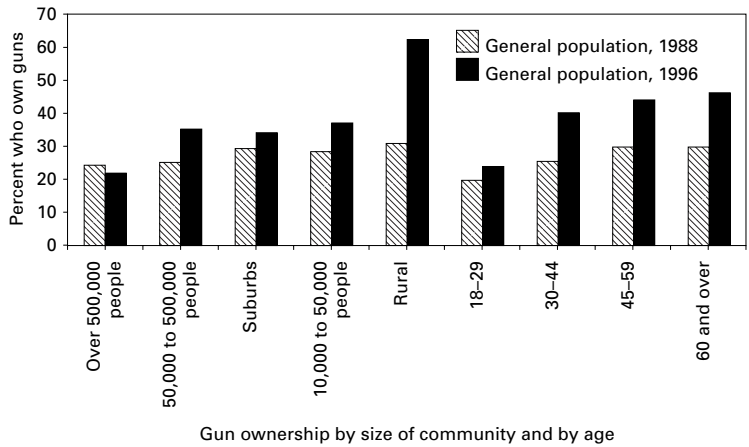


Figure 3.4. Percent of people living in different-size communities and in different age groups who owned guns in 1988 and 1996

Table 3.1 Gun-ownership rates by state

	CBS General Election Exit Poll (November 8, 1988) surveyed 34,245 people			Voter News Service General Election Exit Poll (November 5, 1996) surveyed 3,818 people			Estimated change in states over time	
State	Percent of voting population owning a gun[1] (1)	Percent of state's adults owning a gun[1] (2)	Estimated number of adults owning a gun, using column 2 (3)	Percent of voting population owning a gun[2] (4)	Percent of state's adults owning a gun[2] (5)	Estimated number of adults owning a gun, using column 5 (6)	Change in percent of adults owning a gun (7)	Change in the number of adults owning a gun (8)
United States	27.4%	26%	47.3 million	37%	38.9%	76.7 million	12.9%	29.4 million
California	23%	21%	6 million	33%	32%	10 million	11%	4 million
Connecticut	14%	10%	337,000	10%	12%	377,000	2%	40,000
Florida	28%	29%	3.6 million	35%	31%	4.4 million	2%	800,000
Illinois	19%	17%	1.9 million	34%	36%	4.3 million	19%	2.4 million
Indiana	29%	32%	1.74 million	32%	31%	1.8 million	–1%	60,000
Iowa	29%	31%	847,000	—	—	—	—	—
Maryland	23%	22%	1 million	—	—	—	—	—
Massachusetts	15%	16%	951,000	12%	11%	638,000	–5%	–313,000
Michigan	27%	28%	2.5 million	38%	37%	3.5 million	9%	1 million
Minnesota	33%	28%	1.2 million	—	—	—	—	—
Mississippi	40%	40%	1 million	—	—	—	—	—
Missouri	37%	31%	1.6 million	—	—	—	—	—

Nevada	30%	38%	404,000	—	—	—	—	—
New Jersey	12%	11%	810,000	14%	13%	1.04 million	2%	230,000
New Mexico	38%	41%	608,000	—	—	—	—	—
New York	13%	11%	2 million	20%	18%	3.3 million	7%	1.3 million
North Carolina	35%	32%	2.1 million	45%	43%	4.8 million	11%	2.7 million
Ohio	25%	28%	2.97 million	32%	32%	3.57 million	4%	600,000
Oregon	40%	36%	996,000	—	—	—	—	—
Pennsylvania	24%	19%	2.2 million	30%	29%	3.5 million	10%	1.3 million
Texas	38%	37%	6.1 million	34%	34%	6.4 million	–3%	300,000
Vermont	34%	35%	193,000	—	—	—	—	—
Washington	33%	31%	1.5 million	—	—	—	—	—
Wisconsin	29%	29%	1.4 million	43%	45%	2.3 million	16%	900,000

Source: The polls used are the General Election Exit Polls from CBS (1988) and Voter News Service (1996). The estimated percent of the voting population owning a gun is obtained by using the weighting of responses supplied by the polling organizations. The estimated percent of the general population owning a gun uses a weight that I constructed from the census to account for the difference between the percentage of males and females; whites, blacks, Hispanics and others; and these groups by age categories that are in the voting population relative to the actual state-level populations recorded by the census.

[1]State poll numbers based upon at least 770 respondents per state.

[2]State poll numbers based upon at least one hundred respondents per state. Other states were surveyed, but the number of respondents in each state was too small to provide an accurate measure of gun ownership. These responses were still useful in determining the national ownership rate, even if they were not sufficient to help determine the rate in an individual state.

more people owning guns. Only Massachusetts saw a decline in gun ownership.

States differ significantly in the percentage of people who own guns. On the lower end in 1988, in states like New York, New Jersey, and Connecticut, only 10 or 11 percent of the population owned guns. Despite its reputation, Texas no longer ranks first in gun ownership; California currently takes that title—approximately 10 million of its citizens own guns. In fact, the percentage of people who own guns in Texas is now below the national average.

National Election Pool survey data are available for the 2004 presidential election,[9] but there are two problems. First, the gun ownership survey question changes significantly. Instead of asking whether an individual personally owns a gun, the question is now "Do you or does someone else in your household own a gun?" As noted previously, many respondents in the earlier surveys may have indicated that they owned a gun simply because there was a gun in the home even if the gun was technically owned by another person in the household. Presumably asking if a gun is owned in the household will cause more people to answer "yes" than they had to the question of whether they personally own a gun, but how large the effect is remains an empirical question.

Just as important, there is strong evidence that Republicans and conservatives "refused to be interviewed [by the exit pollsters] in disproportionately higher numbers, thus skewing the results."[10] A similar problem appears to have occurred with the 2000 general voter exit poll, but a study by those who conducted the exit poll concluded: "[This systematic refusal] was higher in 2004 than in previous years for which we have data."[11] This skewness in the survey data generated quite a controversy after the 2004 presidential election, as John Kerry supporters argued that the difference between the actual vote totals for their candidate and what had been predicted by the exit poll survey implied some type of fraud had occurred. Since Republicans and conservatives are much more likely to own guns than the general population, this last bias works to understate the percent of the overall population that owns guns.[12]

There is some evidence that changing questions on gun ownership significantly affected the rate that people say that they own a gun. If the difference between married men and married women had remained unchanged, it would suggest that the results are not greatly affected. While there is still a ten-percentage-point gap between the rates that married men and women report that there are guns in their household (53 to 43 percent), it is quite

a bit smaller than the twenty-seven-percentage point gap (a difference of more than 2 to 1) shown for 1988. I would have greatly preferred making this comparison to 1996, where the overall poll results are more comparable, but it still seems likely that changing the question did cause more people (primarily women) to answer "yes" to the survey question in 2004.

In any case, both the change in questions and the higher rate at which Republicans and conservatives refused to answer survey questions make it difficult to directly compare the 2004 exit polls with those from 1988 and 1996. However, since one error works to overstate gun ownership while another works to understate it, without additional information it is not clear whether these errors work to overstate or understate the true gun ownership rate. With that in mind, it makes more sense to limit the 2004 survey results to making comparisons between different groups in the 2004 sample than to compare the changes between 1996 and 2004.

Given these caveats, the poll results shown in figures 3.5a and 3.5b indicate that 41 percent of Americans live in households with guns (this is slightly higher than the 37 percent who reported owning a gun in 1996). The demographic patterns are very similar to patterns shown for 1988 and 1996. For all the categories, the relative gun ownership rankings of the different groups in 2004 are exactly the same as they were in 1996. Men are more likely than women to say that they live in a household with guns. Conservatives are more likely than moderates to own guns and moderates more likely than liberals, and the same pattern occurs whether they voted for the Democratic or Republican presidential candidate and regardless of political affiliation.

The male/female ratio of gun ownership in 2004 remains virtually the same as in 1996. The percent of married women with guns rose at the same time that it fell for single women. Gun ownership in the most urbanized areas has increased relative to ownership in rural areas (urban areas increased from being 32 percent of the level of rural areas to 41 percent), and white gun ownership has increased slightly relative to black ownership (from 40 percent more to 49 percent more).

The one category of comparisons used earlier that cannot be made here involves gun ownership in union households. Not all respondents were asked whether their household owned guns or whether someone in their household belonged to a union, and there was no overlap between the two samples.

Table 3.2 shows the breakdown in gun ownership rates by state for the thirty-eight states where at least 100 people were asked whether their

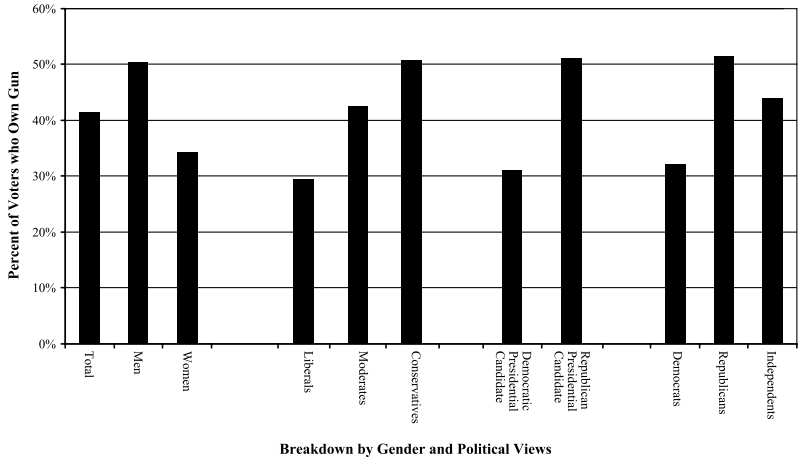


Figure 3.5a. Percent of voters who own guns by gender and political views

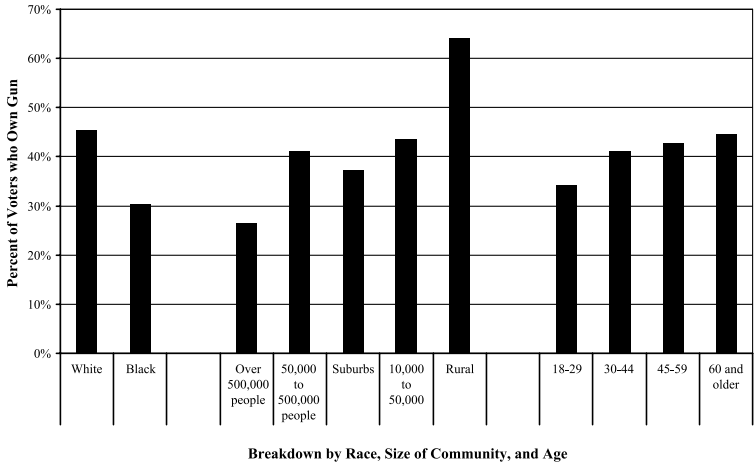


Figure 3.5b. Percent of voters who own guns by race, size of community, and age

household owns a gun. The results show a much wider range of gun ownership than was observed in either of the two previous surveys—ranging from 87 percent in Nebraska to 13 percent in Rhode Island. The seven states with the highest gun ownership rates and the two states with the lowest rates in 2004 had samples that were too small to be included in either of the two previous years. With the exception of slight drops in gun ownership rates in California, Illinois, and New York, all the other states showed an increase from 1996 to 2004.

Table 3.2 Gun ownership rate by state in 2004, using the question in the 2004 presidential exit poll survey "Do you or does someone else in your household own a gun?"

State	Gun ownership rate
Alabama	58%
Arizona	40%
California	31%
Colorado	33%
Connecticut	27%
Delaware	16%
Florida	39%
Georgia	50%
Idaho	47%
Illinois	30%
Indiana	60%
Iowa	59%
Kansas	62%
Louisana	65%
Maine	32%
Maryland	34%
Massachusetts	22%
Michigan	56%
Minnesota	44%
Missouri	45%
Montana	84%
Nebraska	87%
New Hampshire	37%
New Jersey	30%
New York	18%
North Carolina	56%
Ohio	35%
Oklahoma	71%
Oregon	44%
Pennsylvania	33%
Rhode Island	13%
South Carolina	34%
Tennessee	49%
Texas	48%
Utah	69%
Virginia	32%
Washington	56%
Wisconsin	44%
All States	41%

Understanding Different Gun Laws and Crime Rate Data

While murder rates have exhibited no clear trend over the last twenty years, they are currently 60 percent higher than in 1965.[13] Driven by substantial increases in rapes, robberies, and aggravated assaults, violent crime was 46 percent higher in 1995 than in 1976 and 240 percent higher than in 1965. As shown in figure 3.6, violent-crime rates peaked in 1991, but, with the exception of murder, they are still substantially above the rates in previous decades.

Such high violent-crime rates make people quite concerned about crime, and even the recent declines have not allayed their fears. By 2007, stories of people who had used guns to defend themselves had helped motivate thirty-nine states to adopt nondiscretionary (also referred to as "shall-issue," "right-to-carry," or "do-issue") concealed-handgun laws, which require law-enforcement officials or a licensing agency to issue, without subjective discretion, concealed-weapons permits to all qualified applicants (see figures 3.7a and 3.7b for the state laws in 1996 and 2007). This constitutes a dramatic increase from the eight states that had enacted nondiscretionary concealed-weapons laws prior to 1985. The requirements that must be met vary by state, and generally include the following: lack of a significant criminal record, an age restriction of either 18 or 21, various fees, training, and a lack of significant mental illness. The first three requirements, regarding criminal record, age, and payment of a fee, are the most common. Two states, Vermont and Idaho (with the exception of Boise), do not require permits, though the laws against convicted felons carrying guns still apply. In contrast, discretionary laws allow local law-enforcement officials or judges to make case-by-case decisions about whether to grant permits, based on the applicant's ability to prove a "compelling need."

When the data set used in this book was originally put together, county-level crime data was available for the period between 1977 and 1992. During that time, ten states—Florida (1987), Georgia (1989), Idaho (1990), Maine (1985),[14] Mississippi (1990), Montana (1991), Oregon (1990), Pennsylvania (1989), Virginia (1988),[15] and West Virginia (1989)—adopted nondiscretionary right-to-carry firearm laws. Pennsylvania is a special case because Philadelphia was exempted from the state law during the sample period, though people with permits from the surrounding Pennsylvania counties were allowed to carry concealed handguns into the city. Eight other states (Alabama, Connecticut, Indiana, New Hampshire, North Dakota, South Dakota, Vermont, and Washington) have had right-to-carry laws on the

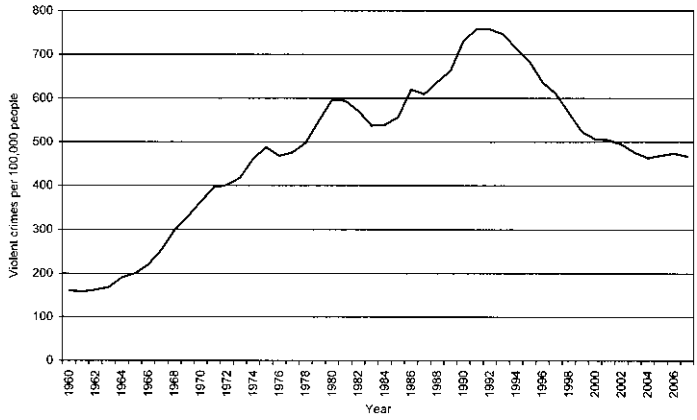
books for decades.[16] Between 1993 and 2007, twenty-one additional states adopted shall-issue laws. The last two chapters of this book will analyze later changes.

Keeping in mind all the serious causation problems discussed earlier for cross-sectional data, table 3.3 provides a first and very superficial look at the data for the last year originally examined in this book (1992) as well as the last year that FBI crime rate data are available. The table shows how crime rates varied with the type of concealed-handgun law. Despite the problem with cross-sectional data, according to the data presented in the table for 1992, the difference is quite suggestive: violent crimes are 81 percent higher in states without nondiscretionary laws. For murder, states that ban the concealed carrying of guns have murder rates 127 percent higher than states with the most liberal concealed-carry laws. After almost all the states have adopted these laws in 2007, the difference is much smaller: just 25 percent for violent crime and 28 percent for murder. States with nondiscretionary laws have less violent crime, but the differences for property crimes are smaller and less consistent.

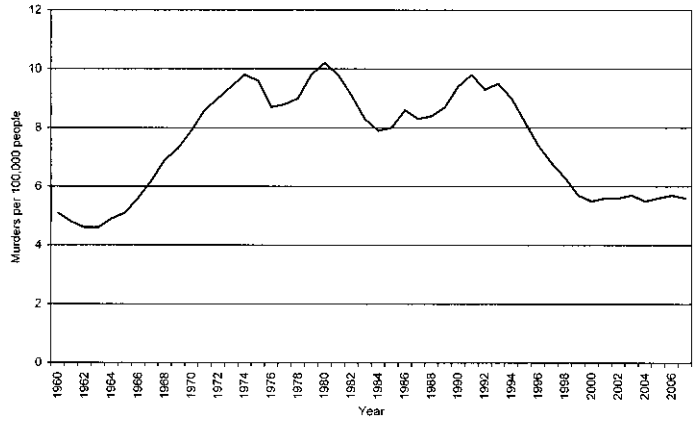
Since the primary data that we will focus on are at the county level, we are asking whether crime rates change in counties whose states adopt nondiscretionary concealed-handgun laws. We are also asking whether the crime rates change relative to other changes in counties located in states without such laws. Using a reference library (Lexis/Nexis) that contains an extensive collection of news stories and state laws, I conducted a search to determine the exact dates on which these laws took effect. Because of delays in implementing the laws even after they went into effect, I defined counties in states with nondiscretionary laws as being subject to these laws beginning with the first full year for which the law was in effect. While all the tables shown in this book use the second measure, both measures produced similar results.

The number of arrests and offenses for each type of crime in every county from 1977 to 1992 was provided by the FBI's *Uniform Crime Reports;* in addition, however, I contacted the state department of corrections, attorney general, secretary of state, and state police offices in every state in an effort to compile data on conviction rates, sentence lengths, and concealed-weapons permits by county. The Bureau of Justice Statistics also released a list of contacts in every state that might provide state-level criminal justice data. Unfortunately, county data on the total number of outstanding concealed-carry pistol permits were available only for Arizona, California,

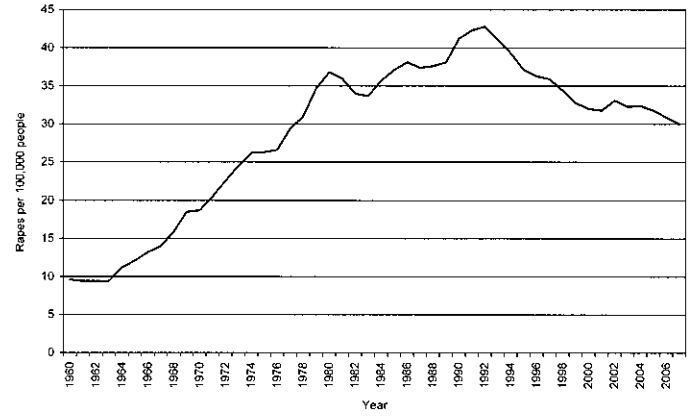
A. Violent crime

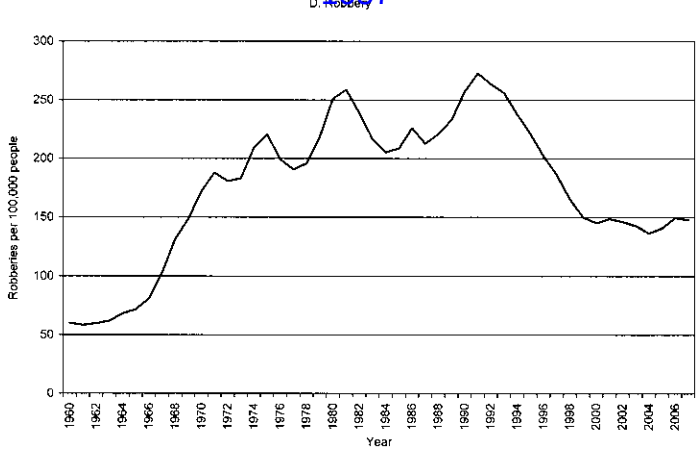


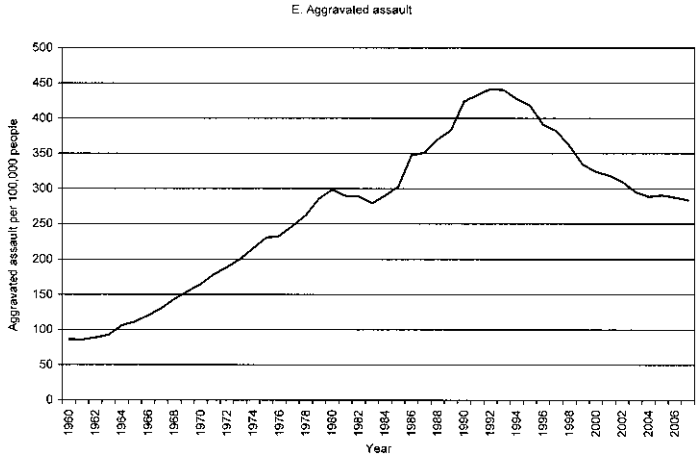
B. Murder



C. Rape







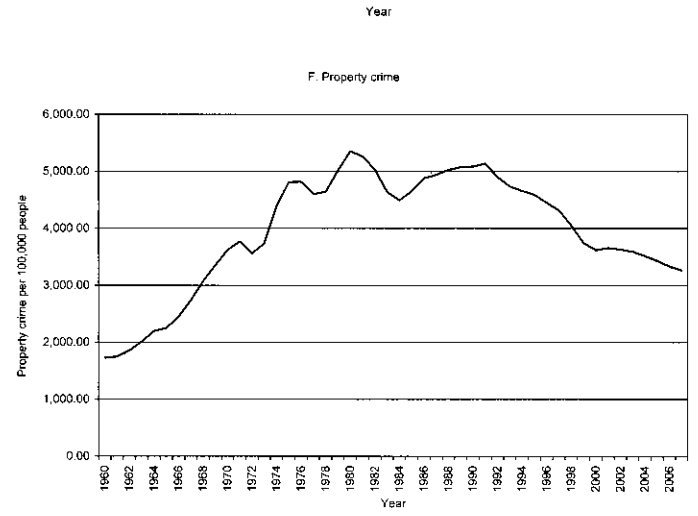


Figure 3.6. U.S. Crime rates from 1960 to 2006 (from FBI's *Uniform Crime Reports*)

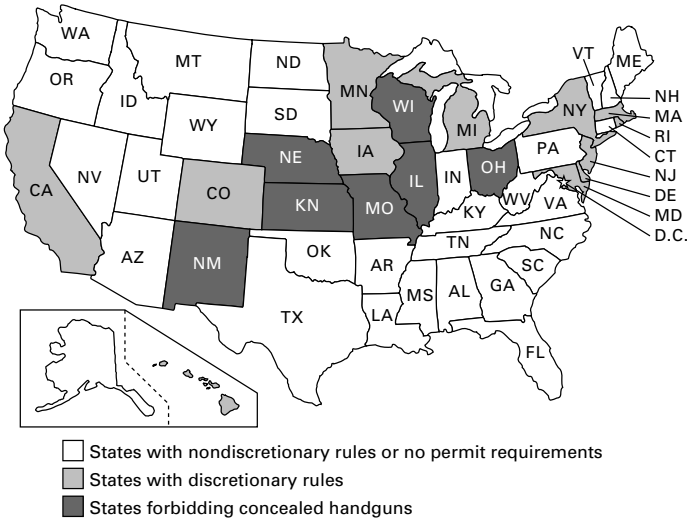


Figure 3.7a. State concealed-handgun laws as of 1996

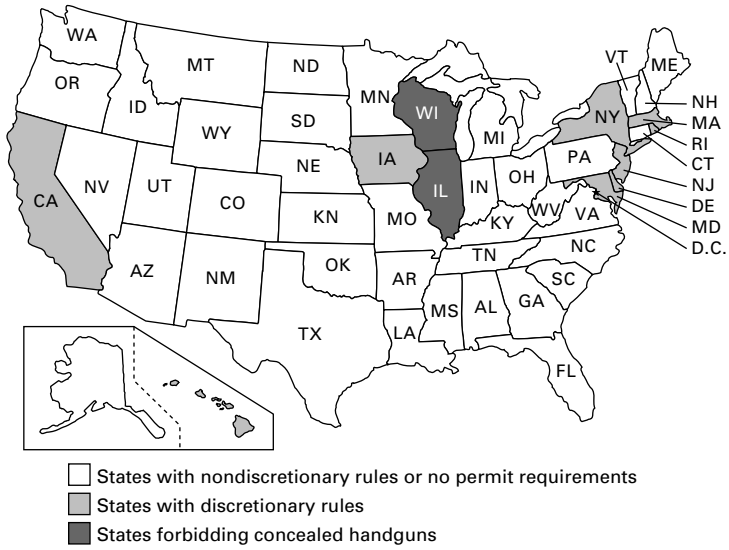


Figure 3.7b. State concealed-handgun laws as of 2007

Table 3.3 Crime rates in states and the District of Columbia that are shall-issue and are more restrictive in letting citizens carry concealed handguns

	1992			2007		
	Crime rate per 100,000 people			Crime rate per 100,000 people		
Type of crime	States with nondiscretionary concealed handgun laws	All other states	Percentage higher crime rate in states without nondiscretionary laws	States with nondiscretionary concealed handgun laws	All other states	Percentage higher crime rate in states without nondiscretionary laws
Violent crime	378.8	684.5	81	403.2	505.2	25
Murder	5.1	9.5	86	5	6.4	28
Rape	35	43.6	25	34.7	25.3	–27
Aggravated assault	339.9	417.4	23	262	286.6	9
Robbery	108.8	222.6	105	101.5	186.9	84
Property crime	3,786.3	4,696.8	24	3244	3,047.4	–6
Auto theft	334.2	533.4	60	314.1	399.2	27
Burglary	840.3	1,074.7	28	714.4	573.8	–20
Larceny	2,611.8	3,088.7	18	2,215.4	2,074.4	–6

Florida, Oregon, Pennsylvania, and Washington, and time-series county data before and after a change in the law were only available for Arizona (1994–96), Oregon (1990–92), and Pennsylvania (1986–92). Since the Oregon nondiscretionary law was passed in 1990, I sought data on the number of permits in 1989 by calling up every county sheriff in Oregon, and 25 of the 36 counties provided that information. (The remaining counties stated that records had not been kept.)[17] For Oregon, data on county-level conviction rates and sentence lengths were also available from 1977 to 1992.

One difficulty with the sentence-length data is that Oregon passed a sentencing-reform act that took effect in November 1989 and required criminals to serve at least 85 percent of their sentences; thus, judges may have correspondingly altered their sentencing practices. This change was phased in over time because the law only applied to crimes committed after it went into effect in 1989. In addition, the Oregon system did not keep complete records prior to 1987, and the percentage recorded decreased as one looked further into the past. One solution to both of these problems is to allow the sentence-length variable to have different effects in each year.[18] A similar problem exists for Arizona, which adopted a truth-in-sentencing reform in the fall of 1994. We must note, finally, that Arizona differs from Oregon and Pennsylvania in that it already allowed handguns to be carried openly before passing its concealed-handgun law; thus, one might expect to find a somewhat smaller response to adopting a concealed-handgun law.

In addition to using separate variables to measure the average crime rate in each county,[19] I collected data from the Bureau of the Census to try to control for other demographic characteristics that might influence the crime rate. These data included information on the population density per square mile, total county population, and detailed information on the racial and age breakdown of the county (percent of population by each racial group and by sex between 10 and 19 years of age, between 20 and 29, between 30 and 39, between 40 and 49, between 50 and 64, and 65 and over).[20] While a large literature discusses the likelihood that younger males will engage in crime,[21] controlling for these other categories allows us to account for the groups considered most vulnerable (for example, females in the case of rape).[22] Evidence reported by Glaeser and Sacerdote confirms the higher crime rates experienced in cities and examines the effects on these rates of social and family influences as well as the changing pecuniary benefits from crime;[23] the present study, however, is the first to explicitly control for population density (see appendix 3 for a more complete discussion of the data).

An additional set of income data was also used. These included real per-capita personal income, real per-capita unemployment insurance payments, real per-capita income-maintenance payments, and real per-capita retirement payments per person over 65 years of age.[24] Unemployment insurance and income-maintenance payments from the Commerce Department's Regional Economic Information System (REIS) data set were included in an attempt to provide annual, county-level measures of unemployment and the distribution of income.

Finally, I recognize that other legal changes regarding how guns are used and when they can be obtained can alter the levels of crime. For example, penalties involving improper gun use might also have been changing simultaneously with changes in the requirements for obtaining permits to carry concealed handguns. In order to see whether such changes might confound my ability to infer the causes of any observed changes in crime rates, I read through various editions of *State Laws and Published Ordinances—Firearms* (published by the Bureau of Alcohol, Tobacco, and Firearms: 1976, 1986, 1989, and 1994). Except for the laws regarding machine guns and sawed-off shotguns, the laws involving the use of guns did not change significantly when the rules regarding concealed-handgun permits were changed.[25] A survey by Marvell and Moody that addresses the somewhat broader question of sentencing-enhancement laws for felonies committed with deadly weapons (firearms, explosives, and knives) from 1970 to 1992 also confirms this general finding.[26] Yet Marvell and Moody's dates still allow us to examine the deterrent effect of criminal penalties specifically targeted at the use of deadly weapons during this earlier period.[27]

States also differ in terms of their required waiting periods for handgun purchases. Again using the Bureau of Alcohol, Tobacco, and Firearms' *State Laws and Published Ordinances—Firearms,* I identified states with waiting periods and conducted a Lexis search on the ordinances to determine exactly when those laws went into effect. Thirteen of the nineteen states with waiting periods instituted them prior to the beginning of the sample period.[28]

4 Concealed-Handgun Laws and Crime Rates: The Empirical Evidence

While our initial comparison of crime rates in states with and without concealed-handgun laws was suggestive, obviously many other factors must be accounted for. The next three chapters use common statistical techniques known as regression analysis to control for these factors. (For those who are interested, a more complete discussion of regressions and statistical significance is provided in appendix 1.) The following discussion provides information on a wide range of law-enforcement activities, but the primary focus is on the link between the private ownership of guns and crime. What gun laws affect crime? Does increased gun ownership increase or decrease murders? How do more lenient gun ownership laws affect accidental deaths and suicide?

The analysis begins by examining both county- and state-level crime data. We then examine how gun ownership benefits different groups, such as women and minorities. To test whether crime-rate changes are a result of concealed-handgun laws, it is not enough simply to see whether these laws lower crime rates; any changes in crime rates must also be linked to the changes in the number of concealed-handgun permits. We must also remember that the laws are not all the same: different states adopt different training and age requirements for obtaining a permit. These differences allow us to investi-

gate whether the form of the concealed-handgun law matters as well as to test the importance of other gun-control laws. Finally, evidence is provided on whether criminals move to other places when concealed-handgun laws are passed.

The book is organized to examine the simplest evidence first and then gradually considers more complicated issues. The first estimates measure whether the average crime rate falls in counties when they adopt concealed-handgun laws. By looking across counties or states at the same time that we examine them over time, we can test not only whether places with the most permits have the greatest reductions in crime, but also whether those with the greatest *increases* in permits have the greatest reductions in crime. Similarly, we can investigate how total gun ownership is related to the level of crime. Tracking gun ownership in individual states over time allows us to investigate how a crime in a state changes as its gun ownership rates change.

Using County and State Data for the United States

The first group of estimates reported in table 4.1 attempts to explain crime rates. There are nine different categories of crime. Each column in the table presents the changes in the crime rate for the crime in the column heading. The numbers in each row represent the impact that a particular explanatory variable has on each crime rate. There are three pieces of information for most of the explanatory variables: (1) the percent change in the crime rate attributed to a particular change in the explanatory variable; (2) the percentage of the variation in the crime rate that can be explained by the variation in the explanatory variable;[1] and (3) one, two, or three asterisks denote whether a particular effect is statistically significant at least at the 1, 5, or 10 percent level, where the 1 percent level represents the most reliable result.[2]

While I am primarily interested in nondiscretionary laws, the estimates also account for many other variables: the arrest rate for each type of crime; population density and the number of people living in a county; measures of income, unemployment, and poverty; the percentage of the population that is a certain sex and race by ten-year age groupings (10 to 19 years of age, 20 to 29 years of age); and the set of variables described in the previous section to control for other county and year differences. The results clearly imply that nondiscretionary laws coincide with fewer murders, aggravated assaults, and rapes.[3] On the other hand, auto-theft and larceny rates rise.

Table 4.1 The effect of nondiscretionary concealed-handgun laws on crime rates: National, County-Level, Cross-Sectional, Time-Series Evidence

	Percent change in various crime rates for changes in explanatory variables								
Change in explanatory variable	Violent crime	Murder	Rape	Aggravated assault	Robbery	Property crime	Burglary	Larceny	Auto theft
Nondiscretionary law adopted	−4.9%*	−7.7%*	−5.3%*	−7.01%*	−2.2%***	2.7%*	.05%	3.3%*	7.1%*
	(1%)	(2%)	(1%)	(1%)	(.3%)	(1%)	(.02%)	(1%)	(1%)
Arrest rate for the crime category (e.g., violent crime murder, etc.) increased by 100 percentage points	−0.48%	−1.39%*	−0.81%*	−0.896%*	−0.57%*	−0.76%*	−2.4%*	−0.18%*	−0.18%*
	(9%)	(7%)	(4%)	(9%)	(4%)	(10%)	(11%)	(4%)	(3%)
Population per square mile increased by 1,000	6%*	−2%	−2%	0.58%	31.6%*	0.48%	−7%*	3.7%*	48%*
	(5%)	(1%)	(1%)	(.4%)	(17%)	(1%)	(9%)	(4%)	(36%)
Real per-capita personal income increased by $1,000	0.79%*	1.63%*	−0.59%***	0.47%	0.47%	−1.02%*	−1.84%*	−1.23%*	1.5%*
	(1%)	(2%)	(1%)	(1%)	(1%)	(3%)	(4%)	(2%)	(2%)
Real per-capita unemployment Ins. increased by $100	−2.2%*	−4.6%*	−4.7%*	−1.9%*	0.7%	3.8%*	6.0%*	1.9%*	2.1%*
	(.07%)	(1%)	(1%)	(.05%)	(.01%)	(2%)	(3%)	(.08%)	(.06%)
Real per-capita income maintenance increased by $100	−0.7%	2.5%**	−1.7%	1.39%	−3.2%*	1.9%*	3.9%*	0.2%	3.3%*
	(.3%)	(1%)	(.7%)	(.7%)	(1%)	(2%)	(4%)	(.1%)	(2%)
Real per-capita retirement payments per person over 65 increased by $1,000	−0.197%	−1.3%	−0.24%	−0.68%	−0.55%	−0.87%	−1.06%	−0.63%	−0.93%
	(.5%)	(3%)	(.4%)	(2%)	(1%)	(4%)	(7%)	(2%)	(2%)
Population increased by 100,000	0.86%	−0.34%*	−2.94%	0.45%*	−0.61%***	−2.18%*	−2.14%*	−3.10%*	−0.04%*
	(1%)	(.4%)	(3%)	(.06%)	(.06%)	(6%)	(5%)	(6%)	(.05%)

Note: The percentage reported in parentheses is the percent of a standard deviation change in the endogenous variable that can be explained by one-standard-deviation change in the exogenous variable. Year and county dummies are not shown, and the results for demographic variables are shown in appendix. All regressions use weighted least squares, where the weighting is each county's population. Entire sample used for all counties over the 1977–1992 period.

*The result is statistically significant at the 1 percent level for a two-tailed *t*-test.

**The result is statistically significant at the 5 percent level for a two-tailed *t*-test.

***The result is statistically significant at the 10 percent level for a two-tailed *t*-test.

Both changes are consistent with my discussion of the direct and substitution effects produced by concealed weapons.[4]

The results are also large, indicating how important the laws can be. When state concealed-handgun laws went into effect in a county, murders fell by about 8 percent, rapes fell by 5 percent, and aggravated assaults fell by 7 percent.[5] In 1992 the following numbers were reported: 18,469 murders; 79,272 rapes; 538,368 robberies; and 861,103 aggravated assaults in counties without nondiscretionary laws. The estimated coefficients suggest that if these counties had been subject to state concealed-handgun laws and had thus been forced to issue handgun permits, murders in the United States would have declined by about 1,400.

What about increased accidental deaths from concealed weapons? The entire number of accidental handgun deaths in the United States in 1988 was only 200 (the last year for which these data are available for the entire United States).[6] Of this total, 22 accidental deaths were in states with concealed-handgun laws, while 178 occurred in states without these laws. The reduction in murders is as much as eight times greater than the total number of accidental deaths in concealed-handgun states. We will revisit the impact of concealed-handgun laws on accidental deaths in chapter 5, but if these initial results are accurate, the net effect of allowing concealed handguns is clearly to save lives, even if concealed handguns were somehow responsible for all accidental handgun deaths.[7]

As with murders, the results indicate that the number of rapes in states without nondiscretionary laws would have declined by 4,200. Aggravated assaults would have declined by 60,000, and robberies by 12,000.[8]

On the other hand, property-crime rates increased after nondiscretionary laws were implemented. If states without concealed-handgun laws had passed such laws, there would have been 247,000 more property crimes in 1992 (a 2.7 percent increase). The increase is small compared to the changes that we observed for murder, rape, and aggravated assault, though it is about the same size as the change for robbery. Criminals respond to the threat of being shot while committing such crimes as robbery by choosing to commit less risky crimes that involve minimal contact with the victim.[9]

It is possible to put a rough dollar value on the losses from crime in the United States and thus on the potential gains from nondiscretionary laws. A recent National Institute of Justice study estimates the costs to victims of different types of crime by measuring lost productivity; out-of-pocket expenses, such as those for medical bills and property losses; and losses from fear, pain, suffering, and lost quality of life.[10] While the use of jury awards

to measure losses such as fear, pain, suffering, and lost quality of life may be questioned, such estimates allow us to compare the reduction in violent crimes with the increase in property crimes.

By combining the estimated reduction in crime from table 4.1 with the National Institute of Justice's estimates of what these crimes would have cost victims had they occurred, table 4.2 reports the gain from allowing concealed handguns to be $5.7 billion in 1992 dollars. The reduction in violent crimes represents a gain of $6.2 billion ($4.2 billion from murder, $1.4 billion from aggravated assault, $374 million from rape, and $98 million from robbery), while the increase in property crimes represents a loss of $417 million ($343 million from auto theft, $73 million from larceny, and $1.5 million from burglary). However, while $5.7 billion is substantial, to put it into perspective, it still equals only about 1.23 percent of the total losses to victims from these crime categories. These estimates are probably most sensitive to the value of life used (in the National Institute of Justice Study this was set at $1.84 million in 1992 dollars). Higher estimated values of life would obviously increase the net gains from the passage of concealed-handgun laws, while lower values would reduce the gains. To the extent that people are taking greater risks regarding crime because of any increased sense of safety produced by concealed-handgun laws,[11] the preceding numbers underestimate the total savings from allowing concealed handguns.

The arrest rate produces the most consistent effect on crime. Higher arrest rates are associated with lower crime rates for all categories of crime. Variation in the probability of arrest accounts for 3 to 11 percent of the variation in the various crime rates.[12] Again, the way to think about this is that the typical observed change in the arrest rate explains up to about 11 percent of the typical change in the crime rate. The crime most responsive to the arrest rate is burglary (11 percent), followed by property crimes (10 percent); aggravated assault and violent crimes more generally (9 percent); murder (7 percent); rape, robbery, and larceny (4 percent); and auto theft (3 percent).

For property crimes, the variation in the percentage of the population that is black, male, and between 10 and 19 years of age explains 22 percent of the ups and downs in the property-crime rate.[13] For violent crimes, the same number is 5 percent (see appendix 5). Other patterns also show up in the data. Not surprisingly, a higher percentage of young females is positively and significantly associated with the occurrence of a greater number of rapes.[14] Population density appears to be most important in explaining

Table 4.2 The effect of nondiscretionary concealed-handgun laws on victims' costs: What if all states had adopted nondiscretionary laws?

	Change in number of crimes if states without nondiscretionary laws in 1992 had adopted them			Change in victims' costs if states without nondiscretionary laws in 1992 had adopted them		
Crime category	Estimates using county-level data	Estimates using county-level data and state time trends	Estimates using state-level data	Estimates using county-level data	Estimates using county-level data and state time trends	Estimates using state-level data
Murder	–1,410	–1,840	–1,590	–$4.2 billion	–5.57 billion	–$4.8 billion
Rape	–4,200	–3,700	–4,800	–$374 million	–$334 million	–$431 million
Aggravated assault	–60,400	–61,100	–93,900	–$1.4 billion	–$1.4 billion	–$2.2 billion
Robbery	–11,900	–10,990	–62,900	–$98 million	–$90 million	–$518 million
Burglary	1,100	–112,700	–180,800	$1.5 million	–$162 million	–$261 million
Larceny	191,700	–93,300	–180,300	$73 million	–$35 million	–$69 million
Auto theft	89,900	–41,500	–11,100	$343 million	–$2 million	–$42 million
Total change in victims' costs				–$5.7 billion	–$7.6 billion	–$8.3 billion

Note: Estimates of the costs of crime are in 1992 dollars, from the National Institute of Justice's study.

robbery, burglary, and auto-theft rates, with the typical variation in population density explaining 36 percent of the typical change across observations in auto theft.

Perhaps most surprising is the relatively small, even if frequently significant, effect of a county's per-capita income on crime rates. Changes in real per-capita income account for no more than 4 percent of the changes in crime, and in seven of the specifications it explains at most 2 percent of the change. It is *not* safer to live in a high-income neighborhood if other characteristics (for example, demographics) are the same. Generally, high-income areas experience more violent crimes but fewer property crimes. The two notable exceptions to this rule are rape and auto theft: high-income areas experience fewer rapes and more auto theft. If the race, sex, and age variables are replaced with separate variables showing the percentage of the population that is black and white, 50 percent of the variation in the murder rate is explained by variations in the percentage of the population that is black. Yet because of the high rates at which blacks are arrested and incarcerated or are victims of crimes (for example, 38 percent of all murder victims in 1992 were black; see table 1.1), this is not unexpected.

One general caveat should be made in evaluating the coefficients involving the demographic variables. Given the very small portions of the total populations that fall into some of these narrow categories (this is particularly true for minority populations), the effect on the crime rate from a one-percentage-point increase in the percentage of the population in that category greatly overstates the true importance of that age, sex, or race grouping. The assumption of a one-percentage-point change is arbitrary and is only provided to give the reader a rough idea of what these coefficients mean. For a better understanding of these variables' impact, relatively more weight should be placed on the second number, which shows how much of the variation in the various crime rates can be explained by the normal changes in each explanatory variable.[15]

We can take another look at how sensitive the results from table 4.1 are and examine how they vary with different subsets of the following variables: the nondiscretionary law, the nondiscretionary law and the arrest rates, and the nondiscretionary law and the variables that account for the national changes in crime rates across years. Each specification yields results that show even more significant effects from the nondiscretionary law, though when results exclude variables that measure how crime rates differ across counties, they are likely to tell us more about which states adopt these laws than about the impact of these laws on crime.[16] The low-crime

states are the most likely to pass these laws, and their crime rates become even lower after their passage. I will attempt to account for this fact later in chapter 6.

To further test the sensitivity of the results to the various control variables used, I reestimated the specifications in table 4.1 without using either the percentages of the populations that fall into the different sex, race, and age categories or the measures of income; this tended to produce similar though somewhat more significant results with respect to concealed-handgun laws. And the estimated gains from passing concealed-handgun laws were also larger.

While these regressions account for nationwide changes in crime rates on average over time, one concern is that individual states are likely to have their own unique time trends. The question here is whether the states adopting nondiscretionary concealed-handgun laws experienced falling crime rates over the entire time period. This cannot be true for all states as a whole, because as figure 3.5 shows, violent crimes have definitely not been diminishing during the entire period. However, if this downward trend existed for the states that adopted nondiscretionary laws, the variables shown in table 4.1 could indicate that the average crime rate was lower after the laws were passed, even though the drop in the average level was due merely to a continuation of a downward trend that began before the law took effect. To address this issue, I reestimated the specifications shown in table 4.1 by including state dummy variables that were each interacted with a time-trend variable.[17] This makes it possible to account not only for the national changes in crime rates with the individual year variables but also for any differences in state-specific trends.

When these individual state time trends were included, all results indicated that the concealed-handgun laws lowered crime, though the coefficients were not statistically significant for aggravated assault and larceny. Under this specification, the passage of nondiscretionary concealed-handgun laws in states that did not have them in 1992 would have reduced murders in that year by 1,839; rapes by 3,727; aggravated assaults by 10,990; robberies by 61,064; burglaries by 112,665; larcenies by 93,274; and auto thefts by 41,512. The total value of this reduction in crime in 1992 dollars would have been $7.6 billion. With the exceptions of aggravated assault and burglary, violent-crime rates still experienced larger drops from the adoption of concealed-handgun laws than did property crimes.

Despite the concerns over the aggregation issues discussed earlier, economists have relied on state-level data in analyzing crime primarily because

of the difficulty and extra time required to assemble county-level data. As shown in tables 2.2–2.4, the large within-state heterogeneity raises significant concerns about relying too heavily on state-level data.

To provide a comparison with other crime studies relying on state-level data, table 4.3 reestimates the specifications reported in table 4.1 using state-level rather than county-level data. While the results in these two tables are generally similar, two differences immediately manifest themselves: (1) the specifications now imply that nondiscretionary concealed-handgun laws lower all types of crime, and (2) concealed-handgun laws explain much more of the variation in crime rates, while arrest rates (with the exception of robbery) explain much less of the variation.[18] Concealed-handgun laws lower both violent- and property-crime rates, but violent crimes are more affected by concealed handguns, falling two-and-one-half times more than those for property crimes.

Suppose we rely on the state-level results rather than the county-level estimates. We would then conclude that if all states had adopted nondiscretionary concealed-handgun laws in 1992, about 1,600 fewer murders and 4,800 fewer rapes would have been committed.[19] Overall, table 4.3 allows us to calculate that the estimated monetary gain from reductions in crime produced by nondiscretionary concealed-handgun laws was $8.3 billion in 1992 dollars (again, see table 4.2 for the precise breakdown). Yet, at least in the case of property crimes, the concealed-handgun law coefficients are sensitive to whether the regressions are run at the state or county level. This suggests that aggregating observations into units as large as states is a bad idea.[20]

Differential Effects across Counties, between Men and Women, and by Race and Income

Let us now return to other issues concerning the county-level data. Criminal deterrence is unlikely to have the same impact across all counties. For instance, increasing the number of arrests can have different effects on crime in different areas, depending on the stigma attached to arrest. In areas where crime is rampant, the stigma of being arrested may be small, and that means that an increase in arrest rates has a correspondingly small effect.[21] To test this, the specifications shown in table 4.1 were reestimated by breaking down the sample into two groups: (1) counties with above-median crime rates and (2) counties with below-median crime rates. Each set of data was reexamined separately.

Table 4.3 Aggregating the data: state-level, cross-sectional, time-series evidence

	Percent change in various crime rates for changes in explanatory variables								
Change in explanatory variable	Violent crime	Murder	Rape	Aggravated assault	Robbery	Property crime	Burglary	Larceny	Auto theft
Nondiscretionary law adopted	−10.1%	−8.62%**	−6.07%**	−10.9%*	−14.21%*	−4.19%**	−0.88%	−8.25%*	−3.14%
	(5.8%)	(5%)	(4.7%)	(6.5%)	(5.7%)	(4.8%)	(.43%)	(7.6%)	(3.8%)
Arrest rate for the crime category increased by 100 percentage points	−8.02%*	−7.3%*	−2.05%***	−15.3%*	−10.5%*	−59.9%	−14.5%*	−71.5%*	−65.7%*
	(1.5%)	(5.3%)	(.69%)	(3.9%)	(14.4%)	(8.1%)	(6.5%)	(7.6%)	(10.4%)

Note: Except for the use of state dummies in place of county dummies, the control variables are the same as those used in table 4.1 including year dummies, though they are not all reported. The percent reported in parentheses is the percent of a standard deviation change in the endogenous variable that can be explained by a one-standard-deviation change in the exogenous variable. All regressions use weighted least squares, where the weighting is according to each state's population. Entire sample used over the 1977 to 1992 period.

*The result is statistically significant at the 1 percent level for a two-tailed *t*-test.

**The result is statistically significant at the 5 percent level for a two-tailed *t*-test.

***The result is statistically significant at the 10 percent level for a two-tailed *t*-test.

As table 4.4 shows, concealed-handgun laws do indeed affect high- and low-crime counties similarly. The coefficient signs are consistently the same for both low- and high-crime counties, though for two of the crime categories—rape and aggravated assault—concealed-handgun laws have statistically significant effects only in the relatively high-crime counties. For most violent crimes—such as murder, rape, and aggravated assault—concealed-weapons laws have much greater deterrent effects in high-crime counties. In contrast, for robbery, property crimes, auto theft, burglary, and larceny, the effect appears to be greatest in low-crime counties.

Table 4.4 also shows that the deterrent effect of arrests is significantly different, at least at the 5 percent level, between high- and low-crime counties for eight of the nine crime categories (the one exception being violent crimes). The results further reject the hypothesis that arrests would be associated with greater stigma in low-crime areas. Additional arrests in low- and high-crime counties generate extremely similar changes in the aggregate category of violent crime, but the arrest-rate coefficient for murder is almost three times greater in high-crime counties than in low-crime counties. If these results suggest any conclusion, it is that for most crimes, tougher measures have more of an impact in high-crime areas.

The effect of gun ownership by women deserves a special comment. Despite the relatively small number of women who obtain concealed-handgun permits, the concealed-handgun coefficient for explaining rapes in the first three sets of results is consistently similar in size to the effect that this variable has on other violent crime. January 1996 data for Washington and Oregon reveal that women constituted 18.6 and 22.9 percent, respectively, of those with concealed-handgun permits.[22] The set of women who were the most likely targets of rape probably chose to carry concealed handguns at much higher rates than women in general. The preceding results show that rapists are particularly deterred by handguns. As mentioned earlier, the National Crime Victimization Survey data show that providing a woman with a gun has a much greater effect on her ability to defend herself against a crime than providing a gun to a man. Thus even if few women carry handguns, the change in the "cost" of attacking women could still be as great as the change in the "cost" of attacking men, despite the much higher number of men who are becoming armed. To phrase this differently, if one more woman carries a handgun, the extra protection for women in general is greater than the extra protection for men if one more man carries a handgun.[23]

These results raise a possible concern as to whether women have the

Table 4.4 Aggregating the data: Do law-enforcement and nondiscretionary laws have the same effects in high- and low-crime areas?

	Percent change in various crime rates for changes in explanatory variables							
Change in explanatory variable	Violent crime	Murder	Rape	Aggravated assault	Robbery	Property crime	Burglary	Larceny	Auto theft
	Sample where county crime rates are above the median								
Nondiscretionary law adopted	–6.0%*	–9.9%*	–7.2%*	–4.5%*	–3.4%*	1.6%*	0.4%	3.0%*	5.2%*
Arrest rate for the crime category increased by 100 percentage points	–5.2%*	–12.3%*	–3.3%*	–6.3%*	–29.4%*	–53.5%*	–56.5%*	–59.6%*	–13.3%*
	Sample where county crime rates are below the median								
Nondiscretionary law adopted	–3.7%**	–4.4%**	–3.0%	–0.3%	–7.9%*	8.8%*	3%**	8.7%*	7.2%*
Arrest rate for the crime category increased by 100 percentage points	–5.2%*	–4.9%*	–6.6%*	–6.8%*	–3.7%*	–13.5%*	–27.1%*	–10%*	–1.4%*

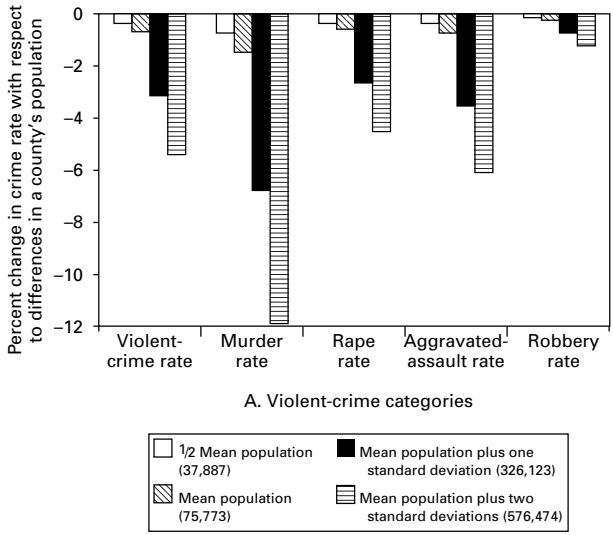
Note: The control variables are the same as those used in table 4.1, including year and county dummies, though they are not reported. All regressions use weighted least squares, where the weighting is each county's population. Entire sample used over the 1977 to 1992 period.
*The result is statistically significant at the 1 percent level for a two-tailed *t*-test.
**The result is statistically significant at the 5 percent level for a two-tailed *t*-test.

right incentive to carry concealed handguns. Despite the fact that women who carry concealed handguns make other women so much safer, it is possible that women might decide not to carry them because they see their own personal gain as much smaller than the total benefit to all women. While the problem is particularly pronounced for women, people in general often take into account only the benefits that they individually receive from carrying a gun and not the crime-reduction benefits that they are generating for others.[24]

As mentioned in chapter 2, an important concern is that passing a nondiscretionary concealed-handgun law should not affect all counties equally. In particular, when states had discretionary laws, counties with the highest populations were also those that most severely restricted people's ability to carry concealed weapons. Adopting nondiscretionary laws therefore produced the greatest change in the number of permits in the more populous counties. Thus, a significant advantage of using this county data is that it allows us to take advantage of county-level variation in the impact of nondiscretionary concealed-handgun laws. To test this variation across counties, figures 4.1 and 4.2 repeat all the specifications in table 4.1 but examine instead whether the effect of the nondiscretionary law varies with county population or population density. (The simplest way to do this is to multiply the nondiscretionary-law variable by either the county population or population density.) While all the other coefficients remain virtually unchanged, this new interaction implies the same crime-reducing effects from the nondiscretionary law as reported earlier. In all but one case the coefficients are more significant and larger.

The coefficients are consistent with the hypothesis that the new laws induce the greatest changes in the largest counties, which have a much greater response in both directions to changes in the laws. Violent crimes fall more and property crimes rise more in the largest counties. The figures indicate how effects vary for counties of different sizes. For example, when counties with almost 600,000 people (two standard deviations above the mean population) pass a concealed-handgun law, the murder rate falls by 12 percent. That is reduced 7.4 times more than for the average county (75,773 people).

Although the law-enforcement officials that I talked to continually mentioned population as being the key variable, I also reexamined whether the laws had different effects in more densely populated counties. Given the close relationship between county population and population density, it is not too surprising to find that the impact of concealed handguns in



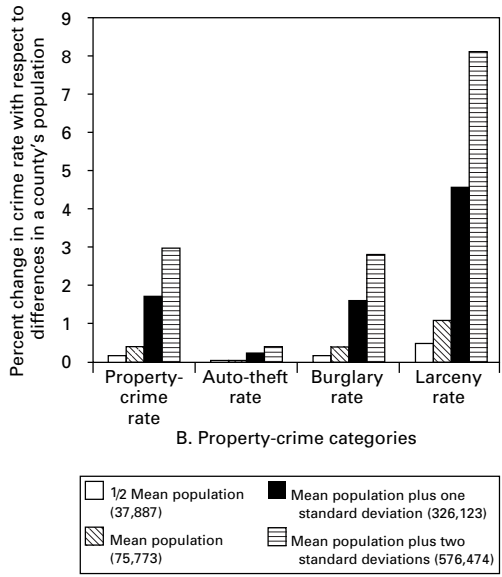
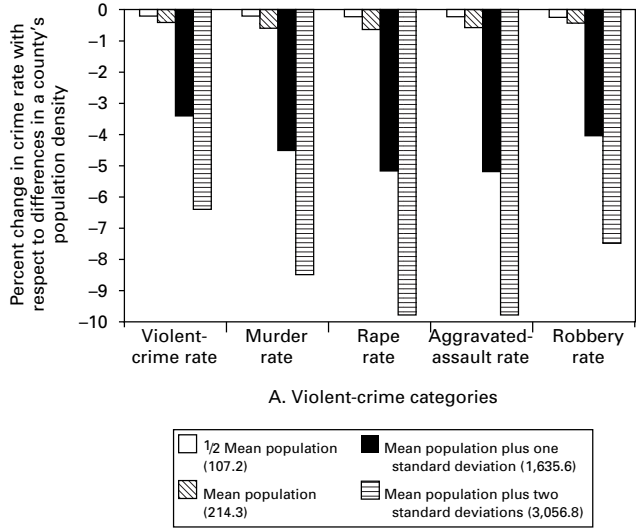


Figure 4.1. Do larger changes in crime rates from nondiscretionary concealed-handgun laws occur in more populous counties?



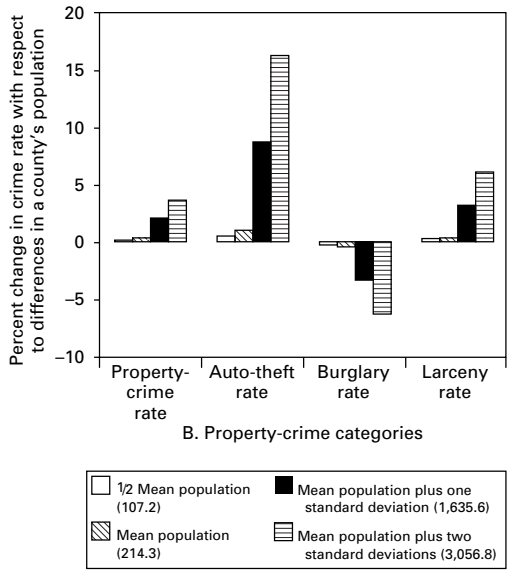


Figure 4.2. Do larger changes in crime rates from nondiscretionary concealed-handgun laws occur in more densely populated counties?

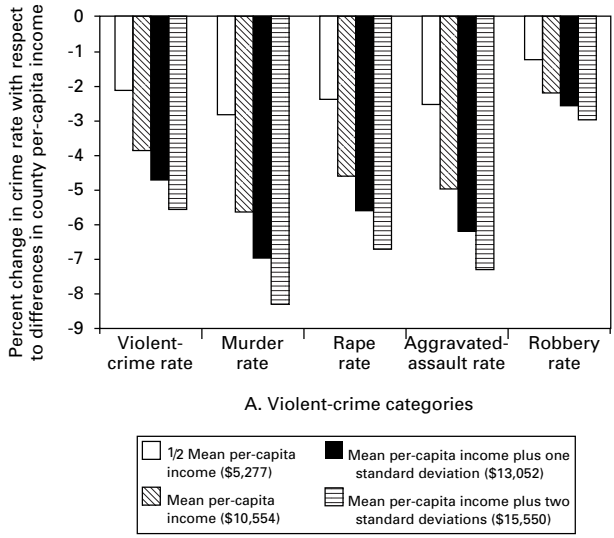
more densely populated areas is greater than in sparsely populated counties. Passing a concealed-handgun law lowers the murder rate in counties with about 3,000 people per square mile (the levels found in Fairfax, Virginia; Orleans, Louisiana, which contains New Orleans; and Ramsey, Minnesota, which contains St. Paul) by 8.5 percent, 12 times more than it lowers murders in the average county. The only real difference between the results for population and population density occur for the burglary rate, where concealed-handgun laws are associated with a small reduction in burglaries for the most densely populated areas.

Figures 4.3 and 4.4 provide a similar breakdown by income and by the percentage of the population that is black. Higher-income areas and counties with relatively more blacks both have particularly large drops in crime associated with concealed-handgun laws. Counties with a 37 percent black population experienced 11 percent declines in both murder and aggravated assaults. The differences with respect to income were not as large.[25]

With the extremely high rates of murder and other crimes committed against blacks, it is understandable why so many blacks are concerned about gun control. University of Florida criminologist Gary Kleck says, "Blacks are more likely to have been victims of crime or to live in neighborhoods where there's a lot of crime involving guns. So, generally, blacks are more pro-control than whites are." Nationally, polls indicate that 83 percent of blacks support police permits for all gun purchases.[26] While many blacks want to make guns harder to get, the irony is that blacks actually benefit more than other groups from concealed-handgun laws. Allowing potential victims a means for self-defense is more important in crime-prone neighborhoods. Even more strikingly, the history of gun control in the United States has often been a series of attempts to disarm blacks.[27] In explaining the urgency of adopting the U.S. Constitution's Fourteenth Amendment, Duke University Law Professor William Van Alstyne writes,

> It was, after all, the defenselessness of the Negroes (denied legal rights to keep and bear arms by state law) from attack by night riders—even to protect their own lives, their own families, and their own homes—that made it imperative that they, as citizens, could no longer be kept defenseless by a regime of state law denying them the common right to keep and bear arms.[28]

Indeed, even in the 1960s, much of the increased regulation of firearms stemmed from the fear generated by Black Panthers who openly carried guns.



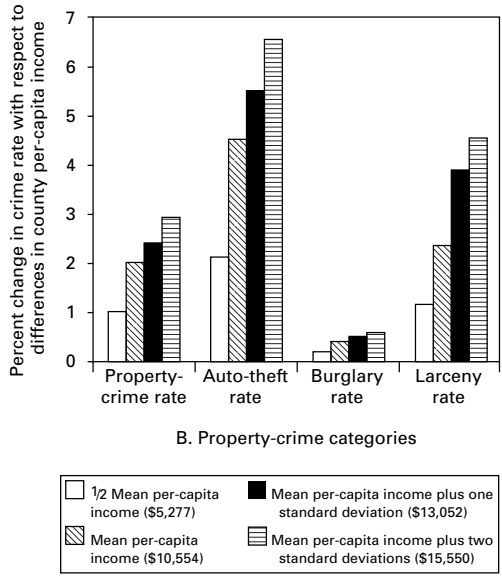
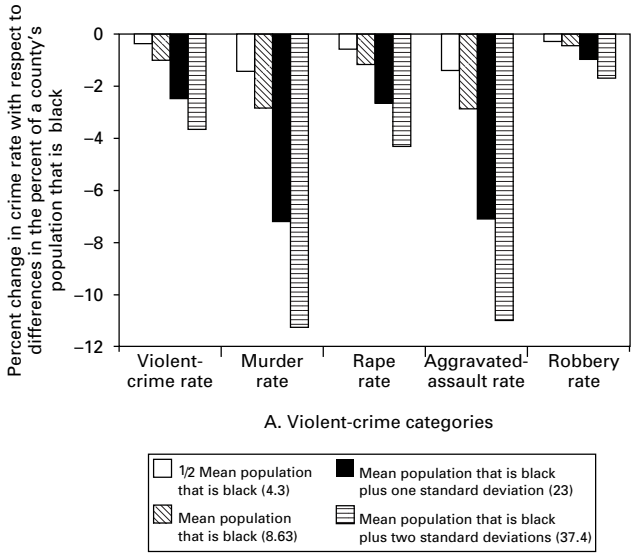


Figure 4.3. How does the change in crime from nondiscretionary concealed-handgun laws vary with county per-capita income?



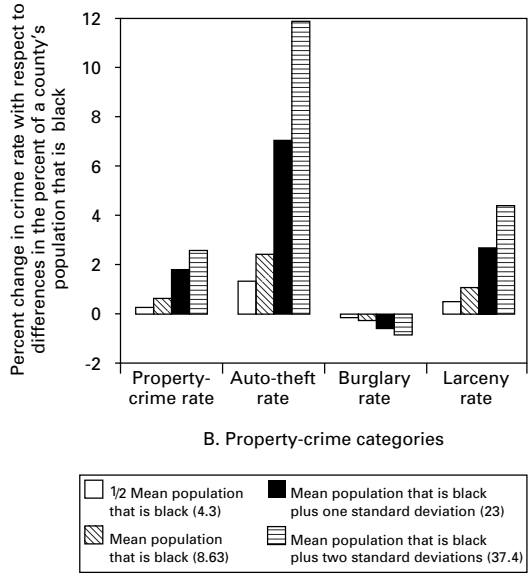


Figure 4.4. How does the change in crime from nondiscretionary concealed-handgun laws vary with the percent of a county's population that is black?

Alexis Herman, the current Secretary of Labor, experienced firsthand the physical risks of growing up black in Alabama. Describing her difficult confirmation hearings, an Associated Press story included the following story:

> Anyone who thought the frustrations of waiting for confirmation would discourage her knew nothing about the lessons Herman learned from her father. They forgot that he sued to integrate the Democratic Party in Alabama, and later became the state's first black ward leader. They never heard about the night he put a pistol in his young daughter's hands and stepped out of the car to confront the Ku Klux Klan.
>
> "He taught me that you have to face adversity. He taught me to stand by my principles," Herman said in the interview. "He also taught me how to work within the system for change."
>
> Herman said her father never raised his voice, but he always kept a small silver pistol under the driver's seat of his DeSoto as he drove from community meeting to community meeting around Mobile. She always sat close by his side, unless the pistol was out. "The only way that I ever knew trouble was around was that the gun would come out from under the driver's seat and he'd put it by his side," she said.
>
> As they left the home of a minister one Christmas Eve, the pistol was on the car seat. She was 5. "It was a dark road, a dirt road to get back to the main highway," she recalled. "We were driven off the road by another car, and they were Klansmen."
>
> She hid on the floor and her father pressed the pistol's white handle into her palm. "He told me, 'If anybody opens this door, I want you to pull this trigger.'" He locked the door behind him and walked ahead to keep them away from the car. She crouched in the dark, listening until the shouts and scuffling died down.
>
> Eventually, the minister came to the car to drive Herman home. Her father, who had been beaten, rode in another car.[29]

Recently, after testifying before the Illinois state House of Representatives on whether to pass a concealed-handgun bill, I was approached by a black representative from Chicago who supported the bill.[30] He told me that, at least for Illinois, he was not surprised by my finding that areas with large minority populations gained the most from these laws. Noting the high rate at which young, black males are stopped by police and the fact that it is currently a felony to possess a concealed handgun, he said

that an honest, law-abiding, young, black male would be "nuts" to carry a concealed handgun in Illinois. He mentioned a case that had occurred just a week earlier: Alonzo Spellman—a black professional football player for the Chicago Bears—had been arrested in Chicago after a routine traffic violation revealed that he had a handgun in his car.[31] Noting the inability of the police to protect people in heavily black areas when "bad guys" already had illegal guns, the representative said he believed that the current power imbalance between law-abiding people and criminals was greatest in black areas.

Perhaps it is not too surprising that blacks and those living in urban areas gain the most from being able to defend themselves with concealed handguns, since the absence of police appears most acute in black, central-city neighborhoods. Until 1983, the American Housing Survey annually asked sixty thousand households whether their neighborhoods had adequate police protection. Black, central-city residents were about twice as likely as whites generally to report that they did not have adequate protection, and six times more likely to say that they had considered moving because of an insufficient police presence in their neighborhoods.[32]

These results should at least give pause to the frequent attempts to pass city ordinances and state laws banning low-cost, "Saturday night specials." Indeed, the results have implications for many gun-control rules that raise gun prices. Law-abiding minorities in the most crime-prone areas produced the greatest crime reductions from being able to defend themselves. Unfortunately, however unintentionally, these new laws risk disarming precisely these poor minorities.

Using Other Crime Rates to Explain the Changes in the Crime Rates Being Studied

Other concerns still exist regarding the specifications employed here. Admittedly, although arrest rates and average differences in individual counties are controlled for, more can be done to account for the changing environments that determine the level of crime. One method is to use changes in other crime rates to help us understand why the crime rates that we are studying are changing over time. Table 4.5 reruns the specifications used to generate figure 4.1A but includes either the burglary or robbery rates as proxies for other changes in the criminal justice system. Robbery and burglary are the violent- and property-crime categories that are the least related to changes in concealed-handgun laws, but

Table 4.5 Using crime rates that are relatively unrelated to changes in nondiscretionary laws as a method of controlling for other changes in the legal environment: controlling for robbery and burglary rates

Change in the explanatory variable	Percent change in various crime rates for changes in explanatory variables								
	Violent crime	Murder	Rape	Aggravated assault	Robbery	Property crime	Burglary	Larceny	Auto theft
	Controlling for robbery rates								
Nondiscretionary law adopted multiplied by county population (evaluated at mean county population)	–2.6%*	–4.3%*	–1.9%*	–2.6%*	—	1.4%*	0.08%	1.3%*	3.7%*
	1%	1.1%	0.4%	0.4%		0.5%	0.04%	0.4%	0.5%
Arrest rate for the crime category increased by 100 percentage points	–0.038*	–0.13*	–0.07*	–0.08*	—	–0.06*	–0.20*	–0.015*	–0.014*
	7%	7%	4%	8%		8%	9%	3%	2%
	Controlling for burglary rates								
Nondiscretionary law adopted multiplied by *county population (evaluated at mean county population)	–2.4%*	–4.3%*	–2.0%*	–2.6%*	0.4%	1.8%*	—	1.4%*	3.6%*
	1%	1.1%	0.4%	0.4%	0.04%	0.7%		0.4%	0.5%
Arrest rate for the crime category increased by 100 percentage points	–0.026*	–0.13*	–0.05*	–0.05*	–0.043*	–0.05*	—	–0.01*	–0.01*
	5%	6%	3%	5%	3%	6%		2%	2%

Note: While not all the coefficient estimates are reported, all the control variables are the same as those used in table 4.1, including year and county dummies. All regressions use weighted least squares, where the weighting is each county's population. Net violent and property-crime rates are respectively net of robbery and burglary rates to avoid producing any artificial collinearity. Likewise, the arrest rates for those values omit the portion of the corresponding arrest rates due to arrests for robbery and burglary. While not reported, the coefficients for the robbery and burglary rates were extremely statistically significant and positive. Entire sample used over the 1977 to 1992 period.

*The result is statistically significant at the 1 percent level for a two-tailed *t*-test.

they still tend to move up and down together with all the other types of crimes.[33]

There is some evidence that changes in burglary or robbery rates reflect other changes in the criminal justice system that are omitted by the other factors already accounted for. This is suggested by their very high correlations with other crime categories.[34] The two sets of specifications reported in table 4.5 closely bound the earlier estimates, and the estimates continue to imply that the introduction of concealed-handgun laws coincided with similarly large drops in violent crimes and increases in property crimes. These results differ from the preceding results in that the nondiscretionary laws are not significant related to robberies. The estimates on the other control variables also remain essentially unchanged.[35]

Crime: Changes in Levels Versus Changes in Trends

The preceding results in this chapter examined whether the average crime rate fell after the nondiscretionary laws went into effect. If changes in the law affect behavior with a lag, changes in the trend are probably more relevant. Therefore, a more important question is: How has the crime trend changed with the change in laws? Examining whether there is a change in levels or a change in whether the crime rate is rising or falling could yield very different results. For example, if the crime rate was rising right up until the law was adopted but falling thereafter, some values that appeared while crime rate was rising could equal some that appeared as it was falling. In other words, deceptively similar levels can represent dramatically different trends over time.

I used several methods to examine changes in the trends exhibited over time in crime rates. First, I reestimated the regressions in table 4.1, using year-to-year changes on all explanatory variables (see table 4.6). These regressions were run using both a variable that equals 1 when a nondiscretionary law is in effect as well as the change in that variable (called "differencing" the variable) to see if the initial passage of the law had an impact. The results consistently indicate that the law lowered the rates of violent crime, rape, and aggravated assault. Nondiscretionary laws discourage murder in both specifications, but the effect is only statistically significant when the nondiscretionary variable is also differenced. The property-crime results are in line with those of earlier tables, showing that nondiscretionary laws produce increases in property crime. Violent crimes decreased by an average of about 2 percent annually, whereas property crimes increased by an average of about 5 percent.

Table 4.6 Results of rerunning the regressions on differences

Exogenous variables	Endogenous variables in terms of first differences of the natural logarithm of the crime rate								
	Δln(Violent-crime rate)	Δln(Murder rate)	Δln(Rape rate)	Δln(Aggravated-assault rate)	Δln(Robbery rate)	Δln(Property-crime rate)	Δln(Burglary rate)	Δln(Larceny rate)	Δln(Auto-theft rate)
	All variables except for the nondiscretionary dummy differenced								
Nondiscretionary law adopted	−2.2%***	−2.6%	−5.2%*	−4.6%*	−3.3%****	5.2%*	3.5%*	5.2%*	12.8%*
First differences in the arrest rate for the crime category	−0.05%*	−0.15%*	−0.09%*	−0.09%*	−0.06%*	−0.08%*	−0.24%*	−0.02%*	−0.02%*
	All variables differenced								
First differences in the dummy for nondiscretionary law adopted	−2.7%*	−3.6%***	−3.9%*	−5.4%*	−0.7%	4.8%*	0.7%	6.2%*	24.2%*
First differences in the arrest rate for the crime category	−0.05%*	−0.15%*	−0.09%*	−0.09%*	−0.06%*	−0.08%*	−.24%*	−0.02%*	−0.02%*

Note: The variables for income; population; race, sex, and age of the population; and density are all in terms of first differences. While not all the coefficient estimates are reported, all the control variables used in Table 4.1 are used here, including year and county dummies. All regressions use weighted least squares, where the weighting is each county's population. Entire sample used over the 1977 to 1992 period.

*The result is statistically significant at the 1 percent level for a two-tailed *t*-test.

***The result is statistically significant at the 10 percent level for a two-tailed *t*-test.

****The result is statistically significant at the 11 percent level for a two-tailed *t*-test.

Table 4.7 Permits granted by state: Florida, Oregon, and Pennsylvania

Year	Florida	Oregon	Pennsylvania
1987	17,000[a]	N.A.	N.A.
1988	33,451	N.A.	267,335[c]
1989	51,335	N.A.	314,925
1990	65,636	N.A.	360,649
1991	67,043	N.A.	399,428
1992	75,578	22,197[b]	360,919
1993	95,187	32,049	426,011
1994	134,008	43,216	492,421
1995	163,757	65,394	571,208
1996	192,016	78,258	N.A.

[a]Estimate of the number of concealed-handgun permits issued immediately before Florida's law went into effect from David McDowall, Colin Loftin, and Brian Wiersema, "Easing Concealed Firearms Laws: Effects on Homicide in Three States," *Journal of Criminal Law and Criminology,* 86 (Fall 1995): 194.
[b]December 31, 1991.
[c]Number of permits issued under discretionary law.

As one might expect, the nondiscretionary laws affected crime immediately, with an additional change spread out over time. Why would the entire effect not be immediate? An obvious explanation is that not everyone who would eventually obtain a permit to carry a concealed handgun did so right away. For instance, as shown by the data in table 4.7, the number of permits granted in Florida, Oregon, and Pennsylvania was still increasing substantially long after the nondiscretionary law was put into effect. Florida's law was passed in 1987, Oregon's in 1990, and Pennsylvania's in 1989.

Reestimating the regression results from table 4.1 to account for different time trends in the crime rates before and after the passage of the law provides consistent strong evidence that the deterrent impact of concealed handguns increases with time. For most violent crimes, the time trend prior to the passage of the law indicates that crime was rising. The results using the simple time trends for these violent-crime categories are reported in table 4.8. Figures 4.5 through 4.9 illustrate how the violent-crime rate varies before and after the implementation of nondiscretionary concealed-handgun laws when both the linear and squared time trends are employed. Comparing the slopes of the crime trends before and after the enactment of the laws shows that the trends become more negative to a degree that is statistically significant after the laws were passed.[36]

These results answer another possible objection: whether the findings are simply a result of so-called crime cycles. Crime rates rise or fall over time. If concealed-handgun laws were adopted at the peaks of these cycles (say, because concern over crime is great), the ensuing decline in crime might have occurred anyway without any help from the new laws. To deal

Table 4.8 Change in time trends for crime rates before and after the adoption of nondiscretionary laws

	Percent change in various crime rates for change in explanatory variable								
	Violent crime	Murder	Rape	Aggravated assault	Robbery	Property crime	Auto theft	Burglary	Larceny
Change in the crime rate from the difference in the annual change in crime rates in the years before and after the change in the law (annual rate after the law – annual rate before the law)	−0.9%*	−3%*	−1.4%*	−0.5%*	−2.7%*	−0.6%*	−0.3%**	−1.5%*	−0.1%

Note: The control variables are the same as those used in table 4.1, including year and county dummies, though they are not reported, because the coefficient estimates are very similar to those reported earlier. All regressions use weighted least squares, where the weighting is each county's population. Entire sample used over the 1977 to 1992 period.

*The result is statistically significant at the 1 percent level for a two-tailed *t*-test.

**The result is statistically significant at the 5 percent level for a two-tailed *t*-test.

with this, I controlled not only for national crime patterns but also for individual county patterns by using burglary or robbery rates to explain the movement in the other crime rates. I even tried to control for individual state trends. Yet the simplest way of concisely illustrating that my results are not merely a product of the "normal" ups and downs in crime rates is to look again at the graphs in figures 4.5–4.9. With the exception of aggravated assault, the drops not only begin right when the laws pass but also take the crime rates well below what they had been before the passage of the laws. It is difficult to believe that, on the average, state legislatures could have timed the passage of these laws so accurately as to coincide with the peaks of crime waves; nor can the resulting declines be explained simply as reversions to normal levels.

Was the Impact of Nondiscretionary Concealed-Handgun Laws the Same Everywhere?

Just as we found that the impact of nondiscretionary laws changed over time, we expect to find differences across states. The reason is the same in both cases: deterrence increases with the number of permits. While the information obtained from state government officials only pertained to why permits were issued at different rates across counties within a given state, the rate at which new permits are issued at the state level may also vary based upon population and population density. If this is true, then it should be possible to explain the differential effect that non-discretionary laws have on crime in each of the states that passed such laws in the same way that we examined differences across counties.

Table 4.9 reexamines my earlier regressions, where I took into account that concealed-handgun laws have different effects across counties, depending upon how lenient officials had been in issuing permits under a previously discretionary system. The one change from earlier tables is that a different coefficient is used for the counties in each of the ten states that changed their laws during the 1977 to 1992 period. At least for violent crimes, the results indicate a very consistent effect of nondiscretionary concealed-handgun laws across states. Nine of the ten states experienced declines in violent-crime rates as a result of these laws, and eight of the ten states experienced declines in murder rates; in the states where violent crimes, murders, or robberies rose, the increases were very small. In fact, the largest increases were smaller than the smallest declines in the states where those crime rates fell.

Generally, the states with the largest decreases in any one category

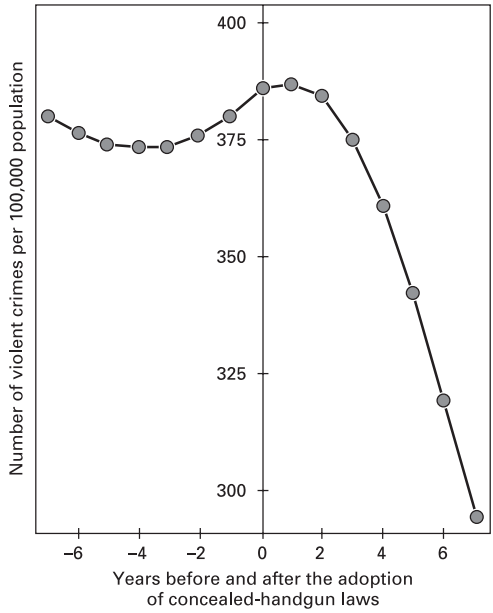


Figure 4.5. The effect of concealed-handgun laws on violent crimes

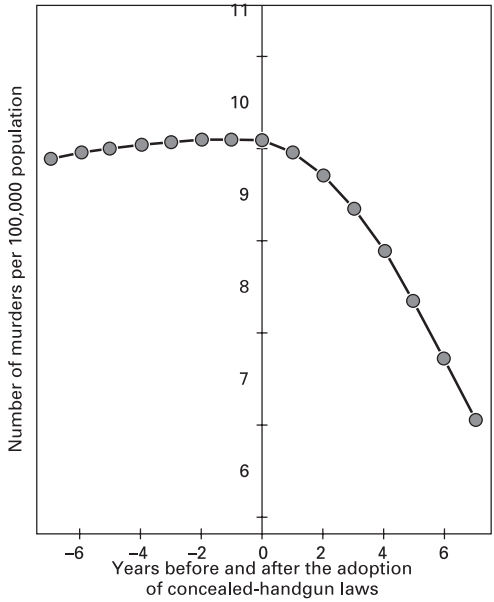


Figure 4.6. The effect of concealed-handgun laws on murders

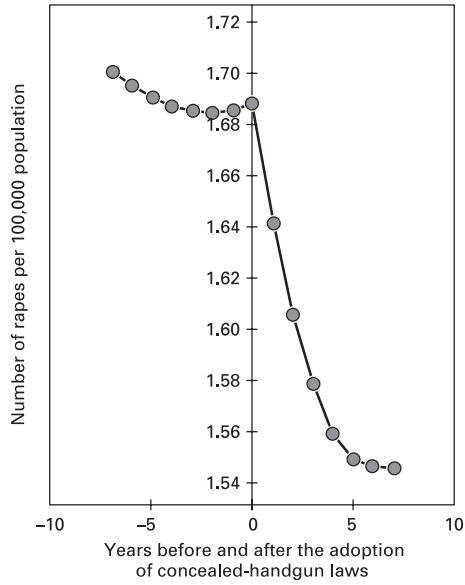


Figure 4.7. The effect of concealed-handgun laws on rapes

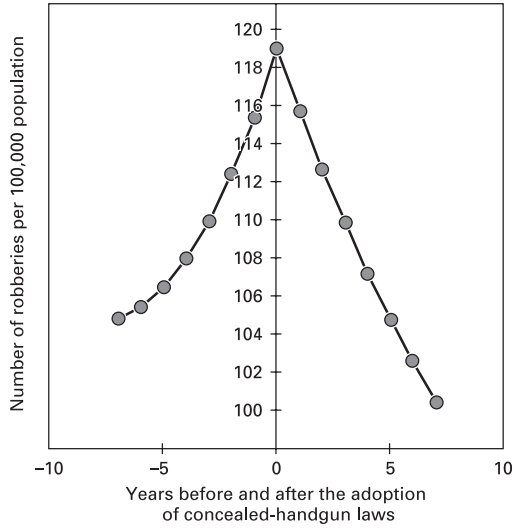


Figure 4.8. The effect of concealed-handgun laws on robberies

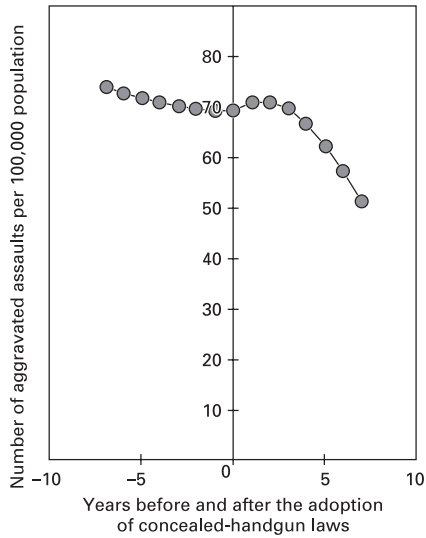


Figure 4.9. The effect of concealed-handgun laws on aggravated assaults

tended to have relatively large decreases across all the violent-crime categories, although the "leader" in each category varied across all the violent-crime categories.[37] Likewise, the states with relatively small crime decreases (for example, Georgia, Oregon, Pennsylvania, and Virginia) tended to exhibit little change across all the categories.

Property crimes, on the other hand, exhibited no clear pattern. Property crimes fell in five states and increased in five states, and the size of any decrease or increase was quite small and unsystematic.

Ideally, any comparison across states would be based on changes in the number of permits issued rather than simply the enactment of the nondiscretionary law. States with the largest increases in permits should show the largest decreases in crime rates. Unfortunately, only a few states have recorded time-series data on the number of permits issued. I will use such data in chapter 5. For the moment, it is still useful to see whether the patterns in crime-rate changes found earlier across counties are also found across states. In particular, we would like to know whether the largest declines occurred in states with the largest or most dense populations, which we believed had the greatest increase in permits. The justification for the county-level differences was very strong because it was based on conversations with individual state officials, but those officials were not asked to

Table 4.9 State-specific impact of nondiscretionary concealed-handgun laws

	Violent crime	Murder	Rape	Aggravated assault	Robbery	Property crime	Auto theft	Burglary	Larceny
Florida	–4%	–10%	–8%	–4%	0.3%	1%	2%	0.3%	2%
Georgia	0.2	–2	0.5	–0.2	0	1	1	1	1
Idaho	–3	–1	0.1	–3	–7	–1	–3	–3	–1
Maine	–17	–5	1	–24	–8	1	–4	–2	2
Mississippi	–3	0.6	3	–8	0	–0.2	3	2	–1
Montana	–10	–5	–10	–12	–6	–4	–5	5	–4
Oregon	–3	–1	–1	–3	–4	–2	3	–4	–2
Pennsylvania	–1	–3	–1	1	–2	1	3	–1	3
Virginia	–2	1	–1	–2	–2	–1	–2	–2	–1
West Virginia	–1	–11	–5	–1	1	3	0	4	2
Summary of the coefficients' signs									
Negative	9	8	6	9	6	5	4	5	5
Positive	1	2	4	1	4	5	6	5	5

Note: The table uses arrest rates adjusted for counties wherein the adoption of nondiscretionary concealed-handgun laws was most likely to represent a real change from past practice by multiplying the nondiscretionary-law variables by the population in each county. The percents are evaluated at the mean county population.

make judgments across states (nor was it likely that they could do so). Further, there is much more heterogeneity across counties, and a greater number of observations. The relationship posited earlier for county populations also seems particularly tenuous when dealing with state-level data because a state with a large population could be made up of a large number of counties with small populations.

With this list of reservations in mind, let us look at the results we get by using state-level density data. Table 4.10 provides the results with respect to population density, and we find that, just as in the case of counties, larger declines in crime were recorded in the most densely populated states. The differences are quite large: the most densely populated states experienced decreases in violent crimes that were about three times greater than the decreases in states with the average density. The results were similar when state populations were taken into account.

Other Gun-Control Laws and Different Types of Concealed-Handgun Laws

Two common restrictions on handguns arise from (1) increased sentencing penalties for crimes involving the use of a gun and (2) waiting periods required before a citizen can obtain a permit for a gun. How did these two types of laws affect crime rates? Could it be that these laws—rather than concealed-handgun laws—explain the deterrent effects? To answer this question, I reestimated the regressions in tables 4.1 and 4.3 by (1) adding a variable to control for state laws that increase sentencing penalties when crimes involve guns and (2) adding variables to measure the impact of waiting periods.[38] It is not clear whether adding an extra day to a waiting period had much of an effect; therefore, I included a variable for when the waiting period went into effect along with variables for the length of the waiting period in days and the length in days squared to pick up any differential impact from longer lengths. In both sets of regressions, the variable for nondiscretionary concealed-handgun laws remains generally consistent with the earlier results.[39] While the coefficients for arrest rates are not reported here, they also remain very similar to those shown previously.

So what about these other gun laws? The pattern that emerges from table 4.11 is much more ambiguous. The results for county-level data suggest that harsher sentences for the use of deadly weapons reduce violent crimes, especially crimes of aggravated assault and robbery. While the same county-level data frequently imply an impact on murder, rape, aggravated

Table 4.10 Effects of concealed-handgun laws across states related to differences in state population density

State population density	Violent crimes	Murder	Rape	Aggravated assault	Robbery	Property crimes	Auto theft	Burglary	Larceny
1/2 Mean	−2.7%	−3.2%	−5%	−1%	−7%	−1%	3%	−5%	1%
179 per square mile									
Mean	−5.4	−6.3	−10	−2	−14	−1	6	−10	2
358 per square mile									
Plus 1 standard deviation	−11.8	−13.7	−21	−4	−29	−3	12	−22	4
778 per square mile									
Plus 2 standard deviations	−18.2	−21.1	−32	−6	−45	−5	19	−33	7
1,197 per square mile									

Note: The regressions used for this table multiplied the variable for whether the law was enacted by that state's population density. The control variables used to generate these estimates are the same as those used in table 4.1, including year and county dummies, though they are not reported, because the coefficient estimates are very similar to those reported earlier. All regressions use weighted least squares, where the weighting is each state's population.

Table 4.11 Controlling for other gun laws

Exogenous variables	Violent crime	Murder	Rape	Aggravated assault	Robbery	Property crime	Burglary	Larceny	Auto theft
	County-level regressions								
Nondiscretionary law adopted	−4.2%*	−8.7%*	−6%*	−5.5%*	−2%	3.6%*	1%	4.5%*	8.2%*
Enhanced sentencing law adopted	−4%	−0.3%	1.1%	−1.5%***	−2.9%***	−0.001%	−2%	1.2%***	−1.8%**
Waiting law adopted	2.3%	23%*	25%*	−9.4%**	−9%***	2%	2%	−0.3%	−8%**
Percent change in crime by increasing the waiting period by one day: linear effect	−0.08%	−9.4%*	−13.6%*	6.5%*	−11%*	−1.5%***	−4.5%*	1.2%	−1%
Percent change in crime by increasing the waiting period by one day: squared effect	−0.08%	0.55%*	0.8%*	−0.5%*	0.73%*	0.019%	0.23%*	−0.17%*	0.099%
	State-level regressions								
Nondiscretionary law adopted	−10.1%*	−8.1%**	−5.7%***	−10.2%*	−13.3%*	−3.4%	−7.6%*	−2.2%	−1%
Enhanced sentencing law adopted	3.5%	3%	3%	−2.8%	1%	3%***	0.5%	3.7%**	2%
Waiting law adopted	10%	6.8%	22%*	2.6%	15%	3.3%	6.5%	2.3%	−3.1%
Percent change in crime by increasing the waiting period by one day: linear effect	−3%	−3%	−10%*	−0.65%	−10%**	−0.95%	−2.2%	−0.53%	−2.4%
Percent change in crime by increasing the waiting period by one day: squared effect	0.12%	−0.13%	0.59%*	−0.041%	0.59%**	−0.021%	0.05%	−0.06%	−0.25%

Note: The control variables are the same as those used in table 4.1, including year and county dummies, though they are not reported, because the coefficient estimates are very similar to those reported earlier. All regressions use weighted least squares, where the weighting is each county's population.

*The result is statistically significant at the 1 percent level for a two-tailed *t*-test.

**The result is statistically significant at the 5 percent level for a two-tailed *t*-test.

***The result is statistically significant at the 10 percent level for a two-tailed .

assault, and robbery, the effects are quite inconsistent. For example, simply requiring the waiting period appears to raise murder and rape rates but lower the rates for aggravated assault and robbery. The lengths of waiting periods also result in inconsistent patterns: longer periods at first lower and then raise the murder and rape rates, with the reverse occurring for aggravated assault. Using state level data fails to confirm any statistically significant effects for the violent-crime categories. First, it reveals no statistically significant or economically consistent relationship between either the presence of waiting periods or their length and violent-crime rates. The directions of the effects also differ from those found using county data. Taken together, the results make it very difficult to argue that waiting periods (particularly long ones) have an overall beneficial effect on crime rates. In addition, one other finding is clear: laws involving sentence length and waiting periods do not alter my earlier findings with respect to nondiscretionary laws; that is, the earlier results for nondiscretionary laws cannot merely be reflecting the impact of other gun laws.

The Importance of the Types of Concealed-Handgun Laws Adopted: Training and Age Requirements

Finally, we need to consider how concealed-handgun laws vary across states and whether the exact rules matter much. Several obvious differences exist: whether a training period is required, and if so, how long that period is; whether any minimum age limits are imposed; the number of years for which the permit is valid; where people are allowed to carry the gun (for example, whether schools, bars, and government buildings are excluded); residency requirements; and how much the permit costs. Six of these characteristics are reported in table 4.12 for the thirty-seven states with nondiscretionary laws in 2005.

A major issue in legislative debates on concealed-handgun laws is whether citizens will receive sufficient training to cope with situations that can require difficult, split-second decisions. Steve Grabowski, president of the Nebraska state chapter of the Fraternal Order of Police, notes that "police training is much more extensive than that required for concealed-handgun permits. The few hours of firearms instruction won't prepare a citizen to use the gun efficiently in a stress situation, which is a challenge even for professionals."[40] Others respond that significantly more training is required to use a gun offensively, as a police officer may be called on to do, than defensively. Law-abiding citizens appear reticent to use their guns

Table 4.12 Characteristics of different nondiscretionary concealed-handgun laws as of 2005

State	Last major change	Permit duration (years)	Training hours (length)	Age requirement	Initial permit fee	Renewal fee	Issuing agency
Alabama	1936	1	0	18	15	15	Sheriff
Alaska	2003	None	None	None	None	None	None
Arizona	2004	5	8	21	60	60	Dept. of Public Safety
Arkansas	1995	4	5	21	144.25	50	State Police
Colorado	2003	5	8	21	52.5	25	Sheriff
Connecticut	1986	5	5	21	35	35	State Police
Florida	1995	5	2	21	117	65	Dept. of State
Georgia	1996	5	0	21	50	50	Judge of probate court
Idaho	1996	4	7	21	56	48	Sheriff
Indiana	1980	4	0	18	10	10	Chief of police or sheriff
Kentucky	1996	5	8	21	60	60	Sheriff
Louisiana	1996	4	9	21	100	100	Dept. of Public Safety
Maine	1985	4	5	18	35	20	Chief of police or sheriff
Michigan	2000	5	8	21	105	105	State Police
Minnesota	2003	5	6	21	100	75	Sheriff
Mississippi	1990	4	0	21	124	74	Dept. of Public Safety
Missouri	2003	3	8	23	100	50	Sheriff
Montana[a]	1991	4	4	18	55	25	Sheriff
Nevada	1995	5	8	21	50	25	Sheriff

New Hampshire	1923	4	0	21	10	10	Chief of police
New Mexico	2004	0	15	18	100	100	Dept. of Public Safety
North Carolina	2002	5	8	21	90	75	Sheriff
North Dakota	1985	3	0	18	25	25	Bureau of Criminal Investigation
Ohio	2004	5	12	21	69	55	Sheriff
Oklahoma	1995	5	8	23	125	85	State Bureau of Criminal Investigation
Oregon	1993	4	5	21	65	50	Sheriff
Pennsylvania	1995	5	0	21	19	19	Chief of police or sheriff
South Carolina	1996	4	6	21	55	55	State Law Enforcement Division
South Dakota	1986	4	0	18	10	10	Chief of police or sheriff
Tennessee	1996	4	9	21	115	50	Dept. of Public Safety
Texas	1995	4	10	21	140	140	Dept. of Public Safety
Utah	1995	5	4	21	64	10	Dept. of Public Safety
Vermont	None	None	None	None	None	None	None
Virginia	1995	5	5	21	50	50	Clerk of circuit court
Washington	1995	5	0	21	60	32	Judge, chief of police, or sheriff
West Virginia	1996	5	5	18	90	90	Sheriff
Wyoming	1994	5	5	18	50	50	Attorney general

[a]Montana does not require a permit outside the city limits for the largest six cities.

and, as noted earlier, in the majority of cases simply brandishing the gun is sufficient to deter an attack.

Reestimating the earlier regressions, I included measures for whether a training period was required, for the length of the training period, and for the age limit.[41] The presence or length of the training periods typically show no effect on crime, and although the effects are significant for robbery, the size of the effect is very small. On the other hand, age limits display quite different and statistically significant coefficients for different crimes. The 21-year-old age limit appears to lower murder rates, but it tends to reduce the decline in rape and overall violent-crime rates that is normally associated with nondiscretionary concealed-handgun laws. Because of these different effects, it is difficult to draw firm conclusions regarding the effect of age limits.

Additional Data on Crime Rates

After I originally put the data together for this study, and indeed after I had written virtually all the first edition of this book, additional county-level data became available for 1993 and 1994 from the FBI's *Uniform Crime Reports*. These data allow us to evaluate the impact of the Brady law, which went into effect in 1994. Four additional states (Alaska, Arizona, Tennessee, and Wyoming) also had right-to-carry laws in effect for at least part of the year. The new information allows us to double-check whether the results shown earlier were mere aberrations.

Table 4.13 reexamines the results from tables 4.1, 4.8, and 4.11 with these new data, and the findings are generally very similar to those already reported. The results in section A that correspond to table 4.1 imply an even larger drop in murder rates related to the passage of concealed-handgun laws (10 percent versus 7.7 percent previously), though the declines in the rates for overall violent crime as well as rape and aggravated assault are smaller. Robbery is also no longer statistically significant, and the point estimate is even positive. As noted earlier, given the inverted V shape of crime-rate trends over time, comparing the average crime rates before and after the passage of these laws is not enough, since crime rates that are rising before the law and falling afterward can produce similar average crime rates in the two periods. To deal with this, section B of table 4.13 corresponds to the results reported earlier in table 4.8. The estimates are again quite similar to those reported earlier. The effect on rape is larger than those previously

Table 4.13 Earlier results reexamined using additional data for 1993 and 1994

	Percent change in various crime rates for changes in explanatory variables				
Change in explanatory variable	Violent crime	Murder	Rape	Aggravated assault	Robbery
Section A: Nondiscretionary law adopted	–4.4%*	–10.0%*	–3.0%*	–5.7%*	0.6%
Section B: The difference in the annual change in crime rates in the years before and after the change in the law (annual rate after the law minus annual rate before the law)	–0.5%*	–2.9%*	–1.7%*	–0.3%*	–2.2%*
Section C: Brady law adopted	3%	–2.3%	3.9%***	3.7%***	–3.9%

Note: This table uses county-level, violent-crime data from the *Uniform Crime Report* that were not available until the rest of the book was written. Here I was not able to control for all the variables used in table 4.1. All regressions use weighted least squares, where the weighting is each county's population. Section C also controls for the other variables that were included in Table 4.11 to account for changes in other gun laws. Section A corresponds to the regressions in table 4.1, section B to those in table 4.8, and section C to those in table 4.11, except that a dummy variable for the Brady law was added for those states that did not previously have at least a five-day waiting period.
*The result is statistically significant at the 1 percent level for a two-tailed *t*-test.
***The result is statistically significant at the 10 percent level for a two-tailed *t*-test.

reported, while the effects for aggravated assault and robbery are somewhat smaller. All the results indicate that concealed-handgun laws reduce crime, and all the findings are statistically significant.

Finally, section C of table 4.13 provides some very interesting estimates of the Brady law's impact by using a variable that equals 1 only for those states that did not previously have at least a five-day waiting period. The claims about the criminals who have been denied access to guns as a result of this law are not necessarily evidence that the Brady law lowers crime rates. Unfortunately, these claims tell us nothing about whether criminals are ultimately able to obtain guns illegally. In addition, to the extent that law-abiding citizens find it more difficult to obtain guns, they may be less able to defend themselves. For example, a woman who is being stalked may no longer be able to obtain a gun quickly to scare off an attacker. Numerous newspaper accounts tell of women who were attempting to buy guns because of threats by former lovers and were murdered or raped during the required waiting period.[42]

The evidence from 1994 indicates that the Brady law has been associated with significant increases in rapes and aggravated assaults, and the declines in murder and robbery have been statistically insignificant. All the other gun-control laws examined in table 4.11 were also controlled for here, but because their estimated impacts were essentially unchanged, they are not reported.

What Happens to Neighboring Counties in Adjacent States When Nondiscretionary Handgun Laws are Adopted?

If you put more resources in one place, it will displace some of the crime.
—Al L'Ecuyer, West Boylston (Massachusetts) Police Chief[43]

Up to this point we have asked what happens to crime rates in places that have adopted nondiscretionary laws. If these laws do discourage criminals, however, they may react in several ways. We already have discussed two: criminals could stop committing crimes, or they could commit other, less dangerous crimes, such as those involving property, where the probability of contact with armed victims is low. Yet, as the epigraph for this section notes, a third possibility is that criminals may commit crimes in other areas where potential victims are not armed. A fourth outcome is also possible: eliminating crime in one area can help eliminate crime in other areas as well. This last outcome may occur if criminals had been using the county that adopted the law as a staging area. Crime-prone, poverty-stricken areas of cities may find that some of their crime spills over to adjacent areas.

This section seeks to test what effect concealed-handgun laws and higher arrest rates have on crime rates in adjacent counties in neighboring states. Since concealed-handgun laws are almost always passed at the state level, comparing adjacent counties in neighboring states allows us to examine the differential effect of concealed-handgun laws. Evidence that changes in a state's laws coincide with changes in crime rates in neighboring states will support the claim that the laws affect criminals. If these laws do not affect criminals, neighboring states should experience no changes in their crime rates.

Although any findings that nondiscretionary concealed-handgun laws cause criminals to leave the jurisdictions that adopt these laws would provide additional evidence of deterrence, such findings would also imply that simply looking at the direct effect of concealed-handgun laws on crime overestimates the total gain to society from these laws. In the extreme, if the entire reduction in crime from concealed-handgun laws was simply transferred to other areas, society as a whole would be no better off with these laws, even though individual jurisdictions benefited. While the evidence would confirm the importance of deterrence, adopting such a law in a single state might have a greater deterrent impact than if the entire nation adopted the law. The deterrent effect of adopting nondiscretionary

Table 4.14 Estimates of the impact of nondiscretionary concealed-handgun laws on neighboring counties

	Percent change in own crime rate		
Type of crime	Own county has nondiscretionary law	Average neighbor has nondiscretionary law	Average neighbor and own county have nondiscretionary law
Violent crime	−5.5%	0	−5.7%
Murder	−7.6%	3.5%	−4.1%
Rape	−6.2%	6%	0
Robbery	−4%	2.8%	−1.1%
Aggravated assault	−7.4%	−3.3%	−10.7%
Property crime	1%	1%	2%
Auto theft	−1.3%	2%	3.4%
Burglary	1%	4.7%	−1%
Larceny	9%	−2%	10.8%

concealed-handgun laws in additional states could also decline as more states adopted the laws.

To investigate these issues, I reran the regressions reported in table 4.1, using only those counties that were within fifty miles of counties in neighboring states. In addition to the variable that examines whether your own state has a nondiscretionary concealed-handgun law, I added three new variables. One variable averages the dummy variables for whether adjacent counties in neighboring counties have such laws. A second variable examines what happens when your county and your neighboring county adopt these laws. Finally, the neighboring counties' arrest rates are added, though I do not bother reporting them, because the evidence indicates that only the arrest rates in your own county, not your neighboring counties, matter in determining your crime rate.

The results reported in table 4.14 confirm that deterrent effects do spill over into neighboring areas. For all the violent-crime categories, adopting a concealed-handgun law reduces the number of violent crimes in your county, but these results also show that criminals who commit murder, rape, and robbery apparently move to adjacent states without the laws. The one violent-crime category that does not fit this pattern is aggravated assault: adopting a nondiscretionary concealed-handgun law lowers the number of aggravated assaults in neighboring counties. With respect to the benefits of all counties adopting the laws, the last column shows that all categories of violent crime are reduced the most when all counties adopt such laws. The results imply that murder rates decline by over 8 percent and aggravated assaults by around 21 percent when a county and its neighbors adopt concealed-handgun laws.

As a final test, I generated the figures showing crime trends before and after a neighbor's adoption of the law by the method previously used, in addition to the time trends for before and after one's own adoption of the concealed-handgun laws. The use of an additional squared term allows us to see if the effect on crime is not linear. Figures 4.10–4.13 provide a graphic display of the findings for the different violent-crime categories, though the results for the individual violent-crime categories are equally dramatic. In all violent-crime categories, the adoption of concealed-handgun laws produces an immediate and large increase in violent-crime rates in neighboring counties. And in all the categories except aggravated assaults the spillover increases over time just as the counties with the nondiscretionary law see their own crime rates continue to to fall. The symmetry and timing between the reduction in counties with non-discretionary laws and increases in neighboring counties without the laws is striking.

Overall, these results provide strong additional evidence for the deter-

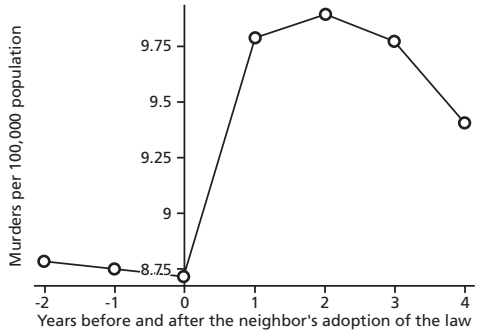


Figure 4.10. Impact on murder rate from a neighbor's adoption of nondiscretionary concealed-handgun law

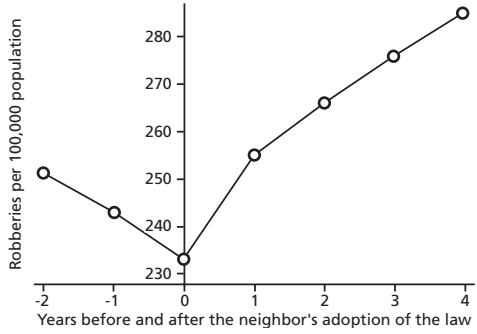


Figure 4.11. Impact on robbery rate from a neighbor's adoption of nondiscretionary concealed-handgun law

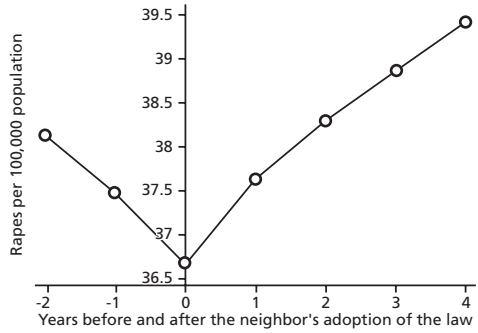


Figure 4.12. Impact on rape rate from a neighbor's adoption of nondiscretionary concealed-handgun law

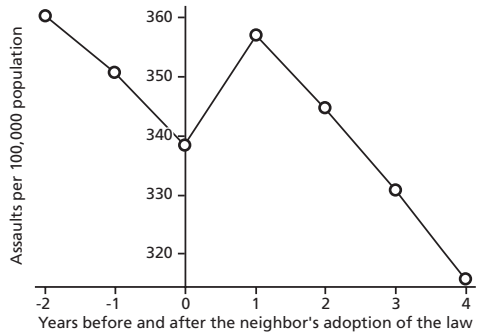


Figure 4.13. Impact on aggravated-assault rate from a neighbor's adoption of nondiscretionary concealed-handgun law

rent effect of nondiscretionary concealed-handgun laws. They imply that the earlier estimate of the total social benefit from these laws may have overestimated the initial benefits, but underestimated the long-term benefits as more states adopt these laws. In the long run, the negative spillover effect subsides, and the adoption of these laws in all neighboring states has the greatest deterrent effect on crime.

Conclusions

The empirical work provides strong evidence that concealed-handgun laws reduce violent crime and that higher arrest rates deter all types of crime. The results confirm what law-enforcement officials have said—that nondiscretionary laws cause a greatest change in the number of permits issued for concealed handguns in the most populous, urbanized counties.

This provides additional support for the claim that the greatest declines in crime rates are related to the greatest increases in concealed-handgun permits. The impact of concealed-handgun laws varies with a county's level of crime, its population and population density, its per-capita income, and the percentage of the population that is black. Despite the opposition to these laws in large, urban, densely populated areas, those are the areas that benefit the most from the laws. Minorities and women tend to be the ones with the most to gain from being allowed to protect themselves.

Some of the broader issues concerning criminal deterrence discussed in chapter 1 were evaluated, and the hypotheses used produced information about the locations where increased police efforts had the most significant deterrent effects on crime. Splitting the data set into high-and low-crime counties shows that arrest rates do not affect crime rates equally in all counties: the greatest return to increasing arrest rates is in the most crime-prone areas.

The results also confirm some of the potential aggregation problems with state-level data. The county-level data explain about six times more variation in violent-crime rates and eight times more variation in property-crime rates than do state-level data. Generally, the effect of concealed-handgun laws on crime appeared much greater when state-level regressions were estimated. However, one conclusion is clear: the very different results for state- and county-level data should make us very cautious in aggregating crime data. The differences in county characteristics show that dramatically greater differences exist among counties within any state than among different states. Whether increased arrest rates are concentrated in the highest-crime counties in a state or spread out equally across all counties makes a big difference in their impact on crime. Likewise, it is a mistake to think that concealed-handgun laws change crime rates in all counties in a state equally. The data should definitely remain as disaggregated as possible.

The three sets of estimates that rely on county-level data, state-level data, or county-level data that accounts for how the law affected different counties have their own strengths and weaknesses. While using county-level data avoids the aggregation problems present with state-level data, the initial county-level regressions rely heavily on variation in state laws and thus are limited to comparing the variation in these fifty jurisdictions. If weight is thus given to any of the results, it would appear that the greatest weight should be given to the county-level regressions that interact the nondiscretionary-law variable with measures of how liber-

ally different counties issued permits under the preexisting discretionary systems. These regressions not only avoid the aggregation problems but also take fullest advantage of the relationship between county-level variations in crime rates and the impact of nondiscretionary laws. They provide the strongest evidence that concealed-handgun laws reduce all types of crime. Despite these different approaches, one point is clear: the results are remarkably consistent with respect to the deterrent effect of nondiscretionary concealed-handgun laws on violent crime. Two of these three sets of estimates imply that concealed-handgun laws also result in lower property-crime rates, although these rates decline less than the rates for violent crimes.

This study represents a significant change in the general approach to crime studies. This is the first study to use cross-sectional time-series evidence at both the county and state levels. Instead of simply using either cross-sectional state- or city-level data, this study has made use of the much larger variations in arrest rates and crime rates between rural and urban areas, and it has been possible to control for whether the lower crime rates resulted from the gun laws themselves or from other differences in these areas (for example, low crime rates) that lead to the adoption of these laws.

5 The Victims and the Benefits from Protection

Concealed-Handgun Laws, the Method of Murder, and the Choice of Murder Victims

Do laws allowing individuals to carry concealed handguns cause criminals to change the methods they use to commit murders? For example, the number of murders perpetrated with guns may rise after such laws are passed, even though the total number of murders falls. While concealed-handgun laws raise the risk of committing murders with guns, murderers may also find it relatively more dangerous to kill using other methods once people start carrying concealed handguns, and they may therefore choose to use guns to put themselves on a more even basis with their potential prey. Using data on the methods of murder from the Mortality Detail Records provided by the U.S. Department of Health and Human Services, I reran the murder-rate regression from table 4.1 on counties with populations over 100,000 during the period from 1982 to 1991. I then separated murders committed with guns from all other murders. Table 5.1 shows that carrying concealed handguns appears to have been associated with approximately equal drops in both categories of murders. Carrying concealed handguns appears to make all types of murders relatively less attractive.

We may also wonder whether concealed-handgun laws have any effect on the types of

Table 5.1 Do concealed-handgun laws influence whether murders are committed with or without guns? Murder methods for counties with more than 100,000 people from 1982 to 1991

Exogenous variables	ln(Total murders)	ln(Murder with guns)	ln(Murders by nongun methods)
Nondiscretionary law adopted	−9.7%*	−9.0%***	−8.9%***
Arrest rate for murder increased by 100 percentage points	−0.15%*	−0.10%*	−0.14%*

Note: While not all the coefficient estimates are reported, all the control variables are the same as those used in table 4.1, including the year and county dummies. All regressions use weighted least squares, where the weighting is each county's population. The first column uses the UCR numbers for counties with more than 100,000 people. The second column uses the numbers on total gun deaths available from the Mortality Detail Records, and the third column takes the difference between the UCR numbers for total murders and Mortality Detail Records of gun deaths. Endogenous variables are in murders per 100,000 population.
*The result is statistically significant at the 1 percent level for a two-tailed *t*-test.
***The result is statistically significant at the 10 percent level for a two-tailed *t*-test.

people who are likely to be murdered. The *Supplementary Homicide Reports* of the FBI's *Uniform Crime Reports* contain annual, state-level data from 1977 to 1992 on the percent of victims by sex, race, and age, as well as information on the whether the victims and the offenders knew each other (whether they were members of the same family, knew each other but were not members of the same family, were strangers, or no relationship was known).[1] Table 5.2, which uses the same setup as in table 4.1, is intended to explain these characteristics of the victims. The regressions indicate no statistically significant relationship between the concealed-handgun law and a victim's sex, race, relationships with offenders, or age (the last is not shown). However, while they are not quite statistically significant, two of the estimates appear important and imply that in states with concealed-handgun laws victims know their nonfamily offenders 2.6 percentage points more frequently than not, and that the number of victims for whom it was not possible to determine whether a relationship existed declined by 2.9 percentage points.

This raises the question of whether the possible presence of concealed handguns causes criminals to prefer committing crimes against people they know, since presumably they would be more likely to know if an acquaintance carried a concealed handgun. The principal relationship between age and concealed handguns is that the concealed weapon deters crime against adults more than against young people—because only adults can legally carry concealed handguns—but the effect is statistically insignificant.[2] Some of the benefits from allowing adults to carry concealed handguns may be conferred on younger people whom these adults protect. In addition, when criminals who attack adults leave states that pass concealed-handgun laws, there might also be fewer criminals left

Table 5.2 Changes in characteristics of murder victims: annual, state-level data from the *Uniform Crime Reports, Supplementary Homicide Reports,* from 1977 to 1992

	Percent change in various endogenous variables for changes in explanatory variables									
	By victim's sex			By victim's race			By victim's relationship to offender			
Change in explanatory variable	Percent of male victims	Percent of female victims	Percent of victims, sex unknown	Percent of victims that are white	Percent of victims that are black	Percent of victims that are Hispanic	Percent of victims where the offender is known to victim but is not in family	Percent of victims where the offender is in the family	Percent of victims where the offender is a stranger	Percent victims where the relationship is unknown
Nondiscretionary law adopted	0.39	−0.44	0.05	0.01	0.70	−0.87	2.58	−0.25	0.54	−2.88
Arrest rate for murder increased by 100 percentage points	0.068	−0.14	0.07	−2.02**	1.32**	0.33	1.74**	−1.45*	0.79	−1.08

To interpret this table, the first coefficient (0.39) implies that the percent of male victims increases by 0.39 percentage points if a state adopts a nondiscretionary concealed-handgun law. While not all the coefficient estimates are reported, all the control variables are the same as those used in table 2.3, including the year and state dummies. All regressions use weighted least squares, where the weighting is each state's population.

*The result is statistically significant at the 1 percent level for a two-tailed *t*-test.

**The result is statistically significant at the 5 percent level for a two-tailed *t*-test.

to attack the children. The earlier evidence from figures 4.10–4.13 indicates that concealed-handgun laws actually drive criminals away, leaving fewer criminals to attack either adults or those under eighteen. Younger people may also benefit from concealed-carry laws simply because criminals cannot always easily determine who is eligible to carry a concealed handgun. Attackers may find seventeen-year-olds difficult to distinguish from eighteen-year-olds.

The arrest rates for murder produce more interesting results. The percent of white victims and the percent of victims killed by family members both declined when arrest rates were increased, while the percent of black victims and the percent killed by non–family members whom they knew both increased. The results imply that higher arrest rates have a much greater deterrent effect on murders involving whites and family members. One explanation is that whites with higher incomes face a greater increase in expected penalties for any given increase in the probability of arrest.

Mass Public Shootings

Chapter 1 noted the understandable fear that people have of mass public shootings like the one at Virginia Tech University. To record the number of mass public shootings by state from 1977 to 1992, a search was done of news-article databases (Nexis) for the same period examined in the rest of this study. A mass public shooting is defined as one that occurred in a public place and involved two or more people either killed or injured by the shooting. The crimes excluded involved gang activity; drug dealing; a holdup or a robbery; drive-by shootings that explicitly or implicitly involved gang activity, organized crime, or professional hits; and serial killings, or killings that took place over the span of more than one day. The places where public shootings occurred included such sites as schools, churches, businesses, bars, streets, government buildings, public transit facilities, places of employment, parks, health care facilities, malls, and restaurants.

Unlike the crime data we have been using, these data are available only at the state level. Table 5.3 shows the mean rate at which such killings occurred both before and after the adoption of the nondiscretionary concealed-handgun laws in the ten states that changed their laws during the 1977 to 1992 period and, more broadly, for all states that either did or did not have such laws during the period. In each case the before-and-after means are quite statistically significantly different at least at the 1 percent

Table 5.3 Mass shooting deaths and injuries

	Mean death and injury rate per year for years in which the states do not have nondiscretionary concealed-handgun laws (1)	Mean death and injury rate per year for years in which the states do have nondiscretionary concealed-handgun laws (2)
	A. Comparing the before-and-after mean mass shooting deaths and injuries for states that changed their concealed-handgun laws during the 1977–1992 period[1]	
Number of mass shooting deaths and injuries for the ten states that changed their laws during the 1977–1992 period	1.63	1.19
Mass shooting deaths and injuries per 100,000 population for the ten states that changed their laws during the 1977–1992 period	0.039	0.012
	B. Comparing the mean mass shooting deaths and injuries for all states with nondiscretionary concealed-handgun laws and those without such laws[2]	
Number of mass shooting deaths and injuries	2.09	0.89
Mass shooting deaths and injuries per 100,000 population	0.041	0.037

[1]Column 1 for section A has 128 observations; column 2 has 32 observations.
[2]Column 1 for section B has 656 observations; column 2 has 160 observations.

level,[3] with the rates being dramatically lower when nondiscretionary concealed-handgun laws were in effect. For those states from which data are available before and after the passage of such laws, the mean per-capita death rate from mass shootings in those states plummets by 69 percent.[4]

To make sure that these differences were not due to some other factor, I reestimated the specifications used earlier to explain murder rates for the state-level regressions with time trends before and after the adoption of the nondiscretionary concealed-handgun laws. The variable being explained is now the total number of deaths or injuries due to mass public shootings in a state.[5]

Figure 5.1 shows that although the total number of deaths and injuries from mass public shootings actually rises slightly immediately after a nondiscretionary concealed-handgun law is implemented, it quickly falls after that, with the rate reaching zero five years after the law is enacted.[6] Why there is an initial increase is not immediately obvious, though during this early period relatively few people have concealed-handgun permits. Perhaps those planning such shootings do them sooner than they otherwise would have, before too many citizens acquire concealed-handgun

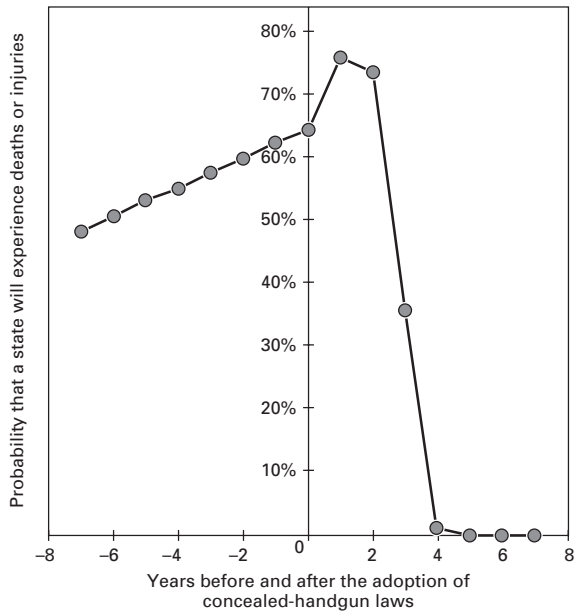


Figure 5.1. Probability that the ten states that adopted concealed-handgun laws during the 1977–1992 period experienced deaths or injuries from a shooting spree in a public place

permits. One additional qualification should also be made. While nondiscretionary concealed-handgun laws reduced deaths and injuries from mass public shootings to zero after five years in the ten states that changed their laws during the 1977 to 1992 period, a look at the mean death and injury rates from mass public shootings in the eight states that passed such laws before 1977 shows that these rates were quite low but definitely not zero. This tempers the conclusion here and implies that while deaths and injuries from mass public shootings fall dramatically after nondiscretionary concealed-handgun laws are passed, it is unlikely that the true rate will drop to zero for the average state that adopts these laws.

County Data for Arizona, Pennsylvania, and Oregon, and State Data for Florida

One problem with the preceding results was the use of county population as a proxy for how restrictive counties were in allowing concealed-handgun permits before the passage of nondiscretionary laws. Since I am still going to control for county-specific levels of crime with county dummies, a better measure would have been to use the actual change in the number of gun permits before and after the adoption of a concealed-handgun law. The per-capita number of permits provides a more direct measure of the expected costs that criminals face in attacking people. Knowing the number of permits also allows us to calculate the benefit from issuing an additional permit.

Fortunately, the information on the number of permits issued by county is available for three states: Arizona, Oregon, and Pennsylvania. Florida also provides yearly permit data at the state level. Arizona and Oregon also provided additional information on the conviction rate and the mean prison-sentence length. However, for Oregon, because the sentence-length variable is not directly comparable over time, it is interacted with all the individual year variables, so that we can still retain any cross-sectional information in the data. One difficulty with the Arizona sentence-length and conviction data is that they are available only from 1990 to 1995, and since the nondiscretionary concealed-handgun law did not take effect until July 1994, we cannot control for all the other variables that we control for in the other regressions.

Unlike Oregon and Pennsylvania, Arizona did not allow private citizens to carry concealed handguns prior to July 1994 (and permits were not actually issued until the end of the year), so the value of concealed-handgun

permits equals zero for this earlier period. Unfortunately, however, because Arizona changed its law so near the end of this period, I cannot control for all the variables that I controlled for in the other regressions. Florida's data are even more limited, but they allow the study of the simple relationship between crime and permits at the state level for a relatively long period of time.

The results in table 5.4 for Pennsylvania and table 5.5 for Oregon provide a couple of consistent patterns.[7] The most economically and statistically important relationship involves the arrest rate: higher arrest rates consistently imply lower crime rates, and in twelve of the sixteen regressions the effect is statistically significant. Five cases for Pennsylvania (violent crime, murder, aggravated assault, robbery, and burglary) show that arrest rates explain more than 15 percent of the change in crime rates.[8] Automobile theft is the only crime for which the arrest rate is insignificant in both tables.

For Pennsylvania, murder and rape are the only crimes for which per-capita concealed-handgun permits explain a greater percentage of the variation in crime rates than does the arrest rate. However, increased concealed-handgun licensing explains more than 10 percent of the variation in murder, rape, aggravated assault, and burglary rates. Violent crimes, with the exception of robbery, show that greater numbers of concealed-handgun permits lower violent crime rates, while property crimes exhibit very little relationship. The portion of the variation for property crimes that is explained by concealed-handgun licensing is only about one-tenth as large as the variation for violent crimes that is explained by such licensing, which is not too surprising, given the much more direct impact that concealed handguns have on violent crime.[9] The regressions for Oregon weakly imply a similar relationship between concealed-handgun use and crime, but the effect is only strongly statistically significant for larceny; it is weakly significant for murder.

The Oregon data also show that higher conviction rates consistently result in significantly lower crime rates. The change in conviction rates explains 4 to 20 percent of the change in the corresponding crime rates;[10] however, for five of the seven crime categories, increases in conviction rates appear to produce a smaller deterrent effect than increases in arrest rates.[11] The greatest differences between the deterrent effects of arrest and conviction rates produce an interesting pattern. For rape, increasing the arrest rate by 1 percent produces more than ten times the deterrent effect of increasing the conviction rate for those who have been arrested by 1 percent.

Table 5.4 Crime and county data on concealed-handgun permits: Pennsylvania counties with populations greater than 200,000

	Crimes per 100,000 population							
Percent change in the crime rate	**Violent crime**	**Murder**	**Rape**	**Aggravated assault**	**Robbery**	**Property crime**	**Auto theft**	**Burglary**	**Larceny**
Due to a 1 percent change in the number of right-to-carry pistol permits/population over 21 between 1988 and each year since the law was implemented	–5.3%***	–26.7%*	–5.7%***	–4.8%***	1.2%	–0.12%	1.5%	–1.4%	0.7%
Due to a 1 percent change in the arrest rate for the crime category	–0.79%*	–0.37%*	–0.08%	–0.76%*	–0.84%*	–0.41%***	–0.065%	–1.1%*	0.13%

Note: While not all the coefficient estimates are reported, all the control variables are the same as those used in table 4.1, including year and county dummies. All regressions use weighted least squares, where the weighting is each county's population. The nondiscretionary-law-times-county-population variable that was used in the earlier regressions instead of the variable for change in right-to-carry permits was tried here and produced very similar results. I also tried controlling for either the robbery or burglary rates, but I obtained very similar results.

*The result is statistically significant at the 1 percent level for a two-tailed *t*-test.

***The result is statistically significant at the 10 percent level for a two-tailed *t*-test.

Table 5.5 Crime and county data on concealed-handgun permits: Oregon data

	Crimes per 100,000 population						
Percent change in the crime rate	**Murder**	**Rape**	**Aggravated assault**	**Robbery**	**Auto theft**	**Burglary**	**Larceny**
Due to a 1 percent change in the number of right-to-carry pistol permits/population over 21 between 1988 and each year since the law was implemented	–.37%****	–6.7%	–4.8%	–4.7%	12%	2.7%	–9%**
Due to a 1 percent change in the arrest rate for the crime category	–0.34%*	–1%*	–0.4%*	–0.4%*	–.04%*	–0.7%*	–0.9%*
Due to a 1 percent change in the conviction rate for the crime category	–0.2%*	–0.09%*	–1.5%***	–0.19%*	–0.37%*	–0.27%*	–0.86%*

Note: While not all the coefficient estimates are reported, all the control variables are the same as those used in table 4.1, including year and county dummies. I also controlled for sentence length, but the different reporting practices used by Oregon over this period make its use somewhat problematic. To deal with this problem, the sentence-length variable was interacted with year-dummy variables. Thus, while the variable is not consistent over time, it is still valuable in distinguishing penalties across counties at a particular point in time. The categories for violent and property crimes are eliminated because the mean sentence-length data supplied by Oregon did not allow us to use these two categories. All regressions use weighted least squares, where the weighting is each county's population.

*The result is statistically significant at the 1 percent level for a two-tailed *t*-test.

**The result is statistically significant at the 5 percent level for a two-tailed *t*-test.

***The result is statistically significant at the 10 percent level for a two-tailed *t*-test.

****The result is statistically significant at the 11 percent level for a two-tailed *t*-test.

For auto theft, arrest seems more important than conviction: a 1 percent increase in the arrest rate reduces crime by about ten times more than the same increase in convictions. These results are consistent with the assumption that arrests produce large penalties in terms of shame or negative reputation.[12] In fact, the existing evidence shows that the reputational penalties from arrest and conviction can dwarf the legally imposed penalties.[13] This is some of the first evidence that the reputational penalties from arrests alone provide significant deterrence for some crimes.

One possible explanation for these results is that Oregon simultaneously passed both the nondiscretionary concealed-handgun law and a waiting period. The statistics in table 4.11 suggest that the long waiting period imposed by the Oregon law (fifteen days) increased murder by 5 percent, rape by 2 percent, and robbery by 6 percent. At least in the case of murder, which is weakly statistically significant in any case, the estimates from tables 4.11 and 5.5 together indicate that if Oregon had not adopted its waiting period, the drop in murder resulting from the concealed-handgun law would have been statistically significant at the 5 percent level.

The results for sentence length are not shown, but the t-statistics are frequently near zero, and the coefficients indicate no clear pattern. One possible explanation for this result is that all the changes in sentencing rules produced a great deal of noise in this variable, not only over time but also across counties. For example, after 1989, whether a crime was prosecuted under the pre- or post-1989 rules depended on when the crime took place. If the average time between when the offense occurred and when the prosecution took place differed across counties, the recorded sentence length could vary even if the actual time served was the same.

Florida's state-level data showing the changes in crime rates and changes in the number of concealed-handgun permits are quite suggestive (see figure 5.2). Cuba's Mariel Boat Lift created a sudden upsurge in Florida's murder rate from 1980 through 1982. By 1983 the murder rate had return to its pre-Mariel level, and it remained relatively constant or exhibited a slight upward trend until the state adopted its nondiscretionary concealed-handgun law in 1987. Murder-rate data are not available for 1988 because of changes in the reporting process, but the available evidence indicates that the murder rate began to drop when the law was adopted, and the size of the drop corresponded with the number of concealed-handgun permits outstanding. Ironically, the first post-1987 upward movement in murder rates occurred in 1992, when Florida began to require a waiting period and background check before issuing permits.

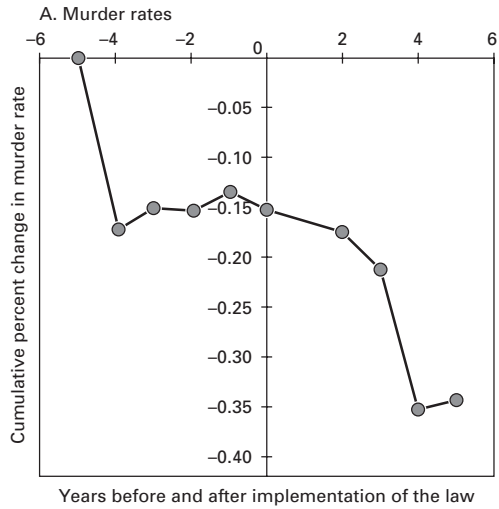


Figure 5.2A. Cumulative percent change in Florida's murder rate

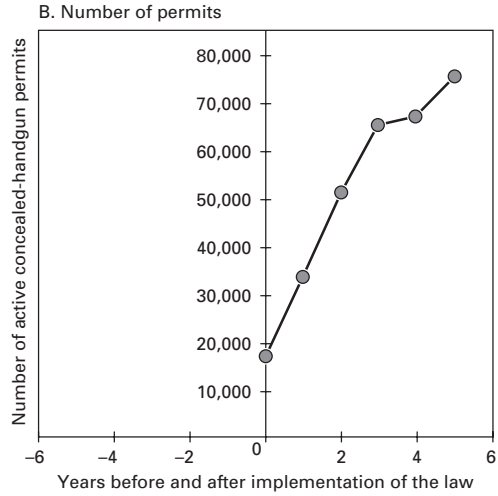


Figure 5.2B. Concealed-handgun permits after implementation of the law in Florida

Finally, a very limited data set for Arizona produces no significant relationship between the change in concealed-handgun permits and the various measures of crime rates. In fact, the coefficient signs themselves indicate no consistent pattern; the fourteen coefficients are equally divided between negative and positive signs, though six of the specifications imply

that the variation in the number of concealed-handgun permits explains at least 8 percent of the variation in the corresponding crime rates.[14] This is likely to occur for several reasons. The sample is extremely small (only 64–89 observations, depending on which specification), and we have only a year and a half over which to observe the effect of the law. In addition, if Arizona holds true to the pattern observed in other states, the impact of these laws is smallest right after the law passes.

The results involving either the mean sentence length for those sentenced in a particular year or the actual time served for those ending their sentences also imply no consistent relationship between sentence length and crime rates. While the coefficients are negative in eleven of the fourteen specifications, they provide weak evidence of the deterrent effect of longer prison terms: only two coefficients are negative and statistically significant.

The Brady law also went into effect during this period.[15] Using the Arizona data to investigate the impact of the Brady law indicates that its only discernible effect was in the category of aggravated assault, where the statistics imply that it increased the number of aggravated assaults by 24 percent and the number of rapes by 3 percent. Yet it is important to remember that the data for Arizona covered only a very short period of time when this law was in effect, and other factors influencing crime could not be taken into account. While I do not believe that the Brady law was responsible for this large increase in assaults, I at least take this as evidence that the law did not reduce aggravated assaults and as confirmation of the belief that relying on this small sample for Arizona is problematic.

Overall, Pennsylvania's results provide more evidence that concealed-handgun ownership reduces violent crime, murder, rape, aggravated assault, and burglary. For Oregon, the evidence implies that murder and larceny decrease. While the Oregon data imply that the effect of handgun permits on murder is only marginally statistically significant, the point estimate is extremely large economically, implying that a doubling of permits reduces murder rates by 37 percent. The other coefficients for Pennsylvania and Oregon imply no significant relationship between the change in concealed-handgun ownership and crime rates. The evidence from the small sample for Arizona implies no relationship between crime and concealed-handgun ownership. All the results also support the claim that higher arrest and conviction rates deter crime, although—perhaps partly because of the relatively poor quality of the data—no systematic effect appears to arise from longer prison sentences.

Putting Dollar Values on the Crime-Reduction Benefits and Private Costs of Additional Concealed-Handgun Permits

By combining evidence that additional concealed handguns reduce crime with the monetary estimates of victim losses from crime produced by the National Institute of Justice, it is possible to attach a monetary value to the benefits of additional concealed-handgun permits. While the results for Arizona imply no real savings from reduced crime, the estimates for Pennsylvania indicate that potential costs to victims are reduced by $5,079 for each additional concealed-handgun permit, and for Oregon, the savings are $3,439 per permit. As noted in the discussion of table 4.2, the results are largely driven by the effect of concealed handguns in lowering murder rates (with savings of $4,986 for Pennsylvania and $3,202 for Oregon).[16]

These estimated gains appear to far exceed the private costs of owning a concealed handgun. The purchase price of handguns ranges from $100 or less for the least-expensive .25-caliber pistols to over $700 for the newest, ultracompact, 9-millimeter models.[17] The permit-filing fees can range from $19 every five years in Pennsylvania to a first-time, $65 fee with subsequent five-year renewals at $50 in Oregon, which also requires several hours of supervised safety training. Assuming a 5 percent real interest rate and the ability to amortize payments over ten years, purchasing a $300 handgun and paying the licensing fees every five years in Pennsylvania implies a yearly cost of only $43, excluding the time costs incurred. The estimated expenses are higher for Oregon, because of the higher fees and the costs in time and money of obtaining certified safety instruction. Even if these annual costs double, however, they are still quite small compared to the social benefits. While ammunition purchases and additional annual training would increase annualized costs, the long life span of guns and their resale value work to reduce the above estimates.

The results imply that handgun permits are being issued at much lower than optimal rates, perhaps because of the important externalities not directly captured by the handgun owners themselves. While the crime-reducing benefits of concealed handguns are shared by all those who are spared being attacked, the costs of providing this protection are borne exclusively by permit holders.

Accidental Deaths and Suicides

Even if nondiscretionary handgun permits reduce murder rates, we are still left with the question of what happens to the rates for accidental death.

As more people carry handguns, accidents may be more likely. Earlier, we saw that the number of murders prevented exceeded the entire number of accidental deaths. In the case of suicide, the nondiscretionary laws increase the probability that a gun will be available when an individual feels particularly depressed; thus, they could conceivably lead to an increase in the number of suicides. While only a small portion of accidental deaths are attributable to guns (see appendix 4), the question remains whether concealed-handgun laws affect the total number of deaths through their effect on accidental deaths.

To get a more precise answer to this question, I used county-level data from 1982 to 1991 in table 5.6 to test whether allowing concealed handguns increased accidental deaths. Data are available from the Mortality Detail Records (provided by the U.S. Department of Health and Human Services) for all counties from 1982 to 1988 and for counties with populations over 100,000 from 1989 to 1991. The specifications are identical to those shown in all the previous tables, with the exceptions that they no longer include variables related to arrest or conviction rates and that the variables to be explained are either measures of the number of accidental deaths from handguns or measures of accidental deaths from all other nonhandgun sources.

While there is some evidence that the racial composition of the population and the level of welfare payments affect accident rates, the impact of nondiscretionary concealed-handgun laws is consistently both quite small economically and insignificant statistically. The first estimate in column 1 implies that accidental deaths from handguns rose by about 0.5 percent when concealed-handgun laws were passed. With only 200 accidental handgun deaths nationwide during 1988 (22 accidental handgun deaths occurred in states with nondiscretionary laws), the implication is that enacting concealed-handgun laws in states that currently do not have them would increase the number of deaths by less than one (.851 deaths). Redoing these tests by adding together accidental handgun deaths and deaths from "unknown" types of guns produces similar results.

With 186 million people living in states without concealed-handgun laws in 1992,[18] the third specification implies that implementing such laws across those remaining states would have resulted in about nine more accidental handgun deaths.[19] Combining this finding with earlier estimates from table 4.1, we find that if the rest of the country had adopted concealed-handgun laws in 1992, the net reduction in total deaths would have been approximately 1,405 to 1,583.

Table 5.6 Did nondiscretionary concealed-handgun laws increase the number of accidental deaths? (1982–91 county-level data)

	Deaths per 100,000 population			
	Ordinary least squares, natural logarithm of endogenous variable		Tobit	
Change in explanatory variable	Accidental deaths from handguns	Accidental deaths from nonhandgun sources	Accidental deaths from handguns	Accidental deaths from nonhandgun sources
Nondiscretionary law adopted	0.48%	9.9%***	0.574 more deaths	1.331 more deaths
Percent change in crime (for Tobit number of deaths per 100,000) for an increase in population of one person per square mile	−0.07%*	0.09%*	−0.004%	−0.016%
Percent change in crime (for Tobit number of deaths per 100,000) for an increase in $1,000 of real per-capita personal income	2.67%	−5.7%*	4.4%	−9%*

Note: While not all the coefficient estimates are reported, all the control variables are the same as those used in table 4.1, including year and county dummies. Absolute t-statistics are in parentheses. All regressions weight the data by each county's population.
*The result is statistically significant at the 1 percent level for a two-tailed *t*-test.
***The result is statistically significant at the 10 percent level for a two-tailed *t*-test.

One caveat should be added to these numbers, however: both columns 2 and 4 indicate that accidental deaths from nonhandgun sources increased by more than accidental deaths from handguns after the nondiscretionary concealed-handgun laws were implemented. To the extent that the former category increased because of uncontrolled factors that also increase accidental deaths from handguns, the results presented here are biased toward finding that concealed-handgun laws have increased accidental deaths from handguns.

Finally, I examined similar specifications using data on suicide rates. The possibility exists that if a person becomes depressed while away from home, the presence of a concealed handgun might encourage that person to act impulsively, whereas an enforced delay might ultimately prevent a suicide. If anything, the results implied a statistically insignificant and small increase in suicides (less than one-tenth of 1 percent). Hence it is reasonable to conclude that no relationship exists between concealed-handgun laws and suicide rates.

Total Gun Ownership and Crime

Traditionally, people have tried to use cross-country comparisons of gun ownership and crime rates to determine whether gun ownership enhances or detracts from safety.[20] Worldwide, there is no relationship between gun ownership and crime rates. Many countries, such as Switzerland, Finland, New Zealand, and Israel, have high gun ownership rates and low crime rates, while many other countries have both low gun ownership rates and either high or low crime rates. For example, in 1995 Switzerland's murder rate was 40 percent lower than Germany's despite having a three-times higher gun-ownership rate. Yet, making a reliable comparison across countries is an arduous task simply because it is difficult to obtain gun ownership data both over time and across countries, and to control for all the other differences across the legal systems and cultures across countries. International comparisons are also risky because polls underreport ownership in countries where gun ownership is illegal, and the polls are conducted by different polling organizations that ask questions in widely differing ways. How crime is measured also varies across countries.

Fortunately, more consistent data are available to investigate the relationship between total gun ownership in the United States and crime. In chapter 3, I presented poll data from general-election surveys that offer consistent polling across states, showing how gun ownership varied across

states for 1988 and 1996. There is broad variation in gun ownership across states, and the crime rates also vary across states and over time. Even with rather few observations, however, these data suggest that we may be able to answer an obvious question: Is the crime rate higher in states with more guns?

To test the relationship between gun ownership and crime, I attempted to examine the relationship between the percentage of the adult population owning guns and the crime rate after accounting for the arrest rate, real personal income, population per square mile, state dummy variables, the percentage of blacks among each state's population, and a variable to pick up the average change in crime rates between 1988 and 1995. This last variable was also intended to help pick up any differences in the results that arise from the slightly different poll methods in the two years. Ideally, one would want to construct the same type of cross-sectional, time-series data set over many years and states that was used in the earlier discussions; unfortunately, however, such extensive poll data on gun ownership are not available. Because we lack the most recent data for the above-named variables, all the variables except for the percentage of the state's adult population that owns guns is for 1995.

As table 5.7 shows, a strong negative relationship exists between gun ownership and all of the crime rates except for rape, and the results are statistically significant for seven of the nine categories. Indeed, the effect of

Table 5.7 The relationship between state crime rates and the general election poll data on the percent of the state's adult population owning guns

Crime rates	Percent change in the crime rate from a 1 percentage point increase in a state's gun ownership rate	Estimated change in victim costs from a 1 percent increase in the number of guns nationwide
Violent crime	–4.1*	
Murder	–3.3*	$2.7 billion
Rape	0	
Aggravated assault	–4.3*	$44 million
Robbery	–4.3*	$200 million
Property crime	–1.5**	
Burglary	–1.6*	$54 million
Larceny	–1.3	$38 million
Auto theft	–3.2*	$17 million
Total savings		$3.1 billion

Note: While the other coefficient values are not reported here, these regression results control for the arrest rate, real personal income, population per square mile, state dummy variables, the percent of the state's population that is black, and a year-dummy variable for 1996 to pick up the average change in crime rate between the years. All regressions use weighted least squares, where the regressions are weighted by the state populations.
*The result is statistically significant at the 1 percent level for a two-tailed *t*-test.
**The result is statistically significant at the 5 percent level for a two-tailed *t*-test.

gun ownership on crime is quite large: a 1 percent increase in gun ownership reduces violent crime by 4.1 percent. The estimates from the National Institute of Justice of the costs to victims of crime imply that increasing gun ownership nationwide by 1 percent would reduce victim costs by $3.1 billion, though we must bear in mind that these conclusions are based on a relatively small sample. Similar estimates for accidental gun deaths or suicides reveal no significant relationships.

Conclusion

Nondiscretionary concealed-handgun laws have equal deterrent effects on murders committed both with and without guns. Despite differences in the rates at which women and men carry guns, no difference exists in the total benefit the two sexes derive in terms of reduced murder rates. The evidence strongly rejects claims that criminals will be more likely to use firearms when their potential victims are armed. Furthermore, the increased presence of concealed handguns under nondiscretionary laws does not raise the number of accidental deaths or suicides from handguns.

As in other countries, people who engage in mass public shootings are deterred by the possibility that law-abiding citizens may be carrying guns. Such people may be deranged, but they still appear to care whether they will themselves be shot as they attempt to kill others. The results presented here are dramatic: states that adopted nondiscretionary laws during the 1977–1992 period virtually eliminated mass public shootings after four or five years. These results raise serious concerns over state and federal laws banning *all* guns from schools and the surrounding area. At least permitting school employees access to guns would seem to make schools less vulnerable to mass shootings.

One prominent concern about leniency in permitting people to carry concealed handguns is that the number of accidental deaths might rise, but I can find no statistically significant evidence that this occurs. Even the largest estimate of nine more accidental deaths per year is extremely small in comparison to the number of lives saved from fewer murders.

The evidence for Pennsylvania and Oregon also provides the first estimates of the annual social benefits that accrue from private expenditures on crime reduction. Each additional concealed-handgun permit reduces total losses to victims by between three and five thousand dollars. The results imply that handgun permits are being obtained at much lower than optimal rates in two of the three states for which I had the relevant

data, perhaps because the individual owners bear all the costs of owning their handguns but receive only a small fraction of the total benefits. The evidence implies that concealed handguns are the most cost-effective method of reducing crime that has been analyzed by economists; they provide a higher return than increased law enforcement or incarceration, other private security devices, or social programs like early educational intervention.[21]

The general-election exit-poll data may also be used to calculate the change in total costs to crime victims when more people own guns. These preliminary estimates are quite dramatic, indicating that, nationwide, each 1 percent increase in the number of people owning guns reduces victim costs by over 3 billion dollars.

The data continue to supply strong evidence supporting the economic notion of deterrence. Higher arrest and conviction rates consistently and dramatically reduce the crime rate. Consistent with other recent work,[22] the results imply that increasing the arrest rate, independent of the probability of eventual conviction, imposes a significant penalty on criminals. Perhaps the most surprising result is that the deterrent effect of a 1 percent increase in arrest rates is much larger than the same increase in the probability of conviction. It was also surprising that while longer prison terms usually implied lower crime rates, the results were normally not statistically significant.

6 What Determines Arrest Rates and the Passage of Concealed-Handgun Laws?

The regressions used in previous chapters took both the arrest rate and the passage of nondiscretionary concealed-handgun laws as given. This chapter deals with the unavoidably complicated issue of determining whether the variables I am using to explain the crime rate are in themselves determined by other variables. Essentially, the findings here confirm the deterrence effect of concealed-handgun laws and arrest rates.

Following the work of Isaac Ehrlich, I now let the arrest rate depend on crime rates as well as on population measures and the resources invested in police.[1] The following crime and police measures were used: the lagged crime rates; measures of police employment and payroll per capita, per violent crime, and per property crime at the state level (these three measures of employment are also broken down by whether police officers have the power to make arrests). The population measures were as follows: income; unemployment insurance payments; the percentages of county population by age, sex, and race (already used in table 4.1); and county and year dummy variables.[2] In an attempt to account for political influences, I further included the percentage of a state's population belonging to the National Rifle Association, along with the percentage voting for the Republican presidential candidate.[3]

Because presidential candidates and political

issues vary from election to election, the variables for the percentage voting Republican are not perfectly comparable across years. To account for these differences across elections, I used the variable for the percentage voting Republican in a presidential election for the years closest to that election. Thus, the percent of the vote obtained in 1980 was multiplied by the individual year variables for the years from 1979 to 1982, the percent of the vote obtained in 1984 was multiplied by the individual year variables for the years from 1983 to 1986, and so on through the 1992 election. A second set of regressions explaining the arrest rate also includes the change in the log of the crime rates as a proxy for the difficulties that police forces may face in adjusting to changing circumstances.[4] The time period studied in all these regressions, however, is more limited than in the previous tables because the state-level data on police employment and payroll available from the U.S. Department of Justices' Expenditure and Employment data set for the criminal justice system covered only the years from 1982 to 1992.

Aside from the concern over what determines the arrest rate, we want to answer another question: Why did some states adopt nondiscretionary concealed-handgun laws while others did not? As noted earlier, if states adopted such laws because crime rates were either rising or expected to rise, our preceding regression estimates (using ordinary least-squares) will underestimate the drop in crime. Similarly, if such laws were adopted because crime rates were falling, the bias is in the opposite direction—the regression will overestimate the drop in crime. Thus, in order to explain whether a county was likely to be in a state that had adopted concealed-handgun laws, I used the rates for both violent crime and property crime, along with the change in those crime rates.[5] To control for general political differences that might affect the chances for the passage of these laws, I also included the percentage of a state's population that belonged to the National Rifle Association; the Republican presidential candidate's percentage of the statewide vote; the percentage of blacks and whites in a state's population; the total population in the state; regional dummy variables for whether the state is in the South, Northeast, or Midwest; and year dummy variables.

The regressions reported here are different from those reported earlier because they allow us to let the crime rate depend on the variables for the concealed-handgun law and the arrest rate, as well as on other variables, but the variables for the concealed-handgun law and the arrest rate are in turn dependent on other variables.[6] While these estimates use the same set of control variables employed in the preceding tables, the results differ from all my previous estimates in one important respect: nondiscretionary concealed-

handgun laws are associated with large, significant declines in all nine crime categories. I tried estimating a specification that mimicked the regressions in Ehrlich's study. Five of the nine crime categories implied that a change of one standard deviation in the predicted value of the nondiscretionary-law variable explains at least 10 percent of a change of one standard deviation in the corresponding crime rates. Nondiscretionary concealed-handgun laws explain 11 percent of the variation in violent crime, 7.5 percent of the variation in murder, 6 percent for rape, 10 percent for aggravated assault, and 5 percent for robbery. In fact, concealed-handgun laws explain a greater percentage of the change in murder rates than do arrest rates.

A second approach examined what happened to the results when the arrest rate was determined not only by past crime rates but also by the change in the crime rate in the previous year. The concern here is that rapid changes in crime rates make it more difficult for police agencies to maintain the arrest rates they had in the past. With the exception of robbery, the new set of estimates using the change in crime rates to explain arrest rates indicated that the effect of concealed-handgun laws was usually more statistically significant but economically smaller. For example, in the new set of estimates, concealed-handgun laws explained 3.9 percent of the variation in murder rates compared to 7.5 percent for the preceding estimates. While these results imply that even crimes involving relatively little contact between victims and criminals experienced declines, nondiscretionary concealed-handgun laws reduced violent crimes by more than they reduced property crimes.

Both sets of estimates provide strong evidence that higher arrest rates reduce crime rates. Among violent crimes, rape consistently appears to be the most sensitive to higher arrest rates. Among property crimes, larceny is the most sensitive to higher arrest rates.

The estimates explaining which states adopt concealed-handgun laws show that the states adopting these laws are relatively Republican with large National Rifle Association memberships and low but rising rates of violent crime and property crime. The set of regressions used to explain the arrest rate shows that arrest rates are lower in high-income, sparsely populated, Republican areas where crime rates are increasing. This evidence calls into question claims that police forces are not catching criminals in high-crime, densely populated areas.

I reestimated the state-level data using similar specifications. The coefficients on the variables for both arrest rates and concealed-handgun laws remained consistently negative and statistically significant. The state-level

data again implied a much stronger effect from the passage of concealed-handgun laws and a much weaker effect from higher arrest rates. In order to use the longer data series available for the nonpolice employment and payroll variables, I even reestimated the regressions without those variables. This produced similar results.[7]

Finally, using the predicted values for the arrest rates allows us to investigate the significance of another weakness of the data. The arrest-rate data suffers not only from some missing observations but also from some instances where it is undefined when the crime rate in a county equals zero. This last issue is problematic only for murders and rapes in low-population counties. In these cases, both the numerator and denominator in the arrest rate equal zero, and it is not clear whether I should count this as an arrest rate equal to 100 or 0 percent, neither of which is correct, as it is truly undefined. The previously reported evidence arising from regressions that were run only on the larger counties (population over 10,000) sheds some light on this question, since these counties have fewer observations with undefined arrest rates. In addition, if the earlier reported evidence that adopting nondiscretionary concealed-handgun laws changed the number of permits the least in the lower-population counties, one would expect relatively little change in counties with missing observations.

The analysis presented in this section allowed us to try another, more appropriate approach to deal with this issue.[8] I created predicted arrest rates for these observations using the regressions that explain the arrest rate, and then I reestimated the regressions with the new, larger samples. While the coefficient for murder declined, implying a 5 percent drop when nondiscretionary laws are adopted, the coefficient for rape increased, implying a drop of more than 10 percent. Only very small changes appeared in the other estimates. All coefficients were statistically significant. The effect of arrest rates also remained negative and statistically significant. As one final test to deal with the problems that arise from using the arrest rates, I reestimated the regressions using only the predicted values for the nondiscretionary-law variable. In this case the coefficients were always negative and statistically significant, and they indicate that these laws produce an even larger negative effect on crime than the effect shown in the results already reported.

Conclusion

Explicitly accounting for the factors that influence a state's decision to adopt a nondiscretionary concealed-handgun law and that determine the

arrest rate only serves to strengthen the earlier results: with this approach, both concealed-handgun laws and arrest rates explain much larger percentages of the changes in the crime rate than they did earlier. Several other facts are clear. Up through the early 1990s, nondiscretionary laws were adopted by relatively low-crime states in which the crime rate is rising. These states have also tended to vote Republican and to have high percentages of their populations enrolled in the National Rifle Association.

For studies that use the number of police officers as a proxy for the level of law enforcement, these results suggest some caution. Property-crime rates appear to have no systematic relationship to the number of police officers either with or without the power to make arrests. For violent crime, the presence of more police officers *with* arrest powers lowers the arrest rate, while a greater number of police officers *without* arrest powers raises the arrest rate.

Neither of these results alone is particularly troubling, because increasing the number of police officers could reduce the crime rate enough so that the arrest rate could fall even if the officers did not slack off. Theoretically, the relationship between the number of police officers and the arrest rate could go either way. Yet in the case of violent crimes, the drop in arrest rates associated with more police officers is too large to be explained by a drop in the crime rate. In fact, the direct relationship between the number of police officers and violent crime implies a positive relationship. There are many possible explanations for this. Quite plausibly, the presence of more police officers encourages people to come forward to report crime. Another possibility is that relatively large police forces tend to be unionized and have managed to require less work from their officers. The bottom line is that using the number of police officers directly as a proxy for the level of law enforcement is at best a risky proposition. We must control for many other factors before we know exactly what we are measuring.

7 The Political and Academic Debate by 1998

The Political Process

When my original study was released in 1996, many commentators were ready to attack it. Anyone who had shown any interest in looking at the article was given a copy while I was in the process of revising it for the *Journal of Legal Studies,* although I quickly learned that it was not common practice to circulate studies to groups on both sides of the gun debate. Few comments were offered privately, but once the paper began to receive national press coverage, the attacks came very quickly.

Before the press coverage started, it was extremely difficult to get even a proponent of gun control to provide critical comments on the paper when I presented it at the Cato Institute in early August 1996. I approached twenty-two pro-control people before Jens Ludwig, a young assistant professor at Georgetown University, accepted my request to comment on the paper.

One of the more interesting experiences occurred when I asked Susan Glick, of the Violence Policy Center, to participate.[1] Glick, whom I called during June 1996, was one of the last people that I approached. She was unwilling to comment on my talk at Cato because she didn't want to "help give any publicity to the paper." Glick said that her appearance might help bring media attention to the paper that it wouldn't otherwise have gotten. When I pointed out that

C-SPAN was likely to cover the event, she said she didn't care because "we can get good media whenever we want." When I asked her if I could at least send her a copy of the paper because I would appreciate any comments that she might have, she said, "Forget it, there is no way that I am going to look at it. Don't send it."[2]

However, when the publicity broke on the story with an article in *USA Today* on August 2, she was among the many people who left telephone messages immediately asking for a copy of the paper. In her case, the media were calling, and she "need[ed] [my] paper to be able to criticize it." Because of all the commotion that day, I was unable to get back to her right away. ABC National Television News was doing a story on my study for that day, and when at around 3:00 P.M. the ABC reporter doing the story, Barry Serafin, called saying that certain objections had been raised about my paper, he mentioned that one of those who had criticized it was Ms. Glick. After talking to Mr. Serafin, I gave Glick a call to ask her if she still wanted a copy of my paper. She said that she wanted it sent to her right away and wondered if I could fax it to her. I then noted that her request seemed strange because I had just gotten off the telephone with Mr. Serafin at ABC News, who had told me that she had been very critical of the study, saying that it was "flawed." I asked how she could have said that there were flaws in the paper without even having looked at it yet. At that point Ms. Glick hung up the telephone.[3]

Many of the attacks from groups like Handgun Control, Inc. and the Violence Policy Center focused on claims that my study had been paid for by gun manufacturers or that the *Journal of Legal Studies* was not a peer-reviewed journal and that I had chosen to publish the study in a "student-edited journal" to avoid the close scrutiny that such a review would provide.[4] These attacks were completely false, and I believe that those making the charges knew them to be false. At least they had been told by all the relevant parties here at the University of Chicago and at the Olin Foundation that the funding issues were false, and the questions about publishing in a "student-edited journal" or one that was not peer-reviewed were well known to be false because of the prominence of the journal. Some statements involved claims that my work was inferior to an earlier study by three criminologists at the University of Maryland who had examined five counties.

Other statements, like those in the *Los Angeles Times,* tried to discredit the scholarliness of the study by claiming that "in academic circles, meanwhile, scholars found it curious that he would publicize his findings before they

were subjected to peer review."[5] In fact, the paper was reviewed and accepted months before media stories started discussing it in August 1996.

The attacks claiming that this work had been paid for by gun manufacturers have been unrelenting. Congressman Charles Schumer (D-N.Y.) wrote as follows in the *Wall Street Journal:* "I'd like to point out one other 'association.' The Associated Press reports that Prof. Lott's fellowship at the University of Chicago is funded by the Olin Foundation, which is 'associated with the Olin Corporation,' one of the nation's largest gun manufacturers. Maybe that's a coincidence, too. But it's also a fact."[6] Others were even more direct. In a letter that the Violence Policy Center mass-mailed to newspapers around the country, M. Kristen Rand, the Center's federal policy director, wrote,

> Lott's work was, in essence, funded by the firearms industry—the primary beneficiary of increased handgun sales. Lott is the John M. Olin fellow at the University of Chicago law school, a position founded by the Olin Foundation. The foundation was established by John Olin of the Olin Corp., manufacturer of Winchester ammunition and maker of the infamous "Black Talon" bullet. Lott's study of concealed handgun laws is the product of gun-industry funding. . . . (See, as one of many examples, "Gun Industry Paid," *Omaha World Herald,* March 10, 1997, p. 8.)[7]

Dan Kotowski, executive director of the Illinois Council Against Handgun Violence, said that "the study was biased because it was funded by the parent company of Winchester, Inc., a firearms manufacturer."[8] Kotowski is also quoted as saying that the claimed link between Winchester and my study's conclusions was "enough to call into question the study's legitimacy. It's more than a coincidence."[9] Similar claims have been made by employees of Handgun Control, Inc. and other gun-control organizations.

Indeed, gun-control groups that were unwilling to comment publicly on my study at the Cato Institute forum had time to arrange press conferences that were held exactly at the time that I was presenting my paper in Washington. Their claims were widely reported by the press in the initial news reports on my findings. A typical story stated that "Lott's academic position is funded by a grant from the Olin Foundation, which is associated with the Olin Corp. Olin's Winchester division manufactures rifles and bullets,"[10] and it was covered in newspapers from the *Chicago Tribune* to the *Houston Chronicle* and the *Des Moines Register,* as well as in "highbrow" publications like *The National Journal*. The Associated Press released a partial correc-

tion stating that the Olin Foundation and Olin Corporation are separate organizations and that the Winchester subsidiary of the Olin Corporation makes ammunition, not guns, but a Nexis search of news stories revealed that only one newspaper in the entire country that had published the original report carried the Associated Press correction.[11]

Congressman Schumer's letter did produce a strong response from William Simon, the Olin Foundation's president and former U.S. Secretary of the Treasury, in the *Wall Street Journal* for September 6, 1996:

> An Insult to Our Foundation
>
> As president of the John M. Olin Foundation, I take great umbrage at Rep. Charles Schumer's scurrilous charge (Letters to the Editor, Sept. 4) that our foundation underwrites bogus research to advance the interests of companies that manufacture guns and ammunition. He asserts (falsely) that the John M. Olin Foundation is "associated" with the Olin Corp. and (falsely again) that the Olin Corp. is one of the nation's largest gun manufacturers. Mr. Schumer then suggests on the basis of these premises that Prof. John Lott's article on gun-control legislation (editorial page, Aug. 28) must have been fabricated because his research fellowship at the University of Chicago was funded by the John M. Olin Foundation.
>
> This is an outrageous slander against our foundation, the Olin Corp., and the scholarly integrity of Prof. Lott. Mr. Schumer would have known that his charges were false if he had taken a little time to check his facts before rushing into print. Others have taken the trouble to do so. For example, Stephen Chapman of the *Chicago Tribune* looked into the charges surrounding Mr. Lott's study, and published an informative story in the Aug. 15 issue of that paper, which concluded that, in conducting his research, Prof. Lott was not influenced either by the John M. Olin Foundation or by the Olin Corp. Anyone wishing to comment on this controversy ought first to consult Mr. Chapman's article and, more importantly, should follow his example of sifting the facts before reaching a conclusion. For readers of the *Journal*, here are the key facts.
>
> The John M. Olin Foundation, of which I have been president for nearly 20 years, is an independent foundation whose purpose is to support individuals and institutions working to strengthen the free enterprise system. We support academic programs at the finest institutions in the nation, including the University of Chicago, Harvard, Yale, Stanford, Columbia, the University of Virginia, and many others. We do not tell scholars what to write or what to say.

The foundation was created by the personal fortune of the late John M. Olin, and is not associated with the Olin Corp. The Olin Corp. has never sought to influence our deliberations. Our trustees have never taken into account the corporate interests of the Olin Corp. or any other company when reviewing grant proposals. We are as independent of the Olin Corp. as the Ford Foundation is of the Ford Motor Co.

The John M. Olin Foundation has supported for many years a program in law and economics at the University of Chicago Law School. This program is administered and directed by a committee of faculty members in the law school. This committee, after reviewing many applications in a very competitive process, awarded a research fellowship to Mr. Lott. We at the foundation had no knowledge of who applied for these fellowships, nor did we ever suggest that Mr. Lott should be awarded one of them. We did not commission his study, nor, indeed, did we even know of it until last month, when Mr. Lott presented his findings at a conference sponsored by a Washington think tank.

As a general rule, criticism of research studies should be based on factual grounds rather than on careless and irresponsible charges about the motives of the researcher. Mr. Lott's study should be evaluated on its own merits without imputing motives to him that do not exist. I urge Mr. Schumer to check his facts more carefully in the future.

Finally, it was incorrectly reported in the *Journal* (Sept. 5) that the John M. Olin Foundation is 'headed by members of the family that founded the Olin Corp.' This is untrue. The trustees and officers of the foundation have been selected by virtue of their devotion to John Olin's principles, not by virtue of family connections. Of our seven board members, only one is a member of the Olin family. None of our officers is a member of the Olin family—neither myself as president, nor our secretary-treasurer, nor our executive director.

This letter, I think, clarifies the funding issue, and I would only like to add that while the faculty at the Law School chose to award me this fellowship, even they did not inquire into the specific research I planned to undertake.[12] The judgment was made solely on the quality and quantity of my past research, and while much of my work has dealt with crime, this was my first project involving gun control. No one other than myself had any idea what research I was planning to do. However, even if one somehow believed that Olin were trying to buy research, it must be getting a very poor return on its money. Given the hundreds of people at the different

universities who have received the same type of fellowship, I have been the only one to work on the issue of gun control.

Unfortunately, as the quote from Ms. Rand's letter and statements by many other gun-control advocates—made long after Simon's explanation—indicates, the facts about funding did little to curtail the comments of those spreading the false rumors.[13]

After these attacks on my funding, the gun-control organizations brought up new issues. For example, during the spring of 1997 the Violence Policy Center sent out a press release entitled "Who Is John Lott?" that claimed, among other things, "Lott believes that some crime is good for society, that wealthy criminals should not be punished as harshly as poor convicts." I had in fact been arguing that "individuals guilty of the same crime should face the same expected level of punishment" and that with limited resources to fight crime, it is not possible to eliminate all of it.[14] I would have thought that most people would recognize these silly assertions for what they were, but they were picked up and republished by publications such as the *New Republic*.[15]

The aversion to honest public debate has been demonstrated to me over and over again since my study first received attention. Recently, for example, Randy Roth, a visiting colleague at the University of Chicago Law School, asked me to appear on a radio program that he does from the University of Hawaii on a public radio station. I had almost completely stopped doing radio interviews a few months before because they were too much of an interruption to my work, but Randy, whom I have known only very briefly from lunch-table conversation, seemed like a very interesting person, and I thought that it would be fun to do the show with him. I can only trust that he doesn't normally have as much trouble as he had this time in getting an opposing viewpoint for his program. In a note that Randy shared with me, he described a conversation that he had with Brandon Stone, of the Honolulu Police Department, whom he had been trying for a while to get to participate. Randy wrote as follows on March 3, 1997:

> Brandon called to say he had not changed his mind—he will not participate in any gun-control radio show involving John Lott. Furthermore, he said he had discussed this with all the others who are active in this area (the Hawaii Firearms Coalition, I think he called it), and that they have "banded together"—none will participate in such a show.
>
> He said he didn't want to "impugn" John's character . . . [and] then he went on to talk about all the money involved in this issue, the fact that

[the] Olin Corp. is in the firearm business and financing John's chair, etc. He said John's study had been given to the media before experts first could discredit it, implying that this "tactic" was used because the study could not withstand the scrutiny of objective scholars.

He said the ideas promoted by John's study are "fringe ideas" and that they are "dangerous." When I pointed out that such ideas not only have been publicly debated in other states, but that some of those states actually have enacted legislation, he basically just said that Hawaii is a special place and other states have sometimes been adversely affected by unfair tactics by the pro-gun lobby.

I kept coming back to my belief that public debate is good and that my show would give him an opportunity to point out anything about John's study that he believes to be incorrect, irrelevant, distorted, or whatever. He kept saying that public debate does more harm than good when others misuse the forum. When he specifically mentions the firearm industry ("follow the money" was his suggestion, to understand what John's study is all about), I reminded him of John's association with the University of Chicago and his outstanding reputation, both for scholarship and integrity. He then said he realized John was "my friend," as though I couldn't be expected to be objective. He also said that John was "out of his field" in this area.

My hunch is that it's going to be extremely difficult finding a studio guest with the credentials and ability to do a good job on the pro-gun-control side.

After talking with Randy and in an attempt to create a balanced program, I also telephoned Mr. Stone. While we did not get into the detail that he went into with Randy, I did try to address his concerns over my funding and my own background in criminal justice as chief economist at the U.S. Sentencing Commission during the late 1980s. Stone also expressed his concerns to me that Hawaiians would not be best served by our debating the issue and that Hawaiians had already made up their minds on this topic. I said that he seemed like an articulate person and that it would be good to have a lively discussion on the subject, but he said that the program "could only do more harm than good" and that any pro-gun-control participation would only lend "credibility" to the discussion.[16]

Before I did my original study, I would never have expected it to receive the attention that it did. None of the refereed journal articles that I had produced had received so much attention. Many people have told me

that this was politically naive. That may be, but this much is clear: I never would have guessed how much people fear discussion of these issues. I never would have known how much effort goes into deliberately ignoring certain findings in order to deny them news coverage. Nor would I have seen, after news coverage did occur, how much energy goes into attacking the integrity of those who present such findings, with such slight reference—or no reference at all—to the actual merits of the research. I was also surprised by the absolute confidence shown by gun-control advocates that they could garner extensive news coverage whenever they wanted.

Criticisms of the Original Study

A second line of attack came from academic, quasi-academic, and gun-control advocacy groups concerning the competence with which the study was conducted. Many of these objections were dealt with somewhere in the original study, which admittedly is very long. Yet it should have been easy enough for critics—especially academics—to check.

The attacks were fairly harsh, especially by the standards of academic discourse. For example,

> "They highlight things that support their hypothesis while they ignore things contrary to their hypothesis," said Daniel Webster, an assistant professor at Johns Hopkins University Center for Gun Policy and Research.
>
> "We think the study falls far short of any reasonable standard of good social science research in making [their] case," said economist Daniel Nagin of Carnegie-Mellon University, who has analyzed Lott's data with colleague Dan Black.[17]

I have made the data I used available to all academics who have requested them, and professors at twenty-four universities took advantage of that. Of those who have made the effort to use the extensive data set, Dan Black and Daniel Nagin have been the only ones to publicly criticize the study.

The response from some academics, particularly those at the Johns Hopkins Center for Gun Policy and Research, was highly unusual in many ways. For instance, who ever heard of academics mounting an attack on a scholarly study by engaging in a systematic letter-writing campaign to local newspapers around the country?[18] One letter from a citizen to the *Springfield (Illinois) State Journal-Register* noted, "Dear Editor: Golly, I'm impressed that the staff at Johns Hopkins University reads our local *State Journal-Register*. I wonder if they subscribe to it."[19]

The rest of this chapter briefly reviews the critiques and then provides my responses to their concerns. I discuss a number of issues below that represent criticisms raised in a variety of published or unpublished research papers as well as in the popular press:

1 *Is the scale of the effect realistic?*

> Large reductions in violence are quite unlikely because they would be out of proportion to the small scale of the change in carrying firearms that the legislation produced. (Franklin Zimring and Gordon Hawkins, "Concealed-Handgun Permits: The Case of the Counterfeit Deterrent," *The Responsive Community* [Spring 1997]: 59, cited hereafter as Zimring and Hawkins, "Counterfeit Deterrent")

In some states, like Pennsylvania in 1996, almost 5 percent of the population has concealed-handgun permits. In others, like Florida, the portion is about 2 percent and growing quickly. The question here is whether these percentages of the population are sufficient to generate 8 percent reductions in murders or 5 percent reductions in rapes. One important point to take into account is that applicants for permits do not constitute a random sample of the population. Applicants are likely to be those most at risk. The relevant comparison is not between the percentage of the population being attacked and the percentage of the entire population holding permits, but between the percentage of the population most vulnerable to attack and the percentage of that population holding permits.

Let us consider some numbers from the sample to see how believable these results are. The yearly murder rate for the average county is 5.65 murders per 100,000 people, that is, .00565 percent of the people in the average county are murdered each year. An 8 percent change in this murder rate amounts to a reduction of 0.0005 percent. Obviously, even if only 2 percent of the population have handgun permits, that 2 percent is a huge number relative to the 0.0005 percent reduction in the murder rate. Even the largest category of violent crimes, aggravated assault, involves 180 cases per 100,000 people in the average county per year (that is, 0.18 percent of the people are victims of this crime in the typical year). A 7 percent change in this number implies that the assault rate declines from 0.18 percent of the population to 0.167 percent of the population. Again, this 0.013 percent change in the assault rate is quite small compared to the observed changes in the number of concealed-handgun permits.

Even if those who carry concealed handguns face exactly the same risk

of being attacked as everyone else, a 2 percent increase in the portion of the population carrying concealed handguns seems comparable to the percentage-point reductions in crime. Bearing in mind that those carrying guns are most likely to be at risk, the drop in crime rates correlated with the presence of these guns even begins to seem relatively small. Assuming that just 2 percent of the population carries concealed handguns, the drop in the murder rate only requires that 0.025 percent of those with concealed-handgun permits successfully ward off a life-threatening attack to achieve the 0.0005 percent reduction in the murder rate. The analogous percentage for aggravated assaults is only 0.65 percent. In other words, if less than seven-tenths of one percent of those with concealed handguns successfully ward off an assault, that would account for the observed drop in the assault rate.

2 *The importance of "crime cycles"*

> Crime rates tend to be cyclical with somewhat predictable declines following several years of increases. . . . Shall-issue laws, as well as a number of other measures intended to reduce crime, tend to be enacted during periods of rising crime. Therefore, the reductions in violent crime . . . attribute[d] to the implementation of shall-issue laws may be due to the variety of other crime-fighting measures, or to a commonly observed downward drift in crime levels towards some long-term average. (Daniel W. Webster, "The Claims That Right-to-Carry Laws Reduce Violent Crime Are Unsubstantiated," The Johns Hopkins Center for Gun Policy and Research, copy obtained March 6, 1997, p. 1; cited hereafter as Webster, "Claims")

Despite claims to the contrary, the regressions do control for national and state crime trends in several different ways. At the national level, I use a separate variable for each year, a technique that allows me to account for the changes in average national crime rates from one year to another. Any national cycles in crime rates should be accounted for by this method. At the state level, some of the estimates use a separate time trend for each state, and the results with this method generally yielded even larger drops in violent-crime rates associated with nondiscretionary (shall-issue) laws.

To illustrate that the results are not merely due to the "normal" ups and downs for crime, we can look again at the diagrams in chapter 4 showing crime patterns before and after the adoption of the nondiscretionary laws. The declines not only begin right when the concealed-handgun

laws pass, but the crime rates end up well below their levels prior to the law. Even if laws to combat crime are passed when crime is rising, why would one believe that they happened to be passed right at the peak of any crime cycle?

As to the concern that other changes in law enforcement may have been occurring at the same time, the estimates account for changes in other gun-control laws and changes in law enforcement as measured by arrest and conviction rates as well as by prison terms. No previous study of crime has attempted to control for as many different factors that might explain changes in the crime rate.

3 *Did I assume that there was an immediate and constant effect from these laws and that the effect should be the same everywhere?*

> The "statistical models assumed: (1) an immediate and constant effect of shall-issue laws, and (2) similar effects across different states and counties." (Webster, "Claims," p. 2; see also Dan Black and Daniel Nagin, "Do 'Right-to-Carry' Laws Deter Violent Crime?" *Journal of Legal Studies* 27 [January 1998], p. 213.)

One of the central arguments both in the original paper and in this book is that the size of the deterrent effect is related to the number of permits issued, and it takes many years before states reach their long-run level of permits. Again, the figures in chapter 4 illustrate this quite clearly.

I did not expect the number of permits to change equally across either counties or states. A major reason for the larger effect on crime in the more urban counties was that in rural areas, permit requests already were being approved; hence it was in urban areas that the number of permitted concealed handguns increased the most.

A week later, in response to a column that I published in the *Omaha World-Herald,*[20] Mr. Webster modified this claim somewhat:

> Lott claims that his analysis did not assume an immediate and constant effect, but that is contrary to his published article, in which the *vast majority* of the statistical models assume such an effect. (Daniel W. Webster, "Concealed-Gun Research Flawed," *Omaha World-Herald,* March 12, 1997; emphasis added.)

When one does research, it is most appropriate to take the simplest specifications first and then gradually make things more complicated. The simplest way of doing this is to examine the mean crime rates before and

after the change in a law. Then one would examine the trends that existed before and after the law. This is the pattern that I followed in my earlier work, and I have followed the same pattern here. The bottom line should be, How did the different ways of examining the data affect the results? What occurs here is that (1) the average crime rate falls after the nondiscretionary concealed-handgun laws are adopted; (2) violent-crime rates were rising until these laws were adopted, and they fell dramatically after that; and (3) the magnitude of the drops, both across counties and states and over time, corresponds to the number of permits issued.

4 *When were these concealed-handgun laws adopted in different states?*

> Lott and Mustard also use incorrect dates of shall-issue law implementation in their analyses. For example, they claim that Virginia adopted its shall-issue law in 1988. . . . Some populous counties in Virginia continued to issue very few permits until 1995 (after the study period), when the state eliminated this discretion. Lott and Mustard identify 1985 as the year in which Maine liberalized its concealed-carry policy. It is unclear why they chose 1985 as the year of policy intervention, because the state changed its concealed-carry law in 1981, 1983, 1985, 1989, and 1991. (Webster, "Claims," p. 3; see also Daniel W. Webster, "Concealed-Gun Research Flawed," *Omaha World-Herald,* March 12, 1997; cited hereafter as Webster, "Flawed.")

I do think that Virginia's 1988 law clearly attempted to take away local discretion in issuing permits, and, indeed, all but three counties clearly complied with the intent of the law. However, to satisfy any skeptics, I examined whether reclassifying Virginia affected the results: it did not. The 1988 law read as follows:

> The court, after consulting the law-enforcement authorities of the county or city and receiving a report from the Central Criminal Records Exchange, *shall issue* such permit if the applicant is of good character, has demonstrated a need to carry such concealed weapon, which need may include but is not limited to lawful defense and security, is physically and mentally competent to carry such weapon, and is not prohibited by law from receiving, possessing, or transporting such weapon [emphasis added].[21]

As with Virginia, I relied on a study by Clayton Cramer and David Kopel to determine when Maine changed its law to a nondiscretionary law. Maine enacted a series of changes in its law in 1981, 1983, 1985, and 1991. The 1985 law did not completely eliminate discretion, but it provided the founda-

tion for what they then considered to be a switch to a de facto shall-issue regime, which was upheld in a number of important state court decisions.[22] The bottom line, however (again, as with Virginia), is that reclassifying Maine (or even eliminating it from the data set) does not change the results much.

5 *Should robbery be the crime most affected by the adoption of the nondiscretionary law?*

> Shall-issue laws were adopted principally to deter predatory street crime, the most common example of which is robbery by a stranger. But [the] results indicate that shall-issue laws had little or no effect on robbery rates. Instead the strongest deterrent effects estimated were for rape, aggravated assault, and murder. (Webster, "Claims," p. 3)

> Is it credible that laws that allow citizens to carry guns in public appear to have almost no effect on robberies, most of which occur in public spaces, yet do reduce the number of rapes, most of which occur outside of public spaces within someone's home. (Jens Ludwig, speaking on *Morning Edition,* National Public Radio, 10:00 A.M. ET December 10, 1996.)

I have two responses. First, as anyone who has carefully read this book will know, it is simply not true that the results show "little or no effect on robbery rates." Whether the effect was greater for robbery or other violent crimes depends on whether one simply compares the mean crime rates before and after the laws (in which case the effect is relatively small for robbery) or compares the slopes before and after the law (in which case the effect for robbery is the largest).

Second, it is not clear that robbery should exhibit the largest impacts, primarily because the term *robbery* encompasses many crimes that are not street robberies. For instance, we do not expect bank or residential robberies to decrease; in fact, they could even rise. Allowing law-abiding citizens to carry concealed handguns makes street robberies more difficult, and thus may make other crimes like residential robbery *relatively* more attractive. Yet not only is it possible that these two different components of robbery could move in opposite directions, but to rank some of these different crimes, one requires information on how sensitive different types of criminals are to the increased threat.

Making claims about what will happen to different types of violent crimes is much more difficult than predicting the relative differences between, say, crimes that involve no contact with victims and crimes that

do. Even here, however, some of these questions cannot be settled *a priori*. For example, when violent crimes decline, more people may feel free to walk around in neighborhoods, which implies that they are more likely to observe the illegal actions of strangers.[23] Criminals who commit violent crimes are also likely to commit some property crimes, and anything that can make an area unattractive to them will reduce both types of crime.

6 *Do concealed-handgun laws cause criminals to substitute property crimes for rape?*

> Lott and Mustard argue that criminals, in response to shall-issue laws, substitute property crimes unlikely to involve contact with victims. But their theory and findings do not comport with any credible criminological theory because theft is the motive for only a small fraction of the violent crimes for which Lott and Mustard find shall-issue effects. It is difficult to rationalize why a criminal would, for example, steal a car because he felt deterred from raping or assaulting someone. (Webster, "Claims," p. 4. See also Jens Ludwig, "Do Permissive Concealed-Carry Laws Reduce Violent Crime?" Georgetown University working paper, October 8, 1996, p. 19, hereafter cited as Ludwig, "Permissive Concealed-Carry Laws.")

No one believes that hard-core rapists who are committing their crimes only for sexual gratification will turn into auto thieves, though some thefts do also involve aggravated assault, rape, or murder.[24] Indeed, 16 percent of murders in Chicago from 1990 to 1995 occurred in the process of a robbery.[25] What is most likely to happen, however, is that robbers will try to obtain money by other means such as auto theft or larceny. Although it is not unusual for rape victims to be robbed, the decline in rape most likely reflects the would-be rapist's fear of being shot.

I am also not completely clear on what Webster means when he says that "theft is the motive for only a small fraction of violent crimes," since robbery accounted for as much as 34 percent of all violent crimes committed during the sample between 1977 and 1992 (and this excludes robberies that were committed when other more serious crimes like murder or rape occurred in connection with the robbery).

7 *Comparing crime rates for two to three years before nondiscretionary laws go into effect with crime rates for two to three years after the passage of such laws*

> If right-to-carry laws have an immediate, substantial impact on the crime rates, the coefficients on the right-to-carry laws immediately after the

> enactment of the law should be substantially different from those immediately preceding the law's enactment. To test formally for the impact of right-to-carry laws, we see if the sum of the coefficients for two to three years prior to adoption is significantly different from the sum for two and three years following adoption. . . . Only in the murder equation do our findings agree with Lott and Mustard. In contrast to Lott and Mustard, we find evidence that robberies and larcenies are reduced when right-to-carry laws are passed and no evidence of an impact on rape and aggravated assaults. (Dan Black and Daniel Nagin, "Do 'Right-to-Carry' Laws Deter Violent Crime?" Carnegie-Mellon University working paper, October 16, 1996, p. 7)

Instead of the approach used earlier in this book (a simple time trend and time trend squared for the number of years before and after the concealed-handgun laws) Black and Nagin used ten different variables to examine these trends. Separate variables were used for the first year after the law, the second year after the law, the third year after the law, the fourth year after the law, and five or more years after the law. Similarly, five different variables were used to measure the effects for the five years leading up to the adoption of the law. They then compared the average coefficient values for the variables measuring the effects two to three years before the law with the average effect for the variables two to three years after the law.

A quick glance at figures 7.1–7.5, which plot their results, explains their findings. Generally, the pattern is very similar to what we reported earlier. In addition, as crime is rising right up until the law is adopted and falling thereafter, it is not surprising that some values when the crime rate is going down are equal to those when it was going up. It is the slopes of the lines and not simply their levels that matter. But more generally, why choose to compare only two to three years before and after to look for changes created by the law. Why not use all the data available?

Examining the entire period before the law versus the entire period after produces the significant results that I reported earlier in the book. Alternatively, one could have chosen to analyze the differences in crime rates between the year before the law went into effect and the year after, but one would hope that if deviations are made from any simple rule, some rationale for doing so would be given.

They claim that their results differ from ours because they find a statistically significant decline. This is puzzling; it is difficult to see why their results would be viewed as inconsistent with my argument. I had indeed also

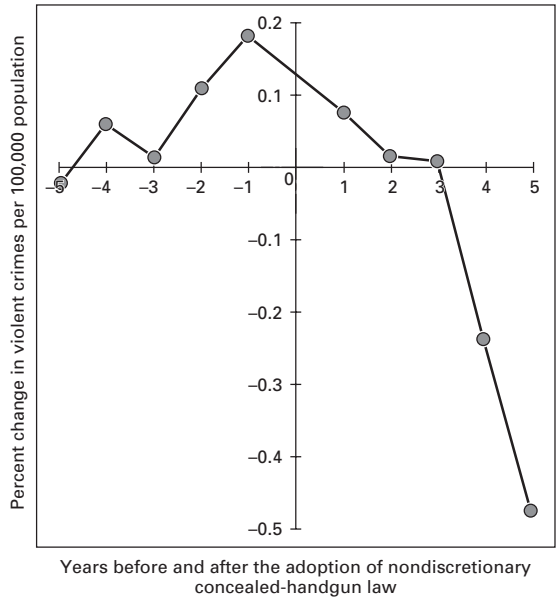


Figure 7.1. Average year-dummy effects for violent crimes, using Black's and Nagin's "full sample"

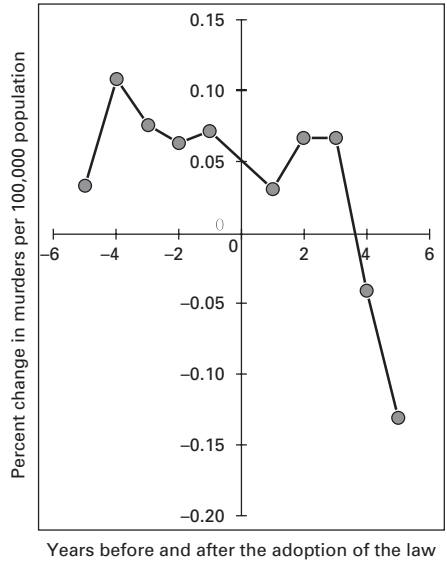


Figure 7.2. The effect of concealed-handgun laws on murder, using Black's and Nagin's "full sample"

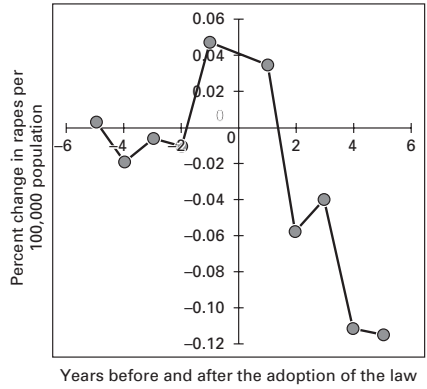


Figure 7.3. The effect of concealed-handgun laws on rapes, using Black's and Nagin's "full sample"

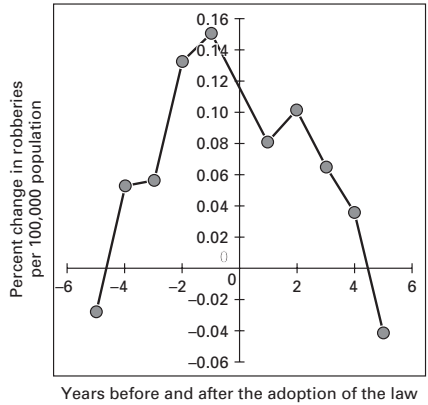


Figure 7.4. The effect of concealed-handgun laws on robbery, using Black's and Nagin's "full sample"

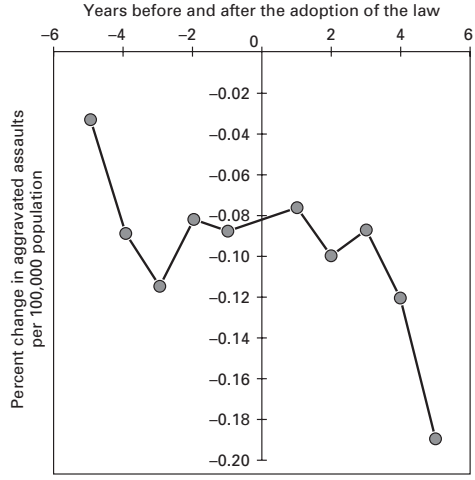


Figure 7.5. The effect of concealed-handgun laws on aggravated assaults, using Black's and Nagin's "full sample"

found some evidence that larcenies were reduced by nondiscretionary laws (for example, see the results using the state-level data or the results using two-stage least squares), but I chose to emphasize those results implying the smallest possible positive benefits from concealed-handgun laws.

The bottom line—even using their choice of the dates that they deem most appropriate—is that murder and robbery rates fall after the passage of the laws and that none of the other violent-crime categories experienced an increase. Looking further at whether violent-crime rates were rising or falling before and after these laws, one finds that violent-crime rates were almost always rising prior to the passage of the law and always falling after it.

8 *The impact of including Florida in the sample*

> Our concern is particularly severe for the state of Florida. With the Mariel boat lift of 1980 and the thriving drug trade, Florida's crime rates are quite volatile. Moreover, four years after the passage of the right-to-carry law in 1987, Florida passed several gun-related measures, including background checks of handgun buyers and a waiting period for handgun purchases. To test the sensitivity of the results to the inclusion of Florida, we reestimated the model . . . without Florida. Only in the robbery equation can we reject the hypothesis that the crime rate two and three years after adoptions is different than the crime rate two and three years prior to adoption. (Dan Black and Daniel Nagin, "Do 'Right-to-Carry' Laws Deter Violent Crime?" Carnegie-Mellon University working paper, October 16, 1996, p. 9)

> In fact, Nagin and Black said they found that virtually all of the claimed benefits of carry laws were attributable to changes in the crime rate in just one state: Florida. (Richard Morin, "Unconventional Wisdom: New facts and Hot Stats from the Social Sciences," *Washington Post,* March 23, 1997, p. C5)

This particular suggestion—that we should throw out the data for Florida because the drop in violent crimes is so large that it affects the results—is very ironic. Well after my work in 1996 got attention, Handgun Control, Inc., and other gun-control groups continued to cite the 1995 University of Maryland study, which claimed that if evidence existed of a detrimental impact of concealed handguns, it was for Florida.[26] If the Maryland study is to be believed, the inclusion of Florida must have biased my results in the opposite direction.[27]

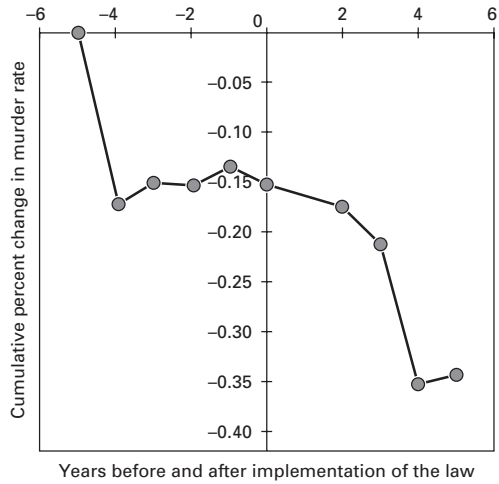


Figure 7.6. Florida's murder rates

More important, as we shall see below, the reasons given by Black and Nagin for dropping Florida from the sample are simply not valid. Furthermore, the impact of excluding Florida is different from what they claim. Figure 7.6 shows the murder rate in Florida from the early 1980s until 1992. The Mariel boat lift did dramatically raise violent-crime rates like murder, but these rates had returned to their pre-Mariel levels by 1982. For murder, the rate was extremely stable until the nondiscretionary concealed-handgun law passed there in 1987, when it began to drop dramatically.

The claim that Florida should be removed from the data because a waiting period and a background check went into effect in 1992 is even weaker. If this were a valid reason for exclusion, why not exclude other states with these laws as well? Why only remove Florida? Seventeen other states had waiting periods in 1992. A more valid response would be to try to account for the impact of these other laws—as I did in chapter 4. Indeed, accounting for these other laws slightly strengthens the evidence that concealed handguns deter crime.

The graph for Florida in figure 7.6 produces other interesting results. The murder rate declined in each consecutive year following the implementation of the concealed-handgun law until 1992, the first year that these other, much-touted, gun-control laws went into effect. I am not claiming that these laws caused murder rates to rise, but this graph surely

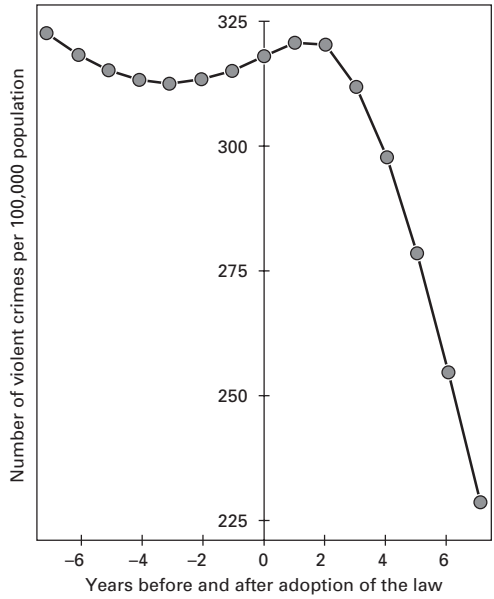


Figure 7.7. The effect of concealed-handgun laws on violent crimes, excluding Florida

makes it more difficult to argue that laws restricting the ability of law-abiding citizens to obtain guns would reduce crime.

While Black's and Nagin's explanations for dropping Florida from the data set are invalid, there is some justification for concern that results are being driven by a few unusual observations. Figure 7.7 shows the relationship between violent-crime rates and concealed-handgun laws when Florida is excluded. A careful comparison of this graph with that of figure 4.5, which includes Florida, reveals only a few very small differences.

As a more systematic response to this concern, I excluded Florida and reestimated all the regressions shown in this book. Indeed, there were eight regressions out of the more than one thousand discussed in which the exclusion of Florida did cause the coefficient for the nondiscretionary variable to lose its statistical significance, although it remained negative. The rest of the regression estimates either remained unchanged or (especially for aggravated assault and robbery) became larger and more statistically significant.

Black and Nagin seem to feel that their role in this debate is to see if they can find some specification using any combination of the data that weakens the results.[28] But traditional statistical tests of significance are based on the assumption that the researcher is not deliberately choosing which re-

sults to present. Even if a result is statistically significant at the 1 percent level, one would expect that one out of every one hundred regressions would not yield a statistically significant result; in other words, out of one thousand regressions, one would expect to find at least ten for which the impact of nondiscretionary concealed-handgun laws was not statistically significant.

> Lott's claims that Florida's concealed-carry law was responsible for lower murder rates in that state is questionable. Florida did not experience reductions in murders and rapes until four or five years after the law was liberalized. Lott attributes this "delayed effect" to the cumulative influence of increases in carrying permits. Other research attributes Florida's declines in murders in the 1990s to laws requiring background checks and waiting periods for handgun purchases that were implemented several years after gun-carrying laws were liberalized. (Webster, "Flawed")

Much of Webster's comment echoes the issues raised previously by Black and Nagin—indeed, I assume that he is referring to their piece when he mentions "other research." However, while I have tested whether other gun-control laws might explain these declines in crime (see table 4.11), Black and Nagin did not do so, but merely appealed to "other research" to support their affirmation. The preceding quotation seems to imply that my argument involved some sort of "tipping" point: as the number of permits rose, the murder rate eventually declined. As figure 7.6 illustrates, however, Florida's decline in murder rates corresponded closely with the rise in concealed-handgun permits: no lag appears in the decline; rather, the decline begins as soon as the law goes into effect.

9 *The impact of including Maine in the sample*

> One should also be wary of the impact that Maine has on Lott's graphs. . . . When Maine was removed from the analyses, the suggested delayed [effects of the law] on robberies and aggravated assaults vanished. (Webster, "Flawed")

This comment is curious not only because Mr. Webster does not cite a study to justify this claim but also because he has never asked for the data to examine these questions himself. Thus it is difficult to know how he arrived at this conclusion. A more direct response, however, is simply to show how the graphs change when Maine is excluded from the sample. As figures 7.8 and 7.9 show, the exclusion of Maine has very little effect.

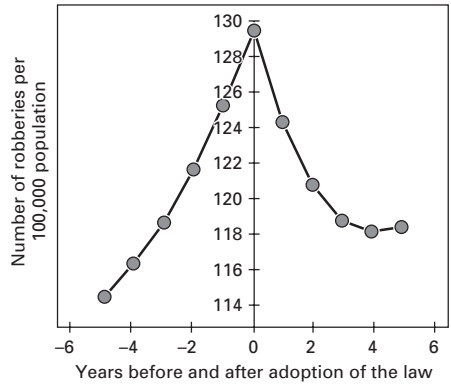


Figure 7.8. The effect of concealed-handgun laws on robberies, excluding Maine

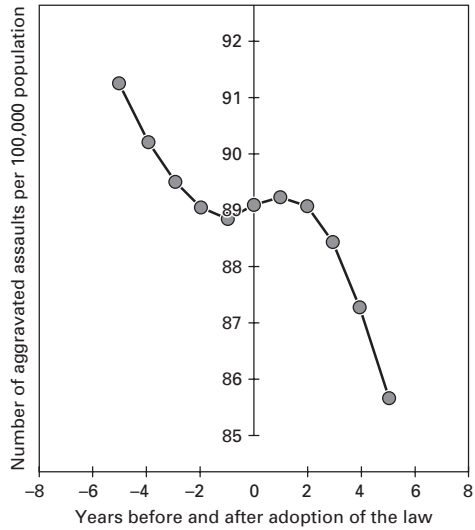


Figure 7.9. The effect of concealed-handgun laws on aggravated assaults, excluding Maine

10 *How much does the impact of these laws vary across states?*

[Dan Black and Dan Nagin] found the annual murder rate did go down in six of the ten states—but it went up in the other four, including a 100 percent increase in West Virginia. Rape dropped in five states—but increased in the other five. And the robbery rate went down in six states—but went up in four. "That's curious," Black said. If concealed weapons laws were really so beneficial, their impact should not be so "wildly" different from

state to state. (Richard Morin, "Unconventional Wisdom: New Facts and Hot Stats from the Social Sciences," *Washington Post,* March 23, 1997, p. C5)

Unfortunately, Black's and Nagin's evidence was not based on statewide crime rates but on the crime rates for counties with over 100,000 people. This fact is important, for instance, in West Virginia, where it means that *only one single county*—Kanawha—was examined. The other fifty-four counties in West Virginia, which include 89 percent of the state's population, were excluded from their estimates. They used only one county for three of the ten states, and only three counties for another state. In fact, Black and Nagin managed to eliminate 85 percent of all counties in the nation in their analysis.

As shown in table 4.9 (see chapter 4), my estimates using all the counties certainly did not yield "wildly" different estimates across states. Violent-crime rates fell in nine of the ten states enacting new nondiscretionary concealed-handgun laws between 1977 and 1992. The differences that did exist across states can be explained by differences in the rates at which concealed-handgun permits were issued. Table 4.10 also provides evidence that the states that issued more permits experienced greater reductions in crime.

11 *Do the coefficient estimates for the demographic variables make sense?*

Perhaps even more surprising are the coefficient estimates for measures of a county's population that is black, female, and between the ages of 40 and 49 or over the age of 65. [Lott and Mustard find] evidence to suggest that these variables have a statistically significant, positive correlation with murder rates . . . and that black females ages 40 to 49 have a statistically significant positive correlation with the aggravated assault rate. . . . There remain two competing explanations for [these] findings. First, middle-aged and elderly African-American women could be actively [engaged] in the commission of car thefts, assaults, and murders across the United States. The more likely explanation is that [their results] are misspecified and, as a result, their coefficient estimates are biased. (Ludwig, "Permissive Concealed-Carry Laws," pp. 20–21. See also Albert W. Alschuler, "Two Guns, Four Guns, Six Guns, More: Does Arming the Pubic Reduce Crime?" *Valparaiso University Law Review* 31 (Spring 1997): 367.)

No, black females ages 40 to 49 are not responsible for a crime wave. Other results in the regressions that were not mentioned in this quotation indicate that the greater the percentage of women between the ages of 10 and 29, the greater the rape rate—but these estimates do not imply that

young women are going out and committing rapes. To show that crime rates are higher where greater percentages of the population are of a certain demographic age group does not imply that the people in that group are committing the crimes. The positive relationship may exist because these people are relatively easy or attractive victims.

If such an objection were valid, it should also apply to my finding that in areas where personal incomes are high, auto-theft rates are also high. Should we infer from this that high-income individuals are more likely to steal cars? Presumably not. What is most likely is that wealthy individuals own cars that are attractive targets for auto thieves.

It is also important to note that the different demographic variables are very highly correlated with each other. The percentage of the population that is male and within a particular race and age grouping is very similar to the percentage that is female within that race and age group. Similar high correlations exist within racial groups across age groups. With thirty-six different demographic categories, determining whether an effect is specifically related to an individual category or simply arises because that category is correlated (whether negatively or positively) with another demographic group is difficult and not the object of this book. What I have tried to do is "overcontrol" for all possible demographic factors to make sure that any effects attributed to the right-to-carry law are not arising because I have accidentally left out some other factor.

12 *Can we compare counties with discretionary and nondiscretionary concealed-handgun laws?*

> Many counties with very permissive permit systems can be found in states with no shall-issue laws, such as Louisiana and California. For example, in El Dorado county in California, 1,289 concealed-carry permits were issued in 1995. With a population of 148,600, this implies that 0.87 percent of this county's population received concealed-carry permits in one year alone. In contrast, a total of 186,000 people in Florida had concealed-carry permits in 1996 out of a total state population of 13,958,000; that is, 1.33 percent of the population was licensed to carry concealed [guns]. Yet under [the] classification scheme used in most of their results, El Dorado county would not be classified as shall-issue, while every county in Florida would be so classified. (Jens Ludwig, "Permissive Concealed-Carry Laws," pp. 20–21.)

The simplest question that we are asking is, What happens to the crime rate when nondiscretionary laws are passed allowing law-abiding citizens to carry concealed handguns? The key here is the *change* in the leniency of

the laws. The regressions have individual variables for each county that allow us to account for differences in the mean crime rate. The purpose of all the other variables is to explain why crime rates differ from this average. Under discretionary laws some counties are extremely liberal in granting permits—essentially behaving as if they had nondiscretionary laws. In the regressions, differences between counties with discretionary laws (including differences in how liberally they issued concealed-handgun permits) are already being partly "picked up" by these individual county variables. For my test to work, it is only necessary for nondiscretionary laws *on average* to increase the number of concealed-handgun permits.

True, the amount of change in the number of permits does vary across counties. As this book has documented, law officials in discretionary states across the country have said that the more rural counties with relatively low populations were much more liberal in granting permits under discretionary laws. Since no usable statistics are available regarding how easily permits are granted, I tested whether nondiscretionary laws changed the crime rates the most in counties with the largest or densest populations. The results confirmed that this was the case (see figure 4.1).

We also tried another approach to deal with this question. A few states did keep good records on the number of concealed-handgun permits issued at either the county or the state level. We reported earlier the results for Pennsylvania and Oregon (see tables 5.4 and 5.5 in chapter 5). Despite the small samples, we accounted for all the variables controlled for in the larger regressions, and the results confirmed that murder rates decline as the number of a permits issued in a county rises.

13 *Should changes in the arrest rate be accounted for when explaining changes in the crime rate?*

> The use of arrest rates as an explanatory variable is itself quite problematic. . . . Since the arrest rate is calculated as the number of arrests for a particular crime divided by the number of crimes committed, unobserved determinants of the crime rate will by construction also influence the arrest rate. When the arrest rate is included as an explanatory variable in a regression equation, this leads to the statistical problem known as "endogeneity," or "simultaneity bias." (Jens Ludwig, "Permissive Concealed-Carry Laws," pp. 7–8)

True, there is an endogeneity "problem." However, on theoretical grounds, the inclusion of the arrest rate is highly desirable. There is strong reason to

believe that crime rates depend on the probability of punishment. In addition, to exclude variables that obviously should be included in the analysis would create even more important potential bias problems. Furthermore, the endogeneity problem was dealt with in the original paper: it was precisely our awareness of that problem that led us to use two-stage least squares to estimate the set of regressions, which is the recognized method of dealing with such a problem. As reported in chapter 6, the two-stage least-squares estimate provided even stronger evidence that concealed handguns deter crime.

The simplest point to make, however, is that excluding the arrest rate does not alter the findings regarding concealed handguns. Reestimating the regressions in tables 4.1 and 4.3 for the same samples and control variables produces virtually identical results. Ironically, two of my strongest critics, Dan Black and Dan Nagin, also tried excluding the arrest rates, and they admitted in early drafts of their paper that their results agreed with ours: "The inclusion of the arrest-rate variable has very little impact on the coefficient estimates of the right-to-carry laws."[29]

14 *Are the graphs in this book misleading?*

> Lott rebuts many of the criticisms of his study by pointing to his simple but misleading graphs. The graphs are visually compelling yet very deceptive. What is not obvious to the casual observer of the graphs is that each data point represents an aggregate average for states that liberalized their gun-carrying laws, but the states that make up the average are not the same each year. Lott examined 10 states he claims adopted "shall-issue" concealed-gun-carrying laws during his sample period. For many of the states studied, data were available for only one to three years after the laws were implemented. (Webster, "Flawed")

The graphs presented in the paper do indeed represent the average changes in crime rates before and after the implementation of these laws. The graphs consistently show that violent-crime rates are rising before these laws go into effect and falling afterward. Since some states only adopted nondiscretionary, "shall-issue" laws toward the end of the sample period, it was not possible to examine all the states for the same number of years after the laws were implemented. I disagree that this is "misleading" or "deceptive." The results were by no means generated by the aggregation itself, and anybody doubting the meaning of the graph can examine the regression results. Since the regressions already control for each county's

average crime rate, any changes refer to deviations from that county's average crime rate.[30]

Ian Ayres and Steven Levitt use similar graphs and find similar results when they look at the deterrent effect of Lojack antitheft devices on cars (these are radio tracking devices that can be activated by police when a car is stolen).[31] In many ways, the theoretical deterrent effect of these devices is the same as that of concealed handguns: because the device is small and easy to hide, a criminal cannot easily know whether a car has the tracking device until the police arrive.

Future studies will be able to track these changes in crime over longer periods of time because more states will have had right-to-carry laws for longer periods of time. Such studies will ultimately help to test my findings. I have used all the data that was available at the time that David Mustard and I put this data set together. With 54,000 observations and hundreds of variables available over the 1977 to 1994 period, it is also by far the largest data set that has ever been put together for any study of crime, let alone for the study of gun control.[32] I find it ironic that my study is attacked for not having enough data when these same researchers have praised previous studies that relied on much shorter time periods for a single state or a few counties. For example, Mr. Webster expresses no such criticism when referring to a study conducted by the University of Maryland. Yet that study analyzed merely five counties and covered a shorter period of time after the law was enacted.[33]

15 *Should concealed-handgun laws have differential effects on the murder rates of youths and adults?*

> Ludwig points out that in many states only adults may carry concealed weapons. So, according to Lott's deterrence theory, adults should be safer than young people. But this hasn't happened, Ludwig says. (Kathleen Schalch describing Jens Ludwig's arguments on *Morning Edition,* National Public Radio, 10:00 A.M. ET Tuesday, December 10, 1996.)

As noted in chapter 4, I tested the hypothesis that murder rates would be lower for adults than for adolescents under nondiscretionary concealed-handgun laws, and reported the results in the original paper. However, the results did not bear out this possibility. Concealed-handgun laws reduce murder rates for both adults and for adolescents. One explanation may simply be that young people also benefited from the carrying of concealed handguns by adults. Several plausible scenarios may explain this.

First, criminals may well tend to leave an area where law-abiding adults carry concealed handguns, and since all age groups live in the same neighborhood, this lowers crime rates for all population groups. Second, when gun-carrying adults are physically present, they may able to protect some youngsters in threatening situations.

Could some other factor be lowering the juvenile murder rate—something that is unrelated to concealed handguns? Perhaps, despite all the factors accounted for, the results of any research may be affected by unknown factors. But it is wrong to conclude, as Ludwig does, that "these findings are not consistent with the hypothesis that shall-issue laws decrease crime through a deterrence effect."[34]

16 *Are changes in the characteristics of victims consistent with the theory?*

> Lott and Mustard offer data on the character of victims in homicide cases. They report (astonishingly) that the proportion of stranger killings increases following the enactment of right-to-carry laws, while the proportion of intrafamily killings declines. That right-to-carry laws deter intrafamily homicides more than they deter stranger homicides is inconceivable. (Albert W. Alschuler, "Two Guns, Four Guns, Six Guns, More: Does Arming the Public Reduce Crime?" *Valparaiso University Law Review* 31 (1997): 369)

> Josh Sugarmann of the Violence Policy Center noted that most murders are committed by people who know each other. "Concealed-weapons laws are not passed to protect people from people they know," Sugarmann said. (Doug Finke, "Sides Stick to Their Guns, Concealed-Carry Bill Set for Showdown in General Assembly," *Springfield State Journal-Register,* March 31, 1997, p. 1)

As noted in the first chapter, the category of acquaintance murder is extremely broad (encompassing shootings of cab drivers, gang members, drug dealers or buyers, and prostitutes or their clients). For the Chicago data that we discussed, the number of acquaintance murders involving friends was actually only a small percentage of the total number of acquaintance murders. If the breakdown found for Chicago provides even the remotest proxy for the national data, it is not particularly surprising that the relative share of acquaintance murders involving friends should rise, because we expect that many of the murders in this category are unlikely to be affected by law-abiding citizens carrying concealed handguns. Fam-

ily members may also find that concealed handguns protect them from other estranged family members. A wife seeking a divorce may find that a concealed handgun provides her protection against a husband who is unwilling to let go of the relationship, and attacks by such people do not always take place in a home. Surely there are many cases of spousal abuse where women fear for their lives and find that a handgun provides them with a significant degree of protection.

A recent case involving a woman who used a handgun to protect herself from an abusive husband created an important new legal precedent in California: for the first time, women are now allowed to use self-defense before they suffer serious blows. The *San Francisco Examiner* reported as follows:

> [Fay] Johnson, a 47-year-old mother of four, said that on July 2, 1995, she feared her 62-year-old husband, Clarence, would beat her as he always did after a weekend of drinking and hanging out with his motorcycle buddies.
>
> She had overspent her budget on supplies for a Fourth of July barbecue and didn't have dinner ready, and the house was not clean—so when she heard her husband's motorcycle pull into the driveway, she decided to take matters into her own hands.
>
> Johnson said she grabbed a loaded gun . . . [and fired,] hitting her husband five times. He survived and testified against her. She was arrested and spent 21 months in prison until her acquittal.
>
> "I regret being in jail, but I just wouldn't tolerate it anymore," said Johnson, a friendly, articulate woman who is celebrating her freedom with her children and six grandchildren. "It would have been suicide."
>
> Johnson said she had endured nearly 25 years of mental and physical abuse at the hands of her husband, whose usual form of punishment was slamming her head into a wall. The beatings got so bad, she said, that she had to be hospitalized twice and tried getting counseling until he found out and forced her to stop. She said the pressure of the abuse had culminated that fateful day.[35]

Pointing to women who use handguns to protect themselves from abusive husbands or boyfriends in no way proves that the primary effects of concealed-handgun laws will involve such uses of guns, but these cases should keep us from concluding that significant benefits for these women are "inconceivable."

With reference to Alschuler's discussion, however, two points must be made clear. First, the diverse breakdown of these groupings makes it diffi-

cult to predict on theoretical grounds how the number of murders among family members, acquaintances, strangers, or unknown cases should necessarily change relative to each other. Second, as Alschuler himself has noted, these estimates are suggestive; they are not statistically significant, in that we cannot say with much certainty how concealed-handgun laws have affected the proportions of victims across the categories mentioned above.

An additional response should be made to Sugarmann's claims. Even if one accepts the claim that nondiscretionary concealed-handgun laws do not reduce the number of murders against people who know each other (and I do not concede this), what about other types of murders, such as those arising from street robbery? For Chicago during the period from 1990 to 1995, 16 percent of all murders involved nonacquaintance robbery. Moreover, one must ask about nonfriend acquaintance murders (excluding prostitution, gang, and drug cases), murders by complete strangers, and at least some of those murders still classified as mysteries (an additional 22 to 46 percent of all murders). Since permitted handguns are virtually never used in crimes against others and they do not produce accidental deaths, should not the reduction of these other types of murders still be deemed important?[36]

17 *Do nondiscretionary concealed-handgun laws only affect crimes that occur in public places?*

> Handguns were freely available for home and business use in all the "shall-issue" jurisdictions prior to the new laws. The new carrying privilege would thus not affect home or business self-defense but should have most of its preventive impact on street crime and offenses occurring in other public places. But the study contains no qualitative analysis of different patterns within crime categories to corroborate the right-to-carry prevention hypothesis. (Zimring and Hawkins, "Counterfeit Deterrent," p. 54)

Contrary to the claim of Zimring and Hawkins, concealed handguns may very well affect crime in homes and businesses in several ways. First, being allowed to carry a concealed gun outside is likely to increase the number of guns owned by law-abiding citizens. Since these guns will be kept at least part of the time in the home, this should have a deterrent effect on crimes committed at home and also at one's business. Second, as some of the evidence suggests, nondiscretionary laws could even increase the number of crimes that occur in the home as criminals turn away from other crimes, like street robbery, for which the risks that criminals face have gone up.

These two effects would thus work in opposite directions. Finally, to the extent that nondiscretionary handgun laws drive criminals out of a certain geographical area, rates for all types of crimes could fall.

Aggregation of the crime categories makes it difficult to separate all the different substitution effects. Still, the results presented here are very consistent with the two primary dimensions that we focused on: whether there is contact between the criminal and the victim, and whether the crime occurs where law-abiding citizens could already legally carry a gun.

18 *Is it reasonable to make comparisons across states?*

> The sort of state that passes a "shall-issue" law in the 1980s is apt to be the same kind of place where ordinary citizens carrying concealed firearms might not be regarded as a major problem even before the law changed. . . . Idaho is not the same sort of place that New York is, and there seem to be systematic differences between states that change standards for concealed weapons and those that do not. (Zimring and Hawkins, "Counterfeit Deterrent," pp. 50–51)

The observed drop in crime rates in states that have enacted nondiscretionary concealed-handgun laws does not by itself imply that we will observe the same effect in other states that adopt such laws later. Several different issues arise here. First, the regressions used in this book have attempted to control for many differences that can explain the level of crime (for example, income, poverty, unemployment, population and population density, demographic characteristics, law enforcement, other gun laws). Admittedly, even my long list of variables does not pick up all the differences between states, which is the reason that a variable is added for each county or state to pick up the average differences in crime rates across places. Individual time trends are also allowed for each state.

Yet despite all these attempts to control for variables, some caution is still in order—especially when dealing with areas that are particularly extreme along dimensions that do not have obvious counterparts in areas with nondiscretionary laws. One obvious example would be New York City. While the regression results show that areas with the largest and most dense populations gain the most from nondiscretionary laws, there is always the possibility that the relationship changes for values of population and density that are different from those in places where we have been able to study the effects of these laws. To date, the fourth and fifth largest cities in the country have passed nondiscretionary laws (Houston and

Philadelphia), and additional experience with large cities may help determine whether these laws would be equally useful in a city like New York. If one were skeptical about the effects in large cities, the laws should first be changed in Los Angeles and Chicago.

A second issue is whether there is something unique about states that have adopted nondiscretionary laws, and whether that characteristic caused them not only to adopt the laws but also reduced the potential problems resulting from adoption. For example, if local legislators in a few states had special information confirming that the citizens in their state were uniquely trustworthy with regard to concealed handguns, that might have led these few states to pass the laws and have little difficulty with them. It could then "falsely" appear that nondiscretionary laws are generally successful. Such an argument may have been plausible at one time, but its force has declined as larger and more varied areas have been covered by these laws. Equally important is the fact that not all jurisdictions have willingly adopted these laws. Many urban areas, such as Atlanta and Philadelphia, fought strongly against them, but lost out to coalitions of rural and suburban representatives. Philadelphia's opposition was so strong that when Pennsylvania's nondiscretionary law was first passed, Philadelphia was partially exempted.

19 *Does my discussion provide a "theory" linking concealed-handgun ownership to reductions in crime? Do the data allow me to link the passage of these laws with the reduction in crime?*

> Two idiosyncratic aspects of the Lott and Mustard analysis deserve special mention. . . . In the first place, there is very little in the way of explicit theory advanced to explain where and when right-to-carry laws should operate as deterrents to the types of crime that can be frustrated by citizens carrying concealed handguns. . . . They have no data to measure the critical intermediate steps between passing the legislation and reductions in crime rates. This is the second important failing . . . that is not a recurrent feature in econometric studies. (Zimring and Hawkins, "Counterfeit Deterrent," pp. 52, 54)

This set of complaints is difficult to understand. The theory is obvious: A would-be criminal act is deterred by the risk of being shot. Many different tests described in this book support this theory. Not only does the drop in crime begin when nondiscretionary laws are adopted, but the extent of the decline is related to the number of permits issued in a state. Nondiscretionary laws reduce crime the most in areas with the greatest increases

in the number of permits. As expected, crimes that involve criminals and victims in direct contact and crimes occurring in places where the victim was previously unable to carry a gun are the ones that consistently decrease the most.

20 *What can we infer about causality?*

> Anyone who has taken a course in logical thinking has been exposed to the fallacy of arguing that because A happened (in this case, passage of a concealed-weapon law) and then B happened (the slowing of the rate of violent crime), A must surely have caused B. You can speculate that the passage of concealed-gun legislation caused a subsequent slowing of the rate of violent crime in various states, but you certainly can't prove it, despite the repeated claims that a University of Chicago law professor's "study" has offered "definitive scholarly proof." (Harold W. Andersen, "Gun Study Akin to Numbers Game," *Omaha World Herald,* April 3, 1997, p. 15)

An obvious danger arises in inferring causality because two events may coincide in time simply by chance, or some unknown factor may be the cause of both events. Random chance is a frequent concern with pure time-series data when there is just one change in a law. It is not hard to believe that when one is examining a single state, unrelated events A and B just happened to occur at the same time. Yet the data examined here involve many different states that changed their laws in many different years. The odds that one might falsely attribute the changes in the crime rate to changes in the concealed-handgun laws decline as one examines more experiences. The measures of statistical significance are in fact designed to tell us the likelihood that two events may have occurred randomly together.

The more serious possibility is that some other factor may have caused both the reduction in crime rates and the passage of the law to occur at the same time. For example, concern over crime might result in the passage of both concealed-handgun laws and tougher law-enforcement measures. Thus, if the arrest rate rose at the same time that the concealed-handgun law passed, not accounting for changes in the arrest rate might result in falsely attributing some of the reduction in crime rates to the concealed-handgun law. For a critic to attack the paper, the correct approach would have been to state what variables were not included in the analysis. Indeed, it is possible that the regressions do not control for some important factor. However, this study uses the most comprehensive set of control variables

yet used in a study of crime, let alone any previous study on gun control. As noted in the introduction, the vast majority of gun-control studies do not take any other factors that may influence crime into account, and no previous study has included such variables as the arrest or conviction rate or sentence length.

Other pieces of evidence also help to tie together cause and effect. For example, the adoption of nondiscretionary concealed-handgun laws has not produced equal effects in all counties in a state. Since counties with easily identifiable characteristics (such as rural location and small population) tended to be much more liberal in granting permits prior to the change in the law, we would expect them to experience the smallest changes in crime rates, and this is in fact what we observe. States that were expected to issue the greatest number of new permits and did so after passing nondiscretionary laws observed the largest declines in crime. We know that the number of concealed-handgun permits in a state rises over time, so we expect to see a greater reduction in crime after a nondiscretionary law has been in effect for several years than right after it has passed. Again, this is what we observe. Finally, where data on the actual number of permits at the county level are available, we find that the number of murders declines as the number of permits increases.

The notion of statistical significance and the number of different specifications examined in this book are also important. Even if a relationship is false, it might be possible to find a few specifications out of a hundred that show a statistically significant relationship. Here we have presented over a thousand specifications that together provide an extremely consistent and statistically significant pattern about the relationship between nondiscretionary concealed-handgun laws and crime.

21 *Concerns about the arrest rates due to missing observations*

> To control for variation in the probability of apprehension, the [Lott and Mustard] model specification includes the arrest ratio, which is the number of arrests per reported crime. Our replication analysis shows that the inclusion of this variable materially affects the size and composition of the estimation data set. Specifically, division by zero forces all counties with no reported crimes of a particular type in a given year to be dropped from the sample for that year. [Lott's and Mustard's] sample contains all counties, regardless of size, and this problem of dropping counties with no reported crimes is particularly severe in small counties with few crimes.

> The frequencies of missing data are 46.6% for homicide, 30.5% for rape, 12.2% for aggravated assault, and 29.5% for robbery. Thus, the [Lott and Mustard] model excludes observations based on the realization of the dependent variable, potentially creating a substantial selection bias. Our strategy for finessing the missing data problem is to analyze only counties maintaining populations of at least 100,000 during the period 1977 to 1992. . . . Compared to the sample [comprising] all counties, the missing data rate in the large-county sample is low: 3.82% for homicide, 1.08% for rape, 1.18% for assault, and 1.09% for robberies. (Dan Black and Daniel Nagin, "Do 'Right-to-Carry' Laws Deter Violent Crime?" *Journal of Legal Studies* 27 [January 1998], forthcoming)

The arguments made by Black and Nagin have changed over time, and some of their statements are not consistent.[37] In part because of the public nature of their attacks, I have tried to deal with all of the different attacks, so that those who have heard them may hear my responses. The problem described immediately above by Black and Nagin is indeed something one should be concerned about, but I had already dealt with the problem of missing observations in the original paper, and I discuss it again here at the end of chapter 6. My original paper and chapter 4 also reported the results when the arrest rate was removed entirely from the regressions. The discussion by Black and Nagin exaggerates the extent of the problem and, depending on the crime category being examined, quite amazingly proposes to solve the missing data problem by throwing out data for between 77 and 87 percent of the counties.

Black and Nagin present a very misleading picture of the trade-offs involved with the solution that examined the more populous counties.[38] The relevant comparison is between weighted numbers of missing observations, not the total number of missing observations, since the regressions are weighted by county population and the missing observations tend to be from relatively small counties, which are given a smaller weight.[39] When this is done, the benefits obtained by excluding all counties with fewer than 100,000 people become much more questionable. The most extreme case is for aggravated assault, where Black and Nagin eliminate 86 percent of the sample (a 29 percent drop in the weighted frequency) in order to reduce weighted missing values from 2.8 to 1.5 percent. Even for murder, 77 percent of the sample is dropped, so that the weighted missing data declines from 11.7 to 1.9 percent. The rape and robbery categories lie between these two cases, both in terms of the number of counties with fewer

than 100,000 people and in terms of the change in the amount of weighted missing data.[40]

Why they choose to emphasize the cut-off that they did is neither explained nor obvious. The current cost-benefit ratio is rather lopsided. For example, eliminating counties with fewer than 20,000 people would have removed 70 percent of the missing arrest ratios for murder and lost only 20 percent of the observations (the weighted frequencies are 23 and 6 percent respectively). There is nothing wrong with seeing whether the estimates provide the same results over counties of various sizes, but if that is their true motivation for excluding portions of the data, it should be clearly stated.

Despite ignoring all these observations, it is only when they *also* remove the data for Florida that they weaken my results for murder and rape (though the results for aggravated assault and robbery are even larger and more statistically significant). Only eighty-six counties with more than 100,000 people adopted nondiscretionary concealed-handgun laws between 1977 and 1992, and twenty of these counties are in Florida. Yet after all this exclusion of data, Black and Nagin still find no evidence that allowing law-abiding citizens to carry concealed handguns increases crime, and two violent-crime categories show a statistically significant drop in crime. The difference between their approach and mine is rather stark: I did not select which observations to include; I used all the data for all the counties over the entire period for which observations were available. When updated data have been available, they have all been used.

22 *What can we learn about the deterrent effect of concealed handguns from this study?*

> The regression study [that Lott and Mustard] report is an all-or-nothing proposition as far as knowledge of legal impact is concerned. If the model is wrong, if their bottom-line estimates of impact cannot withstand scrutiny, there is no intermediate knowledge of the law's effects on behavior that can help us sort out the manifold effects of such legislation. As soon as we find flaws in the major conclusions, the regression analyses tell us nothing. What we know from this study about the effects of "shall-carry" laws is, therefore, nothing at all. (Zimring and Hawkins, "Counterfeit Deterrent," p. 59)

Academics can reasonably differ about what factors account for changes in crime. Sociologists and criminologists, for example, have examined gun control without trying to control for changes in arrest or conviction

rates. Others might be particularly concerned about the impact of drugs on crime. Economists such as myself try to include measures of deterrence, though I am also sympathetic to other concerns. In this book and my other research, my approach has not been to say that only one set of variables or even one specification can explain the crime rate. My attitude has been that if someone believes that a variable is important and has any plausible reason for including it, I have made an effort to include it. This book reports many different approaches and specifications—all of which support the conclusion that allowing law-abiding citizens to carry concealed handguns reduces crime. With each update of this research, I believe that no other study on crime has used as extensive a data set as used here.

23 *Summarizing the concerns about the evidence that concealed-handgun laws deter crime*

> The gun lobby claims to have a new weapon in its arsenal this year—a study by economist John Lott. But the Lott study shoots blanks. In reviewing Lott's research and methodology, Carnegie-Mellon University Profs. Daniel Nagin and Dan Black, and Georgetown University's Prof. Jens Ludwig corrected for the many fatal flaws in Lott's original analysis and found no evidence of his claim that easing restrictions on carrying concealed handguns leads to a decrease in violent crime. Nagin, Black, and Ludwig recently concluded in a televised debate with Lott that "there is absolutely no credible evidence to support the idea that permissive concealed-carry laws reduce violent crime," and that "it would be a mistake to formulate policy based on the findings from Dr. Lott's study." (James Brady, "Concealed Handguns; Putting More Guns on Streets Won't Make America Safer," *Minneapolis Star Tribune,* March 21, 1997, p. 21A)

Unlike the authors of past papers on gun control such as Arthur Kellermann and the authors of the 1995 University of Maryland study, I immediately made my data available to all academics who requested it.[41] To date, my data have been supplied to academics at twenty-four universities, including Harvard, Stanford, the University of Pennsylvania, Emory, Vanderbilt, Louisiana State, Michigan State, Florida State, the University of Texas, the University of Houston, the University of Maryland, Georgetown, and the College of William and Mary.

James Brady's op-ed piece ignores the fact that some of these academics from Vanderbilt, Emory, and Texas paid their own way to attend the December 9, 1996, debate sponsored by his organization—Handgun Control. While Handgun Control insisted on rules that did not allow these academ-

ics to participate, I am sure that they would have spoken out to support the integrity of my original study.

Those who have attempted to replicate the findings in the original *Journal of Legal Studies* paper have been able to do so, and many have gone beyond this to provide additional support for the basic findings. For example, economists at Vanderbilt University have estimated over 10,000 regressions attempting to see whether the deterrent effects of nondiscretionary laws are at all sensitive to all possible combinations of the various data sets on demographics, income, population, arrest rates, and so on. Their results are quite consistent with those reported in this book.[42]

I have tried in this chapter to examine the critiques leveled against my work. In many cases, the concerns they describe were addressed in the original paper. In others, I believe that relatively simple responses exist to the complaints. However, even taking these critics at their worst, I still believe that a comment that I made at the December 9 discussion sponsored by Handgun Control still holds:

> Six months ago, who would have thought that Handgun Control would be rushing out studies to argue that allowing law-abiding citizens to carry concealed handguns would have no effect, or might have a delayed impact, in terms of dropping crimes? (*Morning Edition,* National Public Radio, 10:00 A.M. ET, December 10, 1996.

8 | Some Final Thoughts (1998)

As more than 30 diners sat in Sam's St. John's Seafood [in Jacksonville, Florida] about 7:20 P.M., a masked man entered the eatery and ordered everyone to the floor, said co-owner Sam Bajalia. The man grabbed waitress Amy Norton from where she and another waitress were huddled on the floor and tried to get her to open the cash register.

At that point, [Oscar] Moore stood up and shot him. Another diner . . . pulled out a .22-caliber derringer and fired at the man as he ran out of the restaurant. At least one shot hit the fleeing robber.

[The robber was later arrested when he sought medical care for his wound.] . . .

"I'm glad they were here because if that girl couldn't open the register, and he didn't get [any] money, he might have started shooting," Bajalia said.[1]

[It was] 1:30 A.M. when Angelic Nichole Hite, 26, the night manager, and Victoria Elizabeth Shaver, 20, the assistant manager at the Pizza Hut at 4450 Creedmoor Road, were leaving the restaurant with Marty Lee Hite, 39, the manager's husband. He had come to pick her up after work.

They saw a man wearing a ski mask, dark clothes, gloves, and holding a pistol walking toward them, and the Hites ran back inside the restaurant. Shaver apparently had reached her car already. . . . The couple couldn't close the door behind them because the robber ran up and wedged the barrel of his handgun in the opening. As they struggled to get the door closed, . . . the masked man twice said he would kill them if they didn't open it.

Marty Hite, who carried a .38-caliber handgun, pulled

out his weapon and fired three times through the opening, striking the robber in the abdomen and upper chest. The would-be bandit staggered away, and the Hites locked the door and called police.

The Wake County district attorney will review the shooting, but Raleigh police did not file charges against the manager's husband. Police said it appeared the couple retreated as far as they could and feared for their lives, which would make it a justified shooting.[2]

Many factors influence crime, with arrest and conviction rates being the most important. However, nondiscretionary concealed-handgun laws are also important, and they are the most cost-effective means of reducing crime. The cost of hiring more police in order to change arrest and conviction rates is much higher, and the net benefits per dollar spent are only at most a quarter as large as the benefits from concealed-handgun laws.[3] Even private, medium-security prisons cost state governments about $34 a day per prisoner ($12,267 per year).[4] For concealed handguns, the permit fees are usually the largest costs borne by private citizens. The durability of guns allows owners to recoup their investments over many years. Using my yearly cost estimate of $43 per concealed handgun for Pennsylvanians, concealed handguns pay for themselves if they have only 1/285 of the deterrent impact of an additional year in prison. This calculation even ignores the other costs of the legal system, such as prosecution and defense costs—criminals will expend greater effort to fight longer prison sentences in court. No other government policy appears to have anywhere near the same cost-benefit ratio as concealed-handgun laws.

Allowing citizens without criminal records or histories of significant mental illness to carry concealed handguns deters violent crimes and appears to produce an extremely small and statistically insignificant change in accidental deaths. If the rest of the country had adopted right-to-carry concealed-handgun provisions in 1992, about 1,500 murders and 4,000 rapes would have been avoided. On the other hand, consistent with the notion that criminals respond to incentives, county-level data provide some evidence that concealed-handgun laws are associated with increases in property crimes involving stealth and in crimes that involve minimal probability of contact between the criminal and the victim. Even though both the state-level data and the estimates that attempt to explain why the law and the arrest rates change indicate that crime in all the categories declines, the deterrent effect of nondiscretionary handgun laws is largest for violent crimes. Counties with the largest populations, where the deterrence

of violent crimes is the greatest, are also the counties where the substitution of property crimes for violent crimes by criminals is the highest. The estimated annual gain in 1992 from allowing concealed handguns was over $5.74 billion.

Many commonly accepted notions are challenged by these findings. Urban areas tend to have the most restrictive gun-control rules and have fought the hardest against nondiscretionary concealed-handgun laws, yet they are the very places that benefit the most from nondiscretionary concealed-handgun laws. Not only do urban areas tend to gain in their fight against crime, but reductions in crime rates are greatest precisely in those urban areas that have the highest crime rates, largest and most dense populations, and greatest concentrations of minorities. To some this might not be too surprising. After all, law-abiding citizens in these areas must depend on themselves to a great extent for protection. Even if self-protection were accepted, concerns would still arise over whether these law-abiding citizens would use guns properly. This study provides a very strong answer: a few people do and will use permitted concealed handguns improperly, but the gains completely overwhelm these concerns.

Another surprise involves women and blacks. Both tend to be the strongest supporters of gun control, yet both obtain the largest benefits from nondiscretionary concealed-handgun laws in terms of reduced rates of murder and other crimes. Concealed handguns also appear to be the great equalizer among the sexes. Murder rates decline when either more women or more men carry concealed handguns, but the effect is especially pronounced for women. An additional woman carrying a concealed handgun reduces the murder rate for women by about three to four times more than an additional man carrying a concealed handgun reduces the murder rate for men. Providing a woman with a concealed handgun represents a much larger change in her ability to defend herself than it does for a man.

The benefits of concealed handguns are not limited to those who use them in self-defense. Because the guns may be concealed, criminals are unable to tell whether potential victims are carrying guns until they attack, thus making it less attractive for criminals to commit crimes that involve direct contact with victims. Citizens who have no intention of ever carrying concealed handguns in a sense get a "free ride" from the crime-fighting efforts of their fellow citizens. However, the "halo" effect created by these laws is apparently not limited to people who share the characteristics of those who carry the guns. The most obvious example is the drop in mur-

ders of children following the adoption of nondiscretionary laws. Arming older people not only may provide direct protection to these children, but also causes criminals to leave the area.

Nor is the "halo" effect limited to those who live in areas where people are allowed to carry guns. The violent-crime reduction from one's own state's adopting the law is in fact greatest when neighboring states also allow law-abiding citizens to carry concealed handguns. The evidence also indicates that the states with the most guns have the lowest crime rates. Urban areas may experience the most violent crime, but they also have the smallest number of guns. Blacks may be the racial group most vulnerable to violent crime, but they are also much less likely than whites to own guns.

These estimates make one wonder about all the attention given to other types of gun legislation. My estimates indicate that waiting periods and background checks appear to produce little if any crime deterrence. During the 1990s former president Clinton credited the Brady law with lowering crime because it had, according to him, been "taking guns out of the hands of criminals."[5] During the 1996 Democratic National Convention, Sarah Brady, after whose husband the bill was named, boasted that it "has helped keep more than 100,000 felons and other prohibited purchasers from buying handguns."[6] From 1994 until the Supreme Court's decision in 1997, backers of the Brady law focused almost exclusively on the value of background checks, the one part of the law that the Supreme Court specifically struck down.[7]

Actually, the downward crime trend started in 1991, well before the Brady law became effective in March 1994. With a national law that goes into effect only once, it is difficult to prove empirically that the law was what altered crime rates, because so many other events are likely to have occurred at that same time. One of the major advantages of the large data set examined in this book is that it includes data from many different states that have adopted nondiscretionary laws in many different years.

Others estimate a much smaller effect of the Brady law on gun sales. In 1996 the General Accounting Office reported that initial rejections based on background checks numbered about 60,000, of which over half were for purely technical reasons, mostly paperwork errors that were eventually corrected.[8] A much smaller number of rejections, 3,000, was due to convictions for violent crimes, and undoubtedly many of the people rejected proceeded to buy guns on the street. By the time the background-check provision was found unconstitutional, in June 1997, only four people had gone to jail for violations.

Presumably, no one would argue that rejected permits are meaningful by themselves. They merely proxy for what might happen to crime rates, provided that the law really stops criminals from getting guns. Do criminals simply get them from other sources? Or do the restrictions primarily inconvenience law-abiding citizens who want guns for self-defense? The results presented in this book are the first systematic national look at such gun laws, and if the national *Uniform Crime Report* data through 1994 or state waiting periods and background checks are any indication, the empirical evidence does not bode well for the Brady law. No statistically significant evidence has appeared that the Brady law has reduced crime, and there is some statistically significant evidence that rates for rape and aggravated assault have actually risen by about 4 percent relative to what they would have been without the law.

Yet research does not convince everybody. Perhaps the Supreme Court's June 1997 decision on the constitutionality of the Brady law's national background checks will shed light on how effective the Brady law was. The point of making the scope of the background check *national* was that without it, criminals would buy guns from jurisdictions without the checks and use them to commit crimes in the rest of the country. As these national standards are eliminated, and states and local jurisdictions discontinue their background checks,[9] will crime rates rise as quickly without this provision of the law as gun-control advocates claimed they fell because of it? My bet is no, they will not. If President Clinton and gun-control advocates are correct, a new crime wave should be evident by the time this book is published.

Since 1994, aside from required waiting periods, many new rules making gun ownership by law-abiding citizens more difficult have come into existence. There were 279,401 active, federal gun-dealer licenses in the nation when the new licensing regulations went fully into effect in April 1994. By 2000 there were 100,000, a decline of 64 percent, and by September 2009 it had fallen to 50,630.[10] This has undoubtedly made purchasing guns less convenient. Besides increasing licensing fees from $30 to $200 for first-time licenses and imposing renewal fees of $90, the 1994 Violent Crime Control and Law Enforcement Act imposed significant new regulatory requirements that were probably much more important in reducing the number of licensees.[11]

The Bureau of Alcohol, Tobacco, and Firearms (BATF) supports this decrease largely because it believes that it affects federal license holders who are illegally selling guns. The BATF's own (undoubtedly high) estimate is

that about 1 percent of federal license holders illegally sell guns, and that this percentage has remained constant with the decline in licensed dealers.[12] If so, 155,115 licensees have lost their licenses in order to eliminate 1,551 illegal traffickers. Whether this lopsided trade-off justifies stiffer federal regulation is unclear, but other than simply pointing to the fact that crime continued on its downward course nationally during this period, no evidence has been offered. No attempt has been made to isolate this effect from many other changes that occurred over the same period of time.[13]

Changes in the law will also continue to have an impact. Proposals are being made by the U.S. Department of Justice to "require owners of firearms 'arsenals' to provide notice to law enforcement," where the definition of what constitutes an "arsenal" seems to be fairly subjective, and to "require gun owners to record the make, model, and serial number of their firearms as a condition of obtaining gun insurance." Other proposals would essentially make it impossible for private individuals to transfer firearms among themselves.

What implications does this study have for banning guns altogether? This book has not examined evidence on what the crime rate would be if all guns could be eliminated from society—no data were present in the data set for areas where guns were completely absent for any period of time, but the findings do suggest how costly the transition to that gun-free goal would be. If outlawing guns would primarily affect their ownership by law-abiding citizens, this research indicates that at least in the short run, we would expect crime rates to rise. The discussion is very similar to the debate over nuclear disarmament. A world without nuclear weapons might be better off, but unilateral disarmament may not be the best way to accomplish that goal. The large stock of guns in the United States, as well as the ease with which illegal items such as drugs find their way across borders implies that not only might the transition to a gun-free world be costly (if not impossible), but the transition might also take a long time.

Further, not everyone will benefit equally from the abolition of guns. For example, criminals will still maintain a large strength advantage over many of their victims (such as women and the elderly). To the extent that guns are an equalizer, their elimination will strengthen criminals relative to physically weak victims. As we have seen in discussing international crime data, eliminating guns alters criminals' behavior in other ways, such as reducing their fear of breaking into homes while the residents are there.

All these discussions, of course, ignore the issues that led the founding fathers to put the Second Amendment in the Constitution in the first

place—important issues that are beyond the scope of this book.[14] They believed that an armed citizenry is the ultimate bulwark against tyrannical government. Possibly our trust in government has risen so much that we no longer fear what future governments might do. Having just fought a war for their independence against a government that had tried to confiscate their guns, the founding fathers felt very strongly about this issue.

What Can We Conclude?

How much confidence do I have in these results? The largest previous study on gun control produced findings similar to those reported here but examined only 170 cities within a single year. This book has examined over 54,000 observations (across 3,000 counties for eighteen years) and has controlled for a range of other factors never accounted for in previous crime studies. I have attempted to answer numerous questions. For example, do higher arrest or conviction rates reduce crime? What about changes in other handgun laws, such as penalizing the use of a gun in the commission of a crime, or the well-known waiting periods? Do income, poverty, unemployment, drug prices, or demographic changes matter? All these factors were found to influence crime rates, but no previous gun study had accounted for changing criminal penalties, and this study is the first to look at more than a few of any of these other considerations.

Preventing law-abiding citizens from carrying handguns does not end violence; it merely makes victims more vulnerable to attack. While people have strong views on either side of this debate, and one study is unlikely to end this discussion, the size and strength of my deterrence results and the lack of evidence that holders of permits for concealed handguns commit crimes should at least give pause to those who oppose concealed handguns. In the final analysis, one concern unites us all: Will allowing law-abiding citizens to carry concealed handguns save lives? The answer is yes, it will.

9 Updating the Results in 2000

Updating the Basic Results

I started this research several years ago with data from 1977 to 1992, all the county data that were available at that time. When the book was first published, I had updated the data through 1994. It is now possible to expand the data even further, through 1996. This is quite important, since so many states very recently have passed right-to-carry laws. During 1994, Alaska, Arizona, Tennessee, and Wyoming enacted new right-to-carry laws, and during 1995, Arkansas, Nevada, North Carolina, Oklahoma, Texas, and Utah followed suit.[1] Between 1977 and 1996 a total of twenty states had changed their laws and had them in effect for at least one full year.[2]

Some commentators complained that even though my study was by far the largest statistical crime study ever, there were simply not enough data to properly evaluate the impact of the laws. Others suspected that the findings were simply a result of studying relatively unusual states.[3] Another criticism was that poverty was not properly accounted for.[4]

While the methods I used in the 1998 edition were by far the most comprehensive that I know of, I have continued to look into other methods. By putting together an entirely new data set—using city-level information—it is possible to go beyond my previous efforts and to also control for policing-policy variables such as arrest and

conviction rates, number of police per-capita, expenditures on police per capita, and a proxy for the so-called broken-windows policing policy. The city-level data that I have now compiled include direct information on whether a city has adopted community policing, problem-oriented policing, and/or the broken-windows approach.

One of the commentators on my book suggested that in addition to year-to-year changes in the national crime rate as well as state and county crime trends, another way to account for crime cycles is by measuring whether the crime rates are falling faster in right-to-carry states than in other states in their region rather than compared to just the nation as a whole. While it is impossible to use a separate variable for each year for each individual state, because that would falsely appear to explain all the year-to-year changes in average crime rates in a state, it is possible to group states together. This new set of estimates would account not only for whether the crime rates in concealed-handgun states are falling relative to the national crime rate but now also for whether they are falling relative to the crime rates in their region. To do this, the country is divided into five regions (Northeast, South, Midwest, Rocky Mountains, and Pacific) and variables are added to measure the year-to-year changes in crime by region.[5] All county- and city-level regressions will employ these additional control variables.

Some have criticized my earlier work for not doing enough to account for poverty rates. As a response, I have incorporated in this section of the book state-level measures of poverty and unemployment rates in addition to all the county-level variables that accounted for these factors earlier in this book. The execution rates for murders in each state are now included in estimates to explain the murder rate. Finally, new data on the number of permits granted in different states make it easier to link crime rates to the number of permits granted.

Reviewing the Basic Results

The central question is, How did crime rates change before and after the right-to-carry laws went into effect? The test used earlier in this book examined the difference in the time trends before and after the laws were enacted.[6] With the extended data and the additional variables for the year-to-year changes in crime by region (so-called regional fixed year effects), state poverty, unemployment, and death-penalty execution rates, table 9.1 shows that this pattern closely resembles the pattern found earlier in the

book: violent-crime rates were rising consistently before the right-to-carry laws and falling thereafter.[7] The change in these before-and-after trends was always extremely significant—at least at the 0.1 percent level. Compared to the results for tables 4.8 or 4.13, the effects were larger for overall violent crimes, rape, robbery, and aggravated assaults and smaller for murder. For each additional year that the laws were in effect, murders fell by an additional 1.5 percent, while rape, robbery, and aggravated assaults all fell by about by 3 percent each year. The other variables continued to produce results similar to those that were found earlier.[8]

While no previous crime study accounts for year-to-year changes in regional crime rates, it is possible to go even beyond that and combine different approaches. Including not only the factors accounted for in table 9.1 but also individual state time trends produces similar results. The annual declines in crime from right-to-carry laws are greater for murder (2.2 percent), rape (3.9 percent), and robbery rates (4.9 percent), while the impact on aggravated assaults (0.8 percent) and the property-crime rates (0.9 percent) is smaller.

Figures 9.1–9.5 illustrate how the violent-crime rates vary before and after the implementation of right-to-carry laws when both the linear and squared time trends are employed. Despite expanding the data through 1996 so that the legal changes in ten additional states could be examined, the results are similar to those previously shown in figures 4.5–4.9.[9] As in the earlier results, the longer the laws are in effect, the larger the decline in violent crime. The most dramatic results are again for rape and robbery rates, which were rising before the right-to-carry law was passed and falling thereafter. Robbery rates continued rising during the first full year that the law was in effect, but the rate of increase slowed and began to fall by the second year. It is this continued increase in robbery rates which kept the violent crimes as a whole from immediately declining. While aggravated assaults were falling on average before the right-to-carry law was adopted, figure 9.5 shows that the rate of decline accelerated after the law went into effect.

What Determines the Number of Permits Issued and What Is the Net Benefit from Issuing Another Permit?

The Number of Permits

The relationship between the percentage of the population with permits and the changes in crime rates is central to much of the debate over the right to carry. My previous work was based on the number of permits

Table 9.1 Reexamining the change in time trends before and after the adoption of nondiscretionary laws, using additional data for 1995 and 1996

	Percent change in various crime rates for changes in explanatory variables								
	Violent crime	Murder	Rape	Robbery	Aggravated assault	Property crime	Burglary	Larceny	Auto theft
Change in the crime rate from the difference in the annual change in crime rates in the years before and after the adoption of the right-to-carry law (annual rate of change after the law – annual rate of change before the law)	−2.3%*	−1.5%*	−3.2%*	−2.9%*	−3.0%*	−1.6%*	−2.5%*	−0.9%*	−2.1%*

Note: This table uses county-level violent and property-crime data from the *Uniform Crime Report* that were not available when I originally wrote the book. All regressions use weighted least squares, where the weighting is each county's population. The regressions correspond to those in tables 4.8 and 4.13. The one difference from the earlier estimates is that these regressions now also allow the regional fixed effects to vary by year.

*The result is significant at the 1 percent level for a two-tailed *t*-test.

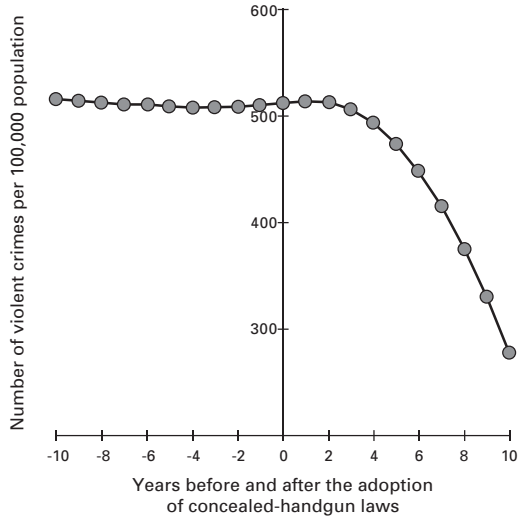


Figure 9.1. The effect of concealed-handgun laws on violent crimes

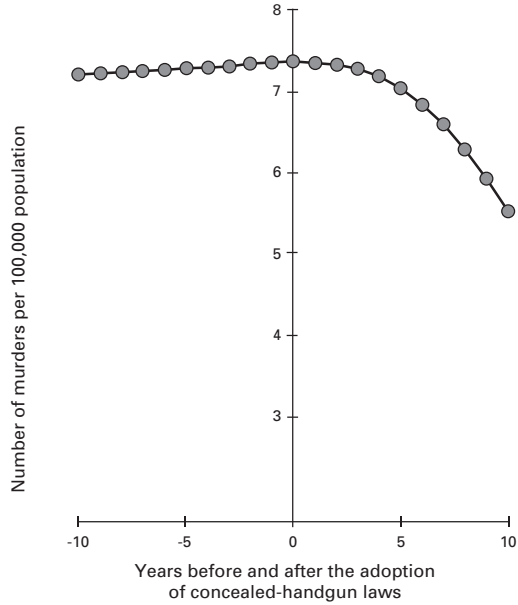


Figure 9.2. The effect of concealed-handgun laws on murders

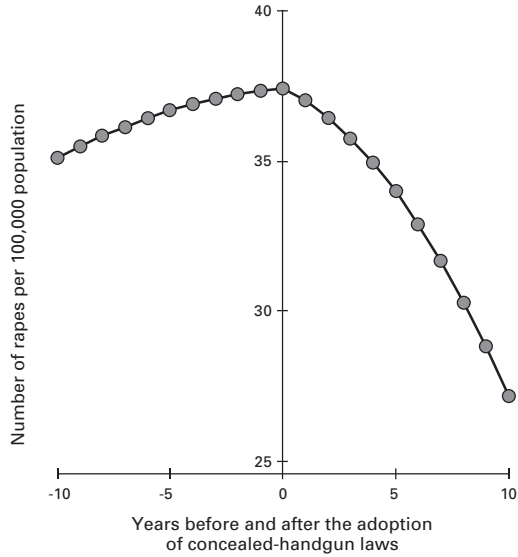


Figure 9.3. The effect of concealed-handgun laws on rapes

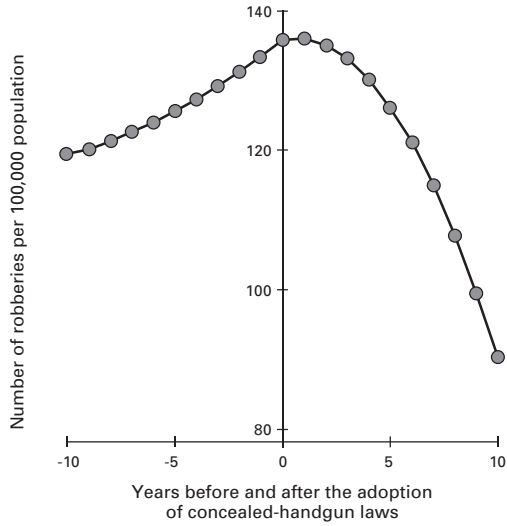


Figure 9.4. The effect of concealed-handgun laws on robberies

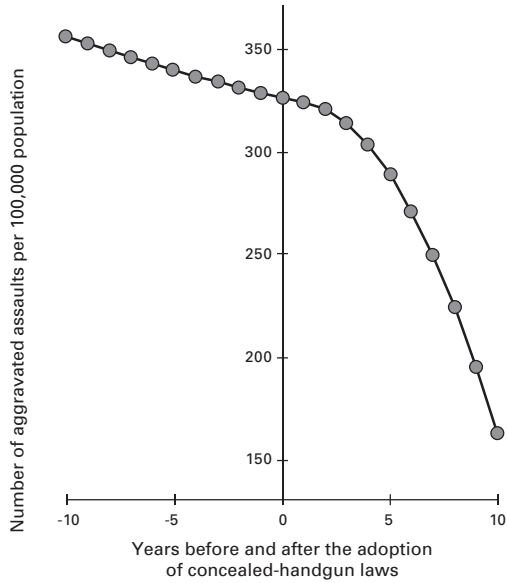


Figure 9.5. The effect of concealed-handgun laws on aggravated assaults

issued for counties in Oregon and Pennsylvania as well as on discussions with various government officials on what types of counties issued the most permits. The comparison across states assumed that what created the difference in permit rates across counties also applied across states. Some more state-level data have now become available on permit rates, but such data are still relatively scarce. In addition to Florida, Oregon, and Pennsylvania, I have also acquired some annual permit-rate data up to 1996 for Alaska, Arizona, Oklahoma, South Carolina, Texas, Utah, and Wyoming, though these states had these rules in effect for no more than a few years.

While these data are limited on the number of permit holders, they allow us to examine what factors determine permitting rates, which in turn lets us link the permitting rate to changes in crime. Permit prices, the amount of training required to get a permit, the length of time that permitting rules have been in effect, and the crime rate are all important factors in determining how many people will get permits. Permitting fees and prices charged for training courses are expected to reduce the number of permits issued, but another important cost of getting a permit is the time spent meeting the requirements. This is not to say that there are not also benefits from training (that is a separate issue), but in the narrow issue of

how many permits will be issued, there is no doubt that longer training requirements discourage some people from getting permits.

What permitting rules are in place largely depends upon when the laws were first enacted. States that adopted right-to-carry laws more recently tend to have more restrictive licensing requirements. For example, the three states (Alaska, Arizona, and Texas) requiring at least ten hours of training adopted their rules during the last few years of the sample, and Arizona is the only right-to-carry state that requires additional training when permits are renewed. Six of the eight states with permitting fees of at least $100 have also enacted the law during the last few years. This raises the concern that the drops in crime from the passage of right-to-carry laws may be smaller in the states that have most recently adopted these laws simply because they have issued fewer permits.

Based on state-level data, table 9.2 shows the impact of permit fees, training requirements, and how long (in years) the law has been in effect. Because the evidence indicates that the number of new permits is likely to trail off over time, the estimates include both the number of years the law has been in effect and the number of years squared. Fees and training requirements were first investigated without square terms. Notice that only a small fraction of the population gets permits, ranging from less than 1 percent to 6 percent. With that in mind, the regression results show that for each $10 increase in fees, the population getting permits is reduced by about one half of a percentage point. And requiring five hours of training (rather than none) reduces the number of permits by about two-thirds of a percentage point. In a typical state without any fees or training requirements, the percentage of the population with permits would grow from about 3 percent to a little less than 6 percent after a decade.

I also ran more complicated specifications including squared terms for fees and training requirements. They gave similar results: fees discourage people from obtaining permits over almost the entire range (until fees go over $130, which is near the highest fee in the sample—$140 for Texas).

Table 9.2 What determines the rate at which people obtain permits?

	$10 increase in permit fee	5-hour increase in training requirement	5 years after the law has passed, assuming no fee or training requirement	10 years after the law has passed, assuming no fee or training requirement
Percentage of the state population with permits	−.5%*	−.6%*	4.8%*	6.1%*

*The result is significant at the 1 percent level for a two-tailed *t*-test.

Anecdotal evidence from newspapers indicates that yet another factor is important: the fear of an attack. Thus, crime and multiple-victim public shootings increase gun sales and concealed-handgun permits.[10] Other variables, such as violent-crime rates, murder rates, the number of multiple-victim public shootings, or the death rate from those attacks, are also important for determining how many people get permits, but they do not alter the impact of the previously mentioned variables. Each additional multiple-victim public shooting increases a state's number of permits by about two-tenths of a percentage point, and each additional person who is killed in such a shooting per 1 million people living in a state increases handgun permits by one-tenth of a percentage point.

The Crime Rate and the Estimated Number of Concealed Handguns

The above estimates allow us to revisit the impact of permits and crime rates. While the time-series data on permits issued in different states covers a relatively limited number of years, we do have detailed information on the factors that help determine the number of permits (the fees, training requirements, and how long the law has been in effect). The results from the specification shown in table 9.2 were used to construct "predicted values." Constructing a predicted percentage of a state's population with permits allows us to do more than relying on how crime rates change over time or on the anecdotal evidence I obtained from surveying different state permitting agencies.

These new results using state-level data, shown in table 9.3, indicate that violent-crime rates fell across the board as more permits were issued, with the largest drop occurring for robberies. These results correspond closely to the diagrams reported in figures 4.6–4.9 and 7.1–7.4, which indicate that robberies and rapes are most dramatically affected by the number of years that right-to-carry laws are in effect. The coefficients imply that for every 1,000 additional people with permits, there are 0.3 fewer murders, 2.4 fewer rapes, 21 fewer robberies, and 14.1 fewer aggravated assaults.[11] On the other hand, with the exception of burglary, property crime remained statistically unchanged as more people obtained permits.

Would society benefit from more people getting permits? As already noted, obtaining a permit costs money and takes time. Carrying around a gun is also inconvenient, and many states impose penalties if the gun does not remain concealed.[12] On the positive side, permit holders benefit from having the gun for protection and might also come to the rescue of others. But perhaps just as important are the benefits to general crime deterrence pro-

duced by concealed-carry laws, for they also help protect others indirectly, as criminals do not know which people can defend themselves until they attack. This raises the real risk that too few people will get permits, as permit holders personally bear all these costs but produce large benefits for others.

Whether too few permits are being issued depends on how the crime rate changes as more and more permits are issued and whether it is the permit holder or the general public who primarily reaps the benefit from more concealed carry.

The impact of increasing the number of permits on crime is shown in table 9.3, column 1. However, the impact does not need to be constant as more people get permits. Indeed, there may well exist what economists call "diminishing returns"—that is, the crime-reducing benefits from another person getting a permit falls as more people get permits. The reason behind this is twofold: first, those most at risk could be the first to get permits; second, once one adult in a public setting (e.g., a store) has a concealed handgun, the additional benefit from a second or third person being armed should be relatively smaller.

But it is also conceivable that the probability that a victim can defend herself must rise above a certain threshold before it does much to discourage criminals. For instance, if only a few women brandish guns, a would-be rapist may believe that a defensive use is simply an exception and go after another woman. Perhaps if a large enough percentage of women defend themselves, the would-be rapist would decide that the risk to himself is too high.

One can test for diminishing returns from more permits by using a squared term for the percentage of the population with permits. The results (shown in column 2) indicate that right-to-carry states experience additional drops in all the violent-crime categories when more permits are issued. For murder, rape, and robbery, all states experience further reductions in crime from issuing more permits, though diminishing returns appear for murder and aggravated assault. (Only one state—Pennsylvania—approaches the number of permits beyond which there would be little further reduction in aggravated assaults from issuing more permits.) An important word of caution is in order here. These particular estimates of the percentage of the population that minimizes crime are rather speculative, because they represent predictions outside the range for which observed permit levels are available. (We thus cannot use these results to predict with confidence what would happen if a state got up to, say, 8 percent having permits.) Still, there is little doubt that issuing additional permits beyond what we have today lowers crime.

Table 9.3 Using the predicted percent of the population with permits to explain the changes in different crime rates for state data

	One-percentage-point change in the share of the state population with permits (1)	Pattern when a quadratic term is added for the percent of the population with permits (2)	Number by which total crimes are reduced when an additional 1 percent of the population obtains permits in 1996, using the estimates from column 1 for states that had a right-to-carry law in effect by that year (3)
Violent crime	–7%*	Drop reaches its maximum when 23% of the population has permits	
Murder	–4%****	Drop reaches its maximum when 8% of the population has permits	432 lives saved
Rape	–7%*	Drop is increasing at an increasing rate as more people get permits	3,862 fewer rapes
Robbery	–13.6%*	Drop tapers off, but so slowly that it is still falling when 100 percent of the population has permits	35,014 fewer robberies
Aggravated assault	–5%**	Drop reaches its maximum when 6 percent of the population has permits	28,562 fewer aggravated assaults
Property crime	–2.6%****	Drop continues at a constant rate	
Burglary	–10%*	Drop is increasing at an increasing rate as more people get permits	144,227 fewer burglaries
Larceny	–.6%	No significant pattern	27,922 fewer larcenies
Auto theft	–3%	Drop reaches its maximum when 3 percent of the population has permits	21,254 fewer auto thefts

Note: Using the National Institute of Justice estimates of what crime costs victims to estimate the net savings from 1 percent more of the population obtaining permits (or of each additional permit) in 1998 dollars, the cost is reduced by $3.45 billion ($2,516 per permit).
*The result is significant at the 1 percent level for a two-tailed *t*-test.
**The result is significant at the 5 percent level for a two-tailed *t*-test.
****The result is significant at the 15 percent level for a two-tailed *t*-test.

Chapter 5 employed county-level permit data from Oregon and Pennsylvania and used the estimated victimization costs from the National Institute of Justice to determine the net benefit to society from issuing an additional permit. Similar estimates can be made for the thirty-one states issuing permits in 1996: each one-percentage-point increase in the population obtaining permits is associated with a $3.45 billion annual net saving to crime victims (in 1998 dollars). Each additional permit produces a total societal benefit of $2,500 per year. While this estimate is smaller than my earlier figures for Oregon and Pennsylvania, the total benefits greatly exceed the total costs of getting a permit. In other words, the numbers suggest that not enough permits are being issued.

The results also indicate that permitting fees are highly detrimental. For each $10 increase in fees, the percentage of the population with permits falls by one half of one percentage point. For the thirty-one states with right-to-carry laws, this increases victimization costs by $1.7 billion. The large effect from higher permitting fees might be due to the poorest and most vulnerable being especially discouraged from obtaining a permit. Blacks living in higher-crime urban areas benefit disproportionately from concealed-handgun permits. High fees are more likely to deter individuals from carrying guns when those individuals are poor. When fees are high, there may be a smaller crime-reduction benefit from right-to-carry laws even if the same percentage of the population were to obtain permits.

To test this, I reestimated the relationship between predicted permits and crime by also including the direct impact of permit fees on the crime rate.[13] The regressions for violent crime, murder, robbery, and aggravated assault all indicate that, holding constant the percentage of the population with permits, higher fees greatly reduce the benefit from right-to-carry laws. For example, the drop in robberies from one percent of the population having permits is about two percentage points smaller when the fee is raised from $10 to $50.

Updating the Evidence on Who Benefits from Permits

While the preceding results relied on state-level data, we know from previous work (already presented in this book) that different parts of states obtained greatly varying benefits from issuing permits. This finding is confirmed with the new, updated data. But I will here discuss a somewhat different specification, linking the changes in crime more closely to the issuing of more permits. The percentage of the population with permits

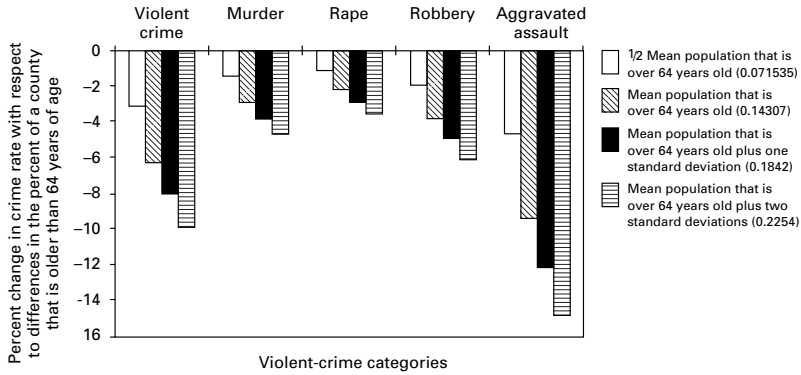


Figure 9.6. How does the change in crime from nondiscretionary concealed-handgun laws occur in counties with relatively more people over age sixty-four?

Figure 9.7. How does the change in crime from nondiscretionary concealed-handgun laws occur in the most densely populated counties?

is interacted with the percentage of the adult population in a county that is over sixty-four years of age, the population density per square mile, the percentage that is black, the percentage that is female, and per-capita personal income. The earlier interactions in chapter 4, reported with county population, are skipped over here because they again produce results that are extremely similar to the regressions with an interaction for population density.[14]

The results reported in figures 9.6–9.9 are all quite statistically significant and imply the same pattern reported earlier when using the data through 1992. The benefits of right-to-carry laws are not uniform across counties. Counties with a high portion of elderly people, blacks, and

Figure 9.8. How does the change in crime from nondiscretionary concealed-handgun laws vary with the percentage of a county's population that is black?

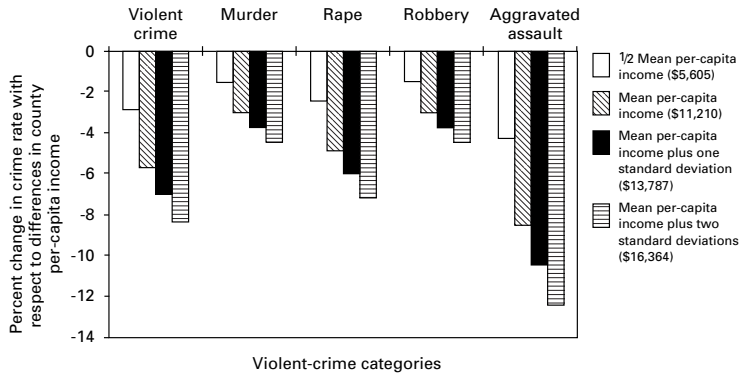


Figure 9.9. How does the change in crime from nondiscretionary concealed-handgun laws vary with county per-capita income?

females—the most vulnerable victims—all benefit disproportionately more from concealed-handgun laws. So do those living in counties that are densely populated.

Certain crime patterns do emerge. For example, in counties with many elderly people (23 percent of the population over age sixty-four) right-to-carry laws have a large deterrent effect against aggravated assaults and robberies but seem to have a relatively small effect on rapes. In contrast, counties with few elderly individuals (7 percent of their population over sixty-four years of age) have only about a third of the drop in violent crime that counties with many elderly people have. Heavily black areas benefit the most through reductions in robberies and rapes, while areas where women

make up a larger share of the population and those living in the wealthiest areas obtain the largest benefits from drops in aggravated assaults and rapes. The benefit for blacks is very large. Increasing the percentage of the black population in a county from half the mean (4.4 percent) to two standard deviations above the mean (37 percent) increases the reduction in violent crime from right-to-carry laws from about one percentage point to over seven percentage points.

Unlike the earlier data presented in chapter 4, which represented crime through 1992, not all the states adopting right-to-carry laws during 1993–1996 moved from a discretionary to a nondiscretionary law. Some states had previously prohibited the carrying of concealed handguns. This is important because one of the reasons that I examined the interactions of population or population density with right-to-carry laws was that state government officials had told me that under a discretionary system lower-population counties had already tended to be more liberal in granting permits. Higher-population counties were thus expected to experience the largest increase in issuing permits and thus the largest drops in violent crime after a non-discretionary system was adopted. In fact, I find that the more populous counties in states changing from discretionary to nondiscretionary laws had a statistically bigger relative drop in violent-crime rates than states that changed from banning concealed handguns to nondiscretionary laws.

These updated results confirm my earlier findings that those who are relatively weaker physically (women and the elderly) and those who are most likely to be crime victims (blacks and those living in urban areas) tend to benefit the most from the passage of right-to-carry laws. Taken together, these results indicate that legislators should be sensitive not only to the costs of running the permitting program, but also to how the rules affect the number and types of people who get permits. Focusing only on setting fees to recoup the costs of the permitting system will end up being financially short sighted.

How Sensitive Are the Results to Different Specifications?

While I have tried to control for all sorts of factors that might explain changes in crime over time, it is indeed possible to get overzealous and account for *too many* variables. Including variables that do not really affect crime can actually create problems similar to excluding factors that should be included. Take a simple example of explaining how the stock market, say the Dow Jones industrials, changes over time. Obvious variables to in-

clude would be the interest rate and the expected growth in the economy, but many other variables—many of dubious importance—could possibly also be included. The problem arises when such variables are correlated to changes in stock prices merely by chance. An extreme case would be including the prices of various grocery store products. A store might sell thousands of items, and one—say, the price of peanut butter—might happen to be highly correlated with the stock prices over the particular period examined. We know that peanut butter has little to do with explaining overall stock prices, but if it just accidentally happens to move up and down with the movements in the stock market, other variables (like the interest rate) may no longer prove to be statistically significant.

There are ways to protect against this "dubious variable" problem. One is to expand the sample period. If no true causal relationship exists between the two variables, the probability that this coincidence will continue to occur during future years is low. And this is exactly what I have done as more data have become available: first by looking at data through 1992, then extending them to 1994, and in the second edition up until 1996. Another approach guarding against the "dubious variable" problem is to replicate the same test in many different places. Again, this is exactly what I have done here: I have studied the impact of right-to-carry laws in different states at different times. As charged by many a critic, it is still conceivable that some other factor just happened to occur also when an individual state passed the law, but the probability of mere coincidence falls as the experiences of more and more states are examined. It is also possible that adding variables that don't belong can cause you to get a more significant result for other factors than is warranted.

Generally, excluding variables that should be included is a more significant problem than including variables that should not be included, and in general I have tried to err on the side of including whatever possible factors can be included. Indeed, a strong case can be made that one must be careful not to include too many variables like state time trends, which can be endlessly added on and have little theoretical justification. Still, I do not consider any of these variables to be similar to the price of peanut butter at the local grocery store in the previous discussion, but obviously some researchers might believe that some variables should not be included. One way to investigate this issue is to include only those variables that different investigators view as relevant. In the early stages of my research, when I presented my original research as a working paper at seminars, I asked participants for other factors that should be included, and some of their

comments were very helpful. I also tried in vain to ask pro-gun-control researchers what variables they wanted me to include in the regressions, but (as discussed in chapter 7) they did not make any suggestions when my initial research was circulated for comments. What comments they made after the publicity broke claimed that I had not controlled for factors that I had indeed accounted for.

Since the original research immediately received a lot of attention, I have let my critics decide for themselves what variables should be included by simply giving them complete access to the data. I know from personal communication that some critics (such as Black and Nagin) did indeed examine numerous different specifications.[15]

A more systematic, if time-consuming, approach is to try all possible combinations of these so-called control variables—factors which may be interesting but are included so that we can be sure of the importance of some other "focus" variables.[16] In my regressions to explain crime rates there are at least nine groups of control variables—population density, waiting periods and background checks, penalties for using guns in the commission of a crime, per-capita income, per-capita unemployment insurance payments, per-capita income maintenance payments, retirement payments per person for those over sixty-five, state poverty rate, and state unemployment rate.[17] To run all possible combinations of these nine groups of control variables requires 512 regressions. The regressions for murder rates also require a tenth control variable for the death-penalty execution rate and thus results in 1,024 combinations of control variables. Given the nine different crime categories, this amounts to 5,120 regressions.

This approach is decidedly biased toward not finding a consistent effect of the right-to-carry laws, because it includes many combinations of control variables that no researcher thinks are correct specifications. Indeed, even the strongest, best-accepted empirical relationships usually fail this test.[18] Since different people will have different preferences for what variables should be included, this massive set of results makes sense only if one knows what variables produce what results. If a range of conflicting estimates are then produced, people can judge for themselves what they think the "true" range of the estimates is.

Two sets of variables have been primarily used to test the impact of right-to-carry laws: crime trends before and after the adoption of right-to-carry laws and the percentage of people with permits. Yet another division is possible by focusing on counties with a large number of people to avoid the difficulty that low-population counties frequently have zero murder

or rape rates and thus have "undefined" arrest rates.[19] Eliminating counties with fewer than 20,000 people removes about 70 percent of the missing arrest ratios for murder while sacrificing 20 percent of the observations (the population-weighted frequencies are 23 and 6 percent, respectively). Dropping out more populous counties reduces the sample size but has virtually no impact on further reducing the frequency of missing arrest rates. Even if I limit the estimates to the full sample and counties with more than 20,000 people, combining that with the two other types of specifications results in 20,480 regressions. Because of all the concerns over possible crime trends, all estimates include variables to account for the average differences across counties and years as well as by year within region as well as the thirty-six demographic variables.[20]

Figures 9.10–9.13 present the range of estimates associated with these different combinations of variables and specifications, both in terms of their extreme bounds and their median value. What immediately stands out when one examines all these estimates is how extremely consistent the violent-crime results are. For example, take figure 9.10. A one-percentage-point change in people with permits lowers violent-crime rates by 4.5–7.2 percent. Indeed, all the estimates (over two thousand of them) for overall violent crime, murder, rape, robbery, and aggravated assault indicate that increases in permits reduce crime. All the combinations of the other ten sets of control variables imply that a one-percentage-point increase in the population holding permits reduces murder rates by 2–3.9 percent annually. Compared to the state-level data, the benefits from right-to-carry laws are much smaller for robbery and much larger for aggravated assaults.

Figure 9.11 uses the simple before-and-after trends to examine the impact of the right-to-carry laws, and the results for the violent-crime rates are generally consistent with those shown in figure 9.10. Again, all the violent-crime-rate regressions show the same direction of impact from the concealed-handgun law. The median estimated declines in violent-crime rates are quite similar to those initially reported in table 9. 1. For each additional year that the right-to-carry laws are in effect, violent crimes decline by 2.4 percent, murders by 1.6 percent, rapes and aggravated assaults by over 3 percent, and robberies by 2.7 percent.

With the notable exception of burglaries, which consistently decline, figures 9.10 and 9.11 provide mixed evidence for whether right-to-carry laws increase or decrease other property crimes. Even when one focuses on estimates of one type, such as those using the percentage of the population with permits, the county- and state-level data yield inconsistent results. Yet,

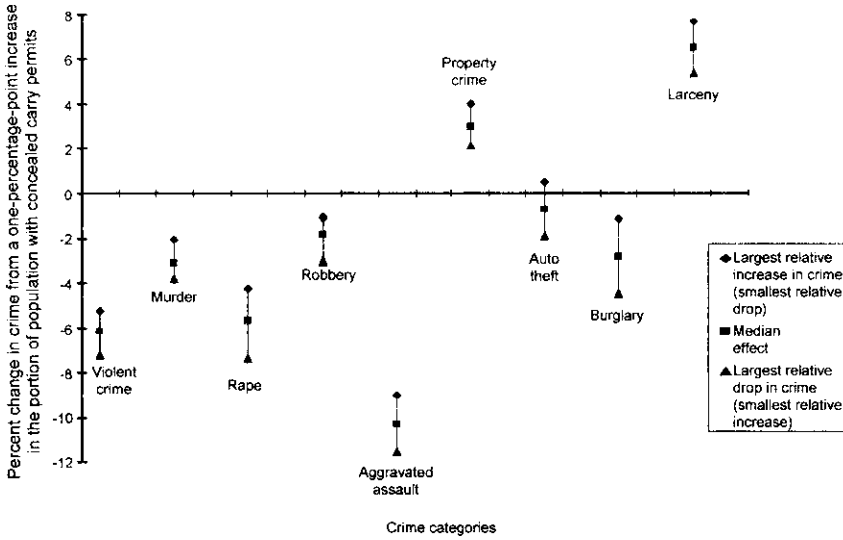


Figure 9.10. Sensitivity of the relationship between the percentage of the population with permits and annual changes in crime rates: data for all counties

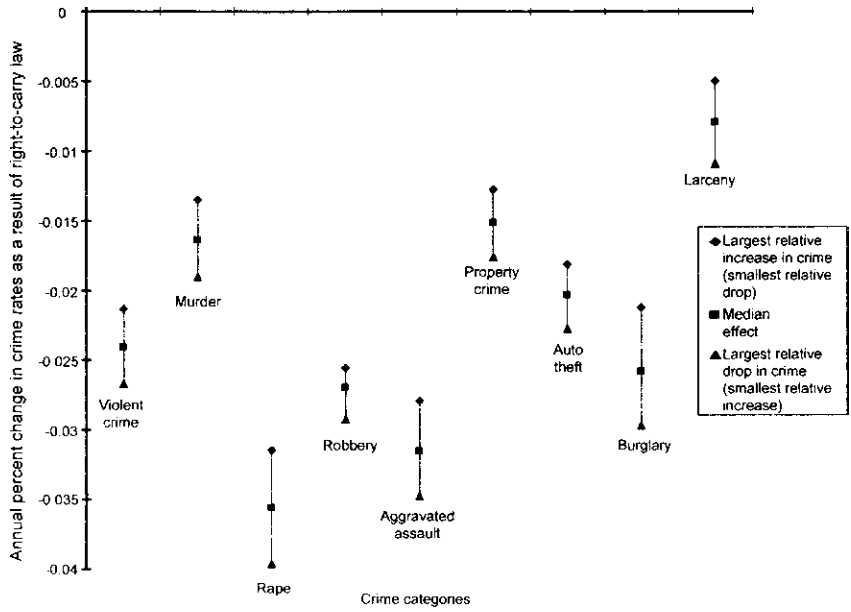


Figure 9.11. Sensitivity of the relationship between right-to-carry laws and annual changes in crime rates: data for all counties

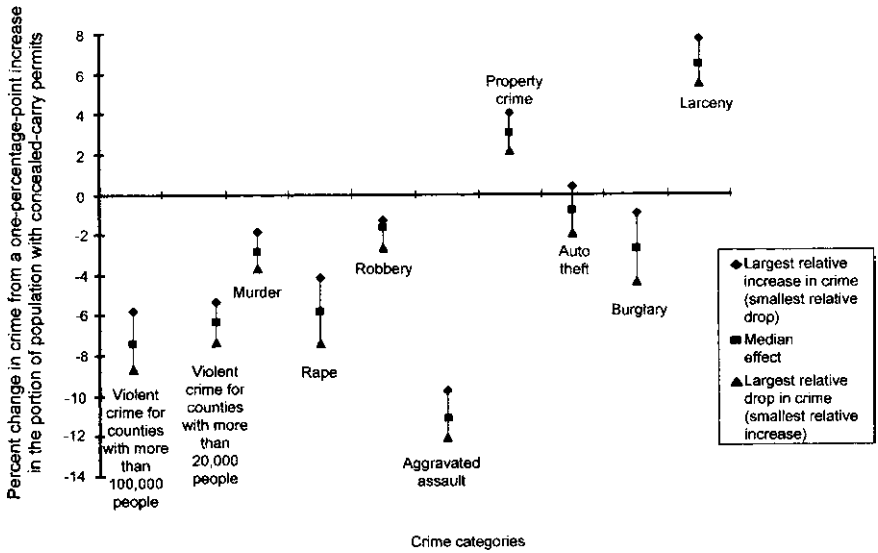


Figure 9.12. Sensitivity of the relationship between the percentage of the population with permits and annual changes in crime rates: data for counties with either more than 20,000 people or more than 100,000 people (all individual crime categories—that is, all categories except "violent crime"—are for counties with more than 20,000 people)

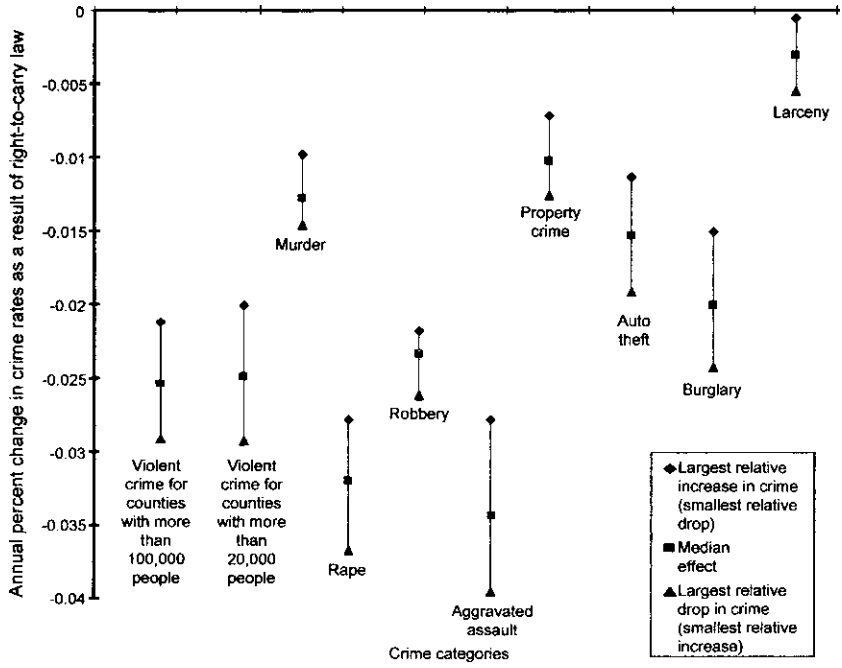


Figure 9.13. Sensitivity of the relationship between right-to-carry laws and annual changes in crime rates: data for counties with either more than 20,000 people or more than 100,000 people (all individual crime categories—that is, all categories except "violent crime"—are for counties with more than 20,000 people)

while the net effect of right-to-carry laws on larceny and auto theft is not clear, one conclusion can be drawn: the passage of right-to-carry laws has a consistently larger deterrent effect against violent crimes than property crimes and may even be associated with increases in property crimes.

Figures 9.12 and 9.13 limit the sample to the more populous counties and continue reaching very similar results. For counties with more than 20,000 people, the estimate ranges are always of the same sign and have magnitudes similar to those results which examined all the counties. Both figures also looked at the sensitivity of the overall violent-crime rate for counties over 100,000. The range of estimates was again very similar, though they implied a slightly larger benefit than for the more populous counties. For example, figure 9.12 shows that in counties with more than 20,000 people, violent crime declines by between 5.4 and 7.4 percentage points for each additional 1 percent of the population with permits, while the analogous drop for counties with more than 100,000 people is between 5.8 and 8.7 percentage points.

A total of 13,312 regressions for the various violent-crime categories are reported in this section. The evidence clearly indicates that right-to-carry laws are always associated with reductions in violent crime, and 89 percent of the results are statistically significant at least at the 1 percent level. The results are not sensitive to including particular control variables and always show that the benefits from these laws increase over time as more people obtain permits. The 8,192 regressions for property crime imply a less consistent relationship between right-to-carry laws and property crime, but even when drops in property crime are observed, the declines are smaller than the decrease in violent crime.

While limiting the sample size to only larger-population counties provides one possible method of dealing with "undefined" arrest rates, it has a serious drawback—information is lost by throwing out those counties with fewer than 20,000 people. Another approach is to control for either the violent- or property-crime arrest rate depending upon whether the crime rate being studied is that of violent or property crime. Even if a county has zero murders or rapes in a particular year, virtually all counties have at least some violent or property crime, thus eliminating the "undefined" arrest rate problem and still allowing us to account for county-level changes over time in the effectiveness of law enforcement. This approach also helps mitigate any spurious relationship between crime and arrest rates that might arise because the arrest rate is a function of the crime rate. Reestimating the 4,096 regressions in figure 9.10 for murder, rape, robbery, aggravated

assault, auto theft, burglary, and larceny with this new measure of arrest rates again produces very similar results.

City Crime Data

County data, rather than city data, allow the entire country to be examined. This is important, since, obviously, not everyone lives in cities. Such data further allow us to deal with differences in how permits are issued, such as the discretion states grant to local law enforcement. Relying on county data allows a detailed analysis of many important factors, such as arrest and conviction rates, the number of police, expenditures on police, (sometimes) prison sentences, and proxies for policing policies like the so-called broken-windows strategy (according to which police focus on less serious property crimes as a means of reducing overall violent crime). Yet a drawback with county data is that policing policies cannot be dealt with well, for such policy decisions are made at the level of individual police departments—not at the county level.[21] With a few exceptions such as San Francisco, Philadelphia, and New York, where county and city boundaries coincide, only city-level data can be used to study these issues.

The focus of my research is guns and crime, but I had to make sure that I accounted for whatever policing policies are being employed.[22] Three policing strategies dominate the discussion: community-oriented policing, problem-oriented policing, and the broken-windows approach. While community-oriented policing is said to involve local community organizations directly in the policing effort, problem-oriented policing is sometimes viewed as a less intrusive version of the broken-windows policy. Problem-oriented policing began as directing patrols on the basis of identified crime patterns but nowadays involves the police in everything from cleaning housing projects and surveying their tenants to helping citizens design parking garages to reduce auto theft.[23] An extensive Westlaw database search was conducted to categorize which cities adopted which policing strategies as well as their adoption and rescission dates.[24]

Other recent research of mine demonstrates the importance of racial and gender hiring decrees on the effectiveness of police departments.[25] When hiring rules are changed so as to create equal pass rates on hiring exams across different racial groups—typically by replacing intelligence tests with what some claim are arbitrary psychological tests—the evidence indicates that the quality of new hires falls across the board. And the longer these new hiring policies are in place, the more detrimental the effect on police de-

partments. As with the right-to-carry laws, simple before-and-after trends were included to measure the changing impact of these rules over time.

Let us return to the main focus, guns and crime. To examine the impact of right-to-carry laws, the following list of variables has been accounted for: city population, arrest rate by type of crime, unemployment rate, percentage of families headed by females, family poverty rate, median family income, per-capita income, percentage of the population living below poverty, percentage of the population that is white, percentage that is black, percentage that is Hispanic, percentage that is female, percentage that is less than five years of age, percentage that is between five and seventeen, percentage that is between eighteen and twenty-five, percentage that is between twenty-six and sixty-four, percentage that is sixty-five and older, median population age, percentage of the population over age twenty-five with a high school diploma, percentage of the population over age twenty-five with a college degree, and other types of gun-control laws (waiting periods, background checks, and additional penalties for using guns in the commission of a crime). As with the earlier county- and state-level data, variables are included to measure the length of state waiting periods, as well as the change in average crime rates from state waiting periods, background checks, penalties for using a gun in the commission of crime, and whether the federal Brady law altered existing state rules. Again, all estimates include variables to account for the average differences across counties and years as well as by year within region.

Table 9.4 provides strong evidence that even when detailed information on policing policies is taken into account, passing concealed-handgun laws deters violent crime. The benefit in terms of reduced murder rates is particularly large, with a drop of 2.7 percent each additional year that the right-to-carry law is in effect. The drop experienced for rapes is 1.5 percent per year. The one violent crime for which the decline is not statistically significant is aggravated assault. On the other hand, property crimes increase after the adoption of right-to-carry laws, confirming some of the earlier findings.

Consent decrees—which mandate police hiring rules that ensure equal pass rates by race and sex—significantly and adversely affect all crime categories but rape. For each additional year that the consent decree is in effect, overall violent crimes rise by 2.4 percent and property crimes rise by 1.9 percent.

The evidence for the before-and-after average crime rates for the different types of policing policies is more mixed, and my research does not

Table 9.4 Accounting for policing policies using city-level data

	Percent change in various crime rates for changes in explanatory variables								
	Violent crime	Murder	Rape	Robbery	Aggravated assault	Property crime	Burglary	Larceny	Auto theft
Change in the crime rate from the difference in the annual change in crime rates in the years before and after the adoption of a right-to-carry law (annual rate of change after the law – annual rate of change before the law)	–1.2%**	–2.7%*	–1.5%**	–1.0%*	–0.6%	0.92%**	1.0%*	0.7%**	1.2%**
Change in the crime rate after imposition of a consent decree regarding the hiring of police officers	2.4%*	0.4%***	–0.27%	3.5%*	1.4%*	1.9%*	2.4%*	2.0%*	0.5%**
Change in the average crime rate after implementation of community policing	–3.3%[a]	2.2%	–4.9%[a]	–1.7%[b]	–2.5%[b]	1.6%[a]	–1.2%[c]	2.5%[a]	7.7%[a]
Change in the average crime rate after implementation of problem-oriented policing	2.4%	–4.1%	–4.5%	3.6%	2.6%	1.8%	–1.8%	–2.7%	24.9%[a]
Change in the average crime rate after implementation of broken-window policing	–0.8%	6.7%[c]	–10.1%[a]	–3.8%	2.3%	–6.4%[a]	–5.6%[a]	–12.3%[a]	18.2%[a]
Average crime rate after adoption of one-gun-a-month purchase rule	9.3%	14.7%[d]	6.8%	7.9%[d]	15.8%[c]	–0.6%	2.7%	–4.9%	11%
Change in the average crime rate in a state after a neighboring state adopts a one-gun-a-month rule	9.6%[a]	18.4%[a]	11.9%	4.1%	17.2%[a]	13%[a]	10.6%[b]	14.3%[c]	11%[c]

[a]The result is significant at the 1 percent level for a two-tailed *t*-test.
[b]The result is significant at the 5 percent level for a two-tailed *t*-test.
[c]The result is significant at the 10 percent level for a two-tailed *t*-test.
[d]The result is significant at the 12 percent level for a two-tailed *t*-test.
*The F-test is significant at the 1 percent level.
**The F-test is significant at the 5 percent level.
***The F-test is significant at the 10 percent level.

attempt to deal with issues of why the different rules were adopted to begin with.[26] In ten cases, the policing policies produce significant reductions in crime, but in six cases there are significant increases in crime. Including cases that are not statistically significant still produces no consistent pattern: the policing policies are associated with declines in crime in fifteen cases and increases in twelve cases. A possible explanation for such results might be that adopting new policing policies reallocates resources within the police department, causing some crime rates to go down while others go up. Indeed, each of the three policing policies is associated with increases in some categories of crime and decreases in others. It is difficult to pick out many patterns, but community policing reduces violent crimes at the expense of increased property crimes.

Revisiting Multiple-Victim Public Shootings

Student eyewitnesses and shooting victims of the Pearl High School (Mississippi) rampage used phrases like "unreal" and "like a horror movie" as they testified Wednesday about seeing Luke Woodham methodically point his deer rifle at them and pull the trigger at least six times. . . . The day's most vivid testimony came from a gutsy hero of the day. Assistant principal Joel Myrick heard the initial shot and watched Woodham choosing his victims. When Woodham appeared headed for a science wing where early classes were already under way, Myrick ran for his pickup and grabbed his .45-caliber pistol. He rounded the school building in time to see Woodham leaving the school and getting into his mother's white Chevy Corsica. He watched its back tires smoke from Woodham's failure to remove the parking brake. Then he ordered him to stop. "I had my pistol's sights on him. I could see the whites of his knuckles" on the steering wheel, Myrick said. He reached into the car and opened the driver-side door, then ordered Woodham to lie on the ground. "I put my foot on his back area and pointed my pistol at him," Myrick testified.[27]

Multiple-victim public shootings were not a central issue in the gun debate when I originally finished writing this book in the spring of 1997. My results on multiple-victim public shootings, presented in chapter 5, were obtained long before the first public school attacks occurred in October 1997. Since that time, two of the eight public school shootings (Pearl, Mississippi, and Edinboro, Pennsylvania) were stopped only when citizens with guns interceded.[28] In the Pearl, Mississippi, case, Myrick stopped the killer from proceeding to the nearby junior high school and continuing his attack there. These two cases also involved the fewest people harmed in any of the attacks. The armed citizens managed to stop the attackers well before

the police even had arrived at the scene—4½ minutes before in the Pearl, Mississippi, case and 11 minutes before in Edinboro.

In a third instance, at Columbine High School in Littleton, Colorado, an armed guard was able to delay the attackers and allow many students to escape the building, even though he was assigned to the school because he had failed to pass his shooting proficiency test. The use of homemade grenades, however, prevented the guard from fighting longer. There is some irony in Dylan Klebold, one of the two killers, strongly opposing the proposed right-to-carry law that was being considered in Colorado at the time of the massacre.[29] In the attack on the Jewish community center in Los Angeles in which five people were wounded, the attacker had apparently "scouted three of the West Coast's most prominent Jewish institutions—the Museum of Tolerance, the Skirball Cultural Center and the University of Judaism—but found security too tight."[30]

It is remarkable how little public discussion there has been on the topic of allowing people to defend themselves. It has only been since 1995 that we have had a federal law banning guns by people other than police within one thousand feet of a school.[31]

Together with my colleague William Landes, I compiled data on all the multiple-victim public shootings occurring in the United States from 1977 to 1999, during which time twenty-three states adopted right-to-carry laws. As with earlier numbers reported in this book, the incidents we considered were cases with at least two people killed or injured in a public place. We excluded gang wars or shootings that were by-products of another crime, such as robbery. The United States averaged twenty-nine such shootings annually, with an average of 1.5 people killed and 2.5 wounded in each incident.

What can stop these attacks? We examined a range of different gun laws, including waiting periods, as well the frequency and level of punishment. However, while arrest and conviction rates, prison sentences, and the death penalty reduce murders generally, they have no significant effect on public shootings. There is a simple reason for this: Those who commit these crimes usually die in the attack. They are killed in the attack or, as in the Colorado shooting, they commit suicide. The normal penalties simply do not apply.

In the deranged minds of the attackers, their goal is to kill and injure as many people as possible. Some appear to do it for the publicity, which is related to the harm inflicted. Some may do it only because they value harming others. The best way to prevent these attacks might therefore be

to limit the carnage they can cause if they do attack. We find only one policy that effectively accomplishes this: the passage of right-to-carry laws.

Even after accounting for the factors that we have used in the other estimates, when different states passed right-to-carry laws during the twenty-three years we studied, the number of multiple-victim public shootings declined by a whopping 67 percent. Deaths from all these shootings plummeted by 75 percent, and injuries by 81 percent. Figure 9.14 demonstrates how the raw number of attacks changes before and after the passage of right-to-carry laws. The extensive research that we have done indicates that these results hold up very well when the long list of factors discussed in this book is taken into account. The very few attacks that still occur in states after enactment of right-to-carry laws tend to occur in particular places where concealed handguns are forbidden, such as schools.

Concealed-handgun laws significantly reduce multiple-victim public shootings in public places (but have no systematic effects on bombings). The estimates imply that the average state passing these laws reduces the total number of murders and injuries per year from 1.91 to .42 and the number of shootings from .42 to .14. Despite expecting a deterrent effect from these laws because of the high probability that one or more potential victims or bystanders will be armed, the drop in murders and injuries is still surprisingly large. And as we shall see, alternative measures of shootings

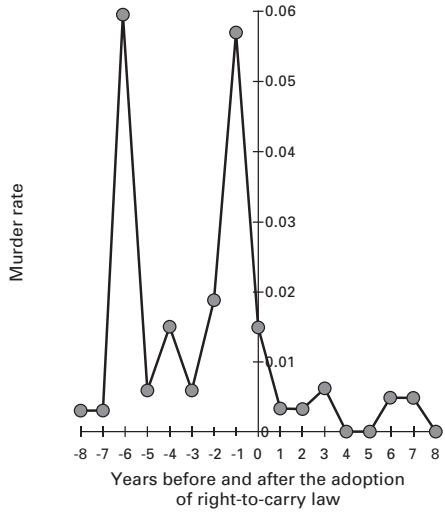


Figure 9.14. Murders from multiple-victim public shootings per 100,000 people: data from 1977 to 1995

and adding other factors that might explain the drop do not seem to reduce the magnitude of the law's effect.[32]

The reason why the deterrent effect on multiple-victim public attacks is greater than on attacks on individual victims is fairly straightforward. Say the probability that a victim has a permitted concealed handgun is 5 percent. That will raise the expected costs to the criminal and produce some deterrence. Yet if one hundred adults are present on a train or in a restaurant, even if the probability that any one of them will be able to offer a defense is only 5 percent, the probability that at least someone there has a permitted concealed handgun is near 100 percent.[33] The results for multiple-victim public shootings are consistent with the central findings of this book: as the probability that victims are going to be able to defend themselves increases, the level of deterrence increases.

Concealed-handgun laws also have an important advantage over uniformed police, for would-be attackers can aim their initial assault at a single officer, or alternatively wait until he leaves the area. With concealed carrying by ordinary citizens, it is not known who is armed until the criminal actually attacks. Concealed-handgun laws might therefore also require fewer people carrying weapons. Some school systems (such as Baltimore) have recognized this problem and made nonuniformed police officers "part of the faculty at each school."[34]

Despite all the debate about criminals behaving irrationally, reducing their ability to accomplish their warped goals reduces their willingness to attack. Yet even if mass murder is the only goal, the possibility of a law-abiding citizen carrying a concealed handgun in a restaurant or on a train is apparently enough to convince many would-be killers that they will not be successful. Unfortunately, without concealed carry, ordinary citizens are sitting ducks, waiting to be victimized.

Other Gun-Control Laws

"Gun control? It's the best thing you can do for crooks and gangsters," Gravano said. "I want you to have nothing. If I'm a bad guy, I'm always gonna have a gun. Safety locks? You will pull the trigger with a lock on, and I'll pull the trigger. We'll see who wins."[35]

—Sammy "the Bull" Gravano, the Mafia turncoat, when asked about gun control

Every couple of years we see a big push for new gun-control laws. Unfortunately, the discussion focuses on only the possible benefits and ignores any costs. Waiting periods may allow for a "cooling-off period," but they may

also make it difficult for people to obtain a gun quickly for self-defense. Gun locks may prevent accidental gun deaths involving young children, but they may also make it difficult for people to use a gun quickly for self-defense.[36] The exaggerated stories about accidental gun deaths, particularly those involving young children, might scare people into not owning guns for protection, even though guns offer by far the most effective means of defending oneself and one's family.

Some laws, such as the Brady law, may prevent some criminals from buying guns through legal channels, such as regular gun stores. Nevertheless, such laws are not going to prevent criminals from obtaining guns through other means, including theft. Just as the government has had difficulty in stopping gangs from getting drugs to sell, it is dubious that the government would succeed in stopping criminals from acquiring guns to defend their drug turf.

Similar points can be made about one-gun-a-month rules. The cost that they impose upon the law abiding may be small. Yet there is still a security issue here: someone being threatened might immediately want to store guns at several places so that one is always easily within reach. The one-gun-a-month rule makes that impossible. Besides this issue, the rule is primarily an inconvenience for those who buy guns as gifts or who want to take their families hunting.

The enactment dates for the safe-storage laws and one-gun-a-month rules are shown in table 9.5.[37] For the implementation dates of safe-storage laws, I relied primarily on an article published in the *Journal of the American Medical Association,* though this contained only laws passed up through the end of 1993.[38] Handgun Control's Web site provided information on the three states that passed laws after this date. The laws share certain common features, such as making it a crime to store firearms in a way that a reasonable person would know allows a child to gain use of a weapon. The primary differences involve exactly what penalties are imposed and the age at which a child's access becomes allowed. While Connecticut, California, and Florida classify such violations as felonies, other states classify them as misdemeanors. The age at which children's access is permitted also varies across states, ranging from twelve in Virginia to eighteen in North Carolina and Delaware. Most state rules protect owners from liability if firearms are stored in a locked box, secured with a trigger lock, or obtained through unlawful entry.

The state-level estimates are shown in table 9.6. Only the right-to-carry

Table 9.5 Enactment dates of other gun control laws

State	Date law went into effect
Safe-storage laws:[a]	
Florida	10/1/89
Iowa	4/5/90
Connecticut	10/1/90
Nevada	10/1/91
California	1/1/92
New Jersey	1/17/92
Wisconsin	4/16/92
Hawaii	6/29/92
Virginia	7/1/92
Maryland	10/1/92
Minnesota	8/1/93
North Carolina	12/1/93
Delaware	10/1/94
Rhode Island	9/15/95
Texas	1/1/96
One-gun-a-month laws:[b]	
South Carolina	1976
Virginia	7/93
Maryland	10/1/96

[a]Source for the dates of enactment of safe-storage laws through the end of 1993 is Peter Cummings, David C. Grossman, Frederick P. Rivara, and Thomas D. Koepsell, "State Gun Safe Storage Laws and Child Mortality Due to Firearms," *Journal of the American Medical Association,* 278 (October 1, 1997): 1084–86. The other dates were obtained from the Handgun Control Web site at http://www.handguncontrol.org/caplaws.htm.
[b]Data were obtained through a Nexis/Lexis search. Lynn Waltz, "Virginia Law Cuts Gun Pipeline to Capital's Criminals," *Norfolk Virginian-Pilot,* September 8, 1996, p. A7.

laws are associated with significant reductions in crime rates. Among the violent-crime categories, the Brady law is only significantly related to rape, which increased by 3.6 percent after the law passed. (While the coefficients indicate that the law resulted in more murders and robberies but fewer aggravated assaults and as a consequence fewer overall violent crimes, none of those effects are even close to being statistically significant.) Only the impact of the Brady law on rape rates is consistent with the earlier results that we found for the data up through 1994.

Safe-storage rules also seem to cause some real problems. Passage of these laws is significantly related to almost 9 percent more rapes and robberies and 5.6 percent more burglaries. In terms of total crime in 1996, the presence of the law in just these fifteen states was associated with 3,600 more rapes, 22,500 more robberies, and 64,000 more burglaries. These increases might reflect the increased difficulty victims have in reaching a

Table 9.6 Evaluating other gun-control laws using state-level data

	Percent change in various crime rates for changes in explanatory variables								
	Violent crime	Murder	Rape	Robbery	Aggravated assault	Property crime	Burglary	Larceny	Auto theft
Change in the crime rate from the difference in the annual change in crime rates in the years before and after the adoption of the right-to-carry law (annual rate of change after the law – annual rate of change before the law)	−2.0%*	−3.2%*	−1.4%*	−3.8%*	−2.3%*	−1.3%*	−2.9%*	−0.8%***	0.06%
Change in the average crime rate after the adoption of Brady law	−2.4%	3.6%	3.6%[a]	0.02%	−4.2%	−0.6%	0.7%	−0.6%	2.5%
Change in the average crime rate after the adoption of safe-storage rules	0.04%	1.3%	8.9%[a]	8.9%[a]	−4.4%	2.5%	5.6%[b]	2.0%	−0.6%

[a]The result is significant at the 1 percent level for a two-tailed *t*-test.
[b]The result is significant at the 5 percent level for a two-tailed *t*-test.
*The F-test is significant at the 1 percent level.
***The F-test is significant at the 10 percent level.

gun to protect themselves. However, a contributing factor might be the horror stories that often accompany the passage of these laws, reducing people's desire to own a gun in the first place. The increase in burglaries is particularly notable. Burglars appeared to be less afraid of entering homes after these laws were passed. Additional state data would be required to answer the question of whether "hot burglaries"—burglaries occurring while the residents are in the dwelling—increased and whether burglars spent less time casing dwellings after these laws were passed. Evidence of these other changes would help confirm that these laws have emboldened criminals.

On the other side of this question is the number of accidental gun deaths that will be prevented. The General Accounting Office reported in 1991 that mechanical safety locks are unreliable in preventing children over six years of age from using a gun,[39] but there is still the question of how many of these children's lives might have been saved, and even if locks are unreliable for older children, some deaths may be prevented. Even if one believes that the high-end estimated benefits are correct, that as many as 31 of the 136 children under age fifteen who had died from accidental gunshots in 1996 would have been saved by nationwide safe-storage laws, table 9.6 implies some caution.[40] The effect for murders was not statistically significant, but it still provides the best estimate that we have and the size of the effect is still instructive. It indicates that in just these fifteen states, 109 lives would be lost from this law. If the entire country had these safe-storage laws, the total lost lives would have risen to 255.

Yet other research that I have done with John Whitley indicates that this is the most optimistic possible outcome from safe-storage laws. We find no support for the theory that safe-storage laws reduce either juvenile accidental gun deaths or suicides. Instead, these storage requirements appear to impair people's ability to use guns defensively. Because accidental shooters also tend to be the ones most likely to violate the new law, safe-storage laws increase violent and property crimes against low-risk citizens with no observable offsetting benefit in terms of reduced accidents or suicides. Just as important, we found that examining the simple before-and-after average effects of the law underestimates the increases in crime that result from safe-storage laws. When the before-and-after trends are accounted for, the group of fifteen states that adopted these laws faced an annual average increase of over 300 more murders, 3,860 more rapes, 24,650 more robberies, and over 25,000 more aggravated assaults during the first five full years after the passage of the safe-storage laws. Using the National Institute of Justice

estimates of victim costs from crime indicates that the average annual costs borne by victims averaged over $2.6 billion.

The one-gun-a-month rule seems to have negative consequences, too. But only three states passed these laws during the twenty years studied, so there is always the issue of whether enough data exist and whether other factors might have played a role. Nevertheless, the passage of these laws was associated with more murders, more robberies, and more aggravated assaults, and the effects appear to be quite large.

One possible suspicion, however, is that the large effect of one-gun-a-month rules merely reflects some regional crime increases, increases that just happen to coincide with the adoption of these laws. To counter this potential problem, I again allowed year-to-year average differences to vary by region, as I had done for the county- and city-level data. The results for right-to-carry laws were essentially unchanged, and the pattern for other gun-control laws remained very similar, though some of the statistical significance declined. The Brady law was still associated with a statistically significant increase in rapes. Using the simple before-and-after averages, safe-storage laws were still associated with statistically significant increases in rape, robbery, and burglary. Indeed, not only did the coefficients remain significant at the 1 percent level, but the results actually implied slightly larger increases in these crime categories, with the effect from state storage laws on rape now increasing to 9 percent, on robbery to 9.9 percent, and on burglary to 6.8 percent.

The Political and Academic Debate Continued

Attacking the Messenger

David Yassky [member of the board of directors of Handgun Control, Inc.]: The people who fund your studies are gun manufacturers.

Lott: That is a lie.

Yassky: That is not a lie. That is not a lie.

Lott: That is a lie.

Yassky: It is paid for by gun manufacturers who manufacture firearms.

—From *Debates/Debates,* a nationally syndicated program on public television that was broadcast during the week of April 22, 1999

Michael Beard [president of the Coalition to Stop Gun Violence]: Yes, and you're unbiased. You work for, what, the Olin Foundation, which manufactures firearms . . .

Lott: No I don't. I work for the University of Chicago.

Beard: Who pays your salary?

Lott: The University of Chicago pays my salary.

Beard: Through the Olin Foundation.

Lott: No, that's not true.

—From *CNN Today,* June 18, 1999; 1:29 P.M. Eastern Time

Gun-control advocates all too frequently use these types of arguments in debates. Often callers on radio shows make similar claims. Even if the claim merely diverts the discussion away from whether guns save more lives than they cost, my guess is that the gun-control organizations view the personal attack as a success.[41] Unfortunately, no matter how many times I deny the charge or explain that no, I did not apply for money from the Olin Foundation; no, I was paid by the University of Chicago; no, the Olin Foundation and the Olin Corporation are separate entities; and no, it was the faculty at the University of Chicago who decided on my appointment and they asked no questions about my future research topics, many people still tune out after these charges are raised.

During 1999, numerous newspaper columns also made similar claims, for instance: "John R. Lott Jr., the latest darling of gun advocates everywhere. He's the Olin Fellow of Law and Economics at the University of Chicago School of Law. (That's 'Olin' as in Olin-Winchester, one of the world's leading manufacturers of ammunition)."[42] Or "They fail to mention that Lott is a John M. Olin fellow. This Olin Foundation is funded through the Olin Corp., the parent company of Winchester Ammunition. Winchester makes more money as the sale of handguns goes up."[43] Letter writers to newspapers have also chimed in: "It was particularly helpful that he exposed Professor John R. Lott Jr. as an intellectually dishonest toady of the bullet manufacturing industry."[44] Even after being given facts to the contrary, some state legislators have continued making claims like "The Lott study's been thrown out. . . . It's a joke. . . . Professor Lott is funded by the Olin Corporation which is funded by Winchester."[45] And, of course, Internet news-group discussions are filled with such assertions.[46] Others bring up the topic only to point out that while others believe it to be important, they do not personally believe that it is relevant.[47]

Gun-control groups have repeatedly attacked me rather than my findings and distorted the research I have done in other areas. State legislators in Michigan, Missouri, Nebraska, and Maryland have begun calling me up to ask whether it is true that I don't think that police departments should hire black or female police officers. Handgun Control and the Violence Policy Center spread claims such as "Lott has argued that the hiring of more

women and minorities in law enforcement has actually increased crime rates."[48] They have made this claim on their Web sites, in debates, and on radio programs.[49] In fact, I had stated that this would be the wrong conclusion to reach. The paper argued: "But it would be a serious mistake not to realize that this simple relationship is masking that the new rules reduce the quality of new hires from other groups."[50] The affirmative action rules which changed the testing standards lowered the quality of new police hires across the board, and that was showing itself in the simple relationship between minority hires and crime.[51]

On the upside, many have come to my defense. One academic review of my book noted, "The personal (and, to those who know him, completely unfounded) attacks on John Lott's integrity were made with such ferocity and in so many media outlets nationwide that one can only conclude that Lott was, with apologies to our gracious First Lady [Hillary Clinton], the target of a vast left-wing conspiracy to discredit his politically incorrect findings."[52] Another academic review wrote: "the ease with which gun-control advocates could get misleading and even false claims published by the press raises important public choice questions. Many of these claims were highly personal and vicious, including outright lies about alleged funding of Lott's research by the firearms industry . . . , about the outlet for his then forthcoming work . . . , about Lott's fringe ideas . . . , and about his lack of qualifications. . . . Most academics probably would have withdrawn back into the sheltered halls of their universities rather than expose themselves to the vicious public attacks that John Lott faced."[53] Other academics have written that "gun control groups attempted to discredit his work by smearing him with accusations that they had to know were patently false"[54] and about the "vicious campaign of lies and distortions."[55] Publications for police officer associations have also been very supportive.[56]

Once in a while, I have come to feel that there is a well-organized campaign to impugn my findings, especially on days when I have done radio talk shows for stations based in different parts of the country and callers state word for word the exact same charge that I have been paid to do my research by gun makers. Originally, I had thought that these personal attacks would fade away after a year or so, but they have now continued for three years, so unfortunately they will probably continue. The most disconcerting aspect of this, especially for my family, has been the numerous physical threats, including an instance of a note on our apartment door.[57]

Yet the gun-control organizations still realized that they had to do more to counter my work. In December 1996, Handgun Control had organized a debate that was broadcast on C-SPAN between myself and three critics: Dan Black, Dan Nagin, and Jens Ludwig. However, none of the researchers that they invited were able to claim that concealed-handgun laws increased crime. I can only imagine that this put Handgun Control in a bind. It is hard to oppose legislation or a referendum by arguing that concealed-handgun laws do no harm. Not being able to find support from the researchers that they work closely with, Handgun Control finally came out with its own numbers in a press release on January 18, 1999, arguing that between 1992 and 1997 violent-crime rates were falling more quickly in the states that most restricted concealed handguns than in the states with more liberal rules.

Their claim was widely and uncritically reported in publications from *Newsweek* to *USA Today,* as well as during the spring 1999 campaign to pass a concealed-handgun law in Missouri.[58] Press coverage and Handgun Control itself usually referred to this contention as coming from the FBI.[59]

Handgun Control examined the change in violent crime between only two years, 1992 and 1997, and strangely enough they chose to classify states according to what their laws were in 1997, at the end of the period. This odd classification makes a considerable difference, for some states' right-to-carry laws did not even go into effect until late 1996, with few permits issued until 1997. It makes no sense to attribute the increase in crime to a law for the five years before the law goes into effect. A third of the states with right-to-carry laws did not enact them until after late 1995. Of course, the way any trained researcher would approach the question is to separate the change in crime rates before and after the different states changed their laws. That is only common sense. Only changes in crime after the law goes into effect can be attributed to the passage of the law.

Given the evidence in this book, I would also argue that since one is examining the change in crime rates, it is important to separate out those states that have had changes in permits and those that have not. If a state has had its right-to-carry law in place for decades, it is extremely unlikely that it will be experiencing any additional growth in permits and thus it should not be expecting any additional changes in its crime rates from this law. Handgun Control also did not account for any other factors that could have influenced crime. Nor did they even classify states consistently across their own press releases issued within months of each other.[60]

During the Missouri campaign, many reporters called me up to comment about the "FBI numbers" on crime rates.[61] When I would point out that the claim was actually based on a report produced by Handgun Control, they said that they didn't know what to do with the conflicting claims. Editorials and news stories in the *St. Louis Post-Dispatch* and the *Kansas City Star* normally just accepted the Handgun Control assertion as established truth.

After repeatedly encountering this response from reporters, I started suggesting to reporters that they ask some local academic (a statistician, criminologist, or economist) to evaluate the two conflicting claims. One reporter with the *St. Louis Post-Dispatch,* Kim Bell, expressed the concern that they might run into a professor with a preconceived bias and that would make the test unfair. I told her that I was willing to take that risk, but that if she were concerned about that problem, she could always approach a few different academics. Others who refused to take me up on this challenge included Bill Freivogel, deputy editor at the *Post-Dispatch,* and Rich Hood, an editor at the *Kansas City Star.* Rather, their newspapers simply presented Handgun Control's claims as fact.

Criticisms of the Book

Some reviewers clearly have not even bothered to read my book, or at least it didn't matter to them whether they read it. A review in the *British Journal of Criminology* claimed that "there is nothing in Lott's study to connect this more general information to the specific county-based data on the issuing of concealed-carry permits," "Lott is dealing with a time frame entirely prior to the introduction of the non-discretionary concealed-carry laws in most of the states which now have them," and "he has pre-occupied himself exclusively with 'good guns' owned by 'good people.'"[62] Another book review, in the *New England Journal of Medicine,* starts off by falsely claiming that I "approvingly" quote Archie Bunker's suggestion to stop airplane hijacking by arming "all the passengers."[63]

As of this writing (September 1999), Handgun Control's Web site still continues to assert the same "major criticisms" of my research—"where are the robbery effects?" "auto theft as a substitute for rape," "Lott fails to account for other initiatives—including other gun control laws," "Lott fails to account for cyclical changes in crime rates"—and the same claims about misclassifying state laws.[64] Ironically, they also continue citing the McDowall et. al. (1995) study that we discussed in chapter 2, which examined a total of only five counties picked from three states, attempted to

account for no other factors that might be changing over the same period of time, and examined only murders with guns.[65]

Time magazine reported that "Other critics raise questions about whether Lott massaged the numbers. One arcane quarrel: for statistical purposes, Lott dropped from his study sample any counties that had no reported murders or assaults for a given year."[66] It also said that "the book does not account for fluctuating factors like poverty levels and policing techniques." After the story on my book ran, I called up the reporter, Romesh Ratnesar, and said that I knew that he had read the book carefully, so I was surprised that he would write these claims as if they were true. I, as well as critics like Black and Nagin, had looked at the evidence once arrest rates were excluded so as to include those counties with zero arrest rates. What was particularly disappointing was that I had spent the time to obtain all the data that were available. The county-level data were used for all the years and for all the counties for which they were available, both when I did the original paper and when I wrote the book. As to the other claim, I had measures of poverty and policing techniques like the broken-window strategy included.

While I appreciated that the *Time* magazine piece was published, claims that "the book does not account" for these factors are clearly wrong. Ratnesar agreed that these issues were dealt with in the book, but that his role was not to serve as a "referee" between the two sides. His job was to report what the claims were.[67]

I keep on being amazed at the absolute faith that so many news media people place in the gun-control organizations and the "facts" issued by them. Take another example: Molly Ivins, a syndicated columnist, asserted that "[Lott] himself admits, he didn't look at any other causative factors—no other variables, as they say."[68] She also argued that "Lott's study supposedly showed that when 10 Western states passed 'right-to-carry' laws between 1985 and 1992, they had less violent crime" and that "according to the author's research, getting rid of black women older than 40 would do more to stop murder than anything else we could try." Syndicated columnist Tom Teepen wrote a very similar column a year earlier in which he also claimed that this book "failed to consider other anti-crime variables in making its cause-and-effect claims, a fundamental gaffe."[69]

I did get a chance to talk with Mr. Teepen, and he told me that he wrote his review without even reading the book. He apparently relied on conversations that he had with people at Handgun Control and the Violence Policy Center. When I talked to Cynthia Tucker, an editor at the *Atlanta Journal-Constitution,* where Mr. Teepen is based, about having a letter respond-

ing to the charges Mr. Teepen made, she found it "unbelievable" that he would have written the review without first looking at the book. She grudgingly said that if it were true, they would publish as a response a short letter, but that she would have to check into it first. Needless to say, the newspaper published my letter the following Sunday.[70] In contrast, unfortunately, Ms. Ivins never returned my telephone calls or responded to my e-mail messages and never corrected her claims.[71]

Undoubtedly, some of the claims constitute simple mistakes, but more than a few reflect columnists and others being too quick to accept whatever gun-control groups tell them. I will spare the reader the long list of other false claims reported in the press.[72] Yet, obviously, many people, particularly those with gun-control organizations, continually make statements that they know are false—safe in the knowledge that only a tiny fraction of readers or listeners ever check the assertions. Unfortunately, the gun-control organizations risk losing significant credibility only with the few who read the book.[73]

Other critiques by academics and the media—some old, some new—require more in-depth discussions. The rest of this section reviews the critiques and then provides my responses.

1 *How do we know that these findings are not a result of the normal ups and downs in crime rates?*

> The central problem is that crime moves in waves, yet Lott's analysis does not include variables that can explain these cycles. (David Hemenway, "Book Review of *More Guns, Less Crime,*" *New England Journal of Medicine,* December 31, 1998)

> Jens Ludwig, assistant professor of public policy at Georgetown University, argued that Lott's data don't prove "anything about what laws do to crime." He noted that crime rates, including homicide, are cyclical: They rise and fall every five to 10 years or so in response to forces that are not well understood. Ludwig suggested that this pattern explains the apparent effectiveness of concealed weapons laws. Imagine, he said, a state where the murder cycle is on the upswing and approaching its peak and public concern is correspondingly high. Then a particularly ghastly mass shooting occurs. Panicked legislators respond by passing a law that allows equally panicked citizens to carry concealed weapons. A year or two later, the murder rate goes down, as Lott's study found. (Richard Morin, "Guns and Gun Massacres: A Contrary View," *Washington Post,* May 30, 1999, p. B5)

> Lott's variables are not good predictors of crime waves. Nor does he provide for any effect of history in the way he models crime. For example, the year 1982 could as well follow 1991 as 1981 in his analyses. (David Hemenway, "More Guns, Less Crime," *New England Journal of Medicine,* May 20, 1999)

Even my most determined critics concede one point: violent-crime rates fell at the point in time that the right-to-carry laws went into effect. The real question is: Why did the crime rates fall? Do these laws simply happen to get passed right when crime rates hit their peaks? Why don't we observe this coincidence of timing for other gun-control laws?

It is logically possible that such coincidental timing could take place. But there is more evidence besides decreases in crime after right-to-carry laws are adopted. First, the size of the drop is closely related to the number of permits issued (as indicated in the first edition and confirmed by the additional data shown here). Second, the additional evidence presented here goes even further: it is not just the number of permits, but also the type of people who obtain permits that is important. For example, high fees discourage the poor, the very people who are most vulnerable to crime, from getting permits. Third, if it is merely coincidental timing, why do violent-crime rates start rising in adjacent counties in states without right-to-carry laws exactly when states that have adopted right-to-carry laws are experiencing a drop in violent crime?

Finally, as the period of time studied gets progressively longer, the results are less likely to be due to crime cycles, since any possible crime "cycles" involve crime not only going down but also "up." If crime happened to hit a peak, say, every ten years, and right-to-carry laws tended to be passed right at the peak, then the reported effect of the law would spuriously show a negative impact right after the enactment. However, five years after that an equally large positive spurious effect on crime would have to show up. Instead, my results reveal permanent reductions in crime that only become larger with time, as more people acquire concealed-carry permits.

Furthermore, my study accounted for possible crime cycles in many ways: individual year variables accounted for average national changes in crime rates, and different approaches in chapter 4 controlled for individual state and county time trends and did not take away the effects of concealed carry. To the contrary, they resulted in similar or even stronger estimates for the deterrence effect. Other estimates used robbery or burglary rates to help account for any left-out factors in explaining other crime rates. Since

crime rates generally tend to move together, this method also allows one to detect individual county trends. In updating the book, I have included estimates that account for the separate average year-to-year changes in five different regions in the country. Despite all these additional controls the deterrence effect continues to show up strongly.

It is simply false to claim, "nor does he provide for any effect of history," as I have variables that account for "changes" in crime rates from previous years. I have variables that measure explicitly the number of years that the law has been in effect as well as the number of years until it goes into effect. In addition, I have used individual state linear time trends that explicitly allow crime rates to change systematically over time.

Earlier discussions in chapter 7 on crime cycles (pp. 134–35) and causality (pp. 157–58) also explain why these concerns are misplaced.

2 *Does it make sense to control for nonlinear time trends for each state?*

> The results suggest that the Lott and Mustard model, which includes only a single national trend, does not adequately capture local time trends in crime rates. To test for this possibility, we generalized the Lott and Mustard model to include state-specific trends in an effort to control for these unobserved factors. . . . we report the results for models with a quadratic time trend. The only significant impact estimate is for assaults, and its sign is positive, not negative. (Dan Black and Dan Nagin, "Do Right-to-Carry Laws Deter Violent Crime?" *Journal of Legal Studies,* January 1998, p. 218)

Much more was controlled for than "a single national trend" in my study (e.g., as just mentioned above, state and county trends as well as other crime rates). While it is reasonable to include individual linear state trends or nonlinear trends for regions, including nonlinear trends for individual states makes no sense. The approach by Black and Nagin is particularly noteworthy because it is the one case in which an academic study has claimed that a statistically significant, even if small, increase in any type of violent crime (aggravated assault) occurs after the law.

Consider a hypothetical case in which the crime rate for each and every state follows the pattern that Black and Nagin found in their earlier paper and that I showed in this book (discussed in chapter 7, pp. 134–38): crime rates were rising up until the law went into effect and falling thereafter. Allowing a separate quadratic time trend for each state results in the time trend picking up both the upward path before the law and the downward path thereafter. If the different state crime patterns all peaked in the

year in which their state law went into effect, the state-specific quadratic trends would account for all the impact of the law. A variable measuring the average crime rates before and after the law would then no longer reflect whether the law raised or lowered the crime rate.[74] This is analogous to the "dubious variable" problem discussed earlier. If enough state-specific trends are included, there will be nothing left for the other variables to explain.

If shall-issue laws deter crime, we would expect crime rates to rise until the law was passed and then to rise more slowly or to fall. The effect should increase over time as more permits are issued and more criminals adjust to the increased risks that they face. But the quadratic specification used by Black and Nagin replicates that pattern, state by state. Their results show not that the effect from the quadratic curve is insignificant, but that the deviation of the law's effect from a quadratic curve over time is generally insignificant.

To see this more clearly, take the hypothetical case illustrated in figure 9.15, in which a state faced rising crime rates.[75] The figure shows imaginary data for crime in a state that passed its shall-issue law in 1991. (The dots in the figure display what the crime rate was in different years.) The pattern would clearly support the hypothesis that concealed-handgun laws deter violent crime, but the pattern can easily be fitted with a quadratic curve, as demonstrated with the curved line. There is no systematic drop left over for any measure of the right-to-carry law to detect—in terms of the figure, the difference between the dots and the curved line shows no particular pattern.

Phrased differently, the deterrence hypothesis implies a state-specific time pattern in crime rates (because different states did or did not pass shall-issue laws, or passed them at different dates). All Black and Nagin have shown is that they can fit such a state-specific pattern with a state-specific

Figure 9.15. Fitting a nonlinear trend to individual states

quadratic time trend, and do this well enough that the residuals no longer show a pattern.

3 *Should one expect an immediate and constant effect from right-to-carry laws with the same effect everywhere?*

> While he includes a chapter that contains replies to his critics, unfortunately he doesn't directly respond to the key Black and Nagin finding that formal statistical tests reject his methods. The closest he gets to addressing this point is to acknowledge "the more serious possibility is that some other factor may have caused both the reduction in crime rates and the passage of the law to occur at the same time," but then goes on to say that he has "presented over a thousand [statistical model] specifications" that reveal "an extremely consistent pattern" that right-to-carry laws reduce crime. Another view would be that a thousand versions of a demonstrably invalid analytical approach produce boxes full of invalid results. (Jens Ludwig, "Guns and Numbers," *Washington Monthly,* June 1998, p. 51)[76]

> We applied a number of specification tests suggested by James J. Heckman and V. Joseph Hotz. The results are available from us on request. The specifics of the findings, however, are less important than the overall conclusion that is implied. The results show that commonly the model either overestimates or underestimates the crime rate of adopting states in the years prior to adoption. (Dan Black and Dan Nagin, "Do Right-to-Carry Laws Deter Violent Crime?" *Journal of Legal Studies,* January 1998, p. 218)

Black and Nagin actually spent only a few brief sentences on this issue at the very end of their paper. Nevertheless, I did respond to this general point in the original book. Their test is based upon the claim that I believe "that [right-to-carry] laws have an impact on crime rates that is constant over time."[77] True, when one looks at the simple before-and-after average crime rates, as in the first test presented in table 4.1 and a corresponding table in my original work with Mustard, this was the assumption that was being made.[78] Figure 9.16 illustrates the crime pattern assumed by that test. But I emphasized that looking at the before-and-after averages was not a very good way to test the impact of the right-to-carry laws (e.g., see p. 92), and I presented better, more complicated specifications, and these showed even larger benefits from these laws. Black and Nagin's test confirms the very criticisms that I was making of these initial simplifying assumptions.

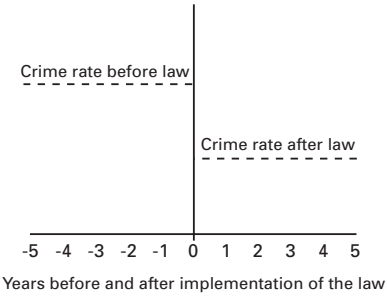


Figure 9.16. What was the crime pattern being assumed in the simple test provided in table 4.1?

Looking at the before-and-after averages merely provides a simplified starting point. If criminals respond to the risk of meeting a potential victim who is carrying a concealed handgun, the deterrent effect of a concealed-handgun law should be related to the number of concealed handguns being carried and that should rise gradually over time. It was precisely because of these concerns that I included a variable for the number of years since the law had been in effect. As consistently demonstrated in figure 1 in my original paper as well as the figures in this book (e.g., pp. 82–83), these estimated time trends confirm that crime rates were rising before the law went into effect and falling afterward, with the effect increasing as more years went by.

As already discussed in the book, I did not expect the impact to be the same across all states, for obviously all states cannot be expected to issue permits at the same rate (see the response to point 3 on pp. 135–36). Indeed, this is one of the reasons why I examined whether the drops in crime rates were greatest in urban, high-population areas.

On this issue David Friedman, a professor at the University of Santa Clara Law School, wrote that "The simplifying assumptions used in one of the regressions reported in the Lott and Mustard paper (Table 3) are not true—something that should be obvious to anyone who has read Lott and Mustard's original article, which included a variety of other regressions designed to deal with the complications assumed away in that one. Black and Nagin simply applied tests of the specification to demonstrate that they were not true."[79] Similar points have also been raised in academic reviews of the book: "Another tactic was to criticize one part of the research by raising issues that Lott actually raised and addressed in another part of the study. Those criticisms that were not uninformed or misleading were generally

irrelevant since taking them into account did not change his empirical results. Nonetheless, they were widely cited by an unquestioning press."[80]

4 *Do right-to-carry laws significantly reduce the robbery rate?*

Q. What's your take on John Lott's study and subsequent book that concludes concealed weapon laws lower the crime rate? (Lott's book is titled "More Guns, Less Crime," University of Chicago Press, 1998.)

A. His basic premise in his study is that these laws encourage private citizens to carry guns and therefore discourage criminal attacks, like homicides and rapes. Think for a second. Most murders and rapes occur in homes. So where would you see the greatest impact if his premise were true? You would see it in armed robbery. But there's no effect on armed robbery. His study is flawed, but it's costing us enormous problems. People are citing it everywhere. (Quote in the St. Paul, Minnesota, newspaper the *Pioneer Planet,* August 3, 1998, from an interview with Bob Walker, president of Handgun Control, Inc.)

Both the preceding quotes and many other criticisms are based on not recognizing that a law can be associated with reduced crime even when the average crime rate in the period after the law is the same as or higher than the average crime rate before the law.[81] For example, look at the four diagrams in figure 9.17. The first two diagrams show dramatic changes in crime rates from the law, but very different before-and-after average crime rates. In the first diagram (17*a*), the average crime rate after the law is lower than the average crime rate before it, while the reverse is true in the second diagram. The second diagram (17*b*) corresponds to an example in which the simple variable measuring the average effect from the law would have falsely indicated that the law actually "increased" the average crime rate, while in actual fact the crime rate was rising right up until the law passed and falling thereafter. If I had another figure where the inverted V shape was perfectly symmetrical, the before-and-after averages would have been the same. (With this in mind, it would be useful to reexamine the earlier estimates for robbery shown in figures 4.8 and 7.4.)

The third diagram (17*c*) illustrates the importance of looking at more than simple before-and-after averages in another way. A simple variable measuring the before-and-after averages would indicate that the average crime rate "fell" after the law was adopted, yet once one graphs out the before-and-after trends it is clear that this average effect is quite misleading—the crime rate was falling until the law went into effect and rising

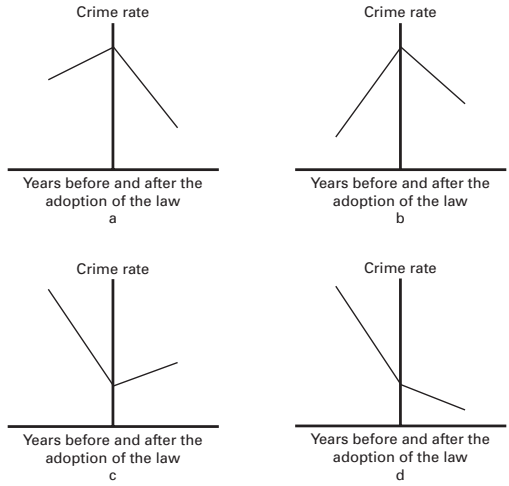


Figure 9.17. Why looking at only the before-and-after average crime rates is so misleading

thereafter. Finally, the fourth diagram (17*d*) shows a case in which the average crime rate is obviously lower after the law than beforehand but the drop is merely a continuation of an existing trend. Indeed, if anything, the rate of decline in crime rates appears to have slowed down after the law. Looking at the simple before-and-after averages provides a very misleading picture of the changing trends in crime rates.

5 *Is the way criminals learn about victims' ability to defend themselves inconsistent with the results?*

Zimring and Hawkins observe that there are two potential transmission mechanisms by which potential criminals respond to the passage of a shall issue law. The first, which they term the announcement effect, changes the conduct of potential criminals because the publicity attendant to the enactment of the law makes them fear the prospect of encountering an armed victim. The second, which they call the crime hazard model, implies that potential criminals will respond to the actual increased risk they face from the increased arming of the citizenry. Lott adheres to the standard economist's view that the latter mechanism is the more important of the two—but he doesn't fully probe its implications. Recidivists and individuals closely tied to criminal enterprises are likely to learn more quickly than non-repeat criminals about the actual probability of encountering a concealed weapon in a particular situation. Therefore, we suspect that

> shall issue laws are more likely to deter recidivists. . . . Thus, if Lott's theory were true, we would also suspect that the proportion of crime committed by recidivists should be decreasing and that crime categories with higher proportions of recidivism—and robbery is likely in this category—should exhibit the highest reductions. Once again, though, the lack of a strong observed effect for robbery raises tensions between the theoretical predictions and Lott's evidence. (Ian Ayres and John J. Donohue III, "Nondiscretionary Concealed Weapons Laws: A Case Study of Statistics, Standards of Proof, and Public Policy," *American Law and Economics Review* 1, nos. 1–2 [Fall 1999]: 458–59)

I have always viewed both the mentioned mechanisms as plausible. Yet the question of emphasis is an empirical issue. Was there a once-and-for-all drop in violent crimes when the law passed? Did the drop in violent crimes increase over time as more people obtained permits? Or was there some combination of these two influences? The data strongly suggest that criminals respond more to the actual increased risk, rather than the announcement per se. Indeed, all the data support this conclusion: table 4.6, the before- and after-law time trends, the county-level permit data for Oregon and Pennsylvania, and the new results focusing on the predicted percentage of the population with permits. The deterrence effect is closely related to the percentage of the population with permits.

I have no problem with Ayres and Donohue's hypothesis that criminals who keep on committing a particular crime will learn the new risks faster than will criminals who only commit crimes occasionally.[82] However, that hypothesis will be difficult to evaluate, for data on the number and types of crimes committed by criminals are known to be notoriously suspect, as they come from surveys of criminals themselves. Some of the criminals appear to be bragging to surveyors and claim many thousands of crimes each year. But one thing is clear from these surveys: criminals often commit many different types of crimes, and hence it is generally incorrect to say that criminals only learn from one type of crime. In any case, even if Ayres and Donohue believe that robbers are more likely to learn from their crimes, the estimated deterrent effect on robbery turns out to be very large when the before-and-after trends are compared.[83]

It is interesting that one set of critiques attacks me for allegedly assuming a once-and-for-all drop in crime from right-to-carry laws (see point 3 above), while at the same time I am attacked for assuming that the drop can be related only to the number of permits issued.

6 *Have prominent "pro-gun" researchers questioned the findings in my book?*

> To dispel the notion that Lott is simply being victimized by the "PC crowd," it may be helpful to mention the reaction of Gary Kleck, a Florida State criminologist known for his generally "pro-gun" views. . . . Kleck argues in his recent book that it is "more likely [that] the declines in crime coinciding with relaxation of carry laws were largely attributable to other factors not controlled in the Lott and Mustard analysis." (Jens Ludwig, "Guns and Numbers," *Washington Monthly,* June 1998, p. 51)

> Even Gary Kleck, a researcher long praised by the NRA and identified as an authority on gun-violence prevention by Lott himself, has dismissed the findings. (Sarah Brady, "Q: Would New Requirements for Gun Buyers Save Lives? Yes: Stop Deadly, Unregulated Sales to Minors, at Gun Shows and on the Internet," *Insight,* June 21, 1999, p. 24)

The quote by Kleck has frequently been mentioned by Jim and Sarah Brady and other members of Handgun Control and the Violence Policy Center.[84] However, it is a rather selective reading of what he wrote. Their claim that Kleck "dismissed the findings" is hard to reconcile with Kleck's comment in the very same piece that my research "represents the most authoritative study" on these issues.[85]

Let me try to explain the meaning of Kleck's quote. I have talked to Gary on several occasions about what additional variables I should control for, but he has been unable to concretely suggest anything; it rather seemed to be more a "feeling" of his that there might be other factors out there. But the issue is more complicated than simply stating that something else should be accounted for: there must exist some left-out factor that just happened to be changing in all the twenty states that had enacted right-to-carry laws for at least a year between 1977 and 1996. Perhaps one can find some left-out national change in some specific year, yet this would not have much of an effect on the regression results.

Gary Kleck has long felt strongly that guns have no net effect on the crime rate. Why he has felt that way has never been clear to me (though I have asked), especially considering his own survey results, which indicate that citizens use guns to stop violent crime about 2.5 million times each year—a large order of magnitude bigger than the reported number of crimes committed with guns.[86] Thus, the couple of sentences that gun-control advocates refer to from what Gary has written about my research did not totally surprise me. Gary told me that he thought it was "quite

amusing" that people from Handgun Control and other gun-control organizations were now starting to cite him as an expert. He also said that he thought that the quotes were being misused, and that he still stood by the blurb for my book—the blurb stating that my research represented "the most extensive, thorough, and sophisticated study we have on the effects of loosening gun control laws."

7 *Are the CBS and Voter News Service polls accurately reflecting how gun ownership rates vary across states?*

> Douglas Weil: But the most important information is that the Voter News Service, which conducted the 1996 poll has said the poll cannot be used in the manner Dr. Lott used it. It cannot be used to say anything about gun ownership in any state, and it cannot be used to compare gun ownership to the earlier 1988 voter poll. ("More Guns, Less Crime? A Debate between John Lott, Author of *More Guns, Less Crime,* and Douglas Weil, Research Director of Handgun Control, Inc.," an on-line debate sponsored by *Time* magazine, transcript from July 1, 1998)

Statistics from the CBS and Voter News Service exit polls (discussed in chapters 3 and 5) were originally "weighted" by these organizations to reflect the share of different racial, sex, and age groups in the national population. For example, white females between thirty and thirty-nine make up 6 percent of the population but may end up accounting for a larger percentage of those surveyed in a poll. If white females in that age group are overrepresented in the calculations made to determine what voters support, the poll will not accurately reflect how voters as a whole will vote in an election. To correct this, polls were adjusted so that different groups are weighted according to their actual shares of either the voting or the general population. It is therefore necessary for the researcher to use a state's demographics to adjust that state's poll results himself, because the shares that different groups make of state populations differ from their shares of the national population. That is precisely what I did.

There were also differences in how the 1988 and 1996 surveys were phrased, and I already discussed those biases right at the beginning of chapter 3. In the notes accompanying that discussion, I mentioned that these biases do not appreciably affect changes in survey results between these two years. The important point is that the changes in how the questions were worded should not alter the relative ranking of states or what types of

people are more likely to own guns. Regressions using data from the two years used variables that account for the average difference across years as well as the average differences across states to account for any biases.

8 *Have I ignored the costs of gun violence?*

> He ignores the huge cost on medical systems that gun violence causes. (Steve Young of the Bell Campaign, an anti-gun group, as quoted in Frank Main, "Economist Says Guns Fight Crime," *Chicago Sun-Times,* July 8, 1999, p. 6)

The costs of crime include medical or other costs of crime, such as lost time from a job or replacement costs for damage and replacement costs for items taken or destroyed. I do not ignore such costs. But unlike my critics, neither do I ignore the crimes that are stopped because people are able to defend themselves. The net effect is what is relevant, and that is directly measured by what happens to the number of crimes. To the extent that people commit crimes with permitted concealed handguns, the number of crimes will rise. To the extent that such handguns deter criminals, the number of crimes will decline. When criminals substitute different types of crimes, the issue then is how the medical and other costs of those different crimes compare. As to the costs of different crimes, I relied on a study produced the National Institute of Justice, rather than produce my own independent numbers.

An interesting contrast to my work is a recent paper published in the *Journal of the American Medical Association* which claimed to show that there were "$2.3 billion in lifetime medical costs for people shot in 1994." Jens Ludwig, one of the authors of the study, argues that "cities such as Chicago could use the study in their lawsuits against the gun industry."[87] But the correct question is not whether guns involve medical costs but whether total medical costs are greater with or without guns. The logic is akin to determining whether police should be allowed to carry guns only by looking at the number of wrongful shootings, and not the times that guns are used to protect officers or deter criminals. Eliminating guns will not eliminate violence and the costs associated with those attacks. Indeed, from a historical perspective, murder rates were higher in England before guns were invented. Medical costs also include costs from suicides and attempted suicides, and the evidence discussed in chapter 5 indicates that suicides will still occur at pretty much the same rate even if guns are not present. For

example, crashing one's car in an attempt to kill oneself can produce substantial medical costs, but even methods like overdosing on sleeping pills or slitting one's wrists with a knife involve medical costs.

9 *What happens to the evidence when Florida and counties with fewer than 100,000 people are removed from the sample?*

> Lott does not respond to Black and Nagin's finding that excluding Florida and small counties (with population less than 100,000) from his samples destroys the statistical significance of all of the violent-crime categories except assault. This suggests that Lott's results are not as robust as he claims. True, Lott's thesis is not embarrassed by varying degrees of deterrence across states (especially since he shows that this variance may be related to the number of permits issued). However, his thesis is shaken by the considerable number of state specific crime categories where concealed-handgun laws are associated with an increase in crime and where the overall significance of his results is undermined by the exclusion of Florida and small counties. (Ian Ayres and John J. Donohue III, "Nondiscretionary Concealed Weapons Laws: A Case Study of Statistics, Standards of Proof, and Public Policy," *American Law and Economics Review* 1, nos. 1–2 [Fall 1999]: 463)

I had clearly dealt with this issue in the first edition of the book. Dropping all counties with fewer than 100,000 people plus Florida reduces the significance in regressions that examine only the average crime rates before and after the law is adopted. Making these changes increases the impact of the law when one examines the before-and-after trends. As the careful reader might guess, the reason that the before-and-after average is not significant for some crimes is that dropping all these observations actually causes the changes to look more like the inverted V that we have so frequently discussed. Picking and choosing which observations to include, which single specification to report, and even which crime categories to report (Black and Nagin do not report the overall violent-crime rates) allows them to knock down the significance of two of the crime categories. (By any standards that I know, a t-statistic of 1.9 for robberies is still statistically significant at better than the 5 percent level, and their coefficient still implies a drop in before-and-after averages of 4.6 percent.) Dropping 87 percent of the sample and reporting only the specifications examining the before-and-after averages may be Black and Nagin's preferred sample and specification, but even these results imply significant benefits and no

cost from passing right-to-carry laws. If they had reported the overall violent-crime rate, they would have shown that overall violent crime fell after the right-to-carry laws were passed.

Table 9.7 uses the updated data to examine the importance of dropping out counties with fewer than 100,000 people as well as Florida. The impact of the law is greater for overall violent-crime rates and aggravated assaults and smaller for the other three violent-crime categories. Each additional year after the law goes into effect produces an additional 3 percent drop in violent-crime rates.

When Black and Nagin break down the differences by individual states, they claim to find three crime categories in which one of the ten states had a statistically significant increase in crime rates (West Virginia for murder, Mississippi for rape, and Pennsylvania for robbery). But their results do not show the variation across states, for they are derived from only a small subset of observations from those states. The West Virginia sample included only one of its fifty-five counties, as it was the only one with more than 100,000 people. The Mississippi data included just three of its eighty-two counties. The results reported earlier in table 4.9 provide the information on how the right-to-carry laws affected the crime rates across states.

10 *Are the results valid only when Maine and Florida are included?*

I will try to summarize the argument here. Ian Ayres and John Donohue are concerned about the inclusion of Maine and Florida for several reasons: (1) the results discussed by Black and Nagin, (2) the issue of whether the crack epidemic might have just happened to cause the relative crime rates to rise in non-right-to-carry states in the late 1980s, and (3) objections to whether Cramer and Kopel were correct in classifying Maine as a right-to-carry state. To satisfy their concerns, Ayres and Donohue use several different approaches, such as dropping both Maine and Florida out of the sample. They also divide the shall-issue dummy variable into two separate variables: a variable to measure the average before-and-after crime rates for those states that adopted their right-to carry laws before December 1987 (Maine and Florida) and a similar variable to measure the average before-and-after crime rates for those states that adopted their crime rates after December 1987.

Ayres and Donohue find that violent-crime rates consistently fall in states adopting right-to-carry laws after 1987, but the effect is often statistically

Table 9.7 What is the impact of removing both counties with fewer than 100,000 people and Florida from the sample?

	Percent change in various crime rates for changes in explanatory variables								
	Violent crime	Murder	Rape	Robbery	Aggravated assault	Property crime	Burglary	Larceny	Auto theft
Change in the crime rate from the difference in the annual change in crime rates in the years before and after the adoption of the right-to-carry law (annual rate of change after the law – annual rate of change before the law)	–3.3%*	–0.45%****	–2.6%**	–3.0%*	–4.7%*	–0.8%	–2.1%**	–0.24%	–1.8%**

*The F-test is significant at the 1 percent level.
**The F-test is significant at the 5 percent level.
****The F-test is significant at the 15 percent level.

insignificant. The drops in violent crime appear much larger and more significant for the earlier states. Indeed, as reported earlier in this book, Maine and Florida experience two of the three largest overall drops in violent crime (see table 4.9). Yet the focus on the before-and-after averages again obscures the benefits from right-to-carry laws.

The results presented in table 9.8 take the two approaches that I have been using: the estimated number of permits issued in a state and the differences between the trends in crime rates before and after the adoption of the right-to-carry laws. With the exception of rape, Maine and Florida experience greater drops in all violent-crime categories, but all the violent-crime rates decline for states adopting right-to-carry laws during the post-1987 period and all but two of these declines are statistically significant at least at the 10 percent level. The estimates using the percentage of the population with permits imply that there were no statistically different effects for the two sets of states for murder and rape.

11 *Was it proper to assume that more permits were issued in the more populous counties after right-to-carry laws were adopted?*

> Since the links between the issuance of permits and the crime reduction that Lott attributes to the shall issue laws is so crucial to establishing causality, more research on this issue is needed. Lott's county population proxies rely on his assumption that population density is a good predictor of the difficulty in obtaining permits under discretionary laws. However, if many states went directly from prohibiting concealed weapons to a nondiscretionary law (like Arizona), Lott's assumed relationship between permits and density would break down. (Ian Ayres and John J. Donohue III, "Nondiscretionary Concealed Weapons Laws: A Case Study of Statistics, Standards of Proof, and Public Policy," *American Law and Economics* Review 1, nos. 1–2 [Fall 1999]: 446)

The original tests shown in figures 4.1 and 4.2 were based upon conversations that I had had with state officials in nondiscretionary states. If the state officials' claims were correct that high-population counties had been much more restrictive in issuing permits than low-population counties, adoption of right-to-carry laws would have seen the biggest issuance of permits in these counties and thus the biggest drops in crime. The results confirmed this prediction. Obviously, this claim depends upon all the states switching from discretionary to nondiscretionary laws, and indeed all the states examined for the tests shown in these earlier figures did make that change.

Table 9.8 Reexamining the claim that states adopting the law before and after December 1987 were differently affected by right-to-carry laws

	Percent change in various crime rates for changes in explanatory variables								
	Violent crime	Murder	Rape	Robbery	Aggravated assault	Property crime	Burglary	Larceny	Auto theft
States adopting law prior to December 1987: one-percentage-point change in the share of the state population with permits to carry concealed handguns	–15.8%[a]	–5.4%[b]	–3.9%[d]	–9.7%[a]	–19.1%[a]	–15.5%[a]	–6.1%[a]	–22.6%[a]	–8.9%[a]
States adopting law after December 1987: one-percentage-point change in the share of the state population with permits to carry concealed handguns	–4.1%[a]	–2.7%[a]	–5.0[a]	–1.0%	–7.8%[a]	4.8%[a]	–2.0%[a]	8.7%[a]	0.34%
States adopting law prior to December 1987: change in the crime rate from the difference in the annual change in crime rates in the years before and after the adoption of the right-to-carry law (annual rate of change after the law – annual rate of change before the law)	–7.2%*	–9.2%*	–0.9%	–7.1%*	–5.7%*	–2.4%*	–1.1%	–4.1%*	–4.1%*
States adopting law after December 1987: change in the crime rate from the difference in the annual change in crime rates in the years before and after the adoption of the right-to-carry law (annual rate of change after the law – annual rate of change before the law)	–1.7%*	–0.7%***	–3.3%*	–2.9%*	–2.4%*	–0.7%*	–2.4%*	0.5%*	–1.4%*

[a]The result is significant at the 1 percent level for a two-tailed *t*-test.
[b]The result is significant at the 5 percent level for a two-tailed *t*-test.
[d]The result is significant at the 12 percent level for a two-tailed *t*-test.
*The F-test is significant at the 1 percent level.
***The F-test is significant at the 10 percent level.

None of the states during 1977–1992 switched from not issuing any permits to nondiscretionary rules. Arizona made its change in late 1994.

The updated results in this chapter have continued to remain conscious of this issue, and I found that the more populous counties in states that changed from discretionary to nondiscretionary laws had bigger relative drops in violent-crime rates than states that changed from banning concealed handguns to nondiscretionary laws.

12 *Did the passage of right-to-carry laws result in more guns being carried in public places?*

> Perhaps by "more guns," Lott means more guns carried in public places. However, surveys indicate that 5–11% of US adults admit to carrying guns, dwarfing the 1% or so of the population that obtained concealed-weapon permits. . . . And if those who got permits were merely legitimating what they were already doing before the new laws, it would mean there was no increase at all in carrying or in actual risks to criminals. One can always speculate that criminals' perceptions of risk outran reality, but that is all this is—a speculation. More likely, the declines in crime coinciding with relaxation of carry laws were largely attributable to other factors not controlled in the Lott and Mustard analysis. (Tim Lambert, "Do More Guns Cause Less Crime?" from his posting on his Web site at the School of Computer Science and Engineering, University of New South Wales [http://www.cse.unsw.EDU.AU/~lambert/guns/lott/])

The survey results mentioned by Lambert refer to all transportation or carrying of guns by Americans. They include not only carrying concealed handguns (whether legally or illegally) but also people who have guns with them to go hunting or who may simply be transporting guns between residences.[88] On the other hand, any survey that focused solely on the illegal carrying of concealed handguns prior to the adoption of the law would find it difficult to get people to admit that they had been violating the law.

The 1 percent figure Lambert picks for carrying concealed handguns is also very misleadingly low. As I have shown in this book, permitting rates depend upon many factors (such as the level of fees and the amount of training required), but they also depend crucially on the number of years that the permitting rules have been in effect. The longer the amount of time that the rules are in effect, the more people who obtain permits. Not everyone who will eventually obtain a permit will apply for it immediately. With the large number of states that have only recently granted permits to people it is misleading to think that the current permit rate tells us the

rate at which people in those states will be carrying concealed handguns even a few years from now.

Given how extremely law abiding these permit holders tend to be, it seems doubtful that most people carrying concealed handguns with permits were illegally carrying concealed handguns before the passage of the right-to-carry law. In many states, illegally carrying a concealed weapon would be the type of violation that would prevent people from ever even getting a permit. There is no evidence that these permit holders have violated this particular law. Yet even if as many as 10 percent of permit holders had previously been illegally carrying a concealed handgun, the coefficients from table 9.3 would still imply that for every 900 additional people with permits there are 0.3 fewer murders and 2.4 fewer rapes.

Finally, while the evidence linking the rate at which permits are issued and the drops in crime rates is important, it is only one portion of the evidence. For example, if there was no change in the number of people carrying concealed handguns, why did violent-crime rates in neighboring counties without the law increase at the same time that they were falling in neighboring counties with the right-to-carry law?

13 *Shouldn't permit holders be required to have the same type of training as police officers?*

> Proponents of [right-to-carry] legislation contend that citizens will be adequately trained to handle firearms responsibly, but this is rarely true. Police departments require officers to go through a great deal of safety and proficiency training before issued a gun—followed by regular refresher courses and qualifications throughout the officer's career. Citizens armed under the provisions of non-discretionary carry laws are not so highly trained, and frequently not trained at all, thereby further increasing the risk of injury and death with a firearm. (From the Web page of Handgun Control, Inc., entitled "Will the Real John Lott Please Stand Up?")

Police officers face a much more difficult job than citizens with concealed handguns. An officer cannot be satisfied if the criminal runs away after he brandishes a gun. Instead, police must act offensively, which is much more dangerous. Citizens are rarely put in situations that require the skill of pursuing an attacker.

There are both costs and benefits to training. Yet the question is ultimately an empirical one. Training requirements improve the deterrence effect for concealed-handgun laws, but the effects are small. What I do find is that longer training periods reduce the number of people obtaining

permits, and the net effect of increased training is clearly to reduce the deterrent effect of adopting right-to-carry laws.

Conclusion

The noise came suddenly from behind early Tuesday—feet rapidly pounding the pavement, voices cursing. Before Jim Shaver could turn around, he was knocked to the ground at East 13th Avenue and Mill Street, fighting off punches from two young men. Police said the assailants figured they'd found a drug dealer to rob, someone who'd have both drugs and money. They couldn't have been more wrong. Their victim was a 49-year-old nurse on his way to work—a nurse with a concealed weapons permit. The fists kept flying, even as Shaver told them—twice, he said—that he had a gun. Fearing for his life, Shaver pulled a .22-caliber revolver out of his coat pocket and fired several shots. One of them hit 19-year-old Damien Alexander Long in the right hip. Long's alleged accomplice, Brandon Heath Durrett, 20, wasn't injured. The pair ran off.[89]

A man who police said kidnapped a 2-year-old child and robbed a disabled elderly woman of a medical monitor was in jail Friday after he was captured and held at gun point by a man with a license to carry a concealed handgun. . . . "I have never pulled a gun on anyone before, and I wouldn't have pulled a gun on this man if he had not run off with that little girl," [the man who stopped the crime] said. "That mother was screaming for her child. She was quite upset."[90]

Awe-struck Phoenix police declared Mr. Vertigan a hero and gave him $500 and a new pistol for catching a cop killer after running out of ammunition in a gunfight with three heavily armed men. Mr. Vertigan . . . came upon three armed Mexican drug-traffickers fatally ambushing a uniformed Phoenix policeman who was patrolling alone in Phoenix's tough Maryvale precinct. Firing 14 shots with his left hand during a slam-and-bump car chase that left the killers' license number imprinted on the front of his own car, Mr. Vertigan emptied his Glock 31 .357 Sig. He wounded the shooter, who was firing at him, and forced the getaway car to crash, slowing the shooter's partners long enough for pursuing police to seize them, as well as a pound of cocaine "eight balls" they were dealing from their white Lincoln. "I always felt that if my life was in danger or anyone around me was in immediate danger I never would hesitate to use that gun. Unfortunately, that day came," Mr. Vertigan said.[91]

A man who tried to commit an armed robbery at a Bensalem convenience store Friday morning was thwarted by a customer who pulled out his own gun and fired five shots at the crook. . . . Fearing he would be killed, police said, the customer began shooting at the suspect. . . . Police said the clerks were "a little shaken up" after the attempted

robbery—but they guessed that the would-be robber was probably just as shocked. "I'll bet he never expected that to happen," said Fred Harran, Bensalem's deputy director of public safety.[92]

All these recent cases involved individuals with permitted concealed handguns. During 1999 concealed permit holders have prevented bank robberies, stopped what could have been a bloody attack by gang members at a teenage girl's high school graduation party, and stopped carjackings.[93] In the couple of months during which I was updating this book, armed citizens have helped capture murderers who had escaped prison; stopped hostage taking at a business, a situation that otherwise surely would have resulted in multiple deaths; and prevented robberies and rapes.[94] Residential attacks that were stopped by citizens with guns during 1999 were extremely common.[95]

One of the bigger puzzles to me has been the news coverage on guns. Admittedly, some of it is easy to explain. Suppose a media outlet has two stories to choose from: one in which there is a dead body on the ground and it is a sympathetic person like a victim, another in which a woman brandishes a gun and the attacker runs away, no shots are fired, no dead bodies are on the ground, and no crime is actually consummated. It seems pretty obvious which story is going to get the news coverage. Yet if we really want to answer the question of which policies will save lives, we must take into consideration not only the newsworthy bad events but also the bad events that never happen because people are able to defend themselves. Unfortunately, the newsworthy bad events give people a warped impression of the costs and benefits from having guns around.

Even when defensive gun uses are mentioned in the press, those mentions do not focus on typical defensive gun uses. The news stories focus primarily on the extremely rare cases in which the attacker is killed, though a few times press stories do mention cases of a gun being used to seriously wound an attacker. News coverage of defensive gun uses in which a would-be victim simply brandished a gun are essentially unheard-of. I don't think one has to rely on a conspiracy explanation to understand why this type of news coverage occurs, for it is not that surprising that dead attackers are considered more newsworthy than prevented attacks in which nobody was harmed. Even so, it is still important to recognize how this coverage can color people's perspective on how guns are used defensively. Since most people probably are very reticent to take a life, if they believe that defensive

gun use almost always results in the death of an attacker, they will become more uncomfortable with guns.

While these examples are easily understood, some other news coverage is not as obvious. Take the case of accidental gun deaths involving young children, which we discussed in chapter 1. My guess is that people believe these events to be much more frequent than they actually are. When I have given talks, I have sometimes asked the audience how many children under age five or ten die from accidental gun shots each year; the answers are frequently in the thousand-plus range. A few answers might mention only hundreds of deaths per year. No one comes close to the Centers for Disease Control numbers: seventeen accidental gun deaths for children under age five and forty-two for children under ten in 1996. The information that forty children under age five drown each year in five-gallon water buckets or that eighty drown in bathtubs always astounds the audience. People remember national news reports of young children dying from accidental handgun shots in the home. In contrast, when was the last time that you heard on the national news of a child drowning in a five-gallon water bucket?[96]

As a father of four boys and one daughter, I can't imagine what life would be like if one of my children died for any reason, including guns. But why so much more attention is given to guns when so many other risks pose a greater threat to our children is not immediately obvious to me. Indeed, it is difficult to think of anything other than guns that is as prevalent around American homes, and that is anywhere near as potentially dangerous, yet is responsible for as low an accidental death rate. With around 80 million people owning a total of 200–240 million guns, the vast majority of gun owners must be extremely careful or such gun accidents would be much more frequent.

I have asked some reporters why they think accidental gun deaths receive so much coverage, and the only answer seems to be that these events get coverage because they are so rare. Dog bites man is simply not newsworthy because it is so common, but man bites dog, well, that is news. Yet this explanation still troubles me, for there are other equally rare deaths involving children that get very little news coverage.

Another puzzle is the lack of coverage given to cases in which citizens with guns have prevented multiple-victim public shootings from occurring. Given the intense concern generated by these attacks, one would think that people would be interested in knowing how these attacks were stopped.

For a simple comparison, take the justified news coverage accorded the heroic actions of Dave Sanders, the Columbine High School teacher who helped protect some of the students and was killed in the process. By the Sunday morning five days after the incident, a Lexis-Nexis search (a type of on-line computer search that includes news media databases) indicates that over 250 of the slightly over 1,000 news stories around the country on this tragedy had mentioned this hero.

Contrast this with other school attacks in which the crimes were stopped well before the police were able to arrive. Take, for example, the October 1997 shooting spree at a high school in Pearl, Mississippi, described at the beginning of this section, which left two students dead. It was stopped by Joel Myrick, an assistant principal. He retrieved his permitted concealed handgun from his car and physically immobilized the shooter for about five minutes before police arrived.

A Lexis-Nexis search indicates that 687 articles appeared in the first month after the attack. Only 19 stories mentioned Myrick in any way. Only a little more than half of these mentioned he used a gun to stop the attack. Some stories simply stated Myrick was "credited by police with helping capture the boy" or that "Myrick disarmed the shooter." A later story reported by Dan Rather on CBS noted that "Myrick eventually subdued the young gunman." Such stories provide no explanation of how Myrick accomplished this feat.

The school-related shooting in Edinboro, Pennsylvania, which left one teacher dead, was stopped only after James Strand, the owner of a nearby restaurant, pointed a shotgun at the shooter when he was finishing reloading his gun. The police did not arrive until eleven minutes later. At least 596 news stories discussed this crime during the next month, yet only 35 mentioned Strand. Once again, the media ignored that a gun was used to stop the crime. The *New York Daily News* explained that Strand "persuaded [the killer] to surrender," while the *Atlanta Journal* wrote how he "chased [the killer] down and held him until police came." Saying that Strand "persuaded" the attacker makes it sound as if Strand were simply an effective speaker.

Neither Myrick nor Strand was killed during their heroics. That might explain why they were ignored to a greater degree than Dave Sanders in the Columbine attack. Yet one suspects a more politically correct explanation—especially when the media generally ignore defensive gun use. With five public-school-related shootings occurring during the 1997–1998 school

year, one might have thought that the fact that two of them were stopped by guns would register in the public debate over such shootings.

The media bias can be amply illustrated by other examples as well. Take the example of the July attack in Atlanta, which left nine people dead. Mark Barton killed people working at two stock brokerages.[97] It did deserve the extensive news coverage that it received. Yet, within the next week and a half, there were three cases around Atlanta in which citizens with guns stopped similar attacks from occurring, and these incidents were given virtually no news coverage. They were an attack at a Lavonia, Georgia, store by a fired worker; an attack by a mental patient at an Atlanta hospital; and an Atlanta truckjacking.[98] The last two incidents were stopped by citizens with permitted concealed handguns. The first was stopped by someone who had only been allowed to buy a gun hours before the attack because of Georgia's instant background check system. Meanwhile, a week after the Atlanta massacre, another attack, which left three people dead at a business in Birmingham, Alabama, again generated national television news coverage on all the networks and was the lead story on the CBS and NBC evening news.[99]

Again, I can see that bad events that never occur are not nearly as newsworthy as actual bad events. Yet multiple-victim attacks using methods other than guns are frequently ignored. On May 3, 1999, Steve Abrams drove his Cadillac into a crowded preschool playground because he "wanted to execute innocent children."[100] Two children died horrible deaths as one was mangled under the wheels and the other pinned to a tree by the car, and another five were badly injured. One woman's son was so badly mauled that "teachers and other parents stepped between [her] and the Cadillac to prevent her from seeing her son's battered body" even though he was still alive. Yet only one television network provided even a passing reference to this attack.[101] One very obvious news angle, it seems to me, would be to link this attack to the various public school attacks. Compare this news coverage with the attention generated by Buford Furrow's August 10, 1999, assault on a Jewish community center, which left five people wounded, three of them young boys.[102] Multiple-victim knife attacks have been ignored by the national media, and few people would realize that there were 1,884 bombing incidents in the United States in 1996, which left a total of 34 people dead and 365 people injured.[103]

The news coverage is also constantly framed as, "Is more gun control the answer?"[104] The question is never asked, "Have increased regulations

encouraged these attacks by making potential victims more vulnerable? Do these attacks demonstrate the importance of letting people be able to defend themselves?"

We are constantly bombarded with pro-gun-control claims. While my research, when it is referred to in the press, is labeled as "controversial" or worse, the claims from the Clinton administration and Handgun Control, Inc., are reported without reference to any academics who might object to them. For years the Clinton administration has been placing public service ads claiming that "thirteen children die every day from guns," linking this claim with elementary school children's voices or pictures. But few of these thirteen deaths fit the image of innocent young children. Nine of these deaths per day involve "children" between seventeen and nineteen years old, primarily homicides involving gang members. Eleven of the deaths per day involved fifteen- to nineteen-year olds. This does not alleviate the sorrow created by these deaths or the 1.9 children under age fifteen that die from guns every day, but it strains credulity to have this number mentioned as evidence justifying the importance of trigger locks.

Much of the debate today is framed so as to blame the greater accessibility of guns in America for the recent school violence. Gun-control groups claim that today "guns are less regulated than toasters or teddy bears."[105] The solutions range from banning gun possession for those under twenty-one to imprisoning adults whose guns are misused by minors under eighteen.

Yet, to the contrary, gun availability has never before been as restricted as it is now. As late as 1967, it was possible for a thirteen-year-old virtually anywhere in the United States to walk into a hardware store and buy a rifle. Relatively few states even had age restrictions for buying handguns from a store. Buying a rifle through the mail was easy. Private transfers of guns to juveniles were also unrestricted.

It was common for schools to have shooting clubs. Even in New York City, virtually every public high school had a shooting club up until 1969. It was common for high school students to take their guns with them to school on the subways in the morning and turn them over to their homeroom teacher or the gym coach so the heavy guns would simply be out of the way. After school, students would pick up their guns when it was time for practice. The federal government would even give students rifles and pay for their ammunition. Students regularly competed in citywide shooting contests, with the winners being awarded university scholarships.

Contrast those days with regulations today. College or elementary students are now expelled from school for even accidentally bringing a water pistol. Schools prohibit images of guns, knives, or other weapons on shirts, on hats, or in pictures. Elementary school students have been suspended for carrying around a mere picture of a gun. High schools have refused to publish yearbook pictures of students sitting on howitzers, even when the picture shows graduating students who are joining the military. School superintendents have lost their jobs for even raising the question of whether someone at a school should have a gun for protection.[106]

Since the 1960s, the growth of federal gun control has been dramatic. Before the Brady law in 1994, background checks and waiting periods were not required in most states. It was not a federal crime for those under eighteen to possess a handgun until 1994. The 1990s saw dramatically higher fees for registered dealers as well as many added paperwork requirements. Federal gun laws in 1930 amounted to only 3,571 words. They expanded to 19,907 words in 1960 and then more than quadrupled to 88,413 words in 1999.[107]

The growth in state laws has kept pace. By 1997, California's gun-control statutes contained an incredible 158,643 words, nearly the length of the King James Version of the New Testament. And in 1999, at least four new gun laws have already been signed into law by the governor. Even a "gun-friendly" state government such as Texas has gun-control provisions containing over 41,000 words. None of this even begins to include the burgeoning local regulations on everything from licensing to mandatory gun locks.

Yet without academic evidence that existing regulations such as gun-free zones, the Brady law, and gun locks produce desirable results, it is surprising that in 2000 we are now debating what new gun-control laws to pass. With that in mind, 294 academics from institutions as diverse as Harvard, Stanford, Northwestern, the University of Pennsylvania, and UCLA released an open letter to Congress during 1999 stating that the proposed new gun laws are "ill advised." They wrote that "With the 20,000 gun laws already on the books, we advise Congress, before enacting yet more new laws, to investigate whether many of the existing laws may have contributed to the problems we currently face."[108]

An effective as well as moving piece I recently read was written by Dale Anema, a father whose son was trapped for hours inside the Columbine High School building during the April 1999 attack. His agony while waiting to hear what happened to his son touches any parent's worst fears. Because

he had witnessed this tragedy, he described his disbelief over the policy debate:

> Two pending gun bills are immediately dropped by the Colorado legislature. One is a proposal to make it easier for law-abiding citizens to carry concealed weapons; the other is a measure to prohibit municipalities from suing gun manufacturers. I wonder: If two crazy hoodlums can walk into a "gun-free" zone full of our kids, and police are totally incapable of defending the children, why would anyone want to make it harder for law-abiding adults to defend themselves and others? . . . Of course, nobody on TV mentions that perhaps gun-free zones are potential magnets to crazed killers.[109]

10 | A Decade Later: Nine More Years of Data and Nine More States

Introduction

Ten years have passed since the second edition of this book. During that time, both the argument and the data have been hotly debated. This debate has often been unpleasant, vociferous, and even disingenuous. To say that my career has suffered as a result is something of an understatement and, alas, an unpleasant warning to other scholars who dare to go against the academic grain. And yet, as this chapter will document, within the scholarly community the research has withstood criticism and remains sound. Further, the additional ten years of data provide continued strong support for the arguments I initially put forward on right-to-carry and other gun-control laws.

I would never have predicted that I would still be working on gun control a decade and a half after I started thinking about the issue. Back in 1993, I had done extensive research on crime, having served as chief economist at the United States Sentencing Commission, but the issues I was interested in were corporate and white-collar crime. I had little interest in the gun-control issue, and I rarely read academic papers on the topic. It is strange how seemingly small decisions can cascade into results that one would never guess. In my case, the impetus came when I was teaching at the Wharton Business School from a couple students asking if I could spend a little

bit of one class talking about gun control and crime. That simple request made me sit down and read many of the existing academic papers. It made me realize how incredibly poorly done the research had been.

Generally, one tries to do work that matters to society, but one is rarely successful. Still, I frequently doubt that the research was personally worth it. The personal attacks and misstatements about my work and other aspects of my life have come fast and furious for years. As Matt Bai, then of *Newsweek*, wrote in 2001, "Lott may be brilliant, but his theories are so controversial that some academics won't so much as look at him when he's standing in a room."[1] One academic paper in 2003 by law professor John Donohue suggested that there could be "blood on Lott and Mustard's hands" because our research may have caused states to adopt right-to-carry laws. Donohue used terms such as "now discredited work," "fraud," and "blight on democracy," and warned I was causing "harm to the democratic process" by "encouraging the adoption of laws on false pretenses."[2] He was still making similar claims in academic seminars five years later.[3]

Others impugned my methodology, insisting that researchers could not replicate my results showing that right-to-carry laws reduce crime. One even alleged that I paid off editors at the University of Chicago Press to publish other research that supported my findings.[4]

Obviously, though, there is much more to this debate than the personal attacks, and we will turn to those empirical issues first. The following sections will revisit the risks and benefits of letting law-abiding people carry concealed handguns. Do bans on concealed handguns make people safer? Do those who carry concealed handguns represent a threat to others? Do they use their guns to commit crime? To look at the impact of right-to-carry laws on crime rates, I will update the data previously examined in this book by nine years—through 2005—to see if it alters the relationship between right-to-carry laws and crime rates.

The more-guns-less-crime thesis will be examined by looking at what happens to crime rates when cities or countries ban guns. The evidence should make gun-control advocates pause, as all the gun bans that I have studied show that murder rates increase after the ban is enacted.

The chapter will then summarize the conclusions that other published studies have reached, and I will discuss the objections raised against my research, including which type of data is best, county- or state-level data. Finally, the impact of other gun-control laws, such as assault weapons bans, gun show regulations, safe-storage rules, and the Castle Doctrine, will be examined. This is the first study to look at the Castle Doctrine, which elimi-

nates the requirement that people in their own home have to retreat as far as possible before defending themselves.

The Continuing Debate

Del. Eleanor Holmes Norton (D-D.C.) and gun control groups are concerned that some visitors attending President-elect Obama's inauguration may try to pack heat because of a rule allowing concealed weapons in national parks.

The Bush administration recently altered federal regulations to allow people with permits to carry concealed firearms while in national parks if the park falls within a state or district that allows concealed weapons.

Washington D.C. does not allow concealed weapons, but Norton and others think confusion over the rule could lead visitors to bring guns to Obama's Jan. 20 inauguration, which will be held on two miles of National Park land.

—*The Hill*, December 27, 2008[5]

On January 9, 2009, the National Park Service was tasked to live by the same rules that the Bureau of Land Management and the U.S. Forest Service and the rest of the nation use [and apply each state's right-to-carry laws to the national parks in that state]. On January 10, 2009, the earth rotated. The sun rose. The Constitution still worked. Law-abiding citizens were still, well, law-abiding. Apparently, we all survived.

—Representative Rob Bishop (R-UT), January 15, 2009[6]

Despite all our experience with permitted concealed handguns, the debate continues. There are always fears about what might happen. Whether it is concealed-handgun laws, assault weapons bans, gun shows, one-gun-a-month rules, banning inexpensive guns, or safe-storage laws, the same issues always reappear. Do guns on balance save lives or cost lives? Will gun-control laws primarily disarm law-abiding citizens or criminals?

In the United States, we have a lot of experience with concealed-handgun permit holders. In 2007, there were about 5 million Americans permitted to carry concealed handguns (see table 10.1). Thirty-nine states have right-to-carry laws and nine have may-issue laws. Only two states, Illinois and Wisconsin, still completely ban people from carrying concealed handguns.[7] That is a big change from just the eight states that had right-to-carry laws in the early 1980s.

The precise number of people legally carrying concealed guns isn't known, because three of the right-to-carry states (Alaska, Montana, and Vermont) do not require permits to carry a concealed handgun in all or

Table 10.1 Permits and percent of adult population with permits by state in 2007

State	Number of permits	Percent of adults with permits
Alabama (estimate)[a]	>281,000	>8%
Alaska (no permit required—permits acquired to carry gun in other states)	9,547	1.91%
Arizona	99,370	2.12%
Arkansas	54,919	2.58%
California	?	
Colorado	28,454	0.78%
Connecticut	133,252	4.94%
Delaware	?	
Florida	445,038	3.13%
Georgia	>300,000	>4.31%
Hawaii	0	0%
Idaho	48,364	4.42%
Illinois	Banned	0%
Indiana	295,643	6.21%
Iowa	28,383	1.25%
Kansas	8,958	0.43%
Kentucky	95,638	2.97%
Louisiana	14,084	0.44%
Maine	?	
Maryland (Feb. 2006)	36,755	0.86%
Massachusetts (fall 2006)[b]	<203,302	<4.04%
Michigan	155,000	2.02%
Minnesota	50,777	1.29%
Mississippi	47,500	2.20%
Missouri	36,105	0.81%
Montana (Dec. 2008); no permit required in 98% of state	17,974	2.44%
Nebraska	2,109	0.16%
Nevada	?	
New Hampshire (nonresidents only; state does not collect county info for residents)	29,609	2.92%
New Jersey (Jan. 13, 2009)[c]	10,821	0.17%
New Mexico	10,566	0.72%
New York City only	2,555	0.02%
North Carolina	95,502	1.39%
North Dakota	8,364	1.68%
Ohio	97,912	1.12%
Oklahoma	57,540	2.12%
Oregon	96,005	3.33%
Pennsylvania	668,372	6.89%
Rhode Island	?	

Table 10.1 (*continued*)

State	Number of permits	Percent of adults with permits
South Carolina	56,715	1.69
South Dakota	41,000	6.87
Tennessee	179,356	3.83
Texas	288,909	1.68
Utah	108,100	5.92
Vermont	No permit required	
Virginia	146,874	2.51
Washington	236,975	4.82
West Virginia	82,000	5.73
Wisconsin	Concealed carry banned	
Wyoming	12,278	3.09
Total	>4,621,625	>2.03

Note: Many of these data were collected by Chris Bird, and I appreciate his sharing this with me. Sources for active concealed-carry permits as of the following dates: Alaska Department of Public Safety as of Aug. 17, 2007; Donna J. Street, administrative supervisor, Arizona Department of Public Safety, Dec. 2007; Arkansas State Police, Aug. 20, 2007; Colorado Sheriffs Association, Dec. 31, 2006; Connecticut State Police, Sept. 6, 2007; Florida Department of Agriculture and Consumer Services, July 31, 2007; Hawaii state senator Sam Slom, Jan. 18, 2009; Sam Knowles, Program Services Bureau, Iowa Department of Public Safety, Dec. 2007; Idaho State Police, Aug. 22, 2007; Kansas Attorney General's Office, Aug. 1, 2007; Kentucky State Police, Aug. 1, 2007; Louisiana State Police, Aug. 28, 2007; for Maryland, http://www.marylandshallissue.org/ccwdata.html; Mississippi Department of Public Safety, Aug. 29, 2007; Missouri Highway Patrol, Aug. 3, 2007; Nebraska State Patrol, Aug. 30, 2007; New Jersey state assemblyman Michael Carroll; New Mexico Department of Public Safety, Aug. 14, 2007; North Carolina Department of Justice, Sept. 12, 2007; Office of North Dakota Attorney General, Bureau of Criminal Investigation, Aug. 30, 2007; Ohio Attorney General's Office, June 30, 2007; Oklahoma State Bureau of Investigation, July 17, 2007; Oregon State Police, Sept. 4, 2007; Pennsylvania State Police, Apr. 27, 2007; South Carolina Law Enforcement Division, Sept. 5, 2007; Tennessee Department of Safety, May 8, 2007; Virginia State Police, Aug. 5, 2007; State of Washington, Department of Licensing, Sept. 5, 2007. Other information: Alabama (Stan Diel, "Metro Zarea Loaded with Concealed Guns Records Show More than 1 in 10 Adults May Have Carry Permits; For Many, 'It's an Insurance Policy,'" *Birmingham News*, Feb. 18, 2007, p. A1; and Christopher Pelton, "Packing Heat, Discretely," *Florence Times Daily*, Aug. 6, 2008), Georgia ("Gun Permit Holders Praise New Georgia Law," WALB News, May 15, 2008 [http://www.walb.com/Global/story.asp?S=8331521&nav=5kZQ]; this information was also confirmed by Ed Stone with GeorgiaCarry.org), Indiana (Lt. Jerry Berkey, Indiana State Police, 317–232–8263), Massachusetts (Bob Hohler, "Many Players Regard Firearm as a Necessity," *Boston Globe*, Nov. 10, 2006 [unable to verify independently]), Michigan (Dawson Bell, "Michigan Sees Fewer Gun Deaths—with More Permits," *Detroit Free Press*, Jan. 6, 2008), Minnesota (http://www.madfi.org/permitcount.asp), Montana (Ted Richardson, customer support technician, Montana Department of Justice; permits as of Dec. 12, 2008), New Hampshire (Rosemary Ruby, counter clerk 4, New Hampshire State Police, Permits and Licenses Unit), New York City (Chris Faherty, "Concealed Pistols Permits Drop in City," *New York Sun*, Aug. 29, 2007), South Dakota (Ben Shouse, "South Dakotans No. 1 in Permits to Conceal Guns," *Argus Leader*, Dec. 17, 2006), Texas (Texas Department of Public Safety, Dec. 31, 2007 [http://www.txdps.state.tx.us/administration/crime_records/chl/PDF/ActLicAndInstr/ActiveLicandInstr2007.pdf]), Utah (Utah Department of Public Safety [http://www.des.utah.gov/bci/brady_statistics.html#]), and West Virginia ("West Virginia Reciprocity Started," *Centre Daily Times* [State College, PA], Aug. 30, 2007).

[a]The estimate for Alabama was obtained from the six counties for which permit information was available. Data were available for six counties. The state rate was obtained assuming that the entire state averaged the lowest rate shown for these counties. One of these counties is Jefferson County, the most densely populated county in the state, where Birmingham is located. Even though urban counties generally tend to have lower rates of permit holding in Jefferson County, 11 percent of adults have permits. If that rate holds for the rest of the state, about 387,000 Alabamans would have permits.

[b]While a class A license to carry firearms is required in order to carry, some licenses are restricted by the issuing police department to prohibit carrying.

[c]9,798 of these were for retired police officers; 1,023 were for private citizens.

virtually all areas in those states. Alaska and Montana grant permits so that their residents can carry concealed handguns in other states, and Montana residents need permits to carry guns only within the city limits of Montana's six largest cities. Vermont doesn't require or grant any permits. In addition, the figures are difficult to determine precisely in states like Alabama, where the permits are issued by the sixty-four local sheriffs' offices and can only be obtained by contacting each office.[8]

Interestingly, no state that has adopted a right-to-carry law has ever rescinded it or even held a state legislative hearing to consider rescinding it.[9] In some states the adoption of right-to-carry laws was controversial, but there seems to be no buyer's remorse. In fact, the only changes that have been made to right-to-carry laws have been to loosen the requirements for getting a permit.

One particularly surprising fact is the high rate at which state legislators seem to have concealed-handgun permits.[10] In South Carolina, 20 percent of the state legislature had permits in early 2008. In Tennessee, 25 percent have permits.[11] Exactly a third of the twenty-four Virginia state legislators from the area around Norfolk, Virginia, have permits.[12]

The debate over concealed-handgun laws has implications for gun-control regulations generally. If permit holders can actually be trusted to carry their guns in public—in restaurants or bars, on buses, at sports stadiums—it is hard to imagine where law-abiding citizens cannot be trusted with guns. The evidence on concealed-handgun laws is also relevant for debates over banning handguns in general—concealed-handgun laws may provide us with the best direct evidence of the costs and benefits of owning handguns.

Even with this level of satisfaction among voters and state legislators, gun-control groups and some liberal academics still strongly advocate more restrictions. The following sections will first examine whether law-abiding concealed-handgun permit holders follow the law and do not pose a threat to others. Then I will present the latest data on how gun-control laws affect crime. I will discuss recent major court cases, including the U.S. Supreme Court 2008 decision to overturn the District of Columbia's handgun ban as well as ongoing legal attempts to overturn the Chicago handgun ban and bans in public housing. As the court cases often turn on the question of whether handgun bans reduce crime, I will discuss this issue at length. I will then turn to answering objections that have been raised against my work and critically discuss some of the recent literature on gun control.

Concealed-Carry Permit Holders: Villains or Saints?

[Obama] differs with McCain and Clinton about whether people should be allowed to carry concealed guns. Clinton and McCain oppose outlawing it.

"I am not in favor of concealed weapons," Obama said. "I think that creates a potential atmosphere where more innocent people could (get shot during) altercations."

—Mike Wereschagin and David M. Brown, "Candidates' Gun Control Positions May Figure in Pa. Vote," *Pittsburgh Tribune-Review*, Apr. 2, 2008

[State Representative Michael] DeBose [a Southside Cleveland Democrat] twice voted against a measure to allow Ohioans to carry concealed weapons. It became law in 2004.

DeBose voted his conscience. He feared that [concealed-handgun] permits would lead to a massive influx of new guns in the streets and a jump in gun violence. He feared that Cleveland would become the O.K. Corral, patrolled by legions of freshly minted permit holders.

"I was wrong," he said Friday.

"I'm going to get a permit and so is my wife.

"I've changed my mind. You need a way to protect yourself and your family."

—Cleveland Plain Dealer, May 15, 2007[13]

The gun-control debate largely focuses on what *might* go wrong, rather than evidence on what actually happens. For example, after 9/11, many were fearful that letting pilots carry guns on planes would endanger passengers' safety. Some worried that a gun being accidentally discharged would lead to an explosive depressurization, causing a plane to crash.[14] Yet Boeing and other airplane manufacturers testified that bullet holes in the airplane's skin would have little effect on cabin pressure and would not cause a crash.[15] Still, the Bush Administration strongly fought against allowing pilots to carry guns.[16]

The debate implies that arming pilots is either something that has not been tried before or that it has been tried and failed. But arming pilots is actually nothing new. Until 1963, American commercial passenger pilots on any flight carrying U.S. mail were *required* to carry handguns.[17] The practice was mandated during the 1920s because the federal government wanted to insure that the U.S. mail would be protected if a plane were forced to land away from an airport. Pilots were still allowed to carry guns as recently as 1987, and the pilots' union for American Airlines and the Airline Pilots Security Alliance claim that up to 10 percent of pilots regularly continued to do so up to that time.[18] Most significantly, there are no recorded instances of any significant gun-related problem arising from a legally armed pilot.[19]

Since 2003, pilots have again been allowed to carry guns on planes, providing they now go through an extensive training and psychological evaluation program.[20] In March 2008, there was one case where a pilot accidentally discharged his gun on a U.S. Airways flight from Denver to Charlotte.[21] The plane experienced no problems and landed safely. Ironically, the accident was caused by federal regulations that require a pilot to put a trigger lock on a loaded gun as the plane was landing.[22] But some academics have used this incident to argue that armed pilots should no longer be countenanced.[23]

Similar fears are expressed over what might happen with guns in school. But many are unaware of the long history of schools permitting concealed handguns. Prior to 1995, when the Federal Safe School Zone Act was passed, many states had allowed concealed-handgun permit holders to carry guns on school property.[24] And even since 1995, Oregon, New Hampshire, and Utah have continued to let permit holders carry guns at school. Many other states adopted limited restrictions, such as allowing a gun only in the school parking lot or when someone is picking up a student.

Yet, over all this time, there has not been a single example of an improper use of a permitted concealed handgun at a school, not even the improper exposure of the gun or an accidental gunshot.[25]

Ignoring this evidence, however, critics continue to argue that permitted concealed handguns make people less safe. Take, for example, the following argument from Brady Campaign president Paul Helmke:

> But the *Sun-Sentinel* found 216 [concealed permit holders] with active warrants, 128 with domestic violence restraining orders, nine people charged with felonies or reckless—or violent reckless demeanors, six red—registered sex offenders, at least one prison inmate, and another 1400 people who had pled guilty or no-contest to felony charges, all had concealed-carry permits in the state of Florida.[26]

Also consider this rather simplified account from John Donohue of our positions on the issue:

> Now, John [Lott] and I have debated on the issue of right to carry laws, laws that say citizens who have not yet been convicted of a felony, or not yet been involuntarily committed to a mental hospital should be allowed to carry a gun wherever they want. John thinks this is a great idea, offers statistics to prove it. Let me just mention that in Texas, even if you have been committed to a mental hospital, if you can get a note from your doctor, they'll give you that concealed handgun permit. North Dakota really

> set a new low. They actually gave one of these permits to a blind permit holder. This is, to be frank, insanity.[27]

These arguments both ignore a crucial question: Did these permit holders actually harm others? I asked Megan O'Matz and John Maines, the reporters who wrote the *South Florida Sun-Sentinel* piece that Helmke refers to, if they had any evidence that any of the individuals they had flagged had committed any crimes while they had a concealed-handgun permit.[28] The newspaper would not allow the reporters to respond directly, but Sally Kestin, their editor, confirmed that there were no examples of these permit holders committing crimes or using their guns improperly while they had permits.[29]

The numbers the *Sun-Sentinel* presented are also somewhat misleading. The 1,400 people referred to who had pleaded guilty or no contest to felony charges had their criminal records expunged by a judge. Presumably, judges only expunge criminal records when they are convinced that there were special circumstances in their cases and that the individuals did not represent a threat to others. Thus, it is not too surprising that these individuals have not committed any crimes. Gun-control groups and these reporters instead claim that a "loophole" allowed those permits to be granted. But the Florida legislature apparently doesn't agree—since the *Sun-Sentinel* article was run in January 28, 2007, no changes have been made to the law. Indeed, no serious discussion even took place in the legislature.

True, some of Florida's then 410,000 permit holders may indeed have been granted permits improperly. But if O'Matz and Maines are correct, those 344 errors amount to merely 0.08 percent of the permits issued. Again, none of those improperly granted permits led to any problems.

As for Donohue's statement, it inaccurately describes the Texas concealed-handgun law.[30] It is simply not true that people are forbidden to obtain a concealed-handgun permit only if they have a felony. It is stricter than that, as one misdemeanor is sufficient for denial. To be eligible for a permit in Texas, a person must not (1) have been convicted of a felony, (2) have been charged with a class A or class B misdemeanor or convicted of such a crime during the previous five years, (3) have been disqualified under federal or state law from purchasing a handgun, (4) have been delinquent in making child support payments or taxes, (5) have been in default on student loans, (6) be chemically dependent, or (7) be under a court protective order or subject to a restraining order. The applicant must also have lived in Texas for at least six months and be at least 21 years old.

The prohibitions on individuals with a history of mental illness are also much more restrictive than Donohue implies. The prohibition applies not only to those involuntarily committed, but also to those who have voluntarily sought psychiatric hospitalization in the preceding two years and to those who have ever been diagnosed "by a licensed physician that the person suffers or has suffered from a psychiatric disorder" related to schizophrenia or delusional disorder, bipolar disorder, chronic dementia, dissociative identity disorder, intermittent explosive disorder, or antisocial personality disorder.

Furthermore, somebody can't simply "get a note from your doctor" to get a permit. The doctor must have his primary practice in psychiatry and must state that the specific "condition is in remission and not reasonably likely to develop at a future time."

Donohue's discussion of the "new low" in North Dakota is also misleading. It is surely easy to ridicule the notion of a legally blind person using a gun for self-defense. Carey McWilliams, the blind permit holder, noted, "A lot of people thought a blind guy with a gun was a funny story. They didn't know the facts or that I've had legitimate training."[31] McWilliams is legally blind, but he has some minimal sight and can differentiate light from darkness. Even with his longtime poor vision, he was able to take target-shooting courses in college. Yet McWilliams only planned for a very limited role in using the gun—"The person would have to actually be attacking me. Then I would put the gun right up against the attacker's body and fire the gun so I wouldn't hit anyone else."

So if McWilliams is denied a gun, how should he defend himself if he's attacked? As McWilliams notes, "It's much easier to attack a blind person than other people."

More important, there have been no problems with McWilliams or with any other legally blind persons with permits, be it in North Dakota or any other state. And there has been no move in the North Dakota legislature to change the law.

We have gone through the numbers before on how law-abiding permit holders are, but given the continued concerns, here are some updated numbers:

ARIZONA. There were 99,370 active permits as of December 1, 2007. During 2007, 33 permits were revoked for any reason—a 0.03 percent rate.[32] There was one case where a permit holder committed murder with a gun in 2002.[33]

FLORIDA. Between, October 1, 1987, and November 30, 2008, Florida issued permits to 1,439,446 people, many of whom have had their permits renewed multiple times. Only 166 had their permits revoked for any type of firearms related violation—about 0.01 percent.[34] These revocations overwhelmingly involved individuals accidentally carrying concealed handguns into restricted areas.

INDIANA. In 2007, there were approximately 300,000 active permits, and 744 were revoked—just under 0.25 percent. According to Indiana State Police Lt. Jerry Berkey, who oversees permit revocations, revocations "rarely" involve the use of a gun. "I have heard of a couple being revoked over the years for improperly discharging their weapons, one involved a player on the Pacers, but I can't think of one involving murder or other violent crime."[35]

KENTUCKY. During 2000, 74 of the 66,000 permits were revoked for any reason—about 0.1 percent. No permit holder was convicted of a violent crime.[36] The most common charge against permit holders, accounting for 20 of the 74 revocations, was a lack of vehicle insurance.

MICHIGAN. During 2007, there were over 155,000 licensed permit holders and 163 revocations—about 0.1 percent.[37] Over the period from July 1, 2001, to June 30, 2007, there was one permit holder convicted of manslaughter, though it did not involve the use of a gun.[38] Three other people were also convicted of "intentionally discharging a firearm at a dwelling." No one was convicted of "intentionally discharging a firearm at or towards another person."

MISSOURI. Ninety-six of the 50,507 permit holders had their permits revoked in 2008—a 0.19 percent rate.[39]

MONTANA. As of December 12, 2008, Montana had 17,974 active permits, and during 2008 there were 20 revocations—0.1 percent. Ted Richardson, who handles the revocation records, commented, "Revocations almost never involve the use of a gun."[40] When asked if any violent crimes were committed by permit holders, Richardson replied, "Not that I have seen."

NEW HAMPSHIRE. Local sheriffs handle permits for New Hampshire residents, so systematic statewide information is only available for nonresident permits. As of December 31, 2007, there were 29,609 active permits held by nonresidents. Rosemary Ruby with the New Hampshire State Police Permits and Licenses Unit said, "The number of revocations is in the range of

2 to 5 per year, never more than five permits." That is a revocation rate of between 0.007 and 0.017 percent per year. She stated that she had "no recollection of revocation for murder or other serious crimes. Simple assault is the worst, nothing even major sticks out."[41]

NORTH CAROLINA. With 246,243 permits issued and 789 revocations, about 0.3 percent of North Carolina permit holders have had their permits revoked over the twelve years from when permits started being issued in December 1, 1995, and December 31, 2008.[42] Local sheriffs revoke the permits and the state only collects the total number of revocations reported to them by the sheriffs. To check the reasons for revocations, I called the sheriff's office for several counties. Tamara Road, an administration support supervisor in Mecklenburg County, the county for Charlotte, reported there were 8,200 permit holders as of December 2008. She said that "no one has had their permit revoked for violent crime. . . . I don't know of any cases where someone has had their permit revoked for improperly using their gun."[43]

Sergeant Bum Gardiner, with the Wake County Sheriff's Office (the county for the state capitol of Raleigh and home of North Carolina State University), has overseen the permit division for seven years and said, "I don't know of one revocation involving the use of a gun. . . . One frequent reason [for revocation] is when the police pull someone over for a traffic violation, [permit holders] fail to tell them that they are a CCW holder." When he was asked why he thought permit holders would forget this, Gardiner said that he couldn't really think of a reason other than they simply forgot they were required to do so. Sheriffs in smaller counties, such as Johnson County, gave similar answers.[44]

OHIO. From April 2004 to the beginning of August 2006, 73,530 permits were issued in Ohio. There were 217 revocations, but 69 of these came from the Cuyahoga County Sheriff's Office after a weapons instructor was accused of not providing the training required by state law.[45] Excluding revocations due to improper training, about 0.2 percent of permit holders had their permits revoked. There were no reported incidents of any permit holder having his permit revoked for committing a violent crime. In 2007, there were 108,386 permits and 171 were revoked—a rate of 0.16 percent. A major reason for revocations was that a licensee moves out of state or dies.[46] The *Cincinnati Post* wrote about the behavior of permit holders, "Toby Hoover of Toledo, director of the Ohio Coalition Against Guns . . . whose group opposes the law, conceded, 'There's been no increase in violence.'"[47]

PENNSYLVANIA. Pennsylvania has more active permits issued than any other state. In April 2007, there were 668,372 permits. The state also had by far the most permit revocations that year: 2,318, translating into a rate of 0.345 percent.[48] However, this was an unusually high year, with the revocation rates during 2003 to 2005 running from 0.23 percent to 0.26 percent.[49]

Interestingly, while Philadelphia accounted for only about 4 percent of the permits issued in the state during 2007, it accounted for 32 percent (744) of all revocations. Philadelphia is the only county in the state that regularly revokes permits for such trivial reasons as parking tickets, notifications to the sheriff's office by the permit holder that he is moving to another address in the city, failure to pay child support, and a host of other, similar reasons.[50] Going to court can usually overturn these revocations, but the process is costly and time-consuming. The easier alternative is to wait one year and then reapply for a new permit.

Still Philadelphia's share of revocations in the state has gone down somewhat over time. In 2000, 40 percent of all revocations in Pennsylvania had taken place in Philadelphia.[51] As noted, by 2007 that share had dropped to 32 percent.

Excluding Philadelphia from the calculation, the revocation rate in 2007 for any reason was 0.25 percent (for the years 2003–2005 the number ranges from 0.15 percent to 0.17 percent). Over half these revocations result from "Protection from Abuse" orders, the vast majority apparently filed in divorce proceedings.[52] Kim Stolfer, chairman of the Pennsylvania-based Firearm Owners Against Crime, states that "a large number of these filings are to obtain concessions in divorce proceedings."[53]

TEXAS. In 2006, there were 258,162 active permit holders. Out of these, 140 were convicted of either a misdemeanor or a felony, a rate of 0.05 percent. That is about one-seventh the conviction rate in the general adult population, and the convictions among permit holders tend to be for much less serious offenses.[54] The most frequent type of revocation, with 33 cases, involved carrying a weapon without their license with them. The next largest category involved domestic violence, with 23 cases.

Similar numbers have been reported in Texas every year. Over the preceding four years, from 2002 to 2005, the average rate at which permit holders were convicted of a misdemeanor or a felony was 0.04 percent.[55]

UTAH. With 134,398 active concealed-handgun permits as of December 1, 2008, there were 12 revocations for any type of violent crime over the pre-

ceding twelve months—a 0.009 percent rate. None of those involved any use of a gun. Thirteen revocations involved any type of firearms-related offense, a revocation rate of less than 0.01 percent. Clark Aposhian, chairman of the State of Utah Concealed Weapon Hearing/Licensing Board, said, "Typically if they just list it as a firearms offense, it is just a more minor offense like carrying a gun into a secure area. The Salt Lake police at the airport handled some of those. If it was intentional, it would have been handled by Homeland Security." In total, 0.22 percent of permit holders had their permits revoked for any reason, and by far the most common reason for revocations involved "alcohol violations."[56]

Since 1994, two permit holders have been convicted of murder, including a police officer who shot his wife.[57] The other murder was not committed with a gun.

WYOMING. Over the four years from 2005 to 2008, 31 permits were revoked. The average yearly revocation rate was 0.06 percent. When asked about the reasons for the revocations, Chris Lynch, a records analyst with the Wyoming Division of Criminal Investigations, noted, "The biggest one that I remember is an individual whose permit was revoked for tax evasion."[58] None of the cases involved a violent crime or the improper use of a gun.

The information that I have collected for other states is more anecdotal and frequently required contacting individual county sheriffs' offices,[59] but the general impression is that their permit holders behave similarly to the permit holders in the states listed above.

With about 5 million people with concealed-handgun permits in the United States in 2007, it is hard to look at these revocation rates and not realize how incredibly law abiding permit holders really are. While all states don't provide detailed records of the reasons that permit are revoked, when a licensee kills someone, it gets extensive news coverage. From 1990 through July 2008, I found twenty-three cases where a permit holder committed murder with a gun (twenty of those cases resulted in convictions, and in the other three murderers died at the scene). Seven permit holders committed murder in Texas; three in Ohio; two in Florida, Pennsylvania, and Utah; and one permit holder committed a murder with a gun in each of the following states: Arizona, Alabama, Indiana, Maine, North Carolina, Tennessee, and Virginia.[60]

Beyond these twenty-three murder cases, one permit holder was convicted of negligent homicide (a misdemeanor), and two cases resulted in con-

victions but no prison time served. These three cases are interesting, showing how even obviously defensive gun uses can result in criminal charges:

Clay Wallace (February 1999)—Wallace, 75 years old, and his friend, Robert Qualls, 65, got into a fight in Black Oak, Arkansas.[61] Qualls knocked Wallace down twice,[62] punching and kicking him.[63] When Wallace was on the ground the second time he pulled out his revolver and shot Qualls once in the chest.[64] Jurors were initially deadlocked on the case, but the judge ordered them back to deliberations, and they came back with a guilty verdict on the misdemeanor charge of negligent homicide.[65] Wallace was sentenced to one year in county jail.

Kenneth Ray Gumm (September 2007)—Gumm, a 67-year-old retired certified security guard with law enforcement education and training, was attacked by Dale Turney, 47, who had a blood alcohol level of .08 and had taken methamphetamines.[66] Turney was angry because he felt that Gumm had cut in front of him in traffic. Turney followed Gumm into a parking lot, where Turney's car blocked Gumm's, preventing him from driving. Turney screamed at Gumm, saying, "You're history," and he chased Gumm around his car. Gumm has "health problems [and] couldn't run away or fight him" and "'backed away from Turney approximately two times around his (Gumm's) car before Turney pushed his shoulder." Claiming, "I thought my life was going to end," Gumm then shot Turney once in his chest. The prosecutor argued that Gumm "was not justified, in the circumstances that existed, to use deadly force in defense of a misdemeanor assault and battery."[67] Convicted of manslaughter, Gumm received a suspended sentence and was placed on probation. No prison time was served and no fine imposed.

Esteban Garza (April 2006)—While eating breakfast at a cafe with his brother-in-law, Garza, 50, argued with others at another table.[68] Fernando Gutierrez, 43, attacked Garza, pushing him to the ground and then shoving him through the glass in the front door.[69] Garza then fatally shot Gutierrez and wounded two bystanders (though no additional charges were ever prosecuted). The prosecutor argued that the response against Gutierrez was excessive. Garza was convicted of second-degree murder and received ten years probation. No prison time was served.[70]

The courts' convictions of Gumm and Garza but refusal to sentence them to jail perhaps indicates the courts' desire not to punish either man seriously, while also discouraging such ready use of a weapon.

The Brady Campaign and other gun-control organizations cherry-pick a few cases, while omitting important facts, such as whether the permit holder was found to have used justifiable force. For instance, in its account of a Texas case from 1997, the Brady Campaign makes it appear as if the permit holder, Pete Kanakidis, shot an innocent bystander named Arroyo: "Arroyo was not involved in the argument and was sitting alone in the driver's seat of a truck."[71]

The Associated Press provided some important details that the jury apparently thought were important about Arroyo's not being a passive bystander:[72]

> Mr. Arroyo drove two other men to Mr. Kanakidis' repair shop in River Oaks on May 30. Mr. Kanakidis had fired one of the men and reportedly planned to fire the other man that day.
>
> Police said the two men beat up Mr. Kanakidis and ran from the shop. Mr. Kanakidis then got his gun from his truck and shot Mr. Arroyo, who was backing up his vehicle. Mr. Kanakidis told police he thought Mr. Arroyo was going to run over him.

The *San Antonio Express-News* described how "[Kanakidis] said his nose was broken, he was knocked to the ground and was choked as the men repeatedly attacked him."[73]

Or take the case of Harold Glover in Tulsa, Oklahoma, in 1997. While the Brady Campaign notes that the person who was shot, Cecil Herndon, had a pocketknife, he is described as just "standing outside the vehicle" and "not acting in a 'life-threatening' manner," and Glover is said to have "acted without cause."[74] Left out of the discussion is that Glover was acquitted, that Herndon had "verbally threatened to cut him with a knife," and that Glover was cleared of acting improperly.[75]

In yet another example, in Austin, Texas, a taxi driver named Wayne Lambert shot two men. The Brady Campaign version of the story fails to note that Lambert suffered a gash over his left eye that required eighteen stitches and that he claimed he fired in self-defense.[76] There was no jury verdict, because Lambert died before trial.

Possibly most telling is that the Brady Campaign and the Violence Policy Center keep track of arrests of permit holders, not convictions—ignoring that defensive gun uses frequently result in arrests simply because a police officer can't be sure what happened.[77] But accurate accounts of these cases are important, because they really demonstrate the exact opposite point that the Brady Campaign is making—that permitted concealed handguns, in fact, help to protect people from getting killed when attacked.

Overall, from 1990 through July 2008, the twenty cases where someone has been convicted of murder with a gun and the three other cases where the killers died at the scene amount to slightly more than one murder per year. Permit holders committed murder at 1/182nd the rate of the general public.[78] This is an amazing difference.

Comparing the states provides some interesting lessons. The variation in state revocation rates is primarily related to the possible reasons that one can have his permit revoked, rather than differences in the rates at which permit holders commit violent crime. Murder and violent crime rates by permit holders are essentially zero whether there are many or a few reasons for screening if someone can get a permit. For example, no permit holder has killed a spouse during divorce proceedings, thus indicating that even if the rules in Pennsylvania could be extended to other states, it clearly wouldn't reduce the number of spousal murders—the number is already zero.

Since permit holders are so very law-abiding and so rarely use their guns to cause harm, it doesn't take many examples of defensive gun use by permit holders to imply there are net benefits. A quick review of news stories over just the month from December 14, 2008, to January 11, 2009, yields articles on ten cases where permitted concealed-handgun holders stopped violent crimes. Here are some of the cases:[79]

> Hammond, Indiana (January 11, 2009)—"An attempted robbery early Sunday morning turned fatal for one of the suspects, police said. A 38-year-old man and his girlfriend were exiting their sport-utility vehicle in the parking lot of McTavern's bar in the 7400 block of Indianapolis Boulevard when two people attempted to rob the couple, according to a Hammond police news release."[80] "Hammond police believe a man who shot and killed a would-be robber outside of a bar there early Sunday legitimately acted in self defense. . . . 'We do believe that his version of the story is true and credible,' Miller said, adding that the man had a valid license to carry the weapon and purchased it legally."[81]

> Orlando, Florida (January 9, 2009)—"Orange County sheriff's deputies said two men with a sawed-off shotgun tried to rob a man at a car wash on Orange Blossom Trail Friday night."[82] "'During this attempted robbery, the victim, who holds a concealed weapons permit, pulled out his weapon and fired shots into the bad guy,' said Orange County Sheriff's Office Commander Paul Hopkins. . . . For the bad guys out there, you never know who you're dealing with,' Hopkins said. 'When you go out to commit this crime, you might be the one who's lying dead in the parking lot.'"[83]

> Ocoee, Florida (January 5, 2009)—"A convenience store customer in Ocoee turned the tables on suspected robbers Monday night, and authorities continued to search for two people Tuesday evening. . . . The customer . . . grabbed his gun and went back inside and saw the suspected robber beating a female clerk with a beer bottle. The customer said he told the assailant he had a gun, but the man turned toward him and the customer fired two shots, killing the suspect. . . . The customer was asked whether he had a permit to carry a concealed weapon, and he responded that he did."[84]
>
> West Park, Florida (December 26, 2008)—"An armed robber who held a West Park man at gunpoint outside his home early this morning was killed when his victim pulled out a gun and fired first, the Broward County Sheriff's Office said. Brian Kelley, 22, was returning to his apartment in the 4100 block of Southwest 19th Street about 4:39 a.m. when he said Kenneth Nelson, 42, came up from behind him and put a gun to his head, according to sheriff's office spokesman Mike Jachles. Kelley pulled out his own handgun and fired, striking and killing Nelson, of Hollywood. Homicides detectives questioned Kelley after the shooting but did not take him into custody. Kelley acted in self-defense, investigators believe."[85]
>
> Terrel, Texas (December 17, 2008)—"A man walking his dog in Terrell on Wednesday fatally shot an armed 17-year-old robber, police said. The incident happened at about 10:20 p.m. . . . Police said, a group of teen robbers surrounded the man. According to investigators, Markee Lamar Johnson pulled out a gun and the 47-year-old man, a licensed concealed handgun owner, fired shots. . . . 'It would be a clear example of someone exercising their rights to protect themselves under the law,' Capt. A. D. Sanson said."[86]
>
> Fort Smith, Arkansas (December 14, 2008)—"She's a woman who knows how to protect herself as two men who tried to rob her found out. What they didn't know was the woman is licensed to carry a concealed weapon . . . and yes, she was packing heat. . . . [Police said] 'When she pulled over to check her tires one of those persons in that other car got out and attempted to rob her at knife point. She pointed that [handgun] at her attacker and he backed away, got in the car and they fled.'"[87]

Obviously, on top of this should be included the crimes that never occured because would-be attackers decided it was too risky to attack in the first place.

So What Has Happened over the Last Decade?

Six years after new rules made it much easier to get a license to carry concealed weapons, the number of Michiganders legally packing heat has increased more than six-fold.

But dire predictions about increased violence and bloodshed have largely gone unfulfilled, according to law enforcement officials and, to the extent they can be measured, crime statistics.

The incidence of violent crime in Michigan in the six years since the law went into effect has been, on average, below the rate of the previous six years. The overall incidence of death from firearms, including suicide and accidents, also has declined.

More than 155,000 Michiganders—about one in every 65—are now authorized to carry loaded guns as they go about their everyday affairs, according to Michigan State Police records.

About 25,000 people had CCW permits in Michigan before the law changed in 2001.

—"Michigan Sees Fewer Gun Deaths—with More Permits," *Detroit Free Press*, January 6, 2008

The first edition of this book studied data up to 1994. The second edition expanded the data up to 1996. Since then a lot has changed. Kentucky, Louisiana, and South Carolina's right-to-carry laws were in effect for their first full year in 1997. There was a four-year hiatus before Michigan adopted its right-to-carry law in 2001, followed by Colorado, Minnesota, and Missouri in 2003 and New Mexico and Ohio in 2004. I have now extended the county- and state-level data up through 2005.[88] Kansas and Nebraska adopted right-to-carry laws the next year. Twenty-nine states that have now adopted right-to-carry laws did so at some point during the twenty-nine-year period I have data for, from 1977 to 2005. On average, states had their laws in effect for just over ten years, with 86 percent of the states having had the laws in effect for at least nine years.

This update will focus heavily on state-level data where the change in crime rates is broken down in yearly intervals. There are several reasons for this. Regardless of which data set is used, the results are similar for both county- and state-level data. Results from the state-level data show smaller estimated benefits from right-to-carry laws. They should not be seen as "the" estimate but rather as a lower bound on the benefits. Nevertheless, as we will see, these estimated benefits are still very large. Some of my critics (e.g., criminologists Michael D. Maltz and Joseph Targonski as well as economists Ian Ayres and John Donohue) insist that they are much more likely to accept state-level than county-level results. Although I disagree with their claims, I will present the data that they believe are best.

It's necessary to look at the year-by-year changes in the crime rates before and after the adoption of right-to-carry laws, especially due to the way some summary statistics have been misused by critics. The bottom line is this: if you see a pattern using trends that you don't observe with the year-by-year breakdowns, it means that there is something wrong with your use of trends. Trends are supposed to help summarize the changes in crime, but if they don't do a good job summarizing more complex relationships, there is a problem. Just as I explained earlier how simple before-and-after averages can hide what is going on with changes in crime rates, some set-ups with a before-and-after average and even a before-and-after trend can also be misleading. We will get deeper into this statistical issue later, but I want to provide some explanation up front, because presenting these more disaggregated changes before and after the right-to-carry laws will take up a lot more space.

One other note should be made before proceeding. Just as was done in the previous chapters, the estimates are going to account for not only all the law enforcement variables (arrest, execution, and imprisonment rates), income and poverty measures (poverty and unemployment rates, per capita real income, as well as income maintenance, retirement, and unemployment payments), the thirty-six measures of demographic changes, and the national average changes in crime rates from year to year and average differences across states (the fixed year and state effects).[89] In addition, the estimates account for the differences in various concealed-handgun laws and other types of gun-control laws. The law enforcement, income and poverty measures, and demographics are exactly the same as described in previous chapters.

Other gun laws besides right-to-carry laws might also affect crime, and the estimates therefore take into account one-gun-a-month regulations, assault weapons bans (whether there are state bans when the federal ban is not enforced), background checks on the private transfer of guns (essentially "closing" the so-called gun show loophole), the Castle Doctrine (which absolves people of having to retreat when they are being threatened with deadly force), one-gun-a-month rules, and bans on relatively inexpensive guns (so-called Saturday night specials).

These gun laws may be important for explaining changes in crime rates. But, perhaps more important, these other gun-control laws appear likely to be hot topics in the near future. Shortly after the November 2008 election, Barack Obama's transition Web site noted that Obama and Joe Biden "support closing the gun show loophole and making guns in this country

childproof. They also support making the federal Assault Weapons Ban that expired in 2004 permanent."[90]

Due to the big differences between concealed-carry laws in different states, as well as differences in the percentages actually getting permits, I also account for the different permit requirements in the estimates. They include the state's permit fee, the number of hours of training required to get the permit, how long the permit lasts, and the minimum age requirement.

Some examples of differences across states help illustrate the need to control for these variables. For example, today in Alaska there are no longer any fees or training requirements (see table 10.2). Yet, in the beginning of this decade Alaska charged fees of over $100 and training took sixteen hours. In 2005, Texas had fees well over $140 with a ten-hour training requirement, but Pennsylvania had a $19 fee and no training requirement. Given those differences, it is not too surprising that Pennsylvania had issued more than twice as many permits as Texas despite a population half the size. And Indiana, with about a quarter of Texas's adult population, has virtually the same rules as Pennsylvania and also has more permit holders than Texas.

As we demonstrated in chapter 9, the rules to get permits do matter. One important pattern stands out. As table 10.3 shows, the states that have passed concealed-handgun permits most recently have made it much more difficult to obtain a permit, and therefore should issue fewer permits overall than states that passed the laws earlier. Later-adopting states had, on average, higher fees for getting a permit (even adjusted for inflation), longer training hours, and older minimum required ages. From the 1980s on, the late-adopting states also have permits with shorter durations.

Further, the most recent right-to-carry states have tended to impose more restrictions on where concealed handguns can be carried. For example, later states more frequently prohibit concealed handguns in restaurants or schools or at amusement parks. Some states allow businesses to prohibit customers from carrying guns on their premises. Such prohibitions make carrying a gun very inconvenient and should further reduce the rate at which people get permits.[91] In sum, the more "reluctant" states have not only waited longer to allow concealed carry, but have also, when finally passing the law, imposed more restrictions.

Permit holders are often still prohibited from carrying a gun in many public places. In Kansas, for example, prosecutors are forbidden to carry their concealed handguns in court. This has further repercussions. If they

Table 10.2 Criteria for concealed-handgun permits in right-to-carry states in 2005

State	Last major change	Permit duration (years)	Training length (hours)	Age requirement	Initial permit fee	Renewal fee	Issuing agency
Alabama	1936	1	0	18	15	15	Sheriff
Alaska	2003	None	None	None	None	None	None
Arizona	2004	5	8	21	60	60	Dept. of Public Safety
Arkansas	1995	4	5	21	144.25	50	State Police
Colorado	2003	5	8	21	52.5	25	Sheriff
Connecticut	1986	5	5	21	35	35	State Police
Florida	1995	5	2	21	117	65	Dept. of State
Georgia	1996	5	0	21	50	50	Judge of probate court
Idaho	1996	4	7	21	56	48	Sheriff
Indiana	1980	4	0	18	10	10	Chief of police or sheriff
Kentucky	1996	5	8	21	60	60	Sheriff
Louisiana	1996	4	9	21	100	100	Dept. of Public Safety
Maine	1985	4	5	18	35	20	Chief of police or sheriff
Michigan	2000	5	8	21	105	105	State police
Minnesota	2003	5	6	21	100	75	Sheriff
Mississippi	1990	4	0	21	124	74	Dept. of Public Safety
Missouri	2003	3	8	23	100	50	Sheriff

Montana	1991	4	4	18	55	25	Sheriff
Nevada	1995	5	8	21	50	25	Sheriff
New Hampshire	1923	4	0	21	10	10	Chief of police
New Mexico	2004	0	15	18	100	100	Dept. of Public Safety
North Carolina	2002	5	8	21	90	75	Sheriff
North Dakota	1985	3	0	18	25	25	Bureau of Criminal Investigation
Ohio	2004	5	12	21	69	55	Sheriff
Oklahoma	1995	5	8	23	125	85	State Bureau of Criminal Investigation
Oregon	1993	4	5	21	65	50	Sheriff
Pennsylvania	1995	5	0	21	19	19	Chief of police or sheriff
South Carolina	1996	4	6	21	55	55	State Law Enforcement Division
South Dakota	1986	4	0	18	10	10	Chief of police or sheriff
Tennessee	1996	4	9	21	115	50	Dept. of Public Safety
Texas	1995	4	10	21	140	140	Dept. of Public Safety
Utah	1995	5	4	21	64	10	Dept. of Public Safety
Vermont	None	None	None	None	None	None	None
Virginia	1995	5	5	21	50	50	Clerk of circuit court
Washington	1995	5	0	21	60	32	Judge, chief of police, or sheriff
West Virginia	1996	5	5	18	90	90	Sheriff
Wyoming	1994	5	5	18	50	50	Attorney general

Table 10.3 Criteria for permits based on when the right-to-carry laws went into effect

First full year that the state's right-to-carry law went into effect	Average permit fee (real 2007 dollars)	Permit duration (years)[a]	Average training time to qualify for a permit (hours)	Average age at which one can get a permit[b]
Prior to 1977	$21.90	3.71	0.7	19.3
1980s	$71.49	4.67	4.0	20.0
1990s	$80.27	4.3	5.45	20.5
2000s	$93.16	4.17	9.5	20.8

Note: Values are for 2005.
[a]Excluding Vermont, which had no permits.
[b]Excluding Vermont, which had no age requirements.

can't take a gun into court, it prevents them from carrying a gun when they enter or leave the building. One prosecutor explained the problem this way:[92]

> [Wyandotte County District Attorney Jerome] Gorman, who has received several written death threats over the years, said he works in two courthouse buildings and he and his staff often walk between them.
>
> "We're out in exposed areas, not behind a locked door," he said.
>
> He recalled that several years ago, he was standing in line at a mall with his wife and daughter waiting to see Santa Claus when he spotted the family of a man he convicted of involuntary manslaughter. When Gorman saw them talking among themselves, he left with his family.
>
> "It was time to get away, but I could have been in a position where they saw me before I saw them," he said.

These restrictions appear to have a large effect on permit-issuing rates. In 2008, applications for permits in Georgia soared by 79 percent, a much larger increase than seen in other states after Obama's election.[93] One of the most prominent reasons given for the increase was "a law change last year that opened up the places where a concealed weapon is allowed."

The fees, training requirements, permit duration, and number of prohibited places all have important implications for studying right-to-carry laws. If criminals are indeed discouraged by a higher probability of a potential victim being able to defend herself, then states that only recently adopted right-to-carry laws should have fewer permit holders and a smaller reduction in crime rates.

A problem of growing inconvenience for future research involves the increasing issuance of nonresidential permits. Two states in particular (Florida and Utah) are giving out a large number of permits to out-of-state residents.

Utah's permits for out-of-state residents are honored in twenty-eight states, while Florida's are honored in twenty-seven states. The changes have been particularly significant during the last few years, and over 100,000 permits in these two states are now held by out-of-state residents.[94] Over time, permits issued by Florida and Utah will have a great impact on researchers knowing how may people in other states possess concealed-handgun permits.

The Results

My primary approach here is to let the estimated changes in crime rates "take their own shape" rather than assume that they fit a straight line or some other particular shape.

As a first pass, the regressions account for all the possible influences and consider the year-by-year effects by two-year intervals, from nine years before the right-to-carry law is passed until fourteen years after the law.[95] Two other variables measure the crime rates ten or more years before the law goes into effect and fifteen or more years afterward. Thus, rather than a before-and-after trend and possibly also a variable to measure any changes in the before-and-after averages, this approach has fourteen variables to measure more precisely how crime rates are changing before and after right-to-carry laws are adopted. Only for 2.6 percent of the sample do states that have adopted a right-to-carry law have their laws in effect for at least fifteen or more complete years. Any changes in crime rates for these right-to-carry states are relative to the changes in crime rates for the states without these laws.

Figures 10.1a–10.1i show some dramatic results. There are large drops in overall violent crime, murder, rape, and aggravated assault that begin right after the right-to-carry laws have gone into effect. In all those crime categories, the crime rates consistently stay much lower than they were before the law.

The murder rate for these right-to-carry states fell consistently every year relative to non-right-to-carry states. When the laws were passed, the average murder rate in right-to-carry states was 6.3 per 100,000 people. By the first and second full years of the law it had fallen to 5.9. And by nine to ten years after the law, it had declined to 5.2. That averages to about a 1.7 percent drop in murder rates per year for ten years. The drops were statistically significant by years 5 and 6.[96] The detailed estimates are shown in appendix 6.

Overall violent crime rates also dropped. On average, states with right-to-carry laws start out at 475 crimes per 100,000 people, then fall to to 436

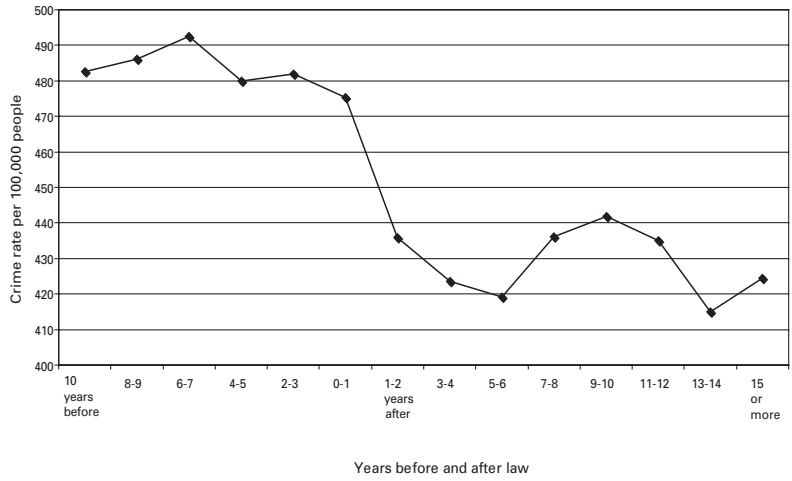


Figure 10.1a. Violent-crime rates before and after adoption of right-to-carry laws

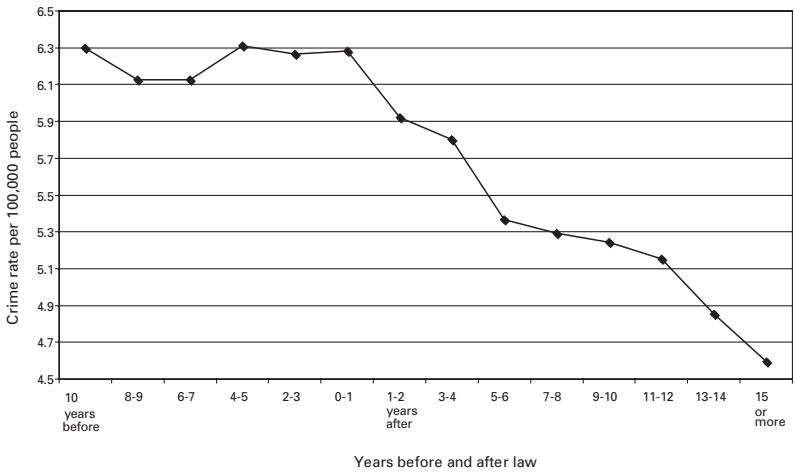


Figure 10.1b. Murder rates before and after adoption of right-to-carry laws

by the first and second full years of the law, and stay down in the 415–40 range after that.[97] A similar pattern holds for rape and aggravated assault. Rape was on average at 40.2 per 100,000 people when the law was passed and ended up at 35.7 (a 12 percent drop) by nine to ten years later.[98]

The story is somewhat more complicated for aggravated assault. While there are large, statistically significant drops in crime as soon as one to two years after the law, assault rates were already declining in the years before the law. If that trend were to have continued in the absence of a

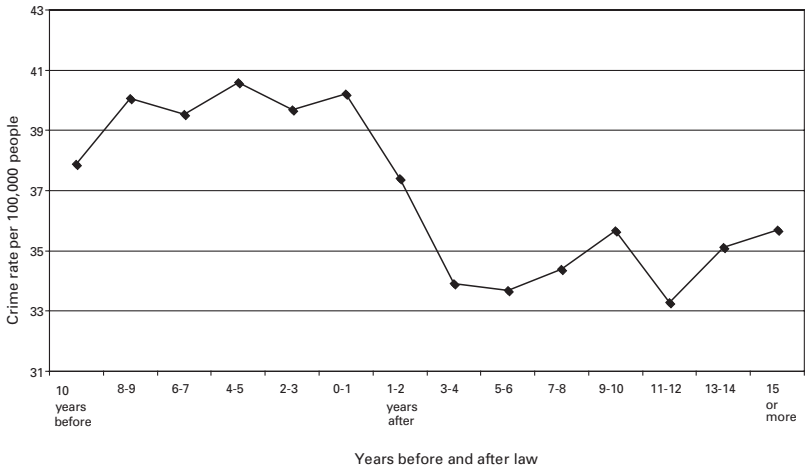


Figure 10.1c. Rape rates before and after adoption of right-to-carry laws

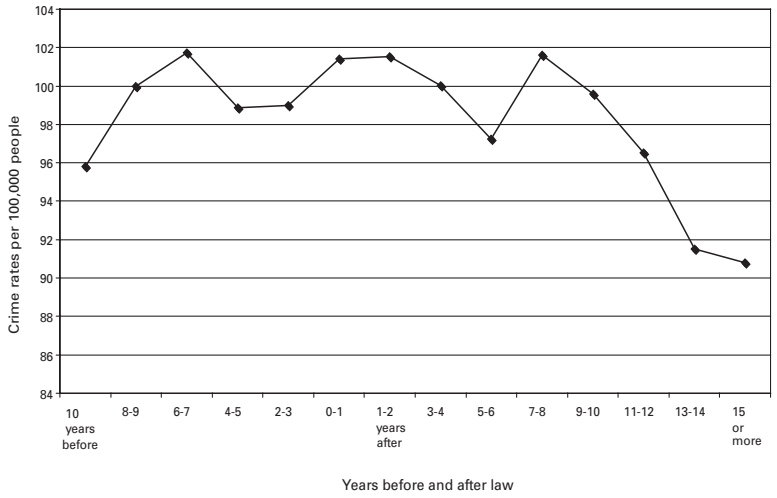


Figure 10.1d. Robbery rates before and after adoption of right-to-carry laws

right-to-carry law, the drop would have finally caught up with the crime rates that we observe by years 11 to 12 after the law. Still, even under that assumption, a right-to-carry law appears to have lowered the number of assaults for ten years.

For robbery, the drop isn't as obvious or as immediate. There are two sets of years in the figure after the law is passed where the robbery rate was slightly higher than it was immediately before the law (years 1 and 2, and

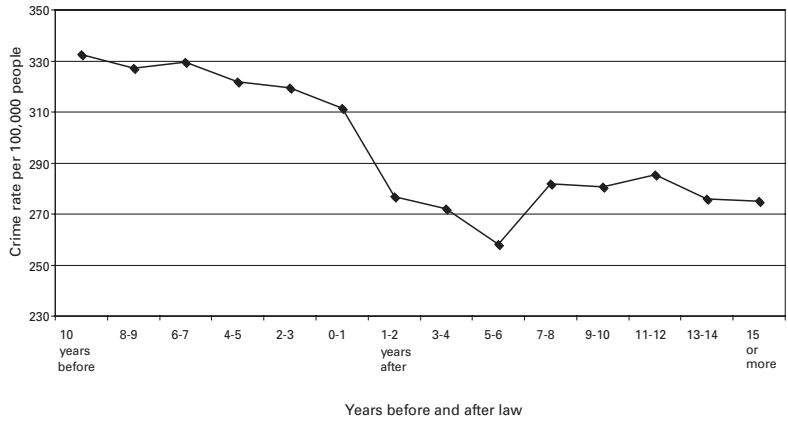


Figure 10.1e. Aggravated-assault rates before and after adoption of right-to-carry laws

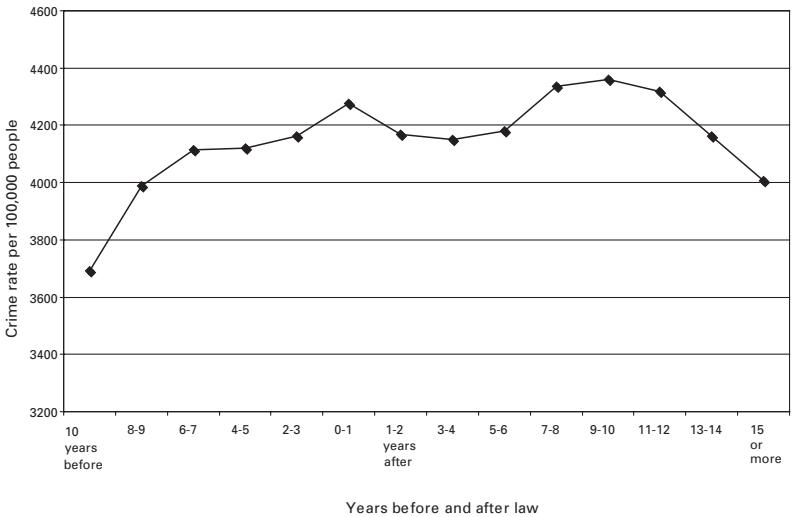


Figure 10.1f. Property-crime rates before and after adoption of right-to-carry laws

years 7 and 8), and there are six observations where it was lower. Robbery is one case where simple before-and-after trends would indicate a much more statistically significant drop in crime rates than is observed for the method being used here. But this disaggregated way provides a more accurate picture, and we have to accept that the results for robbery are not extremely strong.

Consistent with the previously reported research in this book, violent

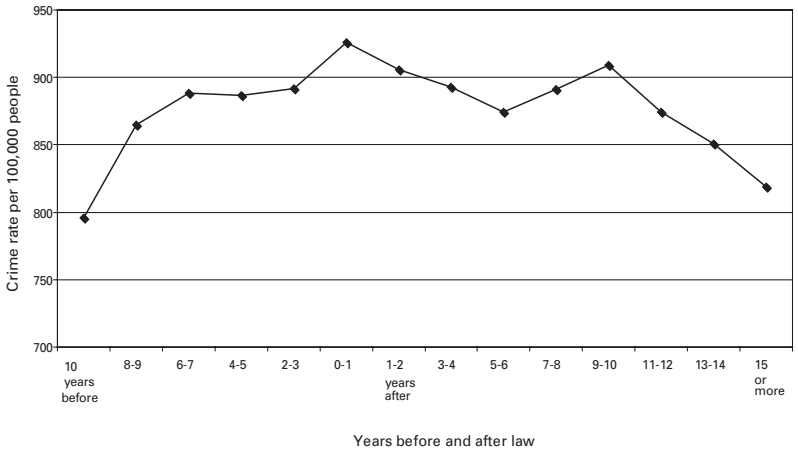


Figure 10.1g. Burglary rates before and after adoption of right-to-carry laws

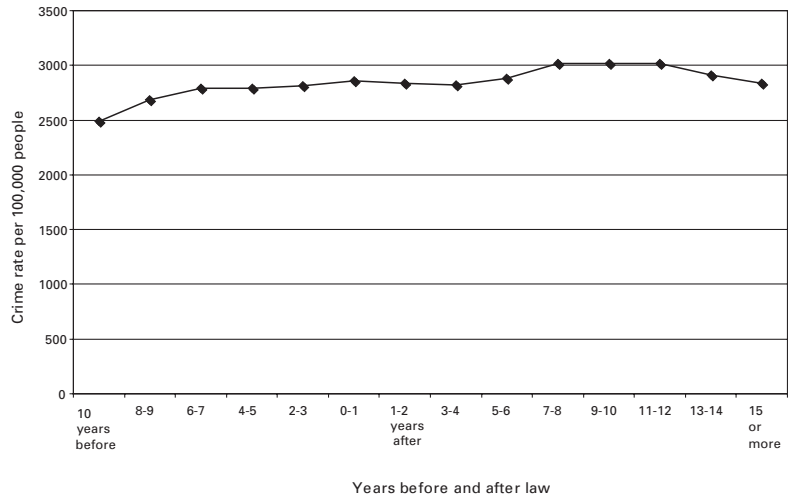


Figure 10.1h. Larceny rates before and after adoption of right-to-carry laws

crime rates are improving relative to property-crime rates, at least larceny and auto theft. If anything, larceny and auto theft keep rising after right-to-carry laws are passed. However, burglary rates go from increasing before the law to generally falling afterward. And indeed, right-to-carry laws should have a bigger impact on violent crimes where victims and criminals come into direct contact.

While we will continue to focus on the year-by-year estimates, the mea-

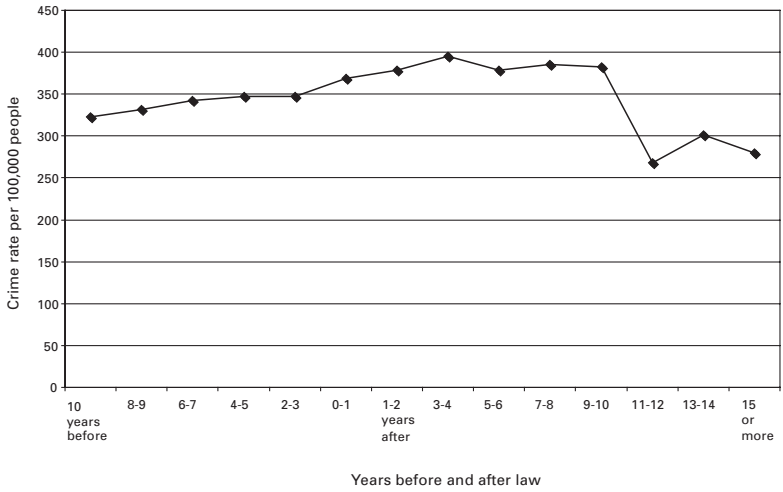


Figure 10.1i. Auto-theft rates before and after adoption of right-to-carry laws

sures used earlier in this book are provided here. For comparison, estimates that do not account for differences in right-to-carry laws are also provided. Table 10.4 generally confirms these previous results for earlier periods by looking at the more restrictive, simple before-and-after average crime rates (with their obvious limitations), the change in the before-and-after crime rate trends, and the combined before-and-after averages and trends. All the results indicate that violent crime falls after right-to-carry laws are passed, but the results vary based on whether one analyzes before-and-after averages or trends. There is a large, statistically significant drop in murder rates across all specifications. The before-and-after average comparison implies that right-to-carry laws reduce murder by roughly 20 percent. In all cases, right-to-carry laws cause the trends in murder, rape, and robbery rates to fall. For each additional year that the law is in effect, the murder rate falls by about 2 percent and the rape and robbery rates fall by about 1 percent relative to states without the law.

An alternative way of asking whether concealed-carry laws reduce crime is to see how crime rate changes vary across different states. (We provided a much simpler version of this previously in table 4.9 on page 80.) These estimates let us check how many states benefit from letting individuals defend themselves.

Tables 10.5a–10.5d show this breakdown using all the control variables for the four violent crime categories for all twenty-nine states that enacted right-to-carry laws between 1977 and 2005. The year-by-year breakdowns for

Table 10.4 The impact of right-to-carry laws on violent crime rates, 1977–2005

	Murder	Rape	Robbery	Aggravated assault
Estimates accounting for everything but differences in right-to-carry laws:				
Simple before-and-after averages	−4.5%[b]	−5.7%[a]	−1%	−5.9%[a]
Change in before-and-after crime trends:				
Change in crime rate trend (annual rate of change after the law minus annual rate of change before the law)	−1.54%*	−1.2%*	−0.6%	−0.7%
Combination of before-and-after averages and trends:				
Shift in crime rates after the law	−0.4%	−8.3%	−0.6%	−3.3%
Change in crime rate trend (annual rate of change after the law minus annual rate of change before the law)	−1.52%*	−0.7%**	−0.7%	0.15%
Estimates accounting for everything including differences in right-to-carry laws:				
Simple before-and-after averages	−20.3%[c]	−14%	−5.7%	−12.9%
Change in before-and-after crime trends:				
Change in crime rate trend (annual rate of change after the law minus annual rate of change before the law)	−2%*	−1%*	−0.8%***	0.5%
Combination of before-and-after averages and trends:				
Shift in crime rates after the law	−12.4%	−12.2%	−2.1%	−13.6%
Change in crime rate trend (annual rate of change after the law minus annual rate of change before the law)	−2%*	−1%*	−1%***	0.55%

* Statistically significant at the 1 percent level for an *F*-test.
** Statistically significant at the 5 percent level for an *F*-test.
*** Statistically significant at the 10 percent level for an *F*-test.
[a] Statistically significant at the 1 percent level for a two-tailed *t*-test.
[b] Statistically significant at the 5 percent level for a two-tailed *t*-test.
[c] Statistically significant at the 10 percent level for a two-tailed *t*-test.

crime rates do not show the results for as many years as in the figures because the tables would prove too large. Instead, I simply put together the year-by-year changes after year 10 after the law into one 10+ year category.

Two important questions can be answered from using this state-by-state breakdown. The first is: following concealed-carry laws being passed, how many states experience a drop versus an increase in crime? Because of random events, one doesn't expect crime in every state to fall. This is likely true even after an extremely effective law is adopted. But what we can do is see if the percentage of states where crime rates fell after the law is large compared to the percentage in states where crime remained the same or increased so that we can rule out randomness as a cause. Secondly, we can study how large the increases or decreases are.

The odds that a typical state experiences a drop in murder or rape after

Table 10.5a Changes in murder rates by state over time

	Years before law in effect						Years after law in effect						Change in murder rates		
	10 or more	9–8	7–6	5–4	3–2	1–0	1–2	3–4	5–6	7–8	9–10	11 or more	Average yearly change before and after law	Net impact by year 6	Net impact by year 10
Alaska	11.7	9.9	9.5	10.7	7.0	9.3	9.1	8.3	7.5	7.4	6.6		−1.91	−9.0	−23.1
Arizona	9.8	11.0	10.6	10.0	7.8	9.0	10.4	8.9	7.6	7.5	8.9		−1.03	−0.4	−5.1
Arkansas	12.7		15.2	16.3	14.8	16.4	8.7	7.6	7.8	8.0	9.6		−6.73	−74.8	−120.3
Colorado	4.7	4.1	4.7	5.5	3.4	4.3									
Florida	19.8	22.4	27.6	27.4	29.2	27.0		10.5	10.7	11.1	11.4	14.2	−14.01	−98.8	−193.5
Georgia	13.9		12.1	13.6	14.3	15.8	11.6	10.3	9.6	9.2	10.6	12.3	−3.36	−47.7	−83.4
Idaho	3.0	1.9	2.3	1.8	2.5	1.8	1.7	1.8	2.6		1.6	1.6	−0.37	2.1	1.5
Kentucky	11.4		8.8	7.7	6.1	6.7	5.8	6.4	6.4	7.3	6.9		−1.56	−4.2	−1.8
Louisiana	15.2	12.6	14.0	14.1	18.5	18.7	15.7	14.5	15.2	16.5			−0.03	−32.4	−95.1
Maine		2.5	2.7		2.5	2.5	2.0	2.5	1.4	1.3	1.7	1.6	−0.79	−4.8	−10.8
Michigan	8.5	7.1	7.0	7.1	7.7	7.4		5.8					−1.66	−4.9	−4.8
Minnesota	3.3		4.5	4.3	3.6	2.8	2.2						−1.51	−1.9	−1.8
Mississippi	17.4		18.7	17.9	15.8	15.9	8.5	14.6	15.3	19.3	16.1	19.4	−1.63	−28.3	−17.7
Missouri	5.5	6.2	6.5	6.5	6.2	6.2									
Montana	4.4	3.4	4.2	3.0	3.1	2.8	2.2			3.2	2.5	2.5	−0.88	−1.9	−1.8
Nevada	14.3	10.6	10.0	10.4	10.8	12.7	13.7	11.9	7.0		7.1		−1.57	−16.8	−33.6
New Mexico	7.2	8.9	9.8	7.5	5.9	6.4	7.4						−0.24	2.9	3

North Carolina	6.3	5.8	6.0	7.6	8.2	9.8	8.5	8.6		9.4	10.6		1.99	–7.6	–6.3
Ohio	3.7	3.2		3.4	4.1	4.5	5.1						1.31	1.8	1.8
Oklahoma	11.1	11.4	10.5	10.0	9.0	12.3	6.8		5.1	5.0	5.1		–5.2	–37.8	–81.3
Oregon	5.8		6.9	8.1	7.2	5.4	4.5	4.3	4.1	4.2	2.9	3.5	–2.76	–9.8	–21
Pennsylvania	7.1	7.4	7.5	7.7	8.0	8.5	6.8	6.5		8.3	7.4	8.1	–0.32	–11.5	–15.9
South Carolina	9.8	9.5	10.1	9.1	8.9	8.5	8.4	8.4	10.2	11.8			0.4	4.9	15
Tennessee	14.9	16.5	15.5		14.8	12.4	10.6	10.7	10	12.1	11.9	12.8	–3.47	–17.7	–20.1
Texas	16.1	15.7	15.2	16	12.6	11.1		5.3	5.9	6.3	6.1		–8.56	–33.1	–62.7
Utah	3.6	4.4	3.9	4.0	3.8	3.6	3.2	3.3	3.3	3.0			–0.7	–3.3	–5.4
Virginia		9.5	9.4	9.7	9	9.1	7.9	7.9	6.6	7.1	7.4	6.0	–2.19	–14.6	–25.5
West Virginia	5.4	5.8	5.8	5.6	6.5	6.8	5.7	6.4	4.4	3.9		4.1	–1.11	–11.9	–20.7
Wyoming	4.1	4.0	2.1	4.0	2.9	2.5	2.1	3.7	2.0	2.4	2.8		–0.64	1.1	1.8
Percentage of states where crime fell													89%	82%	82%
Drop in murders per 100,000 people for states where murder rates fell													–62.25	–473.1	–794.4
Increase in murders per 100,000 people for states where murder rates rose													3.71	12.9	21.6

Note: Blanks indicate that not enough information was available to calculate an estimate for those years.

Table 10.5b Changes in rape rates by state over time

	Years before law in effect						Years after law in effect						Change in rape rates		
	10 or more	9–8	7–6	5–4	3–2	1–0	1–2	3–4	5–6	7–8	9–10	11 or more	Average yearly change before and after law	Net impact by year 6	Net impact by year 10
Alaska	153.8	221.7	190.2	159.9	217.8	218.0	80.3	69.8	77.1	44.9	43.3		−130.5	−426.8	−774.6
Arizona	64.0	61.2	54.2	46.9	35.5	35.6	33.6		29.5	28.3	34.3	34.7	−17.5	−8.1	−16.7
Arkansas	100.8	105.6		127.2	110.0	105.3	41.7	33.8	41.8	39.5	59.2		−66.6	−198.5	−310.4
Colorado	26.7	25.1	38.0	39.5	32.6	41.3									
Florida		217.3	228.3	224.9	215.4	213.1	50.3	58.5	69.3	86.9		93.4	−148.1	−461.1	−587.2
Georgia	41.3	45.8	43.6	47.2	51.5	61.9		43.6	43.4	41.4	44.6	44.5	−5.0	−36.9	−74.7
Idaho	18.3	16.4	17.7	20.3		29.1	29.1	27.0	24.7	26.3	27.2	27.9	6.7	−6.6	−11.4
Kentucky	44.2	43.5	35.1	41.1	36.0	33.8	33.4	31.5	33.4	33.5			−6.0	−3.1	−3.4
Louisiana	70.0	64.6	55.5	49.3	60.0	61.9	41.3	38.1	37.1	45.2			−19.8	−69.1	−85.7
Maine		12.1	8.5		9.9	8.4	14.9	18.9	18.3	18.6	17.5	15.3	7.5	26.8	46.0
Michigan	103.0	101.2	100.0	94.1	96.4	104.4									
Minnesota	42.5		64.3	61.8	61.3	55.6	41.6						−15.5	−14.0	−14.0
Mississippi	57.3	67.4	72.8	63.5	74.1	88.2		51.2	55.1	61.1	74.5	69.8	−8.2	−70.1	−110.8
Missouri	29.3	30.8	31.3	33.9	24.4	28.4									
Montana	30.1	22.9	25.9	18.2	19.0	17.6	21.4			15.7	16.9	18.9	−4.1	3.8	1.2
Nevada	47.0		44.6	40.0	41.1	57.2	53.4	44.1	26.8	24.1	22.4		−11.8	−47.2	−115.1
New Mexico	141.7	184.1	142.9	78.3	66.9	59.3	54.1						−58.1	−5.2	−5.2

North Carolina	18.3	22.5	23.5	26.1	31.7	39.6	31.3	28.6	27.0	28.5	31.2		2.4	−31.9	−51.3
Ohio	39.0	37.4		42.6	45.0	42.6	39.8						−1.5	−2.8	−2.8
Oklahoma	101.3	101.6	97.8	108.5	99.2	103.1	46.8		35.0	33.9	34.7		−64.3	−124.5	−262.1
Oregon	73.2		63.4	63.3	50.4	51.1	53.2	43.9	37.5	41.2	37.2	45.1	−17.3	−18.7	−42.4
Pennsylvania	29.7		28.9	29.3	28.2	26.0	26.4	24.5	24.3	27.7	25.9		−2.7	−2.7	−1.0
South Carolina	50.7	56.4		68.2	60.8	54.5	48.9	42.4	42.1	42.0	35.1		−16.0	−30.1	−61.9
Tennessee	83.4	91.5	90.5	89.5	75.6	80.3	47.1	47.6	44.7	47.9	46.9		−38.3	−101.5	−167.3
Texas	163.8	181.5		159.7	145.7	140.0	43.8	45.3					−113.6	−190.9	−190.9
Utah	39.7	36.0		52.4	47.6	42.0	41.8	40.8	52.1	55.3	65.5		7.6	8.8	45.6
Virginia	23.4	23.3	23.9	22.8	22.7	22.8	27.0	26.6	32.0		28.3	22.3	4.1	17.4	22.9
West Virginia	39.0	37.6	37.2	36.5	36.9	29.2	23.4	20.7	20.4	20.5	20.4	18.3	−15.4	−23.1	−40.6
Wyoming	58.3	41.8	46.1	40.3	32.9	38.9	34.4	32.1	28.8	27.2	21.5	19.6	−15.8	−21.5	−50.7
Percentage of states where crime fell													81%	85%	85%
Drop in rapes per 100,000 people for states where rape rates fell													−776.0	−1,894	−2,980
Increase in rapes per 100,000 people for states where rape rates rose													28.2	52.9	115.6

Note: Blanks indicate that not enough information was available to calculate an estimate for those years.

Table 10.5c Changes in robbery rates by state over time

	Years before law in effect						Years after law in effect						Change in robbery rates		
	10 or more	9–8	7–6	5–4	3–2	1–0	1–2	3–4	5–6	7–8	9–10	11 or more	Average yearly change before and after law	Net impact by year 6	Net impact by year 10
Alaska	130.4	160.3	162.1	134	160.8	194.7	155.1	141.1	112.9	83.5	50.4		−48.4	−525	−1,292
Arizona	189.7	201.0	185.9	176.8	124.4	139.1	173.8	188.9	178.1	194.4	189.2		15.4	371	687
Arkansas	167.9		244.3	253.9	224.6	233.5	114.1	119.6	116.3	148.2	166.5		−91.9	−1,051	−1,508
Colorado	80.7	59.4	72.0	69.3	53.2	70.6									
Florida		617.8	892.8	701.2	810.4	970.5	357.3	398.7	375.2	389.9		425.6	−409.2	−5,341	−7,083
Georgia	197.9		146.7	169.8	225.7	289.3	257.2	225.6	209.6	224.7	248.8	218.6	24.9	−526	−841
Idaho	40.9	27.6	24.7	21.8		13.8	20.5	14.9	22.0	24.0	23.1	18.4	−5.3	48	106
Kentucky	91.9	117.8	72.9	76.3	70.4	84.3	90.7	103.7	100.9	107.8			15.2	127	198
Louisiana	233.6	235	229.1	223.8	260.2	299.2	239.1	244	238.1	201			−16.2	−529	−824
Maine		65.2	46.5		35.9	36.3	28.0	26.1	23.3	22.6	27.0	28.9	−20	−94	−163
Michigan	234.7	194.5	191.5	158.1		191.5	117.9	113.9					−78.2	−454	−454
Minnesota	102.3	118.5	127.2	115.3	84.7	82.6									
Mississippi	159.3	142.4	135.6	125	140.6	159.2		158.3	185	224.9	186.3	226.2	52.5	74	353
Missouri	121.8	136.8	124.5		129.1	114.0	114.0						−11.2		
Montana	34.7		26.5	19.3	22.2	22.2	25.3		22.2	19.5	20.9	27.1	−2	10	−2
Nevada	365.1	337.7	312.8	284.7	325.1	316.2	307.6	269.2	191.8		151.2		−93.7	−541	−1,036
New Mexico	123.6	123.6	149.1	104.5	95.4	80.7									

North Carolina	51.1	65.2	87.7	111.9	131.6	150.9	163.9	191.5		200.7	217.7		93.7	161	511
Ohio	103.4	104.8	105.1	120.7	133.5	139.2									
Oklahoma	160.1	206.8	221.1	196.3	177.3	183.1	106.6		78.2	85.1	97.7		−98.9	−544	−1,095
Oregon	161.5	193.3	227.8	246.6	242	169.7		119.6	124.1	129.2	104.7	136.9	−83.9	−287	−604
Pennsylvania	165.2		196.4	193	178.9	172.6	174.3	158.9	171.7	225.6	201.4	184.0	4.8	−38	207
South Carolina	89.4	104.5		133.1	155.8	161.7	176.2	179.6	186.8	204.4	192.4		59	172	300
Tennessee	264.9	320.2	318.4		286.5	251.9	223.2	210.8	217.1	256.3	266.1	307.1	−41.6	−314	−258
Texas	344.7		474.1	460.8	390.1	354.2	171.5	174.8	200.6	250.7	246.9		−195.9	−1,547	−2,180
Utah	65.1	63.4		54.2	46.5	55.5	68.9	69.2	76.1	69.6	55.0		10.8	143	185
Virginia	101.2	109.7	106.3	97.1	94.9	103.6	107.2	111.8	124.1		134.9	98.1	13.1	97	191
West Virginia	41.5	48.5	51.3	48.9	44.0	45.5	37.7	40.3	36.1	40.6		50.0	−5.7	−68	−83
Wyoming	33.8	27.2	24.4	20.1	16.8	16.4	17.9	18.8	12.1	16.1	15.6		−7	−1	−4
Percentage of states where crime fell													64%	64%	64%
Drop in robberies per 100,000 people for states where robbery rates fell													−1,209.2	−11,861	−1,7426
Increase in robberies per 100,000 people for states where robbery rates rose													289.3	1,203	2,738

Note: Blanks indicate that not enough information was available to calculate an estimate for those years.

Table 10.5d Changes in aggravated assault rates by state over time

	Years before law in effect						Years after law in effect						Change in aggravated assault rates		
	10 or more	9–8	7–6	5–4	3–2	1–0	1–2	3–4	5–6	7–8	9–10	11 or more	Average yearly change before and after law	Net impact by year 6	Net impact by year 10
Alaska	333.9	449.8	423.9	351.5	348.1	457.7	526.3	484.2	574.5	414.8	272.7		60.3	635	−48
Arizona	684.5	719.4	680.2	619.6	523.1	538.4	495.7		381.7	417.1	463.2	509.6	−174	−599	−1,188
Arkansas	332.6		368.6	390.9	394.6	354.7	359.8	344.0	362.9	375.7	449.0		10	8	354
Colorado	285.7	221.2	187.5	184.1	184.8	197.4									
Florida	711.0	665.0	686.9	676.8	675.1	613.3		602.4	626.4	635.4		689.7	−32.9	7	73
Georgia	353	378.4	344.4	354.3	317.7	390.6		413.6	373.8	376.4	337.2	295.9	3	19	−184
Idaho	303	323.9	289.9	220.5	211.9	221.5	227.5	207.2	238.9		244.9	209.4	−36.2	27	97
Kentucky	321.9	339.1	363.3	506.3	415.2	216.7	187	221.5	230.6	214.4			−147	−33	−41
Louisiana	471.3		347.7	381.0	507.8	517.9	559.7	612.5	614.7	470.7	612.5		128.9	700	842
Maine		322.5	262.5		157.3	143.7	102.6	95.6	79.7	74.6	69.0	72.6	−139.2	−460	−892
Michigan	375.5	362.6	315.2	269.6		313.9	362.3	371.7					39.6	319	319
Minnesota	112.1	113.5	112.0	122.2	130.6	127.6									
Mississippi	204.0		183.4	146.0	133.9	121.5	128.6	221.4	303.6	386.2	198.6	183.5	79.2	867	1,893
Missouri	277.3	308.4	285.4	281.7	315.6	366.6									
Montana	208.9		218.9	88.1	85.6	76.5	99.0		76.2	98.8	180.0	237.2	2.7	67	444
Nevada	484.7		369.3	278.9	380.4	587.5	436.6	362.1	304.2	381.8	421.5		−38.9	−1,979	−3,094
New Mexico	752.3		819.1	649.7	641.0	576.6	541.9						−145.8	−104	−104

North Carolina	370.7	305.8	298.2		364.2	363.2	384.5	362.3	270.3	289.3	301.8		–18.8	–218	–623
Ohio	180.7	168.9		139.3	145.0	137.4	143.4						–10.9	18	18
Oklahoma	402.7	366.0	383.5	418.1	461.1	476.9	436.9		414.7	414.0	405.3		–0.3	–307	–710
Oregon	609.8		588.4	470.6	417.3	371.2	297	311.2	117.7	330.5	316.4	268.4	–217.9	–1,163	–1,450
Pennsylvania	206.9	210.5	219.3	219.4	216.7	221.6	223.1	204.8		266.9	288.5	341.8	49.3	–46	291
South Carolina	772.9	667.2	877.5	777.6	822.1	804.7	756.9	616.2	564.5	628.5			–145.5	–1430	–1958
Tennessee	352	355.3	365.2	406.5	473.6	466.8	490.5	544	542.7	613.5	627.9		160.5	530	1,453
Texas	402.2		431.5	479.6	487.1	420.3	421.4	405.3	395.7	428.4	397.1		–34.6	–116	–161
Utah	279	231.9	216.4	227.0	214.3	210.4	218.1	245.6	240.4	247.0			8	218	328
Virginia	269.8	231.3	239.6	233.5	218.4	183.8	172.4	177.5	179.1		231.2	194.5	–38.5	–68	74
West Virginia	101.5	110.2	116.7	119.0	82.4		101.5	125.3	104.9	112.4	197.6	187.7	32.3		
Wyoming	305.7	264.6	277.7	243.7	265.3	201.0	199.8	217.1	203.0	232.5	205.1		–48.2	51	158
Percentage of states where crime fell													58%	48%	48%
Drop in aggravated assaults per 100,000 people for states where aggravated assault rates fell													–1,228.7	–6,522	–10,452
Increase in aggravated assaults per 100,000 people for states where aggravated assault rates rose													573.7	3,465	6,345

Note: Blanks indicate that not enough information was available to calculate an estimate for those years.

a right-to-carry law is passed merely due to randomness is far less than 0.1 percent—more than 1,000 to 1 odds. The odds that these drops in robbery rates are random are also somewhat long—about 11 to 1. For aggravated assault, while the laws still indicate a small overall reduction in crime, the impact on particular states is essentially random. These odds are similar to testing whether a coin is fair. If you flip a coin twenty times and 70 or 80 percent of your tosses come up heads, you would be very foolish not to bet heads for the next coin flip.

The results are again very similar to those provided in figures 10.1b–10.1e, with murder and rape showing the most dramatic reductions. The average murder rate dropped in 89 percent of the states after the right-to-carry law was passed. Over the first six years after the law, 82 percent of the states experienced a net reduction in murders. Over the first ten years after the law, 85 percent saw a drop. Even in the three states where the average rate went up, the increase was very small, averaging just one percentage point, and statistically insignificant.

There was a similar decline in rape rates. While rape rates didn't compare quite as well in terms of the before-and-after averages (81 percent of the states showed declines, compared to 89 percent for murder). Nevertheless, rape rates showed a slightly more consistent drop across states than murder rates when you compare the crime rates after the law to the rates right before the law went into effect (85 percent for both the six- and ten-year comparisons compared to 82 percent). The average drop for the states whose rape rates fell was over twenty-seven times larger than the average increase for the states that experienced an increase in rape.

While the results for robbery are not anywhere near as overwhelming as for murder and rape, states still experienced a drop in robbery rates by almost a two-to-one ratio. Once again, the drops are much larger than the increases—with the average drops in robbery rates being between 4.4 and 9.9 times larger than the average increases. In fact, as we have pointed out many times in this book, the comparison in this case might underestimate the benefit of right-to-carry laws, because robbery rates were rising before the law and falling afterward. If you concentrate on the changes in robbery rates between the period immediately before the law and up to six or ten years afterward, the average drop experienced by states where robbery rates declined is between 6.4 and 9.9 times bigger than the average increase in the states where robbery rates rose.

Victims face real costs from crime. Although the loss of life and physical and psychological damage are the most important, there are lost earnings,

medical care, and the destruction of property that should be included in adding up the costs. Not too surprisingly, murder imposes by far the biggest cost. The estimated total loss from a murder—$3.9 million in 2007 dollars—is far greater than the $115,260 average loss from rape and the $10,758 average loss from robbery. These victimization costs were developed by the National Institute of Justice. If we use these figures, the twenty-nine states that we study save over $30 billion a year, with over half of that coming just from the reduction in murder rates. To put it differently, the average citizen in those states saved $221 each year from other law-abiding citizens carrying concealed handguns. Excluding property crimes, where our results are the least certain, the average citizen still saved $183. Since criminals don't know which people are going to be able to protect themselves from an attack, even those who would never even consider owning a gun, let alone carrying one, benefit from others willing to bear these costs.

The costs are borne by individual gun carriers, who have to pay the price of the gun, licensing fees, and training. These individuals also have to deal with the inconvenience of carrying a gun. It is important to note that higher fees mean that fewer people will carry guns, which reduces the total benefit.[99]

All these tables make another point quite clear. When accounting for the differences in the laws, there is no evidence that states that adopted right-to-carry laws later than others had a different experience. Florida, Maine, and Virginia had the first full year of their laws during the 1980s, but there is little difference in how murder, rape, or robbery rates changed compared with states such as Louisiana, Nevada, and Oklahoma, whose right-to-carry laws started in the 1990s. Even Michigan, whose right-to-carry law was adopted after 2000, experienced substantial benefits.

For example, two-thirds of the states whose right-to-carry laws went into effect during either the 1980s or 1990s saw drops in robbery rates. One of the two states whose laws went into effect after 2000 also showed a drop, while the other, Missouri, showed no change. This pattern casts doubt on the claim that the crack cocaine epidemic during the late 1980s and early 1990s is driving the results, because these results show drops in crime rates whether the right-to-carry laws went into effect before, during, or after the crack epidemic.

Figure 10.2 shows the pattern for murder rates by the decade that the right-to-carry law went into effect. It graphs out what was shown in table 10.5a. Clearly, the murder rates start falling after the law, though

Table 10.6 The impact of right-to-carry laws on victimization costs (millions of 2007 dollars)

	Murder	Rape	Robbery	Aggravated assault	Property crime
Alaska	−$36.91	−$110.29	−$28.61	−$1.26	−$42.19
Arizona	−$67.35	−$19.85	$127.33	−$258.63	$104.84
Arkansas	−$795.17	−$182.53	−$137.97	$38.05	−$264.70
Colorado					
Florida	−$6,652.83	−$1,795.62	−$3,368.81	$40.88	−$2,834.34
Georgia	−$1,490.79	−$118.89	−$208.35	−$53.45	−$0.63
Idaho	$3.50	−$2.61	$3.78	$4.06	$6.84
Kentucky	−$18.56	−$3.15	$28.72	−$6.90	$10.73
Louisiana	−$547.55	−$107.19	−$128.08	$153.92	−$70.19
Maine	−$31.41	$11.93	−$6.57	−$42.26	−$1.13
Michigan	−$289.53		−$149.53	$123.29	−$153.37
Minnesota	−$127.05	−$82.40		$0.00	$0.00
Mississippi	−$122.11	−$68.16	$33.72	$212.79	−$12.96
Missouri			−$0.01		
Montana	−$4.33	$0.24	−$0.08	$16.46	$14.10
Nevada	−$158.07	−$48.00	−$67.20	−$235.82	−$48.57
New Mexico	$70.39	−$11.31		−$8.33	$8.13
North Carolina	−$126.29	−$90.48	$140.17	−$200.92	−$106.80
Ohio	$270.82	−$37.24		$8.73	−$2.81
Oklahoma	−$707.18	−$202.90	−$131.82	−$100.52	−$316.14
Oregon	−$168.72	−$30.24	−$67.03	−$189.06	$38.63
Pennsylvania	−$492.25	−$2.71	$89.37	$147.51	$61.49
South Carolina	$195.55	−$71.77	$43.31	−$332.01	−$42.53
Tennessee	−$282.37	−$209.50	−$50.29	$332.65	−$132.71
Texas	−$3,263.15	−$882.25	−$1,566.65	−$135.67	−$1,619.63
Utah	−$37.02	$27.97	$14.15	$29.44	−$3.00
Virginia	−$421.56	$33.52	$43.51	$19.91	$40.54
West Virginia	−$122.35	−$21.28	−$5.37	$0.00	−$8.16
Wyoming	$2.39	−$5.69	−$0.07	$3.23	$0.17
Total	−$15,419.92	−$4,030.40	−$5,392.38	−$433.91	−$5,374.38
Average per state	−$571.11	−$155.02	−$215.70	−$14.96	−$185.32
Per capita	−$111.38	−$29.11	−$38.95	−$3.13	−$38.82
Excluding Florida:					
Total	−$8,767.10	−$2,234.78	−$2,023.58	−$474.78	−$2,540.04
Average per state	−$337.20	−$89.39	−$84.32	−$16.96	−$90.72
Per capita	−$63.33	−$16.14	−$14.62	−$3.43	−$18.35

Note: Except for the per capita estimates, all dollar amounts are in millions of dollars.
Source: The estimated victimization costs from crime are from Ted R. Miller, Mark Cohen, and Brian Wiersema, "Victim Costs and Consequences: A New Look," Office of Justice Programs, National Institute of Justice, 1996.
Note: Except for the per capita estimates, all amounts are in millions of dollars. All estimates are in 2007 dollars. The victimization costs for the different crime categories are $3,887,200 for murder, $115,260 for rape, $10,758 for robbery, and $12,640 for aggravated assault.

the drop is greater in those states whose laws went into effect in the 1980s than the 1990s. And states that adopted the law after 2000 display a similar pattern—murder rates were rising consistently over the preceding eight years and declined for the first time right after the law went into effect. However, for this last set of states too little time has elapsed to reach any overall conclusion.

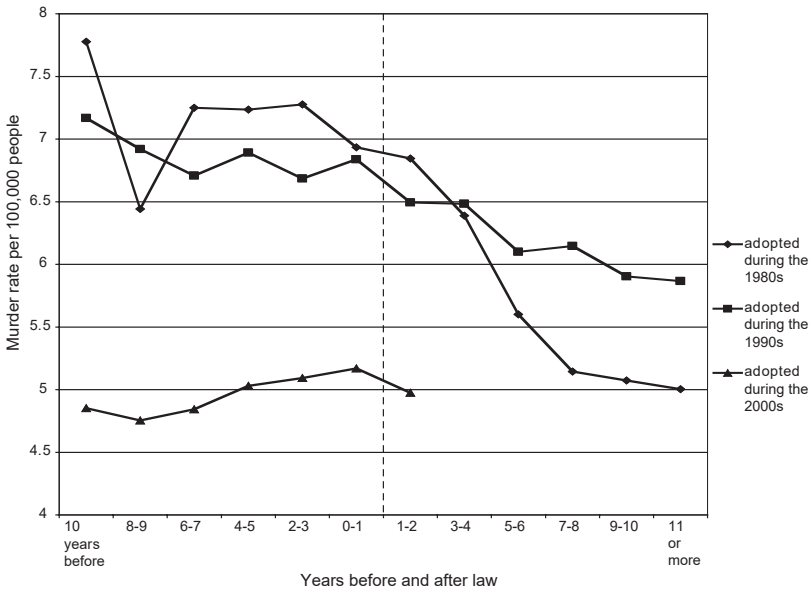


Figure 10.2. Changes in murder rates after adoption of right-to-carry laws by decade of adoption

Obviously, there can be some debate over what factors cause changes in crime rates. Take our graphs for murder. In the nine alternative graphs that follow, I look at many combinations of factors that can explain crime: (a) using the arrest rate in the preceding year; (b) excluding changes in all the other gun-control laws, but keeping everything else; (c) excluding all the measures of demographic change, but keeping everything else; (d) dropping all the measures of income, poverty, unemployment, and population, but keeping everything else; (e) dropping the gun-control laws except the right-to-carry law and dropping all measures of demographics but keeping everything else; (f) dropping the gun-control laws as well as the income, poverty, unemployment, and population variables; (g) dropping the particular demographic variable for the percentage of the population that is neither white nor black males 20–29 years of age; (h) eliminating all control variables but fixed effects; and (i) including crack cocaine use for the 1980–2000 period (see figures 10.3a–10.3i). These different estimates examine how sensitive the results are to the specifications that are used. While I think that most people would accept that the procedures used in the earlier estimates are important in explaining crime rates, all readers might not share that feeling.

The nine figures consistently show that right-to-carry laws reduce murder rates. Given the frequent claims made by critics about cocaine's impact

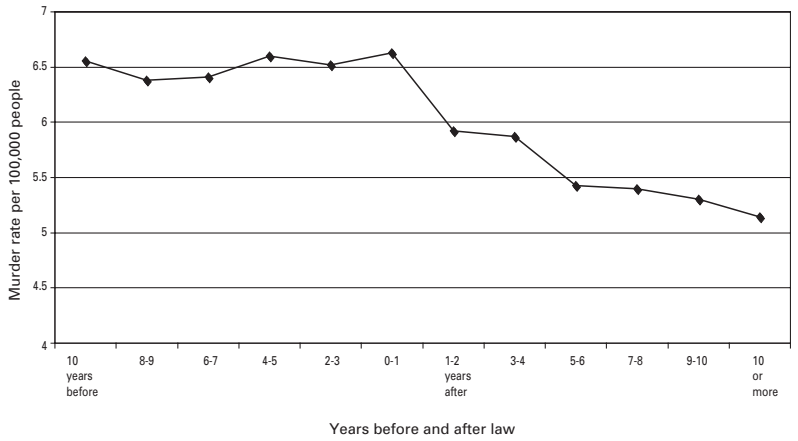


Figure 10.3a. Murder rates before and after right-to-carry law: using the lagged arrest rate

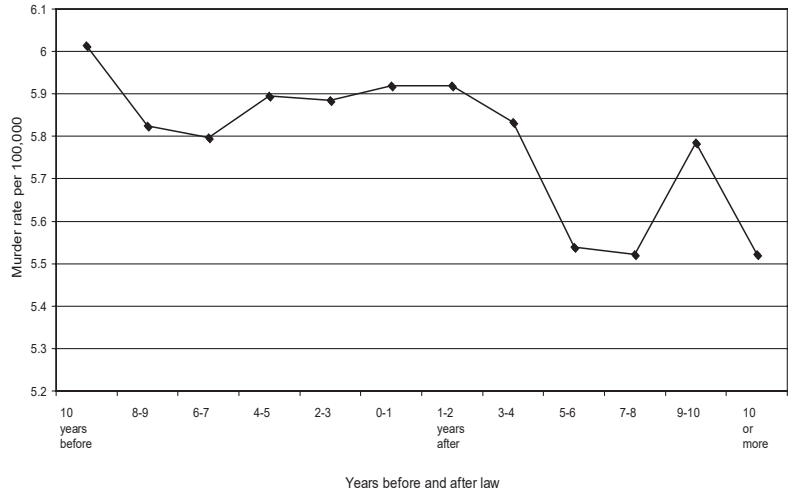


Figure 10.3b. Murder rates before and after right-to-carry law: without gun-control variables

on the measured benefits of right-to-carry laws, it is interesting how large the drop in crime is even when the measure of cocaine use is included. As in the results reported in previous chapters, per capita income and poverty measures have very small and statistically insignificant impacts on crime rates.

Generally, the regulations to obtain permits have effects similar to those

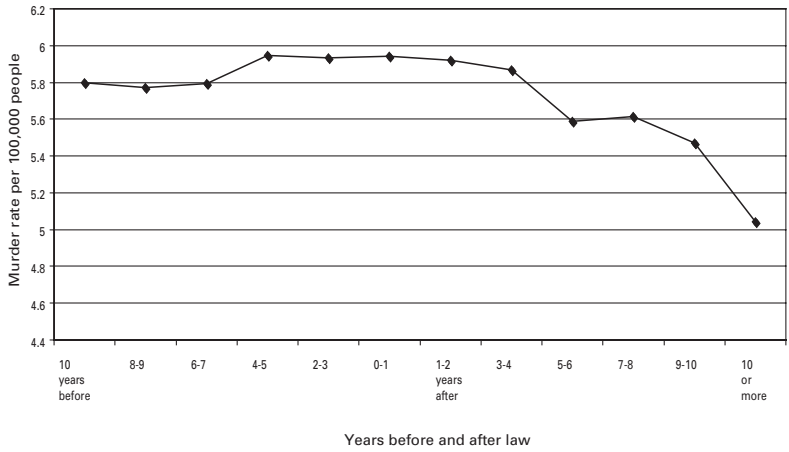


Figure 10.3c. Murder rates before and after right-to-carry law: without demographic control variables

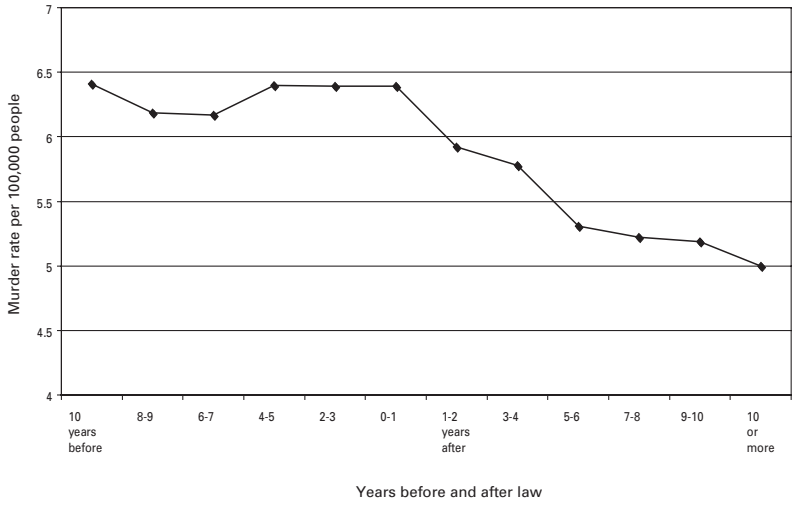


Figure 10.3d. Murder rates before and after right-to-carry law: without the income, poverty, unemployment, and population variables

found in chapter 9. Longer training requirements imply a larger reduction in crime from right-to-carry laws, but the effect drops off at a diminishing rate. There also seems to be an increase in crime when the length of the training requirement is greater than eight hours. Requiring people to train for more than eight hours means that they must attend training for

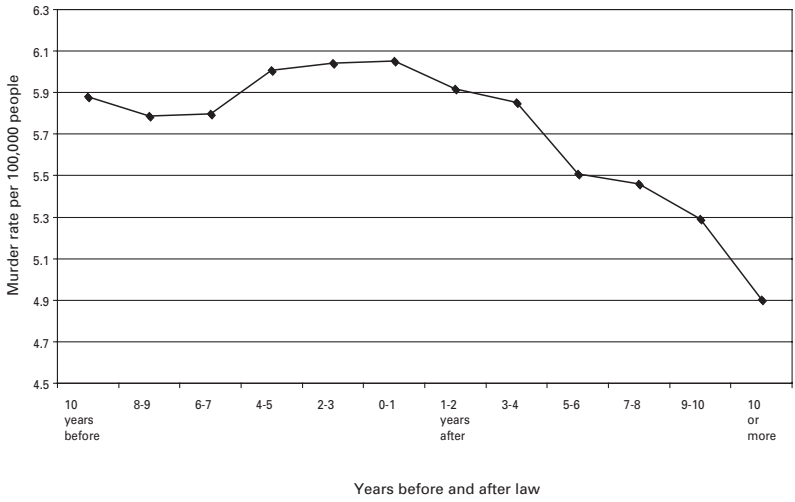


Figure 10.3e. Murder rates before and after right-to-carry law: without the gun-control variables and demographic variables

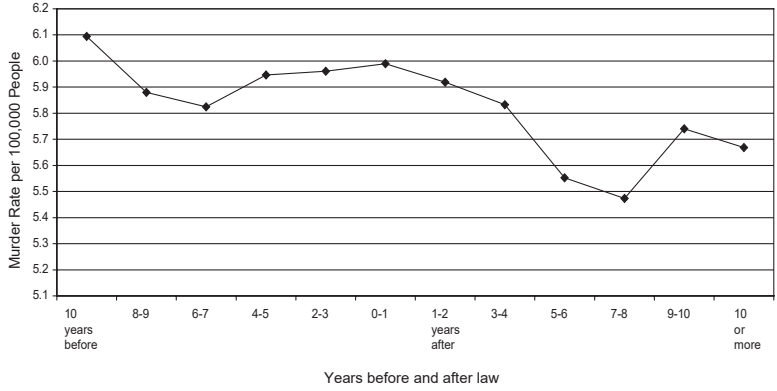


Figure 10.3f. Murder rates before and after right-to-carry law: without the income, poverty, unemployment, population, and gun-control variables

more than one day, and that greatly reduces the number of people who get a permit. The more years a permit lasts, the larger reduction in crime, though the effect is small. Finally, higher age limits before one can obtain a permit appear to be related to more crime, but the effect is only statistically significant for overall violent crime and aggravated assault.

We will discuss these regressions more later in the context of other gun laws.

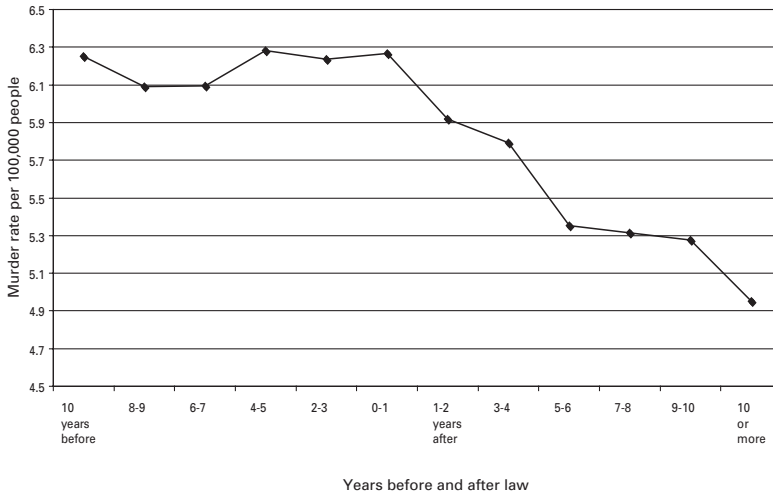


Figure 10.3g: Murder rates before and after right-to-carry law: dropping "percentage of population that is neither white nor black males 20–29 years of age"

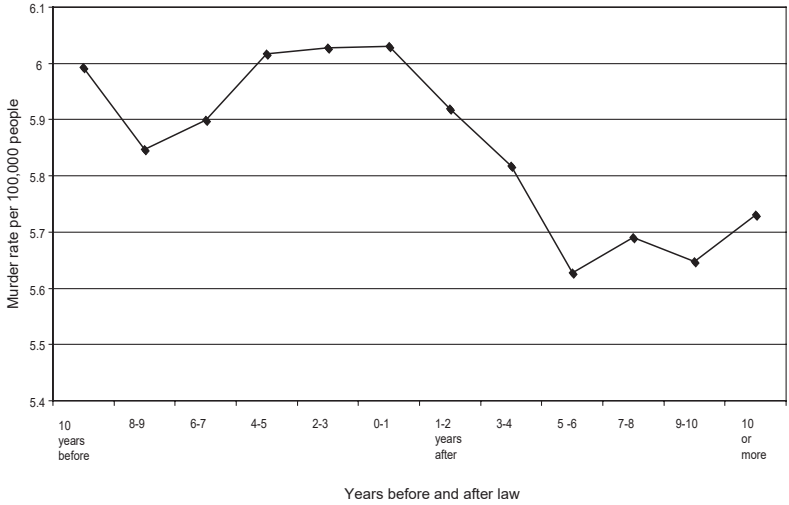


Figure 10.3h. Murder rates before and after right-to-carry law: only accounting for average differences by state and year, no other factors taken into account

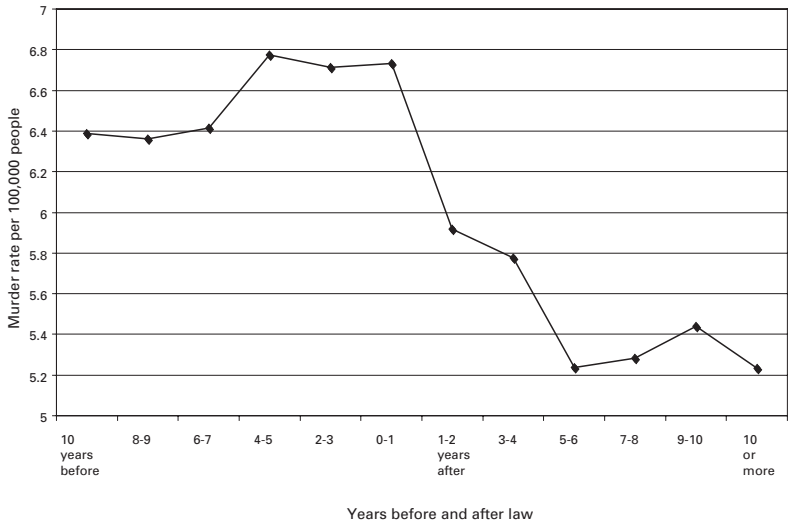


Figure 10.3i. Murder rates before and after right-to-carry law: including crack cocaine, data only available for 1980–2000

Other Research on Guns and Crime

When other scholars have tried to replicate [Lott's] results, they found that the right-to-carry laws simply don't bring down crime.

—Steven Levitt and Stephen Dubner, *Freakonomics* (2005), pp. 133–34

The Numbers

After *More Guns, Less Crime* first appeared in 1998 (with the second edition in 2000), a host of new empirical research was undertaken on the effect of right-to-carry laws.[100] The most recent research continues the earlier pattern of finding reductions in crime, with twelve new refereed studies by economists and criminologists finding reductions in violent crime of various magnitudes. On the other side, one new refereed piece claims that the benefits are small or nonexistent. Weak evidence that concealed handguns may increase crime has been put forward in two nonrefereed publications by Ayres and Donohue.

Here are the results from nine of the twelve studies finding a benefit from right-to-carry laws published since the last edition of this book (see table 10.7; my three articles are excluded from this list):

—Florenz Plassmann and Nicolaus Tideman conclude that "right-to-carry laws do help on average to reduce the number of these crimes."[101]

—Carl Moody states that his findings "confirm and reinforce the basic findings of the original Lott and Mustard study."[102]

—In another paper that Moody coauthored with Thomas Marvell, which studies county crime rates from 1977 to 2000, the authors write that "the evidence, such as it is, seems to support the hypothesis that the shall-issue law is generally beneficial with respect to its overall long run effect on crime."[103]

—Eric Helland and Alex Tabarrok used county crime rates from 1977 to 2000 to find that "shall-issue laws cause a large and significant drop in the murder trend rate" and that "there is considerable support for the hypothesis that shall-issue laws cause criminals to substitute away from crimes against persons and towards crimes against property."[104]

—While calling for more research, David Olsen and Michael Maltz found "a decrease in total homicides," though the different data set they use indicates that the decline was driven entirely by a drop in gun homicides.[105]

—Bruce Benson and Brent Mast argue that their results "are virtually identical to those in [Lott and Mustard]. Therefore, the hypothesis that the [Lott and Mustard estimates] suffer from missing-variable bias owing to the lack of control for the private security industry is rejected."[106]

—David Mustard provides evidence that "after enactment of the right-to-carry laws, states exhibit a reduced likelihood of having felonious police deaths."[107]

—James Q. Wilson, often mentioned as the preeminent criminologist in the United States, reviewed the National Academy of Sciences report on Firearms and Violence and notes that while there might be some debate over some types of violent crime, "I find that the evidence presented by Lott and his supporters suggests that RTC laws do in fact help drive down the murder rate."[108]

—My work with John Whitley concludes that "the longer a right-to-carry law is in effect, the greater the drop in crime."[109]

Different researchers approach the problem from a variety of perspectives by using new statistical techniques, different data sets, or different control variables or by examining a variety of specifications. Despite these differences, the consensus is the same: right-to-carry laws reduce violent crime.[110]

Plassmann and Tideman break down the impact of concealed-handgun laws not only across states but also by each year before and after the law for the years 1977–1992. Their big innovation involves solving what is called

Table 10.7 Modern statistical research on right-to-carry laws and crime by academic economists and criminologists using national panel data

	Studies that found right-to-carry laws reduced violent crime	Studies that claim to find that right-to-carry laws have no discernable effect on violent crime	Studies that found right-to-carry laws increased violent crime
Refereed academic publications	1. Lott and Mustard, *Journal of Legal Studies*, 1997 2. Bartley and Cohen, *Economic Inquiry*, 1998 3. Lott, *Journal of Legal Studies*, 1998 4. Bartley, *Economics Letters*, 1999 5. Benson and Mast, *Journal of Law and Economics*, 2001 6. Moody, *Journal of Law and Economics*, 2001 7. Mustard, *Journal of Law and Economics*, 2001 8. Olsen and Maltz, *Journal of Law and Economics*, 2001 9. Plassmann and Tideman, *Journal of Law and Economics*, 2001 10. Marvel, *Journal of Law and Economics*, 2001 11. Lott and Whitley, *Journal of Law and Economics*, 2001 12. Lott and Whitley, *Journal of Quantitative Criminology*, 2003 13. Helland and Tabarrok, *Advances in Economic Analysis and Policy*, 2004 14. Wilson, *National Academies Press*, 2005 15. Lott and Whitley, *Economic Inquiry*, 2007 16. Moody and Marvel, *Econ Watch*, 2008	1. Black and Nagin, *Journal of Legal Studies*, 1998 2. Ludwig, *International Review of Law and Economics*, 1998 3. Donohue and Levitt, *Quarterly Journal of Economics*, 1999 4. Hood and Neeley, *Social Science Quarterly*, 2000 5. Duggan, *Journal of Political Economy*, 2001 6. Duwe, Kovandzic, and Moody, *Homicide Studies*, 2002 7. Kovandzic and Marvell, *Criminology and Public Policy*, 2003 8. Dezhbakhsh and Rubin, *International Review of Law and Economics*, 2003 9. National Research Council, *National Academies Press*, 2005 10. Kovandzic, Marvell, and Vieraiis, *Homicide Studies*, 2005	None
Nonrefereed publications by academics	1. Bronars and Lott, *American Economic Review*, 1998 2. Plassmann and Whitley, *Stanford Law Review*, 2003 3. Lott and Landes, *The Bias Against Guns*, 2003	1. Ayres and Donohue, *American Law and Economics Review*, 1999 (book reviews, unlike papers in this journal, are not refereed)	1. Ayres and Donohue, *Stanford Law Review*, 2003; see also Donohue, "The Impact of Concealed-Carry Laws," 2003 2. Ayres and Donohue, *Econ Watch*, 2009[a]

[a]Just the original paper by Carl Moody and Teb Marvell was refereed by *Econ Watch*. Correspondence with the editor indicates the comment by Ayres and Donohue was published as it was sent into the journal: no refereeing or editing was done for their piece.

the "truncation problem," which occurs in county-level data sets because in some years many counties do not experience certain types of crimes at all—80 percent have no murders, for instance. If the murder rate in a county is zero before the law goes into effect, simple randomness means that sometimes the crime rate will go up, but the reverse cannot happen—crime rates cannot fall below zero. This could bias results for these regressions toward finding an increase in crime from the law. To avoid that, they exclude counties where there were no crimes committed.

Figure 10.4 graphs the regression results that Plassmann and Tideman obtained for murder. Their results are striking. For the ten states that adopted concealed-handgun laws during the period they studied, murder rates were rising or constant prior to the law. After the law was passed, they were all falling. Indeed, with one exception, all Plassmann and Tideman's estimates for murder, rape, and robbery for the ten states that enacted the law from 1977 through 1992 show that crime rates fell during the first full year that the laws were in effect. Even in the one exception (Oregon for robbery), the robbery rates still were much lower in the first three full years after enactment than in any of the five years before the law. Robbery rates (again with the exception of the single year for Oregon) indicate a bigger drop for each additional year that the law is in effect.

David Olsen and Michael Maltz use county-level data from the Supplemental Homicide Report (SHR). At the time of my initial research with David Mustard, the SHR only provided data at the state level, so we could not use it. Fortunately, the SHR data set has since been improved. The SHR is remarkably rich and includes much more detailed characteristics of the victims and murderers than the FBI's *Uniform Crime Reports* (UCR). Further, county-level data are generally preferable to state level data. When you examine changes in crime rates solely at the state level, you likely miss much of what may be happening within a state.[111] There is no reason to expect that changes in law enforcement or other factors are going to have the same impact on crime in all counties within a state.

The overall drop in homicides that Olsen and Maltz find is roughly similar to what I originally reported using county level UCR data. However, the county level SHR data set does produce different results in terms of how murders are committed and who benefits from gun ownership. Olsen and Maltz's results show that after the passage of concealed-handgun laws, murderers rely much less frequently upon guns to kill people. The results are striking and important: murders with guns fall by 21 percent while non-gun murders actually rise by 10 percent, though this rise is not statistically

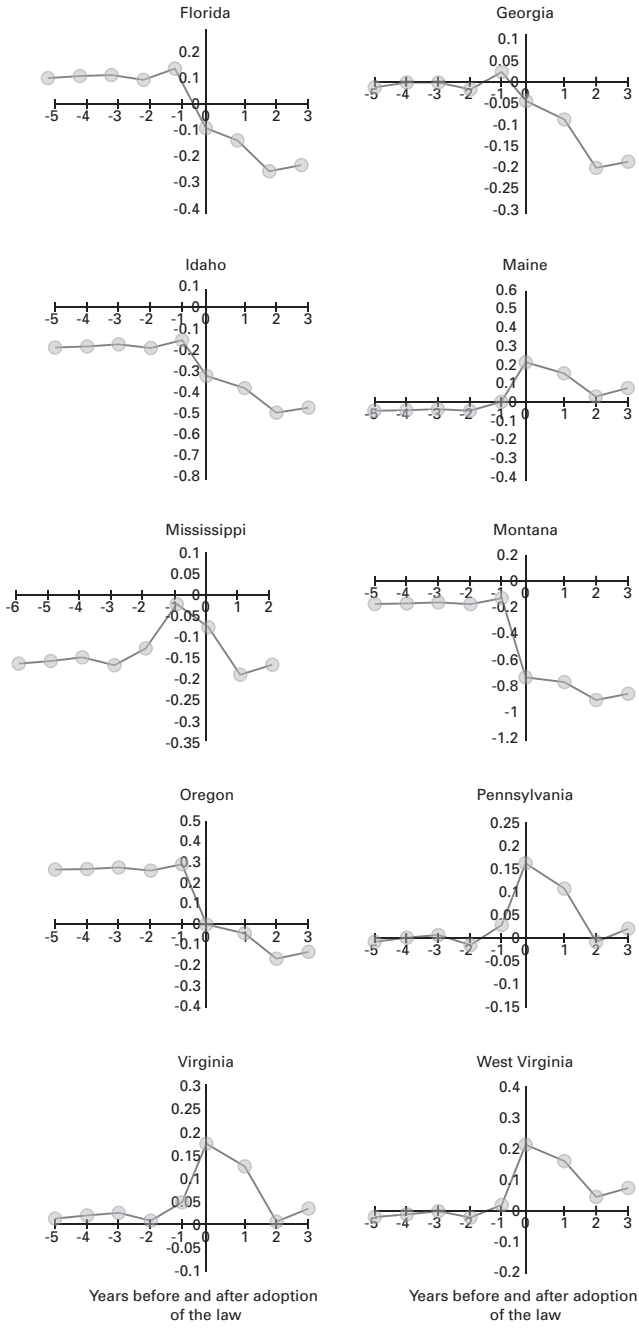


Figure 10.4. Before-and-after trends for murder using Plassmann and Tideman's results

significant. In total, they find that concealed-carry laws lower murder by 7 percent.

Another study presents the effect of right-to-carry laws on police killings. Using data from 1984 to 1996, David Mustard finds that while waiting periods rarely have a significant effect on police deaths, concealed-handgun laws are consistently and significantly related to fewer killings of police. Olsen and Maltz's work dovetails well with Mustard's. If right-to-carry laws result in fewer criminals carrying guns, this might explain why fewer police are murdered.

My paper with John Whitley in 2007 takes into account whether the impact of legalizing abortion on crime altered our results for right-to-carry laws. Using the SHR data from 1980 to 1998, we also find that each additional year that a right-to-carry law was in effect, the murder rate dropped by between 1 and 2.5 percent (with most of the estimates between about 1.9 and 2.5 percent). We also found that legalized abortion led to more single-parent families, which in turn is a well-known cause of delinquent and criminal behavior in children.

The Critics

A number of critics claim that right-to-carry laws have no impact on violent crime. However, there is very little in peer-reviewed professional journals to point to. Even the results that they point to actually hold more evidence that right-to-carry laws reduce crime.

For instance, Mark Duggan claims that my statistically significant results on concealed handguns disappear for several of the violent crime categories when one correctly calculates the statistical significance. However, Duggan has simply misreported his own results and recorded some of his estimated drops in crime as not being statistically significant when in fact they are significant.[112] After those mistakes are corrected, fifteen of his thirty estimates show statistically significant drops in crime, while only one shows a significant increase.

Indeed, all six of his estimates show that right-to-carry laws reduce murder rates, and the results are statistically significant for four of them. These significant reductions occur despite his leaving out all the normal factors that are well known to affect crime rates, such as the arrest rate, the death penalty, prison incarceration rate, poverty, or anything else. These factors are routinely included when economists study crime. In a few estimates, he only accounts for the year-to-year and average county differences in crime rates.

His two estimates that did not yield statistically significant reductions

in murder rates are particularly problematic.[113] In one of them, Duggan compares the murder rate the year before the law with the murder rate the year afterward. However, as shown in previous editions of this book, such an approach gives a very misleading result when crime rates are rising before a law and falling afterward. Obviously, this change in trends, from crime going up to going down, would be consistent with the law's being effective in combating crime. But the before-and-after averages would be the same, meaning that Duggan's calculations would falsely imply the law didn't matter. (Recall the earlier discussion and graphs in chapter 9, critique 4, "Do right-to-carry laws significantly reduce the robbery rate?")

Duggan's second estimate without a statistically significant result stems from his ignoring the "truncation problem," noted above by Plassmann and Tideman, and thus treating counties with no reported murders the same as the others. For example, when most counties have zero murders in any given year, no matter how good the law is, murder rates can't fall any further. But simple randomness can mean that sometimes you will see the crime rate rise from zero even though it had no connection with the right-to-carry laws. In his last set of estimates, his analysis of the different violent crime categories included counties with zero crimes. There are a number of ways to statistically adjust for this problem (Tobit, negative binomials, etc.), but Duggan didn't bother to use these techniques—thus biasing his results against finding a drop in crime.

Let's break down the results from five of the more prominent critical papers. Besides Duggan's work, there are papers by Black and Nagin, Ayres and Donohue (1999), Ludwig, and a book chapter by Donohue (2003) that reproduces the regressions shown in Ayres and Donohue (2003), plus a few more. The results are summarized in table 10.8. Out of 177 estimates reported by these critics, only 7 imply a statistically significant increase in crime after the passage of the law. In contrast, 80 imply no statistically significant change, and 90 imply a statistically significant decline in crime. In other words, half the time these critical studies confirm my results. In only 4 percent of the estimates are the results reversed. And those 4 percent contain numerous problems with their regressions, problems that tend to bias their results against finding a beneficial affect.[114]

Take Black and Nagin's state-by-state breakdown. At the 10 percent level, merely three of their estimates imply a statistically significant increase in crime, twenty-two imply no significant change, and fifteen imply a statistically significant decline.[115] And again, as just mentioned, examining only

Table 10.8 Results on violent crime rates from studies critical of my work

Study	Tables in the study	Finding of reduced crime[a]	Finding of no change	Finding of increased crime
Estimates where there is enough information provided to determine statistical significance:				
Black and Nagin	Tables 1 and 2, national effects	12	8	1
Duggan	Table 12	14[b]	15[b]	1
Ludwig	Tables 4 and 5	0	19	0
Ayres and Donohue	Table 1	30 (27)[c]	13 (16)[c]	0
Donohue	Tables 1–4, examining the period from 1977 to 1997, estimates looking at the average drop in crime or the change in trends	34	25	5
Total		90 (87)	80 (83)	7
	Tables in the study	**Net benefit from reduced crime**		**Net cost from increased crime**
Estimates where there is not enough information provided to determine statistical significance:[d]				
Donohue[e]	Tables 1–4, net benefit or cost estimated by combining both average drop and trends together	4		0
	Tables 5–8, examining the impacts year by year after the law	4		0
Total		8		0

Sources: D. A. Black and D. S. Nagin, "Do Right-to-Carry Laws Deter Violent Crime?" *Journal of Legal Studies* 27 (1998): 209–19; Ian Ayres and John J. Donohue III, "Nondiscretionary Concealed Weapons Laws: A Case Study of Statistics, Standards of Proof, and Public Policy," *American Law and Economics Review* 1 (1999): 436; Mark Duggan, "More Guns, More Crime," *Journal of Political Economy* 109 (2001): 1086; J. Ludwig, "Concealed-Gun-Carrying Laws and Violent Crime: Evidence from State Panel Data," *International Review of Law and Economics* 18 (1998): 239–54; J. J. Donohue, "The Impact of Concealed Carry Laws," in *Evaluating Gun Policy*, ed. J. Ludwig and P. J. Cook (Washington, DC: Brookings Institution, 2003), 287–325.

Note: Values are the national coefficients from the most critical studies listed in footnote 17 of the report.

[a]Some of these negative significant coefficients are a result of the authors' replicating my earlier work. If these were removed, the numbers for negative significant coefficients would be as follows: Black and Nagin, 8; Duggan, 9; Ayres and Donohue, 25 (22); and totals, 42 (39).

[b]Duggan's study has typos mislabeling the statistical significance of two of his results. See column 2 in table 12 (p. 1110) and the results for rape and aggravated assault. For rape a coefficient of –.052 and a standard error of .0232 produce a *t*-statistic of 2.24. For aggravated assault a coefficient of –.0699 and a standard error of .0277 produce a *t*-statistic of 2.52.

[c]Because of downward rounding to 1.6, it is not possible to tell whether the *t*-statistics reported by Ayres and Donohue are statistically significant at the 10 percent level. The values in parentheses assume that a *t*-statistic of 1.6 is not significant at the 10 percent level, while the first values assume that a *t*-statistic rounded off to 1.6 is significant at that level.

[d]Examines the net effect over five years after the right-to-carry law is in effect, for estimates that simultaneously use both a law dummy and trend for the years that the law is in effect.

[e]Calculations taken from Plassmann and Whitley (2003).

Examining the net effect over five years after the right-to-carry law is in effect, for estimates that simultaneously use both a law dummy and trend for the years that the law is in effect.

simple before-and-after averages can be quite misleading, and Black and Nagin, Duggan, Ludwig, and Ayres and Donohue (1999) limit their studies to those averages.

Finally, in a series of papers, none of them peer refereed, Ian Ayres and John Donohue claim that crime rates may possibly rise with the passage of right-to-carry laws. (The book chapter discussed here by Donohue [2003] duplicates the same data and the same regressions.) They provide results for a variety of specifications using data from 1977 to 1997. Their most general results report the relative crime rates by year before and after the adoption of the law and show significant declines in all violent crime categories with patterns that are very similar to those just shown for Plassmann and Tideman.

Nevertheless, Donohue argues that these results provide no evidence that right-to-carry laws reduce violent crime, as the coefficients for crimes like robbery are positive for up to 6 to 7 years after the enactment of the law.[116] But he completely misinterprets his own results. A positive coefficient implies that the crime rates in right-to-carry states are higher than in non-right-to-carry states. But if the coefficient becomes smaller after the passage of the law, as is true here, that means the crime rates in right-to-carry states are falling relative to the crime rates in non-right-to-carry states. The crime rate in right-to-carry states is still higher than in other states, but not by as much as before. Thus, contrary to his own statements, Donohue's study showed that concealed-carry laws actually reduced crime. Figure 10.5 provides fairly dramatic evidence that even Ayres and Donohue's own results show that violent crime rates fall after right-to-carry laws are adopted, and that the drops over the entire period are larger for county- than state-level data. Their results generate similar graphs for the other violent crime categories.

Donohue explains his results this way (pp. 312–13, emphasis added):

> A supporter of the Lott thesis might note that the dummies for the periods more than three years after passage tend to become negative and statistically significant, but in my opinion the coefficient estimates for the dummies lagged beyond three years tend to weaken Lott's case rather than buttress it. . . . The ostensibly growing effect on crime—see the increasingly larger negative numbers after passage in table 8–5—are taken by Lott as evidence that shall-issue laws become more beneficial over time, but something very different is at work. The observed pattern again shows that numerous states *experiencing increases in crime* after passage drop out of the analysis be-

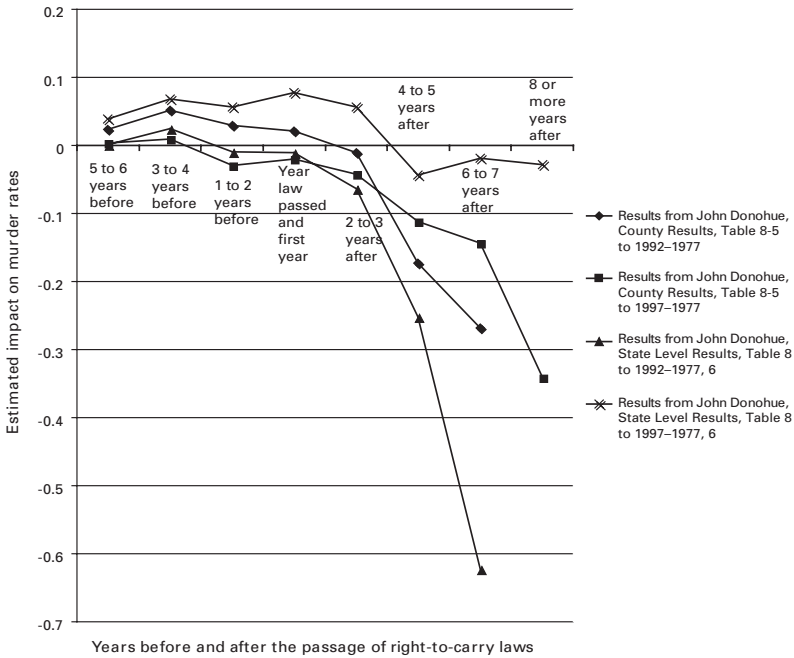


Figure 10.5. Donohue's (2003) estimated impact of right-to-carry laws on murder. From John Donohue, "The Impact of Concealed-Carry Laws," in *Evaluating Gun Policy*, ed. Jens Ludwig (Washington, DC: Brookings Institution Press, 2003), 287–323.

> cause these states' laws were adopted too close to 1997 to be included in the estimate for beyond three years. (Indeed, none of the fourteen shall-issue laws that were adopted after the period for inclusion in Lott's original work affect the estimates of these "after three years" dummies).

As mentioned, Donohue makes a significant mistake here when interpreting his own results. True, the coefficients were positive for some of these estimates in the years immediately after passage of right-to-carry laws. As I explained, however, this simply means that the states that passed right-to-carry laws tended to be states with high crime rates. The crucial point here is that the number of crimes still fell—that immediately after the law was passed, crime rates in right-to-carry states were still higher than in other states but by a smaller amount. As the crime rates in right-to-carry states continued to fall, they eventually fell below the crime rates in non-right-to-carry states, and that is when the coefficients become negative. Thus, Donohue's own results clearly show that right-to-carry laws reduce crime.

Is There an Initial Increase in Crime? How Not to Fit a Line to a Curve

While we do not want to overstate the strength of the conclusions that can be drawn from the extremely variable results emerging from the statistical analysis, if anything, there is stronger evidence for the conclusion that these laws increase crime than there is for the conclusion that they decrease it.

—Ian Ayres and John Donohue, *Stanford Law Review*, 2003, p. 1202

Only one article, by Ayres and Donohue who employ a model that combines a dummy variable with a post-law trend, claims to find that shall-issue laws increase crime. However, the only way that they can produce the result that shall-issue laws increase crime is to confine the span of analysis to five years. We show, using their own estimates, that if they had extended their analysis by one more year, they would have concluded that these laws reduce crime.

—Carl Moody and Thomas Marvell, *Econ Journal Watch*, September 2008, p. 291

The above quotes speak for themselves. Donohue's claim that crime rates initially spike up after right-to-carry laws are adopted is made frequently in Ayres and Donohue's research. We have already discussed many issues such as why looking at simple average crime rates before and after the right-to-carry laws go into effect can lead to highly misleading conclusions on how crime rates are changing over time (p. 216). But there is nothing magical about adding simple before-and-after crime rate trends to these before-and-after averages. Indeed, the best way of analyzing the data is to simply look at how the crime rates change year by year. The reason to look at trends is to allow a simple statistical test to see if the before-and-after trends differ from each other.

Ayres and Donohue added a twist to their tests by combining the before-and-after averages with the before-and-after trends, what they call their "hybrid" estimate. There is nothing inherently wrong with this—after all, it is just another way of summarizing the patterns in the data. Nevertheless, one has to be careful that the actual changes in crime rates fit the pattern implied by whatever approach we are using.

Ayres and Donohue argue that when right-to-carry laws are adopted, there is an initial increase in crime and then a gradual decrease. But since none of the year-by-year estimates for violent crime show this initial jump in crime when the law starts, how do Ayres and Donohue reach this conclusion? The answer: It is really just an artifact of how they tried to fit a straight line to a curve.

Take a look at figure 10.6. Ayres and Donohue claim that crime rates are

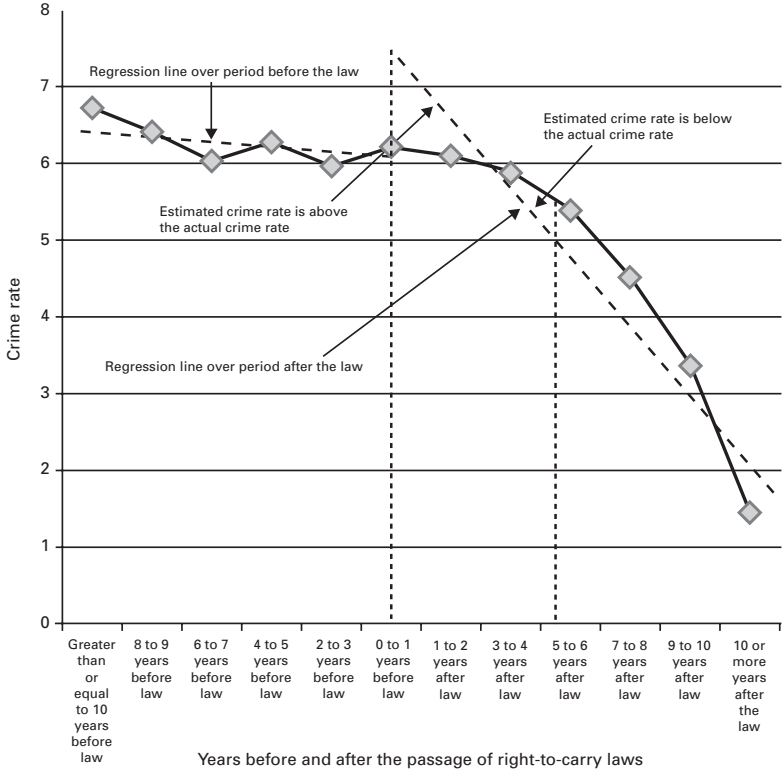


Figure 10.6. Fitting a line to crime rates that are nonlinear

very slightly declining up until the right-to-carry law is passed and then they start to fall much more dramatically. Suppose that you wanted to fit two lines to the figure (say, the two dashed regression lines in the diagram). One straight line shows how the crime rate changes in the years before the law, and one straight line shows how it changes after the law. The first line is very easy to fit. The second one requires some arbitrary choices. The way Ayres and Donohue choose to position this line is so that it goes right through the middle of the curve for the after-law crime rates. An alternative would have been to have this second line start where the first one had finished (the approach that I had taken in the first and second editions in looking at before-and-after trends). This "predicted" crime rate line for the after-law period thus lies above the true crime rate immediately after the law, falls below the actual crime rate when you get out to year 4 after the law, and then again lies above the actual crime rate when you get out past year 9.

The key is that this "predicted" crime rate does not remotely resemble

how the actual crime rate is changing. All this might not have been much of a problem if Ayres and Donohue had tried to reconcile these results with their reported year-by-year estimates (as I discussed at the end of the section "Other Research on Guns and Crime"). But they also made another mistake. Oddly, they used data for states that had these laws in effect for over a decade in order to estimate this after-law regression line, but then they only looked at the net change in crime rates for the first five years that the law was in effect. This only matters because for three of those first five years they are overpredicting the crime rate. Limiting the time that you examine to a period when you are greatly overpredicting the crime rate is what causes the large upward bias in their estimates of how right-to-carry laws alter crime rates.

Steve Levitt has claimed: "When the original Lott and Mustard (1997) data set is extended forward in time to encompass a large number of additional law enactments, the results disappear (Ayres and Donohue, 2003)."[117] That is wrong. It was not because they added additional years of data that these results disappear. The second edition of this book examined crime rates from 1977 to 1996, and the Ayres and Donohue paper added only one year, 1997, to the data set that I had given them. Adding one year of data onto twenty years didn't make a difference in the results. The issue is how they fitted a straight line to crime rate data that weren't straight.

This is where Carl Moody and Ted Marvell enter the debate. Moody and Marvell point out that even using Ayres and Donohue's own estimation over the period of time that they chose, summing the net effects over the first six years shows an overall benefit from the law.

So what is the moral of the story? When you know that the underlying data don't show an initial increase in crime but the simplified method that you are using to test the changes in crime rates implies there is, you had better go back and figure out what you did wrong.

Multiple-Victim Public Shootings

Finally, we should discuss the research by Grant Duwe, Tom Kovandzic, and Carl Moody on multiple-victim public shootings.[118] The three authors claim that the drop in multiple-victim public shootings after right-to-carry laws are adopted is not statistically significant. The problem is that they only looked at a very small subset of attacks, those that left four or more victims killed. Indeed, my earlier work with Bill Landes had also not found a statistically significant result for that one type of attack, but the reason is simple—this way of defining the dependent variable greatly reduces the

number of public shootings to just thirty-six incidents over the entire 1977 to 1995 period.[119] Landes and I had examined a range of different types of attacks: two or more murders, three or more murders, two or more injuries or murders, three or more injuries or murders, and four or more injuries or murders. And the results consistently found that right-to-carry laws reduced the number of attacks and the number killed or injured.

Conclusion

In light of all the studies of right-to-carry laws, it is remarkable *that not a single refereed academic study by economists or criminologists* has found a bad effect from these laws. It's also noteworthy that no one has challenged the results that I have gotten for all the other gun-control laws that I have studied in this book.

The Continuing Debate

Clearly, the weight of research in refereed scholarly journals indicates that the basic results have been replicated, which is a central scientific criterion for evaluating an argument. Critics of the more-guns-less-crime thesis have not been content, however, to limit themselves to whether the basic findings stand up against legitimate examinations by others. Instead, they have sought to find chinks in the armor. When even that has not succeeded, they have engaged in misrepresentations and the casting of aspersions. To be blunt, the debate, such as it is, has unfortunately become personalized rather than sticking to the merits of the case—on which my opponents have no case to make. This section will take up some of the issues raised by critics seeking any way they can to call into question the findings both I and others have made.

Can We Trust the Data? Are the County-Level Crime Data Unusable?

Due to problems in the reporting of [county] crime data there are many gaps that need to be filled. The organization that prepared the data filled these gaps using two different estimation (i.e., imputation) procedures; moreover, the change in these procedures was not recognized by and incorporated into the MGLC analysis. Thus, there are so many problems with the county- level crime data sets used in MGLC that its analyses are called into question. We note, however, that the second edition of *More Guns, Less Crime* (Lott, 2000) includes state- and city-level analyses, which are not subject to this particular problem. . . . Not all police agencies provide 12 months of crime data to the FBI . . . And some agencies may not fill out crime reports simply because they rarely have any crime to report. [The

> FBI only tries to fill in these gaps for data at the] state, regional, and national level. . . . At this point, county-level crime data cannot be used with any degree of confidence.
> —Michael D. Maltz and Joseph Targonski, "A Note on the Use of County-Level UCR Data," *Journal of Quantitative Criminology*, September 2002, pp. 298–99

Maltz and Targonski have a point—there is missing information in the county level-data. But their conclusion "that county-level crime data, as they are currently constituted, should not be used, especially in policy studies" is unjustified. While people who work with data might not want to admit it, all data have errors in them. For example, per capita income in a regression doesn't perfectly measure people's income; people do not report all their income to the government, and there are often typos in the recording of various numbers. These figures are useful, but they are not a perfect measure. Nor does the measure of poverty perfectly evaluate poverty. This is true for every factor that we examine.

Thus, pointing out the mere existence of measurement errors isn't enough. Errors can work to artificially weaken or strengthen results, but normally, if they are random, they simply make it difficult to find any relationship that might exist. But Maltz and Targonski do not ask whether any errors actually affected my results, and that is the key question. The fact that similar results are obtained for city and state data should provide confidence that any problems are minimal. Maltz and Targonski do acknowledge in their article that the "state- and city-level analyses . . . are not subject to [the problem]" for counties.[120] Indeed, one reason why I used city-, county-, and state-level data in the second edition of the book was specifically to double-check that the results are not sensitive to any particular errors in the data.

Fortunately, for each county the FBI provides information on the size of this recording error. Perhaps unsurprisingly, rural counties with few people and small police departments tend to have the most problems reporting their crime numbers accurately. Research that John Whitley and I published found that studying only the counties with the fewest errors produced stronger evidence that right-to-carry laws reduce violent crime.

For example, eliminating those states where at least 20 percent of their counties have unreported crime numbers for cities with at least 30 percent of those county's populations implied that for each additional year after right-to-carry laws were introduced all the violent crime rates fell: murder by 4.8 percent, rape by 1 percent, robbery by 3.5 percent, and aggravated assault by 0.5 percent. The drops for murder, rape, and robbery are statistically

significant.[121] Eliminating those states with even smaller levels of error also continued to imply similar drops in violent crime.

Even if county-level data have more errors, there are still trade-offs. For example, Maltz and Targonski acknowledge the point first made in this book, that "aggregating statistics over an entire state can mislead the true nature of a state's population characteristics."

Unfortunately, my other critics have completely ignored the strong results that I obtained from the city-level data that were shown in chapter 9.[122]

Do the Sales of the Fourth-Largest Gun Magazine Accurately Measure Gun Ownership?

My findings demonstrate that changes in gun ownership are significantly positively related to changes in the homicide rate, with this relationship driven almost entirely by an impact of gun ownership on murders in which a gun is used. The effect of gun ownership on all other crime categories is much less marked.

—Mark Duggan, "More Guns, More Crime," October 2001[123]

Other critics have sought other sources of data to test my findings. Mark Duggan's argument would be troubling were it based on gun survey data or actual gun ownership. But Duggan simply relies on sales of the fourth-largest gun magazine, *Guns and Ammo*, as a proxy for ownership—that is, he assumes that sales are connected to gun ownership—and finds that when magazine sales change, murder rates change. The magazine that he used happens to be the only one for which increased sales correspond to increased crime (see table A7.1).

Duggan claimed that he focused on this one magazine for two reasons: "*Guns & Ammo* is focused relatively more on handguns than [*American Rifleman*, *American Hunter*, and *North American Hunters*]," and "sales data for this magazine are available annually at both the state and the county levels." But other magazines better meet those criteria, such as the two largest exclusively handgun-oriented magazines, *Handguns Magazine* and *American Handgunner*. These magazines also have county and state sales figures.

There is a simple reason why *Guns and Ammo* gives such a different result from other gun magazines. It was the only one of the top seven largest gun magazines that experienced a drop in sales during the 1990s.[124] Its drop was smaller than the increase in the other six magazines. During 2001, Skip Johnson, a vice president for *Guns and Ammo's* parent company, Primedia, told me that in the 1990s anywhere from 5 to 20 percent of its national sales in a particular year were purchased by the magazine itself in order to meet its guaranteed sales to advertisers.[125] The copies were then given away for free to dentists' and doctors' offices.

But importantly, Johnson noted that these magazine self-purchases were not distributed in all states or counties, nor were they done randomly. These self-purchased copies were distributed in areas where the magazine thought that gun purchases were going to increase, and this included areas where they thought that crime rates were going up. Thus, it is not very surprising that *Guns and Ammo* is the one magazine whose sales are associated with more homicides. In statistical language this is known as a spurious relationship.

Did Crack Cocaine Confound the Results?

But an alternative explanation is that the crack cocaine problem drove up crime. . . . The regression would identify a relationship between higher crime and the failure to adopt a shall-issue law when the real cause would have been the influence of crack.
—Ian Ayres and John Donohue, *Stanford Law Review*, 2003[126]

One of Ayres and Donohue's greatest concerns is the apparent failure of previous research to account for the differential geographic impact of cocaine on crime. Lott's book (and the Lott and Mustard paper) reported that including price data for cocaine did not alter the results. Using yearly county-level pricing data (as opposed to short-run changes in prices) has the advantage of picking up cost but not demand differences between counties, thus measuring the differences in availability across counties. Research conducted by Steve Bronars and John Lott examined the crime rates for neighboring counties . . . on either side of a state border. When the counties adopting the law experienced a drop in violent crime, neighboring counties directly on the other side of the border without right-to-carry laws experienced an increase. . . . Ayres and Donohue argue that different parts of the country may have experienced differential impacts from the crack epidemic. Yet, if there are two urban counties next to each other, how can the crack cocaine hypothesis explain why one urban county faces a crime increase from drugs, when the neighboring urban county is experiencing a drop? Such isolation would be particularly surprising as criminals can easily move between these counties. . . . Even though Lott gave Ayres and Donohue the cocaine price data from 1977 to 1992, they have never reported using it.
—Florenz Plassmann and John Whitley, *Stanford Law Review*, 2003

The elephant in the room was crack cocaine. The states that did not pass the right-to-carry laws were states that had a big problem with crack-cocaine which had an enormous influence in running up crime.
—John Donohue, "Do Guns Reduce Crime?" debate on National Public Radio, November 5, 2008[127]

One persistent criticism of my work has been that it did not account for an alternative explanation, namely, that crack cocaine explains increases

and variation in crime rates, especially murder. Alas, as Plassmann and Whitley note, my critics fail to discuss or even recognize the efforts that David Mustard, others, and I have made to address the drug question. In my own mind, those who have found that right-to-carry laws lower crime rates have shown that the results are not affected by the impact of crack cocaine. The critics have also not done anything themselves to try to show how crack cocaine might explain the results. It is one thing to say that they disagree with how I have dealt with the issue, but to keep asserting that the drug problem has not been accounted for is annoying.

This chapter adds new material to our argument, using new data from Steven Levitt and other academics that purports to measure crack cocaine use.[128] There are some serious questions regarding whether the data actually measure cocaine use. Given how much Levitt's coauthors emphasizes DC's crime problems, it is also surprising that their data ignore DC. (DC's crime problems will be discussed in the next section.) The data are also unexplainably limited to the period from 1980 to 2000. Nevertheless, accounting for their measure of crack cocaine use does not reduce the estimated benefits of right-to-carry laws on crime.

Sensitivity of the Results

The results of the state data regressions were incredibly contingent on an array of factors, such as the inclusion or omission of rather innocuous right-hand side controls. We found a similar fragility with regard to these county data regressions. Indeed, as we were double-checking our results, we discovered that we had omitted one of Lott's original controls: Percentage of County Population That Was Not Black or White but Was Male and Aged 20–29; and we were surprised to find that adding this extremely innocuous demographic variable decreased our estimate of the dollar impact that the law's passage had on crime (that we are about to report) by more than twenty percent.

—Ayres and Donohue, *Stanford Law Review*, 2003, p. 1281

"Using a more customary set of demographic controls would have reversed Lott's results."

—John Donohue, "Can You Believe Econometric Evaluations of Law, Policy, and Medicine?" October 16, 2008

How sensitive the results are to the inclusion or exclusion of specific variables or changes in the values of certain variables is important. For were it the case that reasonable changes in the specifications reversed or eliminated the findings presented in this book and elsewhere, it would undermine the

argument that more guns lead to less crime. However, the earlier editions of this book have run thousands of estimates to see if the results change with different combinations of control variables. For example, figure 9.11 on page 188 summarizes the results of 20,480 regressions, which took several months to estimate. The results for murder demonstrate that passing right-to-carry laws causes drops in that crime ranging from about 5 to 7.5 percent, a substantial 40 percent range. There should be nothing surprising or troubling about the 20 percent variation that Ayres and Donohue point to, and it is smaller than the range that I show.

The debate really shouldn't be whether right-to-carry laws reduce crime rates by 4 or 5 or 6 or 7 percent. The debate should be over whether the laws reduce or increase crime. Of course, it is better to have larger drops, but as long as crime rates decline, right-to-carry laws protect potential victims.

It makes little sense to study crime rates without trying to account for the many factors already known to affect crime. But as we showed earlier, the empirical results for murder, even those using state-level data, are not sensitive (when properly analyzed) to eliminating even factors that should obviously be included, such as demographics or law enforcement.

Perhaps not too surprisingly, I can't find any work by Ayres, Donohue, or Levitt that subjects their own work to the types of specification searches that they require my work to stand up to. In none of their papers has any of them tried to run all possible combinations of the different factors that might explain crime rates.

But others beside myself have looked at the sensitivity of my results, too. William Bartley and Mark Cohen examine well over sixteen thousand possible combinations of the control variables. Later works by Bruce Benson and Brent Mast, Carl Moody, and Eric Helland and Alex Tabarrok have attempted to test how sensitive the results are in many different ways. Like myself, they have found the results remarkably consistent. As shown earlier in figures 10.3a–10.3i, redoing the possible combinations of the control variables with the data from 1977 through 2005 proved no different.

The National Academy of Sciences Report

So far the debate has focused on technical issues, including the reliability of the data and the sensitivity of the statistical analysis. But there is also a political side. The importance of the right-to-carry debate eventually led the Clinton administration to set up a National Academy of Sciences panel to investigate the relationship between firearms and violence. When the NAS undertakes an examination of a pressing issue, its findings have an

authority that is distinctive—based on perceptions of unbiased and fair evaluation of all sides of an issue leading to a consensus among leading experts. Unfortunately, the 2004 National Academy of Sciences (NAS) report on Firearms and Violence[129] has been misrepresented.

Consider the following radio debate:

> John Donohue (Yale University): The National Academy of Sciences convened a panel of talented experts who spent two years looking at John Lott's work, Gary Kleck's work. . . . They concluded the scientific evidence does not support the more guns, less crime proposition. The lone dissenter was someone who was not an econometrician, who admitted in his dissent that he wished he knew more econometrics, and who had previously testified as an expert witness on behalf of the *execrable* NRA. . . . But, again, this is exactly what the National Academy of Sciences looked at. And, they concluded the opposite, that the data did not—
>
> Gary Kleck (Florida State): They did not conclude the opposite.
>
> John Donohue: They concluded that the data does not support the proposition that we're debating today which is that guns reduce crime.
>
> Gary Kleck: No, actually what that report persistently said was, we don't have strong enough evidence to draw firm conclusions about virtually every issue they addressed, so, that was more of a no-decision decision than it was reaching the opposite conclusion, they did not reach the conclusion that making it easy to get a carry permit increases crime. They did not conclude that John Lott was wrong, and basically, you know, you learn nothing from what that particular panel said.
>
> John Donvan (ABC News): I've read the same report and I have to say, Gary, that I read it the same way, actually, it was a bit of a Pontius Pilate moment that didn't know who was right or who was wrong.[130]

Kleck and Donvan were correct that Donohue mischaracterized the 2004 NAS report on firearms and violence.[131] Contrary to Donohue's account, the report actually concluded, "The data available on these questions are too weak to support unambiguous conclusions or strong policy statements." The majority of the panel advocated that more money be available to academics to fund additional research.

James Q. Wilson, the panel's "dissenter," is possibly America's most preeminent criminologist, and he vigorously denies the claim that the NRA ever hired him as an expert witness.[132] Wilson, who had previously served on four similar panels, concluded: "I find that the evidence presented by Lott

and his supporters suggests that [right-to-carry] laws do in fact help drive down the murder rate." He argued that the NAS panel simply "show[ed] that different authors have reached different conclusions." Wilson wanted a more critical inquiry, one "to analyze carefully not only the studies by John Lott but those done by both his supporters and his critics. Here, only the work by Lott and his coauthors is subject to close analysis."[133]

Are Those Who Possess a Gun More Likely to Be Shot in an Assault Than Those Not Having a Gun?

People who carry guns are far likelier to get shot—and killed—than those who are unarmed, a study of shooting victims in Philadelphia, Pennsylvania, has found.

—Ewen Callaway, "Carrying a Gun Increases Risk of Getting Shot and Killed," *New Scientist*, October 6, 2009

A study by Charles Branas and coauthors looked at people who had been shot during assaults in Philadelphia between October 15, 2003, and April 16, 2006.[134] The data for this test consists of a "case sample" (677 victims who had been shot in an assault) and a "control" group (684 "matched" individuals who were the same sex, race, and age as those assaulted). The authors conclude that "on average, guns did not protect those who possessed them from being shot in an assault" and that successful defensive gun uses are unlikely. The study, though, suffers from the exact same problems that plagued Arthur Kellermann's work, as we noted early in chapter 2. It is analogous to the hypothetical test we discussed there of whether sick people who have been to a hospital are more likely to die than healthy individuals who never felt the need to go to a hospital to begin with. Presumably that wouldn't be taken as evidence that going to a hospital increases the probability of death. If those who are most likely to be assaulted are more likely to own a gun, it also explains the Branas claim. Guns could make those who own them safer, but not as safe as those who weren't and didn't feel the need to own a gun to begin with.

The "case study" approach makes sense for testing the efficacy of drugs where you are able to randomly determine which patients receive the drug and which receive the placebo. But gun ownership isn't detemined randomly. It is the reason why economists look at changes in people's behavior that occur because of forces beyond their direct control, such as the costs of obtaining a gun. For example, if you institute a ban on handguns, some people who would have owned a gun no longer do so, and researchers can examine the impact that such a change has on crime rates.

As to the claim that successful defensive gun uses are unlikely, the study really provides no evidence on that, because "successful" defensive gun users may completely avoid having to go to police as assault victims. If criminals run away after potential victim brandish their handguns, the events would never be included among their sample of gunshot victims. What one needs to answer this question are surveys on defensive gun use that attempt to identify people who were threatened with crime (not necessarily that they were victims) and that their response protected them. Indeed, the published academic refereed research to make such direct comparisons has found that victims who resisted with guns were less likely to be injured that those who did not.[135]

Data and the Truth

Fundamentally, any academic debate boils down to two issues: how good the data are and whether the tests were done correctly. In capsule form, these issues have been succinctly stated by Steven Levitt and Stephen Dubner in their best-selling *Freakonomics*:

> Then there was the troubling allegation that Lott actually invented some of the survey data that support his more-guns/less-crime theory. Regardless of whether the data were faked, Lott's admittedly intriguing hypothesis doesn't seem to be true. When other scholars have tried to replicate [Lott's] results, they found that the right-to-carry laws simply don't bring down crime. (Pp. 133–34)

As noted above, Levitt's suggestion that my work has not been replicated is totally wrong. Survey data were not even used in my regressions and thus could not have affected the results. In any case, the fact that results have been replicated by those who have put the data together themselves indicates that data could not have been faked. Unfortunately the debate has not stayed on the merits.

Steven Levitt has been one of my most fervent critics for some time, and the above quote from his book was just one of his many attacks on my work. As a result of these allegations, I filed a defamation lawsuit against Levitt and his publishers. The evidence in the lawsuit included a series of e-mail messages exchanged between Levitt and economist John McCall, during which Levitt incorrectly stated that I had "stocked" with only my supporters a conference examining the more-guns-less-crime thesis, that I then had to "buy an issue" of the highly prestigious *Journal of Law and Economics* to publish the results of the conference, and that there was no peer review.[136]

As a result of this litigation, however, Levitt was required to release a letter retracting and clarifying the allegations he made. In what the *Chronicle of Higher Education* called "a doozy of a concession," Levitt conceded that he "knew that scholars with varying opinions had been invited to participate" in the conference because "I received an email from Dr. Lott inviting my own participation." In addition, Levitt acknowledged that I did not engage in "bribery or otherwise exercised improper influence on the editorial process" in the special issue and that "the articles that were published in the Conference Issue were reviewed by referees engaged by the editors of the JLE [*Journal of Law and Economics*]. In fact, I was one of the peer referees."[137]

Nonetheless, charges of faulty data have continued to be made. For example, in his recent book, a coauthor of Levitt's, Ian Ayres, writes that "we found that Lott had made a computer mistake in creating some of his underlying data. For example, in many of his regressions, Lott tried to control for whether the crime took place in a particular region (say, the Northeast) in a particular year (say, 1988). But when we looked at his data, many of these variables were mistakenly set to zero. When we estimated his formula on the corrected data, we again found that these laws were more likely to increase the rate of crime."

One might think from Ayres's comment that there is a mistake in my work with David Mustard or in the earlier editions of this book. However, the research that Ayres is really discussing was not a paper of mine, but a paper published by Florenz Plassmann and John Whitley in the *Stanford Law Review*. Plassmann and Whitley thank me for helping them with their study ("We thank John Lott for his support, comments and discussion"), but this is their own paper, published under their own names.

As to the data mistake, Plassmann and Whitley used the data presented in the second edition of this book, covering the years from 1977 to 1996. There was nothing wrong with those data. The data entry mistakes for their paper arose in the years that were added when they extended the data from 1997 to 2000. Out of over 7 million data entries, about 180 had accidentally been left blank. The significance of some results in one of their tables (table 10.3A) was decreased. Despite the tenor of the critics, this data error was not even crucial for their findings. Further, Plassmann and Whitley had explicitly noted the results in that particular table were biased against finding a decrease in crime, and they had argued that those results should not be given much weight.

Correcting the small data entry errors did not alter the results that Plassmann and Whitley said were the focus of their paper, and their conclu-

sion was still the same: more concealed-handgun permits reduce crime. And Plassmann and Whitley had excellent reasons for the estimates that they thought should be used: "Including all counties with zero crime rates [with the statistical test that Ayres and Donohue use] will bias the estimated benefit of the concealed handgun law towards finding an increase in crime, because no matter how good the law is, it cannot lower the crime rate below zero." Unfortunately, Ian Ayres and John Donohue's responses to Plassmann and Whitely completely ignore these arguments on the proper test as well as the estimates they provide.

This is not the only incorrect claim that Ayres and Donohue make. They have—repeatedly—falsely asserted that I withdrew from coauthorship with Plassmann and Whitley once I saw their criticism.[138] Ayres and Donohue claimed that "we hope that this indicates that the arguments in our Reply have caused the primary proponent of the more guns, less crime hypothesis to at least partially amend his views." It is quite an amazing slur against Plassmann and Whitley to claim that they insisted on publishing research despite a coauthor's withdrawing from the paper over errors.

The inaccuracy of Ayres and Donohue's claim was such that the *Stanford Law Review* felt it necessary to run a very unusual "Clarification," where the editors said that they might not have originally made things sufficiently clear to Ayres and Donohue and noted that "the Editors feel that the impression that some have gotten from Ayres and Donohue's Reply piece is incorrect, unfortunate, and unwarranted."[139]

Being a target of inaccurate accusations has been an unfortunate and unpleasant experience. It certainly would have been preferable if the debate had stuck to the data and their analysis. The hypothesis that more guns connects to less crime has stood up against massive efforts to criticize it.

Fewer Guns, More Crime

If a resident has a handgun in the home that he can use for self-defense, then he has a handgun in the home that he can use to commit suicide or engage in acts of domestic violence. If it is indeed the case, as the District believes, that the number of guns contributes to the number of gun-related crimes, accidents, and deaths, then, although there may be less restrictive, less effective substitutes for an outright ban, there is no less restrictive equivalent of an outright ban. . . . In my view, there simply is no untouchable constitutional right guaranteed by the Second Amendment to keep loaded handguns in the house in crime-ridden urban areas.

—Justice Stephen Breyer, dissenting in *District of Columbia v. Heller*, June 26, 2008[140]

The Supreme Court was sharply divided in its recent 5–4 decision striking down DC's handgun ban as unconstitutional. While in the minority, Justice Breyer's strongly worded dissent represented many people's concern about guns. To him, the Second Amendment to the Constitution did not guarantee an individual's right to own a handgun, but even if it did, he believes that such a right could be overridden by the public interest of reducing gun crimes and suicides. The possible harm from guns was central to his dissent, and the words "crime," "criminal," "criminologist," "homicide," "murder," "rape," "robbery," and "victim" were used a total of 109 times in forty-four pages. The term "suicide" was used thirteen times.

Despite the Supreme Court's decision, DC is still fighting to make it extremely difficult for people to own handguns.[141] Nor is the court's decision just one of historical curiosity. As of this writing, the gun ban litigation question has moved on to Chicago and its ban on handguns. A federal district court has upheld Chicago's ban, and an appeals court will next look at the case.[142] When the Chicago case eventually goes before the Supreme Court, it is going to be important whether gun bans are linked to crime and suicides. Studying the effects of recent bans is also useful because it represents the simplest test for the more-guns-less-crime hypothesis.

Gun bans are not imposed just on cities or nations. There are bans for everything from public housing to city parks or schools and universities. Seattle Mayor Greg Nickles announced late in 2008 that he was moving to ban guns on city property, at sporting events, and street fairs.[143] Court cases have recently been filed over gun bans in places from Western Oregon University to public housing in San Francisco.[144] Given all these pushes for gun bans, a systematic discussion across many places that have instituted them seems long overdue.

Comparing DC's Murder Rate to Other Places

So what is the evidence? DC's handgun ban policy has had ample time in the thirty years since it became effective in February 1977 to reveal any beneficial effects. Yet, looking at the data, there is absolutely no evidence that DC's gun ban reduced murder rates.[145] Indeed, there is only one single year after the ban started that the murder rate is below what it was in 1976. The bad crime figures after 1977 cannot be explained away by any general increase that has been occurring in other large cities, the neighboring states of Virginia and Maryland, or the United States generally.

In 1976, DC's murder rate was fifteenth among America's fifty most populous cities. In only one of the years after the ban (1985) did DC rank as low

as fifteenth. In fifteen out of the next twenty-nine years after the ban, DC's murder rate had risen to first or second place; in another four years it was fourth. DC's murder rate rose relative to the average city murder rate.

Of course, there are many reasons for crime rates to change. For example, DC's police department underwent many changes, and some were likely detrimental, such as instituting new rules for hiring and promotion.[146] Changes in income and poverty can also matter. It is simple enough to see how DC's crime rates change over time relative to the rest of the country in our regression analysis, with all the factors that we have already talked about accounted for. This is particularly important given the demographic and income changes that were occurring in DC. Running a regression accounting for all these other factors, including demographics, reveals that each additional year that the DC ban was in effect saw DC's relative murder rate rise by 6 percent, and the effect is statistically significant at better than the 1 percent level.

Three simple graphic comparisons show how DC's murder rate rose: DC's murder rate relative to other large cities, as just mentioned, DC's murder rate relative to neighboring Maryland and Virginia, and DC's murder rate relative to the rest of the United States.

Justice Breyer's dissent put a great deal of emphasis on a study published seventeen years earlier in the *New England Journal of Medicine*. This study by Colin Loftin, David McDowall, Brian Wiersema, and Talbert Cottey compared the mean homicide rates before and after the ban.[147] They looked at the period from 1968 through 1987 and claimed that the handgun ban lowered homicide and suicide rates. But there are real questions about how they did their analysis, and we are now fortunate enough to have more and better evidence of what happened after the ban.

Our primary focus will be on murders, not homicides, since homicides include justifiable killings by police and civilians. A drop in civilian justifiable homicides after the handgun ban should actually be viewed as a bad sign because fewer crimes would have been prevented.

THE FIFTY LARGEST CITIES. Figure 10.7 shows how DC's murder rate changed over time relative to the other forty-eight largest cities without a ban on handguns. (Chicago was thus excluded because it was the only other major city among the fifty most populous cities that also banned handguns starting in 1983, and we want to compare cities with a ban to cities without a ban.) City-level data from the FBI are only readily available from 1974 on, so that is the period we start with. During the three years from 1974 to 1976, DC's

murder rate averaged 28.5 percent more than the other cities. In 1976, the last year before the ban, its murder rate was not nearly as high, 16 percent greater than the average murder rate for the other large cities. The vertical line in the diagram shows the point at which the ban went into effect, at the beginning of 1977. After the ban, DC's murder rate between 1977 and 1987 averaged 55 percent greater than the average for these other cities, immediately rising to 50 percent above the average and only getting as low as 29.4 percent greater once, in 1985—there is not one single year after the ban when the ratio of DC's murder rate to the average for other cities fell below the preban ratio. Even if we limit ourselves to the period studied by Loftin and his coauthors, it is clear that murder rates rose after the ban.

Extending the data out past 1987 shows how DC's murder rate explodes above the rate in the rest of the cities (figure 10.8). After the ban from 1977 to 2005, the murder rate in DC was on average 91 percent greater than in the other cities. But two criminology professors, James Alan Fox and David McDowall, state:[148]

> [Others have] argued that the rise in violent crimes in the District from 1980 to 1997 establishes that the DC Gun Control Law was ineffective. However, the entire nation experienced an increase in violent crimes during this period because of the emergence of the crack cocaine market and related gang activity.

This is hardly a unique perspective. John Donohue recently claimed:[149]

> John [Lott] mentions what happened in the District of Columbia and it is true that DC did have quite a problem with crime in the late 1980s as did almost all urban areas in the United States because of the crack cocaine problem. Nobody thinks that the crack cocaine problem was a problem caused by a lack of guns, and simply as John does so much in his work where he is a—looking at data in a way that can support a very tendentious conclusion.

Yet DC's murder rate increased relative to other cities even before crack cocaine became an issue in the last half of the 1980s. In addition, crack cocaine affected cities nationwide, and, after 1987, DC's murder rate still increased dramatically relative to the murder rate in other cities. While the crack cocaine epidemic clearly increased DC's murder rate, it is hard to see how cocaine can explain DC's increase in murder rates relative to all other cities either any time from 1977 to 1987 or afterward. DC has continued to

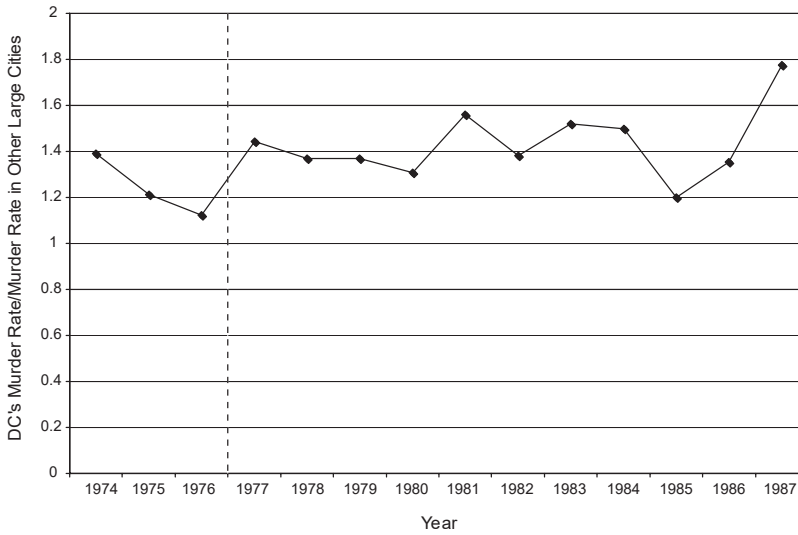


Figure 10.7. Changes in DC's murder rate relative to the other forty-eight largest cities (excluding Chicago from top fifty list, weighted by population)

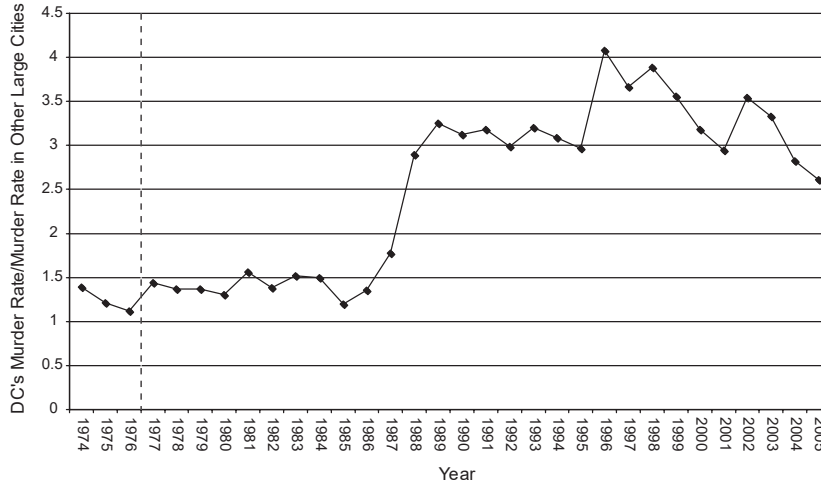


Figure 10.8. Changes in DC's murder rate relative to the other forty-eight largest cities (excluding Chicago from top fifty list, weighted by population)

get worse and worse relative to other cities even since the crack epidemic abated in the early 1990s. DC's murder rate fell much more slowly than other cities. As late as 2007, it was still almost two times that of the average of other large cities.

But to be careful to avoid other possible explanations for DC's rise in murder rates, including crack cocaine, we can re-examine the murder rate regressions and limit the period studied to 1977 to 1987. Would doing so alter our results of the handgun ban's detrimental effects? No, actually the results become even more dramatic—implying that each additional year that the ban was in effect DC's murder rate rose relative to the rest of the United States by 29.9 percent.

If crack was the cause of DC's higher murder rate after 1987, it was a problem that seems to have affected DC dramatically more than other large cities. DC's murder rate not only rose relative to other cities in the late 1980s, but also stayed much higher. How can the crack cocaine problem, which was a national problem affecting many cities, explain DC rising from having the fifteenth highest murder rate in 1976 to place number 1 almost continually from 1988 to 1999 (the exception was only three years from 1993 to 1995, when placed second)? Even if we only concentrated on this later period, their argument would have us believe that crack cocaine dramatically changed DC in a way that it changed no other city.

COMPARISON TO MARYLAND AND VIRGINIA. Perhaps there were regional effects of some nature that influenced crime generally around the DC or mid-Atlantic area. In such a case, the worsening ranking for DC might not be due to gun control, but to the general regional decline. To examine this, we can compare DC and the two states that surround it, Maryland and Virginia. Figure 10.9 examines the period from 1968 through 1987. In the last year before the 1977 ban, DC's murder rate was 197 percent greater than the average murder rate in Maryland and Virginia. Indeed, there was not one single year after the ban was in place when DC's murder rate relative to Maryland and Virginia was as low as it was in 1976.[150] The average murder rate in DC from 1977 to 1987, the period when the ban was in effect, was 257 percent greater than the average for these two states.[151]

Including data past 1987 shows a dramatic additional increase in DC's murder rate relative to Maryland and Virginia (figure 10.10). DC's murder rate averaged 450 percent more than Maryland and Virginia's from 1977 to 2006 (figure 10.10), over twice the ratio of DC to Maryland and Virginia from 1968 to 1976.

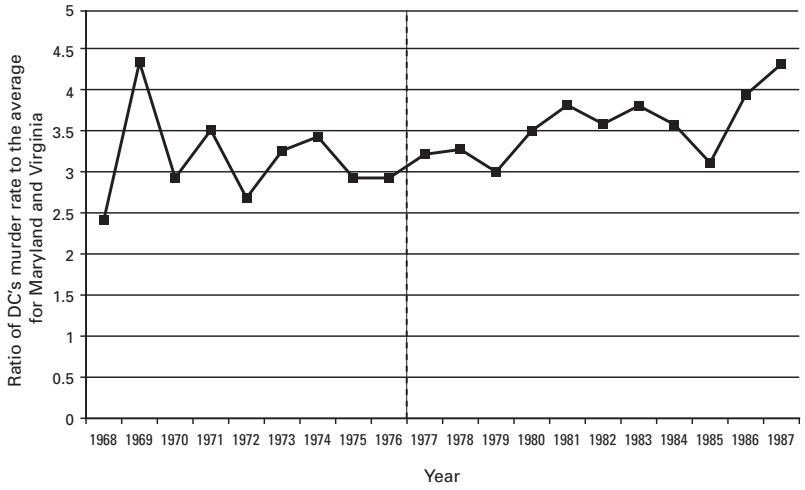


Figure 10.9. Ratio of DC's murder rate to the average for Maryland and Virginia from 1968 to 1987

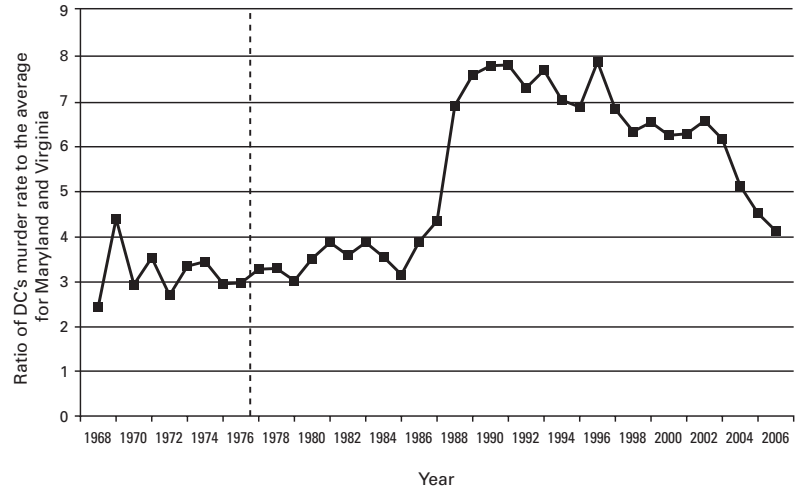


Figure 10.10. Ratio of DC's murder rate to the average for Maryland and Virginia from 1968 to 2006

Again, comparing DC to Maryland and Virginia provides no evidence that the ban reduced DC's murder rate. If anything, DC's murder rate increased after the ban.

COMPARING DC'S MURDER RATE TO THAT FOR THE UNITED STATES. Examining DC's murder rate relative to the United States' from 1968 to 1987 shows that

DC's murder rate was declining before the ban and rising afterward (figure 10.11). The ban seems to be associated with this adverse change in DC's murder rate relative to the rest of the country. By 1976, DC's murder had fallen to being 3.08 times greater than the United States'. There are only two years after that (1979 and 1985) when the ratio of DC's to the United States' murder rate fell below what it was in 1976. Expanding the data to include the period after 1987 (figure 10.12) continues to show the increase in DC's murder rate relative to the United States' that we observed in figures 10.8 and 10.10. Whether one is looking at the period from 1968 to 1987 or including the later period, there is no evidence that the DC gun ban reduced DC's murder rates.

Comparing Murder Rates to the Number of Murders

The *New England Journal of Medicine* study that Justice Breyer cites didn't look at crime rates; it looked at the number of crimes. That is an important distinction. Imagine a city where the number of murders falls by 10 percent but its population declines by 50 percent. Does that demonstrate that crime conditions are improving? Of course not. The crime rate is usually a much better measure of safety than the number of murders.

Yet, not everyone agrees. John Donohue defended the *New England Journal of Medicine* study's approach:[152]

> If you look at the numbers that John [Lott] had put up, which was interesting, if he had actually showed you the number of murders in DC, they had actually dropped. He showed you the rate. And what was interesting about that was, DC was de-populating tremendously in the seventies, and it was largely the flight of the affluent. So, the group that had the lowest likelihood of engaging in crime. So, crime was going to be, if you used the rates that John showed, it was going to be trending up, because the people remaining in the city had a much, much higher risk of crime. And so, when you make those adjustments, the conclusions are opposite to what John suggested.

There are two responses. One is purely factual. The number of murders didn't "drop" after the handgun ban. You don't get the "opposite" of what I argued. During the first six years after the gun ban went into effect, despite a large drop in population, there was only one year when the absolute number of murders fell below what it was in 1976 (and even then it was drop of only eight murders, a drop of 4 percent). Indeed between 1977 and

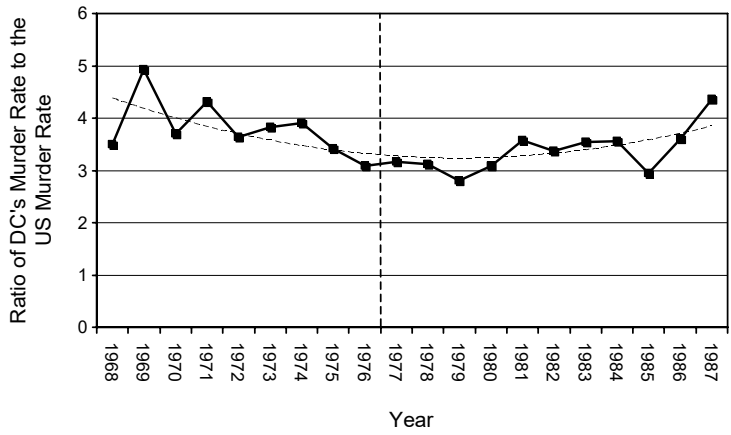


Figure 10.11. Ratio of DC's murder rate to the U.S. murder rate from 1968 to 1987. The dotted line is a simple polynomial curve fitted to these data.

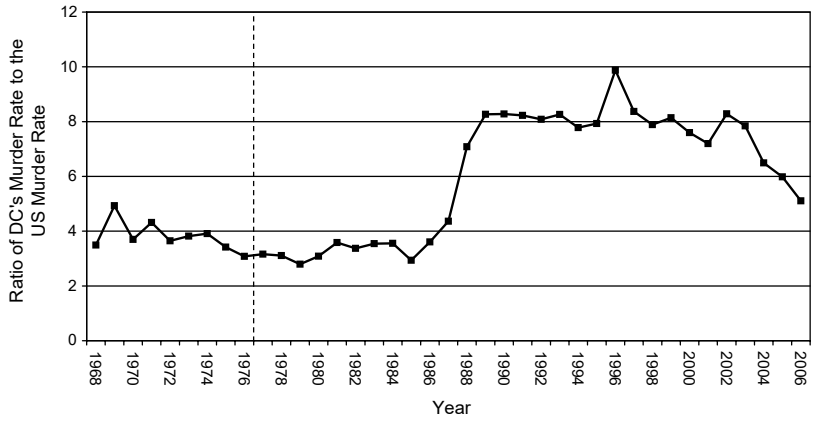


Figure 10.12. Ratio of DC's murder rate to the U.S. murder rate from 1968 to 2006

2005, despite a 20 percent drop in population, there were only four years when the total number of murders was lower than it was in 1976.

The second point concerns demographics. Does the composition of the population matter? Sure it does. And the first and second editions of this book have actually spent a lot of time—more than any previous study—evaluating demographic changes when studying crime. We know that young males commit more crime than other groups. There is more crime in heavily African-American areas. But those concerns are the reason why you look at regressions that account for these changes. As we have already

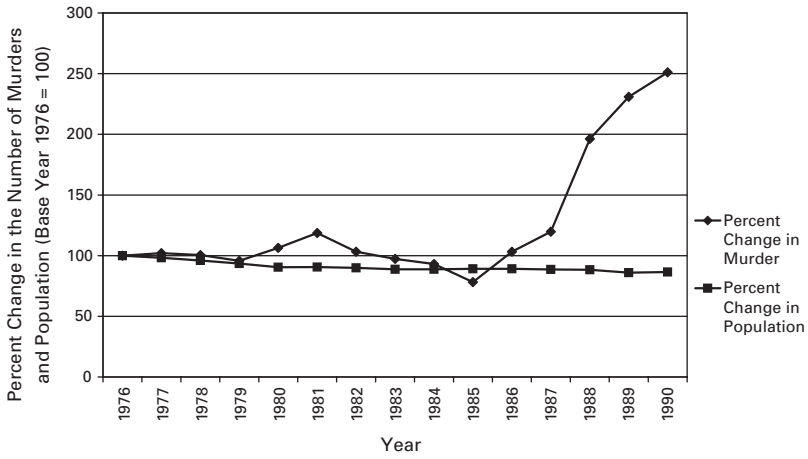


Figure 10.13. Comparison of the change in the number of murders in DC to the change in the number of people living in the city prior to the crack cocaine problem

reported above, DC's murder rates rose relative to the rest of the country even after all the factors—from law enforcement to income and poverty to demographics—have been accounted for.

It Is Not Just DC

The statistics do show a soaring District crime rate. And the District's crime rate went up after the District adopted its handgun ban. But, as students of elementary logic know, after it does not mean because of it. What would the District's crime rate have looked like without the ban? Higher? Lower? The same? Experts differ; and we, as judges, cannot say.

—Justice Stephen Breyer, dissenting in *District of Columbia v. Heller*, June 26, 2008[153]

Justice Breyer is exactly right. DC's crime rates rose after the ban doesn't necessarily mean that the ban caused the increase. Even if the increase wasn't caused by crack cocaine, there could be some other left-out factor that just happened to change at the same time. This is true even though we have run regressions that have attempted to take these different factors into account. One big difference between the earlier work on right-to-carry laws and the current discussion on gun bans is that with thirty-nine states passing right-to-carry laws we have had the same experiment over and over again in many different years in many different places. While it is possible that some left-out factor explains the results in one state or even a few states, the odds that that left-out factor occurred again and again be-

come progressively remote. Of course, there is lots of other evidence for right-to-carry laws: the predicted different effects across different types of crime (multiple-victim shootings should decline more than simple murder), that the size of the drop increases with the percentage of the population with concealed-handgun permits, the comparison of adjacent counties, and so on.

Yet the phenomenon of bans resulting in more crime isn't unique to DC. Every place around the world that has banned guns appears to have experienced an increase in murder and violent crime rates.

Surely DC has had many problems that contribute to crime, but even cities with far better police departments have seen murder and violent crime soar in the wake of handgun bans. Chicago has banned virtually all new handguns since the beginning of 1983 and it now also faces a Supreme Court case challenging its ban.[154] But that handgun ban didn't work at all when it came to reducing violence. Chicago's murder rate fell from 39 to 22 per 100,000 in the eight years before the law and then rose slightly to 23.[155] During the seventeen years from 1983 through 1999, there has been only one year when Chicago's murder rate fell below what it was in 1982, the last year before the ban. Over that same time, the U.S. murder rate fell by 31 percent, from 8.3 to 5.7, and the murder rate for the other nine largest cities dropped by 34 percent, from 17.8 to 11.7 (figure 10.14). Chicago's murder rate doesn't fall below its 1982 murder rate until 2002. It is hard to attribute this eventual drop to the ban, which went into effect twenty years earlier.

Just as it was possible to compare DC's murder rate to other cities, neighboring jurisdictions, and the United States as a whole, one can make the same comparisons for Chicago. Compare Chicago's murder rate to those in other cities among the ten largest or the fifty largest (DC is excluded from this comparison). In both cases, Chicago's murder rate falls relative to the murder rate in other cities up until 1982, when it falls to its lowest value relative to other cities and then rises after that (figure 10.15).

There is a similar relationship when one compares Chicago's murder either to its neighboring counties or to the United States as a whole (figures 10.16, 10.17).[156] If anything, Chicago's murder rate exploded even faster relative to the murder rates in adjacent counties than relative to any of the other comparisons. It is very difficult to see how there is any comparison that can be made that shows that Chicago's murder rate fell after the ban started at the beginning of 1983.

In addition, the experience in other countries is the same, even for island nations that have banned handguns and where borders are easy to moni-

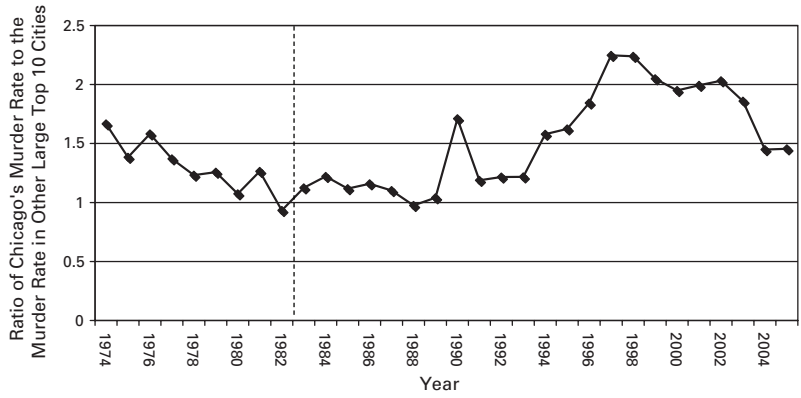


Figure 10.14. Chicago's murder rate relative to the other nine largest cities (weighted by population)

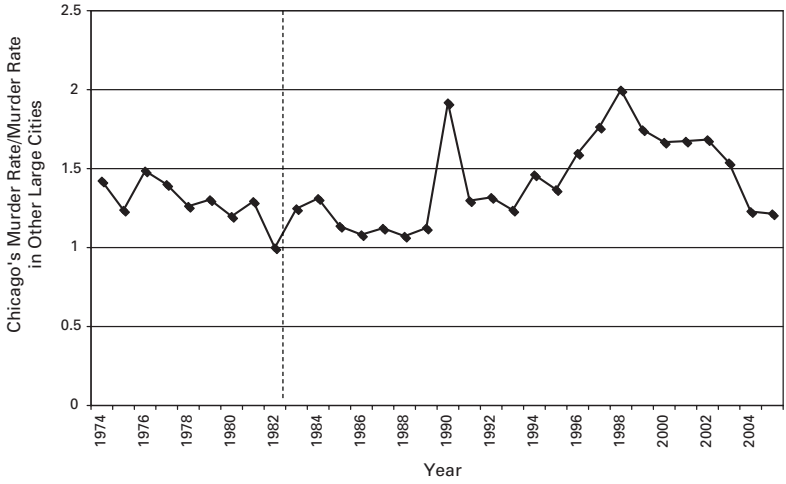


Figure 10.15. Changes in Chicago's murder rate relative to the other forty-eight largest cities (excluding DC from the top fifty list, weighted by population)

tor. These are places that just can't blame the United States or neighboring states for the failure of their gun-control laws. Not only did violent crime and murder not decline as promised; they actually increased.

Great Britain banned handguns in January 1997. But the number of deaths and injuries from gun crime in England and Wales increased an incredible 340 percent in the seven years from 1998 to 2005.[157] The rates of serious violent crime, armed robberies, rapes, and homicide have soared.[158]

The Republic of Ireland and Jamaica also experienced large increases in murder rates after enacting handgun bans in 1972 and 1974, respectively

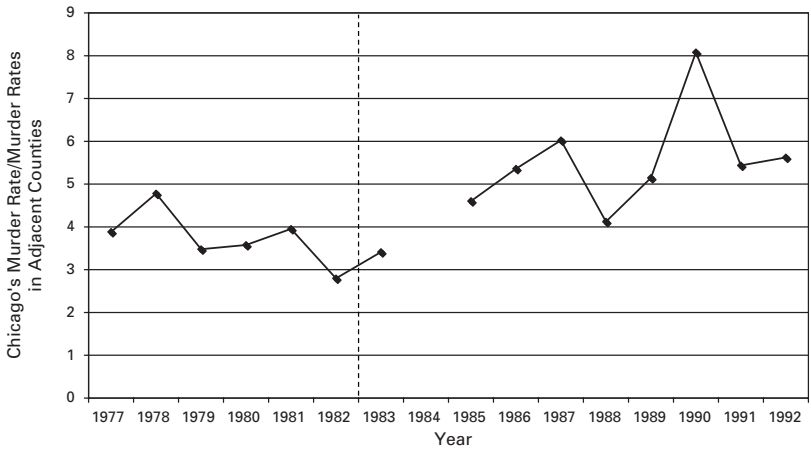


Figure 10.16. Comparison of Chicago's murder rate to the murder rate in adjacent counties (weighted by population)

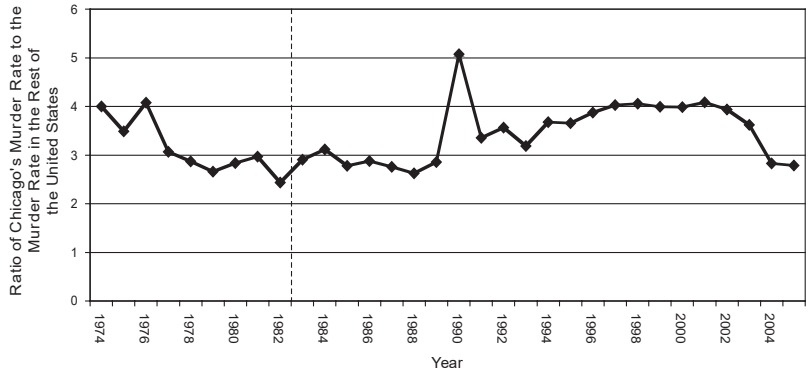


Figure 10.17. Chicago's murder rate relative to the U.S. murder rate

(figures 10.18, 10.19).[159] Since the gun ban, Jamaica's murder rate has soared to become one of the highest in the world, currently at least double that in other Caribbean countries.[160] Jamaica's murder rate hasn't been below 10 murders per 100,000 people since before the gun ban went into effect.[161]

Enforcement efforts have been largely futile. For instance, the weapons the Canadian border guards seize at the U.S. border are overwhelmingly from unwitting U.S. tourists.[162] Few criminals smuggling guns are caught. Jamaica clearly shows that just as drug gangs can bring drugs into a country, they can bring in the guns necessary to protect that valuable property.

The irony is that as drug enforcement increases, the inelastic demand for illegal drugs means that the value gangs attach to protecting their drug turf rises. The same is true in other countries. With recent estimates that up to 80 percent of U.S. crime is gang related—and that, primarily drug gang related—it is likely to be as difficult to remove guns as drugs from these gangs.[163]

Suicides and Guns

One would think that a ban on guns would surely cause a drop in gun suicides. But a graph of the percentage of suicides committed with guns shows no obvious consistent change in the share of suicides committed with guns (figure 10.20).[164] Gun suicides started falling in DC in the early1970s, a couple of years before the gun ban, but so did nongun suicides (figure 10.21). It is difficult to see any benefit as the drop was virtually identical for both gun and nongun suicides.

What makes these graphs particularly surprising is that a gun ban should, everything else equal, actually cause nongun suicides to rise simply because at least some (if not all) of those who would use guns to commit suicide would use some other way of doing so. After all, the ultimate public policy goal would seem to be to reduce overall suicides and not just one method of committing suicide.

Yet even more perverse results have been obtained. David Cutler, Edward Glaeser, and Karen Norberg have conducted by far the largest study on what factors are related to suicides by juveniles.[165] They find some evidence of a relationship between higher gun ownership and suicide, but that relationship not only disappears but is in fact reversed when they include a variable for the rate at which people go hunting. The higher suicide rate is in fact related to the higher rates at which people in certain counties go hunting, not whether people own a gun. They are unable to discern whether the effect is due to something that arises in areas with a lot of hunters or some other factor, but the evidence clearly indicates that suicide rates are actually lower when gun ownership rates are higher.

Conclusion

Everyone wants to take guns away from criminals. However, the problem with bans is who is most likely to obey them. If the ban primarily disarms law-abiding citizens and not criminals, the ban can have the opposite effect of what was intended.

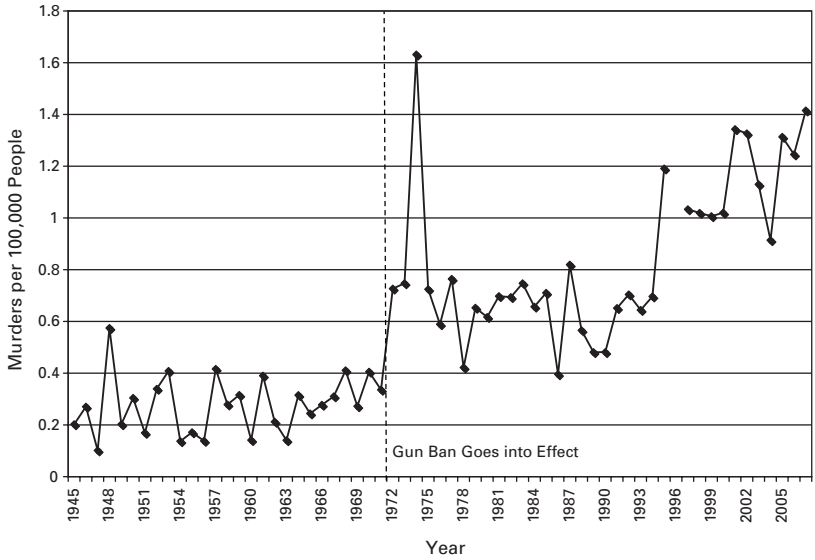


Figure 10.18. Ireland's murder rate. While murder rates in the United States and Jamaica include both murders and manslaughter, Ireland's numbers include only murder. Including manslaughter would probably roughly double the measured murder rate for Ireland for most years. (Murder rate data are not available for 1996.)

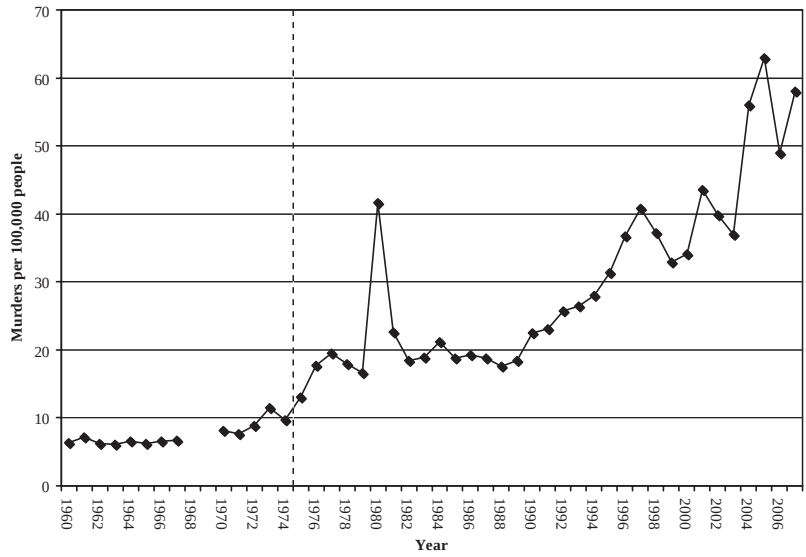


Figure 10.19. Jamaica's murder rate. (Murder rate data are not available for 1968 and 1969.)

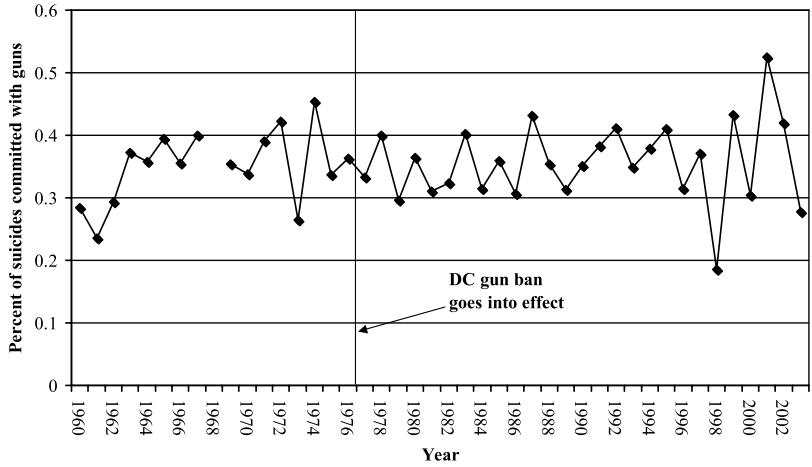


Figure 10.20. Percentage of suicides with guns from 1960 to 2004 for DC

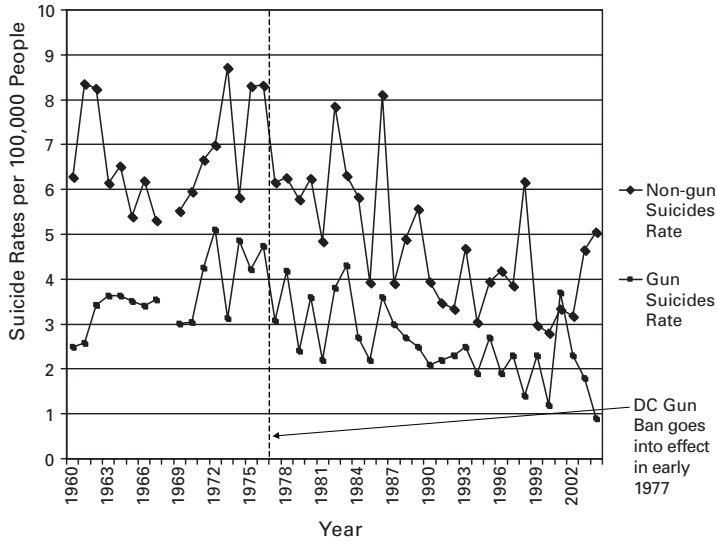


Figure 10.21. Gun and nongun suicide rates in DC from 1960 to 2004

Whether one examines murder rates in DC or Chicago or other countries around the world, there is no evidence that a gun ban reduces murder. Indeed, if anything, the evidence points to the opposite conclusion. DC's rising murder rate cannot be explained as a result of the crack cocaine epidemic during the late 1980s, because this increase started right after the ban was instituted, long before crack cocaine became an issue. Nor can

crack cocaine explain why DC's or Chicago's murder rates rose relative to other large cities.

For those interested in evaluating these crime numbers themselves, many of the data can be easily accessed here:

http://www.disastercenter.com/crime/dccrime.htm
http://www.disastercenter.com/crime/mdcrime.htm
http://www.disastercenter.com/crime/vacrime.htm
http://www.disastercenter.com/crime/uscrime.htm
http://johnrlott.tripod.com/Data_for_Graphs.xls

Gun-Free Zones and Permitted Concealed Handguns

In Mumbai, India, on November 26, 2008, cowering armed policemen failed to fire back at terrorists who were attacking the city. A photographer at the scene described his frustration: "There were armed policemen hiding all around the station but none of them did anything. At one point, I ran up to them and told them to use their weapons. I said, 'Shoot them, they're sitting ducks!' but they just didn't shoot back."[166] Unfortunately, only those police who were directly being threatened by the terrorists chose to fire back. At the hotels targeted by the terrorists, security was equally ineffective; while the hotels "had metal detectors . . . none of its security personnel carried weapons because of the difficulties in obtaining gun permits from the Indian government."[167] India has extremely strict gun-control laws, and citizens are effectively banned from being able to carry guns.[168]

Thus, we see that what holds true in America holds true in other countries as well: that law-abiding citizens, not terrorists and criminals, obey gun-control laws.

Israel provides another quite different example. Up until the early 1970s, the Jewish state had to deal with the cold reality of terrorists who would take machine guns into shopping malls, schools, and synagogues and open fire. That type of attack doesn't occur anymore. Why? Israelis realized that armed citizens could stop such attackers before the attackers could shoot many people. Previously, even large numbers of armed soldiers and police had failed to stop the attacks for a simple reason: terrorists have the option of deciding when to attack and whom to attack first. They would either wait for the police and soldiers to leave the area or shoot them first.

Currently, about 10 percent of Jewish Israeli adults are licensed to carry weapons, so determined terrorists have to resort to less effective, secretive

routes of attack, such as bombings.[169] Prior to letting citizens carry concealed handguns, terrorist attacks in Israel were committed almost entirely with machine guns. Afterward, bombs were almost always used. The reason for the change was simple. Armed citizens can quickly immobilize a gun-wielding attacker, but no one can respond to a bomber once the bomb explodes. Nevertheless, armed citizens do still stop some bombings before the bombs go off.[170] During waves of terror attacks, Israel's national police chief will call on all permitted citizens to carry their firearms at all times.

Multiple-victim public shooters, like terrorists generally, are kamikaze-like killers who seek to maximize carnage. Even if the killers expect to die anyway (and the vast majority of multiple-victim public shooters do), letting potential victims carry guns can help deter these crimes in the first place simply by reducing the level of carnage the killers believe they can inflict.

Americans have learned this lesson the hard way. Consider the disturbed lone shooter who committed the Northern Illinois University attack in February 2008. One thing was clear: Six minutes proved too long.[171] That's how long it took before police officers were able to enter the classroom. In those short six minutes, five people were murdered, and sixteen wounded. And six minutes is actually record-breaking speed for the police arriving at such an attack. At the Virginia Tech massacre the previous year, hours went by between the first attack and the killer's eventual suicide.[172]

Shortly after the Northern Illinois University attack, five people were killed in the city council chambers in Kirkwood, Missouri. This was despite a police officer being present.[173] In Kirkwood, as often happens in these kinds of attacks, the police officer was the one killed first when the attack started.[174] People cowered or were reduced to futilely throwing chairs at the killer.

Over the last three years there have been shootings at the Westroads Mall in Omaha,[175] the Trolley Square Mall in Salt Lake City,[176] and the Tinley Park Mall in Illinois.[177] These tragedies have one thing in common: they took place in "gun-free zones" where private citizens are not allowed to carry guns.

The malls in Omaha and Salt Lake City were in states that, in principle, let people carry concealed handguns. However, these states let private property owners ban guns provided they post clear signs. These malls were among the very few places in their states that posted such bans.[178] Likewise, the slaughter at Virginia Tech and the other public schools oc-

curred in some of the few areas within their states where people were not allowed to carry concealed handguns. Notably, in the Trolley Square attack, an off-duty police officer fortunately carried a handgun—in violation of the ban—and shot the attacker before other officers arrived.

Just as we found in chapter 9, extending the results up through 1999 showed that when states passed right-to-carry laws, the rate of multiple-victim public shootings fell by 60 percent. Deaths and injuries from multiple-victim public shootings fell even further, on average by 78 percent, as the remaining incidents tended to involve fewer victims per attack.[179]

That killers often choose gun-free zones for their attacks is not a new phenomenon. Thirteen were killed in the Columbine High School shooting in 1999; twenty-three were shot dead at Luby's Cafeteria in Killeen, Texas, in 1991; and twenty-one were slain at a McDonald's in Southern California in 1984.[180]

Similar horrible incidents occur in other gun-free zones around the world. The Mumbai massacre left 165 victims dead.[181] Since 2001, many European countries—including Finland, France, Germany, and Switzerland—have each suffered at least two major multiple-victim shootings. The worst school shooting in Germany resulted in seventeen killed (four more than were killed at the Columbine High School attack); in Switzerland, one attacker fatally shot fourteen legislators in a regional parliament building; in Finland in 2008, an attack took the lives of ten victims.[182] During a period of just a couple of weeks in April 2009, there were multiple-victim public shootings at a college in Athens, a crowded café in Rotterdam, and a supermarket in Moscow.[183]

Overall, the problem with gun-control laws is not too little regulation, but rather that the regulations disarm law-abiding citizens. Consider a criminal who is intent on massacring people and then planning on taking his own life. He would unlikely be deterred by any penalties for violating gun regulations. For example, expelling students or firing professors for violating campus gun-free zones represent a real life-changing experience for law-abiding citizens—especially since other academic institutions will not admit or hire people who have such gun offenses on their records. But even assuming the killer survives the attack, it is absurd to imagine that after facing multiple life prison sentences or death penalties for killing people, the threat of expulsion from school will be the penalty that ultimately deters the attack.

But citizens and police who pack heat do help, because they can stop a

shooting while it is happening. Nevertheless, opposition to guns on campuses is so extreme that some universities even oppose having armed police officers on campus. In the wake of the Virginia Tech shooting, campus police at Brandeis University asked that they be armed to prevent similar tragedies. But the president of the Brandeis Student Union retorted that "the sense of community and the sense of safety would be disturbed very much by having guns on campus."[184] Similarly, Columbus (Ohio) State Community College president Val Moeller worried that "when someone comes on campus and sees armed public safety officers, it indicates that the campus is not safe."[185] Similar objections have been voiced on other campuses.[186]

On or off campus, police with guns are certainly helpful in catching criminals, but there are limits to what we should expect them to accomplish. For example, during the attack at Virginia Tech, each officer on duty had to patrol, on average, well over 250 acres.[187]

Passing right-to-carry laws is only one way to utilize guns to help fight terrorism and other violent crime. President Bush's revival of the Federal Air Marshal Program on airplanes is another. This program for domestic flights started in 1970, but ended in the early 1990s.[188] Evidence indicates that it worked well. There were thirty-eight hijackings in America in 1969, but in 1970—as the marshals were employed—the number of hijackings fell into the twenties for each of the next three years, before finally declining to low single digits. Empirical research by Bill Landes suggests that the marshal program substantially contributed to this drop.[189]

While right-to-carry laws—now operating in thirty-nine states—do reduce violent crime generally, the effect is much larger for multiple-victim shootings. Normally about 2–7 percent of adults in any state have permits, and for most crimes, that means some deterrence. But for a shooting in a public place, where there might be dozens or even hundreds of people present, it will almost ensure that at least someone—someone who is unknown to the attacker—will be able to stop the attacker.

Even when an attack begins, civilians with concealed handguns help limit the carnage. A major factor in how many people are killed or injured is how much time elapses between when the attack starts and when someone—be it citizen or police—arrives on the scene with a gun.[190]

Take the Colorado Springs church shooting in December 2007. A parishioner who had the minister's permission to carry her concealed gun into the church quickly stopped the slaughter before the killer was able to

enter the area where thousands of members of the congregation sat.[191] Or take an attack earlier that year on a busy street in downtown Memphis.[192] Or attacks at the Appalachian Law School or high schools in such places as Pearl, Mississippi.[193] In all these incidents, concealed-handgun permit holders stopped what would have clearly escalated into multiple-victim public shootings well before uniformed police could arrive. During 2008, at Israeli schools, armed teachers stopped one terrorist attack and an armed student stopped yet another.[194]

There has been much fear about concealed-handgun permit holders accidentally shooting an innocent bystander when they stop these attacks. This is a legitimate concern. Yet the evidence clearly demonstrates that in practice this is not a problem. Out of all the multiple-victim public shootings that have been stopped by permit holders, no one has indentified a single such incident.[195]

We also have a lot of experience with permitted concealed handguns in schools. Prior to the 1995 Safe School Zone Act, states with right-to-carry laws let teachers and others carry concealed handguns at school. I have not found a single instance when a permitted concealed handgun was improperly used at a school. And neither the National Education Association nor the American Federation of Teachers has been able to point to a problem.

Though in a minority, a number of universities—large public schools such as Colorado State and the University of Utah—let permit holders (both faculty and students) carry concealed handguns on school property.[196] Some other schools, from Dartmouth College to Boise State University, let professors carry concealed handguns.[197] Most of the prohibitions on firearms on college campuses appear to have gone into effect during the early 1990s.[198] Again, no problems have been reported.

Gun-free zones are a magnet for deadly attacks. This applies not only to terrorist attacks, but to crimes generally. Here is one question to think about: If a killer were stalking your family, would you feel safer putting a sign out front announcing, "This home is a gun-free zone"? Probably not, but that is effectively what many places do.

Other Gun-Control Laws

Except for one single study that looks at the Brady Act, researchers following my work have focused exclusively on the impact of concealed handguns. Unfortunately, the work that I did that simultaneously accounted

for many gun-control laws—such as state waiting periods and background checks, one-gun-a-month rules, and penalties for using guns in the commission of a crime—has so far been ignored.

But different gun policies all fit together, and it is hardly obvious that one can properly test the impact of right-to-carry laws without attempting to account for these other laws. Since different gun-control laws sometimes get passed at the same time as right-to-carry laws, inclusion of these other laws is the only way to separate out which law is causing the change in crime rates. The singular focus on right-to-carry laws in so many of these studies suggests that these authors don't believe that these other gun-control laws matter. I have made available the data that David Mustard and I put together as well as the later data in this book on the subject so others could without much effort examine the impact of these other laws.

The other major gun-control laws that we will turn our attention to are gun show regulations, bans on so-called Saturday night specials (inexpensive guns), the assault weapons ban, and the Castle Doctrine. In each section below, I will report the results that were obtained from accounting for these laws in the regressions used to produce figures 10.1a–10.1i. Those figures factored in the impact of all these other gun control laws on the crime rate.

Assault Weapons Ban

Despite many studies of bans on so-called assault weapons, economists and criminologists have yet to find any benefit from either state or federal bans. Analyzing the impact of the 1994 federal ban during its first seven years, Christopher Koper, Daniel Woods, and Jeffrey Roth wrote:

> We cannot clearly credit the ban with any of the nation's recent drop in gun violence. And, indeed, there has been no discernible reduction in the lethality and injuriousness of gun violence, based on indicators like the percentage of gun crimes resulting in death or the share of gunfire incidents resulting in injury, as we might have expected had the ban reduced crimes with both [assault weapons] and [large capacity clips].[199]

Banning some semiautomatic guns when there exist other semiautomatic guns that fire the same bullets at the same rapidity and do the same damage cannot be expected to have much of an impact.

During the 2004 presidential campaign, Senator John Kerry would remark: "I never contemplated hunting deer or anything else with an AK-47."[200] Governor Howard Dean explained his support for extending

the assault weapons ban the same way: "Deer hunters don't need to have assault weapons."[201] The assault weapons ban unfortunately conjures up images of machine guns used by the military. Such weapons are surely not very useful in hunting deer. Yet the 1994 federal assault weapons ban had nothing to do with machine guns, only semiautomatics, which fire one bullet per pull of the trigger. The AK-47s banned by the assaults weapons ban were civilian, semiautomatic versions of the gun. The firing mechanisms in semiautomatics and machine guns are completely different. The entire firing mechanism of a semi-automatic gun has to be gutted and replaced to turn it into a military AK-47.

Does the assault weapons ban have any impact on crime after all? I used two different ways to estimate the impact of both the state and federal assault weapons bans on crime rates (tables 10.9, 10.10). One measures the simple before-and-after average crime rate and the other measures the before-and-after crime rate trends. The simple averages were used in the results shown in figures 10.1a–10.1i. Only using trends shows a significant impact of the law on crime rates, and the longer the ban has been in effect, the greater the increase in murder and robbery. The effects are actually quite large, indicating that each additional year the ban remains in effect raises both murder and robbery rates by around 3 percent. Rape also rises, but only slightly.

Presumably if assault weapons are to be used in committing any particular crime, they will be used for murder and robbery, but the data appear more supportive of an adverse effect of assault weapons bans on murder and robbery rates.

Gun Show Regulations

Despite the impression created by the term gun show "loophole," there are no different rules for buying a gun at a gun show than anywhere else.[202] Gun-control groups, such as Third Way (formerly Americans for Gun Safety) identify eighteen states that have closed the loophole, but interestingly, prior to 2000, only three of these states had laws that even mentioned gun shows.

So how can a state close a gun show loophole if the laws didn't even mention the term "gun show"? The issue is really private handgun transfers. What usually constitutes "closing the loophole" is mandating background checks for private transfers of handguns. Since 1994, federal law has required background checks for all handguns purchased through dealers. The checks were extended to long guns in 1998. But regulating transfers

Table 10.9 Enactment dates of state assault weapons bans

State	Date law went into effect	Penalty for violation
California	Jan. 1, 1990	Felony: 4–8 years in prison
California	Mar. 4, 1998—state appellate court ruled that the 1990 ban was unconstitutional	
California	Jan. 1, 2000—a new assault weapons bill went into effect	Felony: 4–8 years in prison
Hawaii	July 1, 1992	Class C felony: 5 years in prison
Maryland	June 1, 1994	Fine of $1,000–10,000 and/or 1–10 years imprisonment
Massachusetts	Oct. 21, 1998	Felony: not more that 3 years or $5,000 or both
New Jersey	May 30, 1990	Crime of the 3rd degree, knowingly violating regulatory provisions is a crime of the 4th degree
New York	Nov. 1, 2000	Class D violent felony: criminal possession of a weapon in the 3rd degree
Federal assault weapon ban	Sept. 13, 1994, through Sept. 13, 2004	

Table 10.10 Two simple ways of looking at the impact of the assault weapons bans

	Murder	Rape	Robbery	Aggravated assault
Change in the average crime rate when the ban goes into effect	0.4%	–3.0%	3.0%	–2.1%
Change in the crime rate calculated from the difference in the annual change in crime rates in the years before and after adoption of an assault weapon ban	3.2%*	1%**	2.7%*	0.1%

Note: The specifications reported earlier for figures 10.1a–10.1i use the simple dummy variable approach reported here, but using the before-and-after trends does not alter the earlier results.
* Statistically significant at least at the 1 percent level for an *F*-test.
** Statistically significant at least at the 5 percent level for an *F*-test.

by private individuals—such as those occurring at gun shows—has been left to the states (see table 10.11).

The theory linking "gun show loopholes" and crime is fairly straightforward. To the extent that background checks on private transfers prevent criminals from getting guns, crime rates will be reduced. But its impact depends upon two factors: how many criminals actually get guns from gun shows and the ability of criminals to get guns from substitute sources. There is also a trade-off: Increased regulations on private transfers can reduce the number of gun shows and make it more difficult for law-abiding citizens to get guns—guns that could have been used to protect against crime.

To help determine where criminals obtained their firearms, the Bureau

Table 10.11 Enactment dates of state laws requiring background checks on the private transfer of handguns

State	Date enacted	Type of penalty for not conducting check	Type of penalty for providing false information
California	Jan. 1, 1991	Misdemeanor	Misdemeanor
Colorado	Mar. 31, 2001	Class 1 misdemeanor	Class 1 misdemeanor
Connecticut	Oct. 1, 1994	Class D felony	Fine of not more than $500 and/or imprisonment for not more than 3 years
Hawaii	Before 1977	Misdemeanor	Class C felony
Illinois	Before 1977	Class A misdemeanor	Perjury
Indiana	Until Nov. 11, 1998	Class B misdemeanor	Class C felony
Iowa	July 1, 1991	Simple misdemeanor	Class D felony
Maryland	Oct. 1, 1996	Misdemeanor	Misdemeanor
Massachusetts	Before 1977	Felony	Fine of $500–$1,000 and/or 6 months to 2 years imprisonment
Michigan	Before 1977	Felony	Felony
Missouri	Sept. 28, 1981	Class A misdemeanor	Class A misdemeanor
Nebraska	Sept. 6, 1991	Class 1 misdemeanor	Class 4 felony
New Jersey	Before 1977	Crime of the 4th degree	Crime of the 3rd degree
New York	Before 1977	Class A misdemeanor	Class A misdemeanor
North Carolina	Dec. 1, 1995	Class 2 misdemeanor	Class H felony
Oregon	Dec. 7, 2000	Class A misdemeanor	Class A misdemeanor
Pennsylvania	Oct. 11, 1995	Misdemeanor of the 2nd degree	Felony of the 3rd degree
Rhode Island	Before 1977	Fine of not more than $1,000 and/or imprisonment of up to 5 years	Imprisonment of up to 5 years
Tennessee	Until Nov. 11, 1998	Class A misdemeanor	Class A misdemeanor

of Justice Statistics conducted a survey of eighteen thousand state prison inmates in 1997, the largest survey of inmates ever conducted.[203] Fewer than 1 percent of inmates (0.7 percent) who possessed a gun indicated that they obtained it at a gun show. When combined with guns obtained from flea markets, the total rises to 1.7 percent. These numbers are dwarfed by the 40 percent who obtained their guns from friends or family and the 39 percent who obtained them on the street or from illegal sources. These numbers are also very similar to a 1991 survey, a survey that indicated that only 0.6 percent of inmates had obtained their guns from guns shows and 1.3 percent from flea markets. Other surveys of criminals report a similar range of estimates.[204] A detailed discussion of the research on gun show regulations as well as the costs that background checks impose on gun sales is provided in chapter 8 of my book *The Bias Against Guns*.

Despite all the emphasis on gun shows, there is no empirical research

linking gun show regulations to decreased crime rates. If anything, the evidence points in the other direction. Earlier work that I have done using state-level data from 1977 to 2000 implies that gun show regulations not only significantly reduce the number of gun shows by up to 24 percent, but also increase murder and robbery rates. I found no statistically significant impact of background checks for handguns purchased through dealers, a result also found consistently in research by others. Enacting the assault weapons ban and instituting waiting periods did have one effect in my research: it significantly reduced the number of gun shows.[205]

The number of gun shows in the United States peaked in 1996 at 2,907 and has continually fallen since then under both the rest of the Clinton administration and the Bush administration.[206] By 2005, there were 1,792 gun shows, a drop of 38 percent from the peak and just slightly below the 1,800 gun shows that took place in 1990.

Mark Duggan, Randi Hjalmarsson, and Brian Jacob have conducted more recent work. They find that gun shows modestly reduce homicides and have no impact on suicides within twenty-five miles of the gun show.[207] If their result is correct, the reduction in gun shows that I find from closing the gun show loophole may explain why closing the loophole could increase murder and robbery rates. Closing down gun shows is more likely to deprive law-abiding citizens of a relatively inexpensive source of guns than to prevent criminals from getting guns.

The results in table 10.12 imply little impact from closing the gun show loophole. While murder and robbery rates appear to rise, neither increase is statistically significant. Nor is the change in aggravated assaults significant. Although rape is reduced and the reduction is significant, it is unclear how to interpret this lone result, since guns are very rarely used in the commission of rape. In fact, unlike the other violent crime categories, the FBI *Uniform Crime Reports* don't even list how many rapes are committed with guns. What is probably most clear from this result is that closing the gun show loophole does not reduce defensive gun uses that stop rapes.

Castle Laws

Fourteen states adopted Castle Laws in just 2006 alone. It is hard to think of any gun-control law that has been adopted in so many states in just a single year. Yet, this book represents the first research on the impact that the so-called Castle Doctrine, or Castle Law, has on crime rates. These Castle Laws eliminate the requirement that people in their own homes retreat as far

Table 10.12 The impact of "closing" the gun show loophole on violent crime rates

	Murder	Rape	Robbery	Aggravated assault
Change in the average crime rate after the gun show loophole is closed (%)	2%	−3.0%***	3.0%	−2.1%

Note: Examining the before-and-after average crime rates from closing this loophole was accounted for in figures 10.1a–10.1i.

***Statistically significant at least at the 10 percent level for two-tailed *t*-test.

as possible before defending themselves. By removing this requirement, victims do not risk misjudging how far they should retreat. As a result, committing crimes in a home becomes more risky for the criminal. Castle Laws take away worries for a law-abiding citizen about breaking the law by not retreating as far as possible, a delay that might be potentially harmful. Indeed, three of the cases discussed earlier in the section on prosecutions of concealed-handgun permit holders ("Concealed-Carry Permit Holders: Villains or Saints?") noted judgment calls where prosecutors apparently didn't believe that the victims had done enough to avert the attack prior to firing their guns.

On the other hand, there could be some unintended consequences where gun owners might shoot too quickly—leading to the shooting being classified as either murder or manslaughter and thus potentially increasing crime rates. One recent case in Colorado Springs during the end of December 2008 has been pointed to as the type of tragedy that might arise from the Castle Doctrine, and it received widespread international attention.[208] As initially reported, 22-year-old Sean Kennedy had been drinking heavily; he had gotten lost and knocked on the back door of the wrong house and got shot.

But later reports indicated that the residents might have had some justification for their shooting, noting that Kennedy "had broken a window and was trying to get inside a back door when he was shot and killed by the homeowner Sunday night" while those inside were screaming at him to leave.[209] The residents had also reportedly called 911 and "reported that someone was trying to break into the house" before they fired their gun.[210] There is also a mention that Kennedy had "broken" the door. In any case, it appears that the improper actions with guns that many feared would happen after the law are quite rare.

While thirty states have now passed Castle Laws, there are not yet many changes in the law to study. Only seven states enacted such laws during our sample period, between 1977 and the end of 2005, and three of the seven adopted the laws between 2003 and 2005 (table 10.13). While the results (table

Table 10.13 Enactment dates of Castle Doctrine laws

State	Date law went into effect
Alabama	June 1, 2006
Alaska	June 20, 2006
Arizona	Apr. 24, 2006
California	Before 1977
Colorado	June 6, 1995
Connecticut	1973
Delaware	Before 1977
Florida	Oct. 1, 2005
Georgia	July 1, 2006
Hawaii	Before 1977
Idaho	Oct. 1, 2006
Illinois	July 28, 2004
Indiana	July 1, 2006
Kansas	May 26, 2006
Kentucky	July 12, 2006
Louisiana	Aug. 15, 2006
Massachusetts	Before 1977
Michigan	July 26, 2006
Minnesota	Before 1977
Mississippi	July 1, 2006
Montana	Before 1977
New Mexico	1978
North Carolina	1993
Oklahoma	Oct. 1, 2006
Rhode Island	Before 1977
South Carolina	June 9, 2006
South Dakota	July 1, 2006
Utah	2003
Virginia	Before 1977
Washington	1999[a]

Source: Information from "Summary Of 'Duty to Retreat' Law in All 50 States" (NRA/ILA Office of Legislative Counsel, Fairfax, VA, 2008) and factiva searches.

[a]No law; legal precedent only: "The law is well settled that there is no duty to retreat when a person is assaulted in a place where he or she has a right to be"(Washington State Supreme Court, citing a 1999 ruling; http://www.washapp.org/Opinion.aspx?id=16).

10.14) suggest an overall reduction in violent and property crimes, too few years with the law in place are available to examine the before-and-after trends in crime rates.

Conclusion

Only rarely does a man of ideas witness in his own lifetime, the opportunity to actually see one of his ideas change history. For a scholar who wrote a controversial book in

Table 10.14 The impact of the Castle Doctrine on violent crime rates

	Murder	Rape	Robbery	Aggravated assault	Property crime	Burglary	Larceny	Auto theft
Change in the average crime rate after adoption of Castle Doctrine	−9%*	−18%*	6.7%***	−14.1%*	−6%*	−3.5%	−8.8%*	13%*

Note: The before-and-after average crime rates from enacting this law were accounted for in figures 10.1a–10.1i.
* Statistically significant at least at the 1 percent level for two-tailed *t*-test.
*** Statistically significant at least at the 10 percent level for two-tailed *t*-test.

the 1990s, arguing that, where there is more gun ownership there is actually less crime, that history-making experience took place. Legislatures across the country took hold of the ideas in that book, and passed laws allowing for the carrying of concealed weapons, that indeed was history-making. The author of that idea and of the book that contained those ideas, is our first debater tonight, speaking for the motion, "Guns reduce crime," John Lott.

—John Donvan of *ABC News Nightline* giving the introduction to the Intelligence Squared U.S. debate on October 28, 2008[211]

John Donvan's judgment in the above quote is extremely generous—indeed, probably too generous. There are lots of factors that went into this debate, and it is hard to evaluate how important each one was. The most crucial thing, though, is clear: if permit holders weren't extremely law abiding and if there were problems with right-to-carry laws, most states would not have adopted them. It would also have sparked calls for repealing concealed-handgun laws that had already been adopted, but no state has even held a legislative hearing on doing that. The lack of serious political debate on these points reflects that the evidence has been so clear. David Mustard and I may have noticed the facts before others, but the evidence has also spoken for itself.

It is easy to see why some people think that banning guns will make others safer, but gun-free zones, whether on college campuses or at the city or country level, have not disarmed criminals. Everyone wants to disarm criminals, but the problem we face is one faced with all types of gun-control laws: who is most likely to obey the law? Time after time, it is the most law-abiding citizens, the people who we don't have anything to worry about, who are disarmed and made vulnerable, not the criminals. However well meaning, banning guns only makes the lives of criminals easier.

During the past year, gun control has become a heated issue again. Calls for rebanning so-called assault weapons and regulating gun shows are again all the rage. The media have also gotten desperate trying to promote gun control. Take an ABC show from April, 2009. The network aired a heavily promoted, hour-long *20/20* special called "If I Only Had a Gun." It is ABC's equivalent of NBC's infamous exploding gas tanks in General Motors pickups, where NBC rigged the truck to explode. With states debating whether to eliminate gun-free zones at universities, there are few sacred cows in the gun-control debate that are not being questioned.

The show started and ended by claiming that allowing potential vic-

tims to carry guns would not help keep them safe—not even with hundreds of hours of practice firing guns. No mention was made of the actual multiple-victim public shootings stopped by people with concealed handguns, nor did the reporters describe who actually carried out such shootings. Instead, ABC presented a rigged experiment where one student in a classroom had a gun.

But sometimes even the best editors can't hide everything the camera sees. The experiment was set up to make the student fail. It did not resemble a real-world shooting. The same scenario is shown three times, but in each case the student with the gun is seated in the same seat—the center seat in the front row. The attacker not only is a top-notch shooter—a firearms expert who teaches firearms tactics and strategy to police—but also obviously knows precisely where the student with the gun is sitting.

Each time the experiment is run, the attacker first fires two shots at the teacher in the front of the class and then turns his gun directly on the very student with the gun. The attacker wastes no time trying to gun down any of the unarmed students. Thus, very unrealistically, between the very first shot setting the armed student on notice and the shots at the armed student, there is at most two seconds. The armed student is allowed virtually no time to react and, unsurprisingly, fails under the same circumstances that would have led even experienced police officers to fare poorly.

But in the real world, a typical shooter is not a top-notch firearms expert and has no clue about whether or not anyone might be armed and, if so, where that person is seated. If you have fifty people—a pretty typical college classroom—and the armed student is unknown to the attacker, he or she is given a tremendous advantage. Actually, if the experiment run by *20/20* seriously demonstrated anything, it was the problem of relying on uniformed police or security guards for safety: the killer instantly knows whom to shoot first.

Yet, in the ABC experiment, the purposefully disadvantaged students are not just identified and facing (within less than two seconds) an attacker whose gun is already drawn. They are also forced to wear unfamiliar gloves, a helmet, and a holster. This only adds to the difficulties the students face in handling their guns.

Given this odd setup, the second student, Danielle, performed admirably well. She shot the firearms expert in his left leg near the groin. If real bullets had been used, that might well have disabled the attacker and cut short his shooting spree.

What this book has tried to do is describe real-world cases where people have used guns to defend themselves and others. We have tried to use hard data to answer questions about what rules and regulations will save lives. Ten years have passed since this book was first written. While the previous editions involved the largest studies of crime at that time, even more data are now available, and many more states have adopted right-to-carry laws. A lot more work by many academics has been done on gun-control regulations. There are certain points that are beyond dispute.

1. By any measure, concealed-handgun permit holders are extremely law abiding.
2. Even the number of anecdotal news stories of defensive gun uses completely dwarfs any possible bad actions by permit holders with their concealed handguns.
3. No refereed academic articles by economists or criminologists claim that right-to-carry laws have a significant bad effect on crime rates.

Regarding the academic debate, it is obvious that a nerve has been struck. The language and accusations used by some can be distracting and disturbing, but the strongest opponents of allowing people to defend themselves have made what are at best simple mistakes that, when corrected, show the opposite of what they claim.

Refereed academic journal articles by economists and criminologists continue to show estimates that range from indicating large benefits from right-to-carry laws to claiming to show no change in crime rates. Yet, even those studies that claim that there is no benefit provide more evidence of benefits than no effect and much more evidence of benefits than costs.

At some point the risk of gun-free zones is going to have to be seriously discussed. Whether one looks at city or country gun bans or even smaller gun bans involving malls or schools, bans increase violence and murder.

The gun-control debate has changed dramatically over the last decade. In the past the question was how much guns caused crime. The debate now is over whether there are benefits from gun ownership and how large those benefits are.

APPENDIX ONE

How to Account for the Different Factors That Affect Crime and How to Evaluate the Importance of the Results

The research in this book relies on what is known as *regression analysis,* a statistical technique that essentially lets us "fit a line" to a data set. Take a two-variable case involving arrest rates and crime rates. One could simply plot the data and draw the line somewhere in the middle, so that the deviations from the line would be small, but each person would probably draw the line a little differently. Regression analysis is largely a set of conventions for determining exactly how the line should be drawn. In the simplest and most common approach—ordinary least squares (OLS)—the line chosen minimizes the sum of the squared differences between the observations and the regression line. Where the relationship between only two variables is being examined, regression analysis is not much more sophisticated than determining the correlation.

The regression *coefficients* tell us the relationship between the two variables. The diagram in figure A1.1 indicates that increasing arrest rates decreases crime rates, and the slope of the line tells us how much crime rates will fall if we increase arrest rates by a certain amount. For example, in terms of figure Al, if the regression coefficient were equal to –1, lowering the arrest rate by one percentage point would produce a similar percentage-point increase in the crime rate. Obviously, many factors account for how

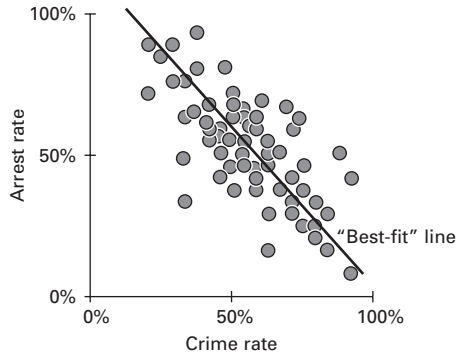


Figure A1.1. Fitting a regression into a scatter diagram

crime changes over time. To deal with these, we use what is called *multiple regression analysis.* In such an analysis, as the name suggests, many explanatory (or exogenous) variables are used to explain how the endogenous (or dependent) variable moves. This allows us to determine whether a relationship exits between different variables after other effects have already been taken into consideration. Instead of merely drawing a line that best fits a two-dimensional plot of data points, as shown in figure A1.1, multiple regression analysis fits the best line through an *n*-dimensional data plot, where *n* is the number of variables being examined.

A more complicated regression technique is called *two-stage least squares.* We use this technique when two variables are both dependent on each other and we want to try to separate the influence of one variable from the influence of the other. In our case, this arises because crime rates influence whether the nondiscretionary concealed-handgun laws are adopted at the same time as the laws affect crime rates. Similar issues arise with arrest rates. Not only are crime rates influenced by arrest rates, but since an arrest rate is the number of arrests divided by the number of crimes, the reverse also holds true. As is evident from its name, the method of two-stage least squares is similar to the method of ordinary least squares in how it determines the line of best fit—by minimizing the sum of the squared differences from that line. Mathematically, however, the calculations are more complicated, and the computer has to go through the estimation in two stages.

The following is an awkward phrase used for presenting regression results: "a one-standard-deviation change in an explanatory variable explains a certain percentage of a one-standard-deviation change in the various

crime rates." This is a typical way of evaluating the importance of statistical results. In the text I have adopted a less stilted, though less precise formulation: for example, "variations in the probability of arrest account for 3 to 11 percent of the variation in the various crime rates." As I will explain below, standard deviations are a measure of how much variation a given variable displays. While it is possible to say that a one-percentage-point change in an explanatory variable will affect the crime rate by a certain amount (and, for simplicity, many tables use such phrasing whenever possible), this approach has its limitations. The reason is that a 1 percent change in the explanatory variable may sometimes be very unlikely: some variables may typically change by only a fraction of a percent, so assuming a one-percentage-point change would imply a much larger impact than could possibly be accounted for by that factor. Likewise, if the typical change in an explanatory variable is much greater than 1 percent, assuming a one-percentage-point change would make its impact appear too small.

The convention described above—that is, measuring the percent of a one-standard-deviation change in the endogenous variable explained by a one-standard-deviation change in the explanatory variable—solves the problem by essentially normalizing both variables so that they are in the same units. Standard deviations are a way of measuring the typical change that occurs in a variable. For example, for symmetric distributions, 68 percent of the data is within one standard deviation of either side of the mean, and 95 percent of the data is within two standard deviations of the mean. Thus, by comparing a one-standard-deviation change in both variables, we are comparing equal percentages of the typical changes in both variables.[1]

The regressions in this book are also "weighted by the population" in the counties or states being studied. This is necessitated by the very high level of "noise" in a particular year's measure of crime rates for low-population areas. A county with only one thousand people may go through many years with no murders, but when even one murder occurs, the murder rate (the number of murders divided by the county's population) is extremely high. Presumably, no one would believe that this small county has suddenly become as dangerous as New York City. More populous areas experience much more stable crime rates over time. Because of this difficulty in consistently measuring the risk of murder in low-population counties, we do not want to put as much emphasis on any one year's observed murder rate, and this is exactly what weighting the regressions by county population does.

Several other general concerns may be anticipated in setting up the regression specification. What happens if concealed-handgun laws just hap-

pen to be adopted at the same time that there is a downward national trend in crime rates? The solution is to use separate variables for the different years in the sample: one variable equals 1 for all observations during 1978 and zero for all other times, another equals 1 for all observations during 1979 and zero otherwise, and so on. These "year-dummy" variables thus capture the change in crime from one year to another that can only be attributed to time itself. Thus if the murder rate declines nationally from 1991 to 1992, the year-dummy variables will measure the average decline in murder rates between those two years and allow us to ask if there was an additional drop, even after accounting for this national decline, in states that adopted nondiscretionary concealed-handgun laws.

A similar set of "dummy" variables is used for each county in the United States, and they measure deviations in the average crime rate across counties. Thus we avoid the possibility that our findings may show that nondiscretionary concealed-handgun laws appear to reduce crime rates simply because the counties with these laws happened to have low crime rates to begin with. Instead, our findings should show whether there is an additional drop in crime rates after the adoption of these laws.

The only way to properly account for these year and county effects, as well as the influences on crime from factors like arrest rates, poverty, and demographic changes, is to use a multiple-regression framework that allows us to directly control for these influences.

Unless we specifically state otherwise, the regressions reported in the tables attempt to explain the natural logarithms of the crime rates for the different categories of crime. Converting into "logs" is a conventional method of rescaling a variable so that a given absolute numerical change represents a given percentage change. (The familiar Richter scale for measuring earthquakes is an example of a base-10 logarithmic scale, where a tremor that registers 8 on the scale is ten times as powerful as one that registers 7, and one that registers 7 is ten times as powerful as one that registers 6.) The reason for using logarithms of the endogenous variable rather than their simple values is twofold. First, using logs avoids giving undue importance to a few, very large, "outlying" observations. Second, the regression coefficient can easily be interpreted as the percent change in the endogenous variable for every one-point change in the particular explanatory variable examined.

Finally, there is the issue of *statistical significance.* When we estimate coefficients in a regression, they take on some value, positive or negative. Even if we were to take two completely unrelated variables—say, sunspot activity

and the number of gun permits—a regression would almost certainly yield a coefficient estimate other than zero. However, we cannot conclude that any positive or negative regression coefficient really implies a true relationship between the variables. We must have some measure of how certain the coefficient estimate is. The size of the coefficient does not really help here—even a large coefficient could have been generated by chance.

This is where statistical significance enters in. The measure of statistical significance is the conventional way of reporting how certain we can be that the impact is different from zero. If we say that the reported number is "positive and statistically significant at the 5 percent level," we mean that there is only a 5 percent chance that the coefficient happened to take on a positive value when the true relationship in fact was zero or negative.[2] To say that a number is statistically significant at the 1 percent level represents even greater certainty. The convention among many social scientists is usually not to affirm conclusions unless the level of significance reaches 10 percent or lower; thus, someone who says that a result is "not significant" most likely means that the level of significance failed to be as low as 10 percent.

These simple conventions are, however, fairly arbitrary, and it would be wrong to think that we learn nothing from a value that is significant at "only" the 11 percent level, while attaching a great deal of weight to one that is significant at the 10 percent level. The true connection between the significance level and what we learn involves a much more continuous relationship. We are more certain of a result when it is significant at the 10 percent level rather than at the 15 percent level, and we are more certain of a result at the 1 percent level than at the 5 percent level.

APPENDIX TWO Explanations of Frequently Used Terms

ARREST RATE: The number of arrests per crime.

CRIME RATE: The number of crimes per 100,000 people.

CROSS-SECTIONAL DATA: Data that provide information across geographic areas (cities, counties, or states) within a single period of time.

DISCRETIONARY CONCEALED-HANDGUN LAW: Also known as a "may-issue" law; the term *discretionary* means that whether a person is ultimately allowed to obtain a concealed-handgun permit is up to the discretion of either the sheriff or judge who has the authority to grant the permit. The person applying for the permit must frequently show a "need" to carry the gun, though many rural jurisdictions automatically grant these requests.

ENDOGENOUS: A variable is endogenous when changes in the variable are assumed to caused by changes in other variables.

EXOGENOUS: A variable is exogenous when its values are as given, and no attempt is made to explain how that variable's values change over time.

EXTERNALITY: The costs of or benefits from one's actions may accrue to other people. External benefits occur when people cannot capture the beneficial effects that their actions produce. External costs arise when people are not made to bear the costs that their actions impose on others.

NONDISCRETIONARY CONCEALED-HANDGUN LAW: Also

known as a "shall-issue" or "do-issue" law; the term *nondiscretionary* means that once a person meets certain well-specified criteria for obtaining a concealed-handgun permit, no discretion is involved in granting the permit—it must be issued.

POOLED, CROSS-SECTIONAL, TIME-SERIES DATA: Data that allow the researcher not only to compare differences across geographic areas, but also to see how these differences change across geographic areas over time.

REGRESSION: A statistical technique that essentially lets us fit a line to a data set to determine the relationship between variables.

STATISTICAL SIGNIFICANCE: A measure used to indicate how certain we can be that the impact of a variable is different from some value (usually whether it is different from zero).

TIME-SERIES DATA: Data that provide information about a particular place over time. For example, time-series data might examine the change in the crime rate for a city over many years.

APPENDIX THREE Description of the Data

This appendix provides a detailed discussion of the variables used in this study and their sources. The number of arrests and offenses for each crime in every county from 1977 to 1992 were provided by the FBI's *Uniform Crime Reports* (UCR). The UCR program is a nationwide, cooperative statistical effort by over 16,000 city, county, and state law-enforcement agencies to compile data on crimes that are reported to them. During 1993, law-enforcement agencies active in the UCR program represented over 245 million U.S. inhabitants, or 95 percent of the total population. The coverage amounted to 97 percent of the U.S. population living in Metropolitan Statistical Areas (MSAs) and 86 percent of the population in non-MSA cities and in rural counties.[1] The *Supplementary Homicide Reports* of the UCR supplied the data on the sex and race of victims and on whatever relationship might have existed between victim and offender.[2]

The regressions report results from a subset of the UCR data set, though we also ran the regressions with the entire data set. The main differences were that the effect of concealed-handgun laws on murder was greater than what is reported in this study, and the effects on rape and aggravated assault were smaller. Observations were eliminated because of changes in reporting practices or definitions of crimes; see *Crime in the United States* for the years 1977 to 1992. For example, from 1985

to 1994, Illinois operated under a unique, "gender-neutral" definition of sex offenses. Another example involves Cook County, Illinois, from 1981 to 1984, which experienced a large jump in reported crime because of a change in the way officers were trained to report crime.

The additional observations that were either never provided or were dropped from the data set include those from Arizona (1980), Florida (1988), Georgia (1980), Kentucky (1988), and Iowa (1991). Data for counties containing the following cities were also eliminated for the crime rates listed: violent crime and aggravated assault for Steubenville, Ohio (1977–89); violent crime and aggravated assault for Youngstown, Ohio (1977–87); violent crime, aggravated assault, and burglary for Mobile, Alabama (1977–85); violent crime and aggravated assault for Oakland, California (1977–90); violent crime and aggravated assault for Milwaukee, Wisconsin (1977–85); all crime categories for Glendale, Arizona (1977–84); violent crime and aggravated assault for Jackson, Mississippi (1977 and 1982); violent crime and aggravated assault for Aurora, Colorado (1977 and 1982); violent crime and aggravated assault for Beaumont, Texas (1977 and 1982); violent crime and aggravated assault for Corpus Christi, Texas (1977 and 1982); violent crime and rape for Macon, Georgia (1977–81); violent crime, property crime, robbery, and larceny for Cleveland, Ohio (1977–81); violent crime and aggravated assault for Omaha, Nebraska (1977–81); all crime categories for Eau Claire, Wisconsin (1977–78); all crime categories for Green Bay, Wisconsin (1977); and all crime categories for Little Rock, Arkansas (1977–79).

The original *Uniform Crime Report* data set did not have arrest data for Hawaii in 1982. These missing observations were supplied to us by the Hawaii UCR program. In the original data set several observations included two observations for the same county and year identifiers. The incorrect observations were deleted from the data.

For all of the different crime rates, if the true rate was zero, we added 0.1 before we took the natural log of those values. It is not possible to take the natural log of zero, because any change from zero is an infinite percentage change. For the accident rates and the supplementary homicide data, if the true rate was zero, we added 0.01 before we took the natural logs of those values.[3]

The number of police in a state, the number of officers who have the power to make arrests, and police payrolls for each state by type of officer are available for 1982 to 1992 from the U.S. Department of Justice's *Expenditure and Employment Data for the Criminal Justice System*.

The data on age, sex, and racial distributions estimate the population

in each county on July 1 of the respective years. The population is divided into five-year age segments, and race is categorized as white, black, and neither white nor black. The population data, with the exception of 1990 and 1992, were obtained from the U.S. Bureau of the Census.[4] The estimates use modified census data as anchor points and then employ an iterative proportional-fitting technique to estimate intercensal populations. The process ensures that the county-level estimates are consistent with estimates of July 1 national and state populations by age, sex, and race. The age distributions of large military installations, colleges, and institutions were estimated by a separate procedure. The counties for which special adjustments were made are listed in the report.[5] The 1990 and 1992 estimates have not yet been completed by the Bureau of the Census and made available for distribution. We estimated the 1990 data by taking an average of the 1989 and 1991 data. We estimated the 1992 data by multiplying the 1991 populations by the 1990–91 growth rate of each county's population.

Data on income, unemployment, income maintenance, and retirement were obtained by the Regional Economic Information System (REIS). Income maintenance includes Supplemental Security Insurance (SSI), Aid to Families with Dependent Children (AFDC), and food stamps. Unemployment benefits include state unemployment insurance compensation, Unemployment for federal employees, unemployment for railroad employees, and unemployment for veterans. Retirement payments include old-age survivor and disability payments, federal civil employee retirement payments, military retirement payments, state and local government employee retirement payments, and workers compensation payments (both federal and state). Nominal values were converted to real values by using the consumer price index.[6] The index uses the average consumer price index for July 1983 as the base period. County codes for twenty-five observations did not match any of the county codes listed in the ICPSR codebook. Those observations were deleted from the sample.

Data concerning the number of concealed-weapons permits for each county were obtained from a variety of sources. Mike Woodward, of the Oregon Law Enforcement and Data System, provided the Oregon data for 1991 and after. The number of permits available for Oregon by county in 1989 was provided by the sheriff's departments of the individual counties. Cari Gerchick, Deputy County Attorney for Maricopa County in Arizona, provided us with the Arizona county-level conviction rates, prison-sentence lengths, and concealed-handgun permits from 1990 to 1995. The Pennsylvania data were obtained from Alan Krug. The National Rifle Associa-

tion provided data on NRA membership by state from 1977 to 1992. The dates on which states enacted enhanced-sentencing provisions for crimes committed with deadly weapons were obtained from a study by Marvell and Moody.[7] The first year for which the enhanced-sentencing variable equals 1 is weighted by the portion of that first year during which the law was in effect.

For the Arizona regressions, the Brady-law variable is weighted for 1994 by the percentage of the year for which it was in effect (83 percent).

The Bureau of the Census provided data on the area in square miles of each county. Both the total number of unintentional-injury deaths and the number of those involving firearms were obtained from annual issues of *Accident Facts* and *The Vital Statistics of the United States*. The classification of types of weapons is from *International Statistical Classification of Diseases and Related Health Problems,* vol. 1, 10th ed. The handgun category includes guns for single-hand use, pistols, and revolvers. The total includes all other types of firearms.

The means and standard deviations of the variables are reported in appendix 4.

APPENDIX FOUR National Sample Means and Standard Deviations

Table A4.1 National Sample Means and Standard Deviations

Variable	Observations	Mean	Standard deviation
Gun ownership information:			
Nondiscretionary law dummy	50,056	0.16	0.368
Arrests rates (ratio of arrests to offenses)			
Index crimes[a]	45,108	27.43	126.73
Violent crimes	43,479	71.31	327.25
Property crimes	45,978	24.03	120.87
Murder	26,472	98.05	109.78
Rape	33,887	57.83	132.80
Aggravated assault	43,472	71.37	187.35
Robbery	34,966	61.62	189.50
Burglary	45,801	21.51	47.299
Larceny	45,776	25.57	263.71
Auto theft	43,616	44.82	307.54
Crime rates (per 100,000 people)			
Index crimes	46,999	2,984.99	3,368.85
Violent crimes	47,001	249.08	388.72
Property crimes	46,999	2,736.59	3,178.41
Murder	47,001	5.65	10.63
Murder rate with guns (from 1982 to 1991 in counties with more than 100,000 people)	12,759	3.92	6.48
Rape	47,001	18.78	32.39
Robbery	47,001	44.69	149.21
Aggravated assault	47,001	180.05	243.26
Burglary	47,001	811.8642	1,190.23
Larceny	47,000	1,764.37	2,036.03
Auto theft	47,000	160.42	284.60
Causes of accidental deaths and murders (per 100,000 people)			
Rate of accidental deaths from guns	23,278	0.151	1.216175
Rate of accidental deaths from causes other than guns	23,278	1.165152	4.342401
Rate of total accidental deaths	23,278	51.95	32.13482
Rate of murders (handguns)	23,278	0.44	1.930975
Rate of murders (other guns)	23,278	3.478	6.115275

Table A4.1 (*continued*)

Variable	Observations	Mean	Standard deviation
Income data (all values in real 1983 dollars)			
Real per-capita personal income	50,011	10,554.21	2,498.07
Real per-capita unemployment insurance	50,011	67.58	53.10
Real per-capita income maintenance	50,011	157.23	97.61
Real per-capita retirement (over age 65)	49,998	12,328.5	4,397.49
Population characteristics			
County population	50,023	75,772.78	250,350.4
County population per square mile	50,023	214.33	1421.25
State population	50,056	6,199,949	5,342,068
State NRA membership (per 100,000 people)	50,056	1098.11	516.0701
Percent voting Republican in presidential election	50,056	52.89	8.41

[a]Index crimes represent the total of all violent and property crimes.

Table A4.2 Average percent of the total population in U.S. counties in each age, sex, and race cohort from 1977 to 1992 (50,023 observations)

	10–19 years of age	20–29 years of age	30–39 years of age	40–49 years of age	50–64 years of age	Over 65 years of age
Black male	0.9%	0.8%	0.5%	0.4%	0.4%	0.4%
Black female	0.9%	0.8%	0.6%	0.4%	0.5%	0.6%
White male	7.3%	6.8%	6.4%	4.9%	6.5%	5.4%
White female	6.8%	6.6%	6.3%	5.0%	6.9%	7.5%
Other male	0.2%	0.2%	0.2%	0.1%	0.1%	0.1%
Other female	0.2%	0.2%	0.2%	0.1%	0.1%	0.1%

APPENDIX FIVE

Continuation of the Results from Table 4.2: The Effect of Demographic Characteristics on Crime

Table A5.1 The effect of demographic characteristics on crime

The following assume a 1 percent change in the portion of the population in each category	Violent-crime rate	Murder rate	Rape rate	Aggravated assault rate	Robbery rate	Property-crime rate	Burlgary rate	Larceny rate	Auto-theft rate
Percent of population that is black, male, and in the following age ranges:									
10–19	6% (5%)	11% (8%)	4% (3%)	9%*** (7%)	11%*** (6%)	13%* (22%)	7%** (7%)	17%* (25%)	5% (6%)
20–29	0% (0%)	7% (4%)	8%** (4%)	−5%* (3%)	−1% (0%)	−1% (2%)	−2% (2%)	−1% (1%)	1% (1%)
30–39	4% (2%)	11%** (4%)	−8%* (3%)	20%* (9%)	1% (0%)	4% (4%)	−1% (0%)	0% (0%)	15%* (11%)
40–49	−2% (1%)	−34%* (9%)	90%* (22%)	−37%* (11%)	−1% (0%)	−2% (1%)	−3% (1%)	19%* (10%)	−68%* (32%)
50–64	18%** (7%)	−35%* (11%)	−15% (4%)	29%* (10%)	−1% (0%)	−5% (4%)	9% (4%)	−13%*** (9%)	6% (3%)
65 and over	12% (5%)	−14% (4%)	44%* (12%)	11% (4%)	17% (4%)	−4% (3%)	6% (3%)	−10% (6%)	−34%* (18%)
Percent of population that is black, female, and in the following age ranges:									
10–19	0% (0%)	4% (3%)	4% (2%)	−7% (6%)	−18%* (11%)	8%* (14%)	2% (2%)	16%* (23%)	−18%* (22%)
20–29	−10%* (7%)	−22%* (13%)	18%* (9%)	−19%* (13%)	−22%* (11%)	−10%* (14%)	−17%* (13%)	−1% (1%)	−25%* (26%)
30–39	12%* (7%)	−8% (4%)	15%* (6%)	9%* (5%)	38%* (14%)	13%* (14%)	27%* (16%)	9%* (9%)	17%* (14%)

Table A5.1 (*continued*)

The following assume a 1 percent change in the portion of the population in each category	Violent-crime rate	Murder rate	Rape rate	Aggravated assault rate	Robbery rate	Property-crime rate	Burlgary rate	Larceny rate	Auto-theft rate
40–49	1% (0%)	59%* (19%)	−74%* (21%)	27%* (10%)	−7% (2%)	6% (4%)	−5% (2%)	−3% (2%)	48%* (27%)
50–64	−21%* (11%)	20%*** (9%)	10% (4%)	−5% (3%)	7% (2%)	−2% (2%)	−22%* (12%)	1% (1%)	12% (9%)
65 and older	−20%* (11%)	31%* (14%)	−52%* (21%)	−16%** (8%)	−37%* (14%)	−20%* (22%)	−39%* (23%)	−12%** (12%)	24%* (19%)
Percent of population that is white, male, and in the following age ranges:									
10–19	−1% (1%)	−3% (2%)	1% (0%)	4%** (4%)	0% (0%)	−1% (1%)	1% (1%)	0% (0%)	−6%* (8%)
20–29	1% (1%)	6%* (5%)	4%* (3%)	2% (2%)	4%* (3%)	0% (1%)	2%** (2%)	0% (1%)	−2% (3%)
30–39	−1% (0%)	−1% (1%)	−4% (2%)	7%* (6%)	−7%* (4%)	−5%* (8%)	−3%*** (2%)	−6%* (8%)	−6%* (7%)
40–49	−1% (1%)	−2% (1%)	9%* (4%)	−4% (2%)	−11%* (5%)	−15%* (17%)	−10%* (7%)	−13%* (13%)	−10%* (8%)
50–64	−1% (0%)	−5% (3%)	4% (2%)	−9%* (7%)	−14%* (8%)	−13%* (20%)	7%* (6%)	−11%* (14%)	−27%* (31%)
65 and over	−13%* (15%)	2% (2%)	4% (4%)	−17%* (18%)	4% (3%)	−14%* (33%)	−12%* (15%)	−14%* (28%)	−11%* (19%)

Percent of population that is white, female, and in the following age ranges:									
10–19	2% (2%)	5% (3%)	7%* (5%)	−1% (1%)	6%** (4%)	8%* (15%)	8%* (8%)	9%* (14%)	9%* (12%)
20–29	1% (1%)	−4%*** (3%)	6%* (4%)	4%** (4%)	1% (1%)	−1% (2%)	−4%* (5%)	3%** (5%)	−3%*** (4%)
30–39	2% (2%)	4% (3%)	14%* (8%)	3% (2%)	−1% (0%)	4%* (6%)	2% (1%)	7%* (9%)	−10%* (12%)
40–49	−9%* (5%)	0% (0%)	−7%** (3%)	0% (0%)	−2% (1%)	6%* (7%)	−4%* (2%)	7%* (7%)	−2% (1%)
50–64	0% (0%)	1% (1%)	2% (1%)	8%* (6%)	3% (2%)	10%* (17%)	6%* (6%)	11%* (16%)	11%* (13%)
65 and over	6%* (9%)	−7%* (8%)	6%* (7%)	8%* (12%)	−9%* (9%)	2%** (6%)	5%* (8%)	4%* (9%)	−5%* (10%)
Percent of population that is other males in the following age ranges:									
10–19	25%** (11%)	66%* (23%)	56%* (18%)	19% (7%)	54%* (16%)	16%*** (13%)	27%* (13%)	15% (11%)	60%* (38%)
20–29	−12%** (4%)	14% (4%)	−17%** (4%)	−6% (2%)	1% (0%)	8%* (5%)	0% (0%)	20%* (11%)	−41% (18%)
30–39	23%** (6%)	−30% (6%)	−19% (4%)	40%* (9%)	−10% (2%)	−18%** (9%)	−43%* (12%)	−4% (2%)	65%* (24%)
40–49	13% (2%)	−36% (5%)	−24% (3%)	−19% (3%)	78%* (9%)	3% (1%)	24%** (4%)	−23%*** (7%)	46%* (11%)
50–64	−9% (2%)	−16% (3%)	24% (4%)	−28% (5%)	−40%** (6%)	−2% (1%)	27%** (6%)	−20% (7%)	−42%** (13%)
65 and over	35%** (6%)	−26% (3%)	87%* (10%)	102%* (15%)	−27% (3%)	−8% (2%)	19% (3%)	−23%*** (6%)	−18% (4%)

Table A5.1 (*continued*)

The following assume a 1 percent change in the portion of the population in each category	Violent-crime rate	Murder rate	Rape rate	Aggravated assault rate	Robbery rate	Property-crime rate	Burlgary rate	Larceny rate	Auto-theft rate
Percent of population that is other females in the following age ranges:									
10–19	–3% (1%)	–73%* (25%)	–11% (3%)	12% (5%)	–35%** (10%)	–18%** (14%)	–29%* (13%)	–23%** (16%)	–27%** (17%)
20–29	–13% (4%)	–33%** (8%)	21%* (5%)	9% (3%)	–30% (6%)	–15%** (9%)	–32%** (11%)	–33%** (18%)	–56%* (26%)
30–39	–22%* (6%)	–11% (2%)	16% (3%)	–17% (4%)	–22% (4%)	–9% (4%)	27%* (8%)	–28%* (12%)	–75%* (28%)
40–49	–14% (2%)	57%** (8%)	8% (1%)	18% (3%)	–48%** (6%)	25%** (8%)	28%* (5%)	70%* (20%)	–15% (4%)
50–64	–10% (2%)	44% (7%)	–66%* (10%)	–27% (5%)	37% (5%)	–5% (2%)	–49%* (11%)	16% (6%)	31%*** (9%)
65 and over	44%* (7%)	6% (1%)	–37%** (4%)	–44%* (6%)	–36%** (4%)	–11% (3%)	–14% (2%)	–5% (1%)	–59%* (13%)

*The result is statistically significant at the 1 percent level for a two-tailed *t*-test.
**The result is statistically significant at the 5 percent level for a two-tailed *t*-test.
***The result is statistically significant at the 10 percent level for a two-tailed *t*-test.

APPENDIX SIX Data and Additional Results for Chapter 10

Table A6.1 Means and standard deviations for variables for state-level data set from 1977 to 2005

Variable	Observations	Mean	Standard deviation	Minimum	Maximum
Natural log of murder rate	1,477	1.663805	0.7535024	−2.302585	4.394449
Natural log of rape rate	1,470	3.436527	0.4527471	0	4.897914
Natural log of robbery rate	1,477	4.606105	0.9641285	1.166271	7.398854
Natural log of aggravated assault rate	1,477	5.465097	0.6679146	1.997418	7.350902
Arrest rate for murder	1,443	82.51287	49.81053	0	1363.16
Arrest rate for rape	1,422	28.34408	15.90256	0	368.171
Arrest rate for robbery	1,439	34.38413	17.31153	0	310.63
Arrest rate for aggravated assault	1,446	42.64555	18.83171	0	343.5685
Execution rate	1,476	0.0018315	0.0069163	0	0.0997791
Prisoners as a percentage of the population	1,478	0.0031395	0.0029579	0.0000262	0.0561262
Greater than or equal to 10 years before law	1,479	0.1670047	0.3731063	0	1
8–9 years before law	1,479	0.0385396	0.1925599	0	1
6–7 years before law	1,479	0.0392157	0.1941734	0	1
4–5 years before law	1,479	0.0392157	0.1941734	0	1
2–3 years before law	1,479	0.0392157	0.1941734	0	1
0–1 years before law	1,479	0.0392157	0.1941734	0	1
1–2 years after law	1,479	0.0378634	0.1909304	0	1
3–4 years after law	1,479	0.0324544	0.1772634	0	1
5–6 years after law	1,479	0.0311021	0.1736524	0	1
7–8 years after law	1,479	0.0311021	0.1736524	0	1
9–10 years after law	1,479	0.0290737	0.16807	0	1
11–12 years after law	1,479	0.0162272	0.1263909	0	1

13–14 years after law	1,479	0.0135227	0.1155371	0	1	
15 or more years after the law	1,479	0.0148749	0.1210932	0	1	
Training hours required	1,479	1.252874	2.821413	0	16	
Training hours required squared	1,479	9.524679	29.10097	0	256	
Training hours required >8	1,479	0.1156187	0.3198752	0	1	
Permit duration in years	1,479	1.323867	1.955158	0	8	
Age required for permit	1,479	6.853955	9.533732	0	23	
Permit fees	1,479	22.67433	42.28191	0	219.0849	
Permit fees squared	1,479	2300.677	6100.163	0	47,998.21	
One-gun-a-month rule	1,479	0.0365112	0.186718	0	1	
Neighboring state has one-gun-a-month rule	1,479	0.0993915	0.2975878	0	1	
Assault weapons ban	1,479	0.354977	0.4666294	0	1	
Castle Doctrine	1,479	0.2151761	0.4105261	0	1	
Saturday night special	1,479	0.0892495	0.2852	0	1	
Regulations on private transfers	1,479	0.2734909	0.4446851	0	1	
State population	1,479	4,988,618	5,505,221	0	3.62E+07	
Unemployment rate	1,479	5.945571	2.022835	2.2	18	
Poverty rate	1,479	13.2191	5.488303	2.9	129	
Real per capita income	1,479	14,367.35	2,863.493	8,587.184	27,041.21	
Real per capita unemployment insurance payments	1,479	68.08383	41.86245	10.48574	403.6979	
Real per capita income maintenance payments	1,479	185.6606	70.61742	45.84409	552.4575	

Table A6.1 (*continued*)

Variable	Observations	Mean	Standard deviation	Minimum	Maximum
Percentage of the population by sex, race, and age:					
Black male 10–19 years of age	1,479	0.8328599	0.8871636	0.01642	5.45238
White male 10–19 years of age	1,479	6.257005	1.431695	1.21827	9.55684
Neither male 10–19 years of age	1,479	0.4455137	0.7688774	0.02724	5.72339
Black female 10–19 years of age	1,479	0.8740414	0.9543196	0.02013	6.31461
White female 10–19 years of age	1,479	6.370946	1.497998	1.38595	9.92206
Neither female 10–19 years of age	1,479	0.4427511	0.7457205	0.02888	5.39289
Black male 20–29 years of age	1,479	0.8814284	1.017257	0.01565	7.25909
White male 20–29 years of age	1,479	6.428597	1.493398	1.85564	10.59611
Neither male 20–29 years of age	1,479	0.4574014	0.8148456	0.02696	6.57483
Black female 20–29 years of age	1,479	0.9458942	1.147497	0.01144	8.40541
White female 20–29 years of age	1,479	6.561392	1.588738	1.8211	10.00352
Neither female 20–29 years of age	1,479	0.4594413	0.791113	0.02851	6.24559

Black male 30–39 years of age	1,479	0.801526	0.8907323	0.00809	5.69006
White male 30–39 years of age	1,479	6.255831	1.217634	1.67978	9.51396
Neither male 30–39 years of age	1,479	0.4593404	0.8215571	0.01779	5.90577
Black female 30–39 years of age	1,479	0.8726103	1.010259	0.00463	6.53929
White female 30–39 years of age	1,479	6.415199	1.258567	1.67469	9.44145
Neither female 30–39 years of age	1,479	0.4669661	0.8115325	0.01889	5.72763
Black male 40–49 years of age	1,479	0.6467276	0.7169999	0.00586	4.44528
White male 40–49 years of age	1,479	5.288351	1.298221	1.28408	8.65968
Neither male 40–49 years of age	1,479	0.3694927	0.6834877	0.01525	5.41537
Black female 40–49 years of age	1,479	0.7102121	0.8227117	0.00204	5.44682
White female 40–49 years of age	1,479	5.439538	1.286688	1.49004	8.52743
Neither female 40–49 years of age	1,479	0.3814887	0.6850796	0.01623	5.3861
Black male 50–64 year of age	1,479	0.6855771	0.7729429	0.00236	4.95675
White male 50–64 year of age	1,479	5.867447	1.255093	1.6931	9.81465

Table A6.1 (*continued*)

Variable	Observations	Mean	Standard deviation	Minimum	Maximum
Percentage of the population by sex, race, and age:					
Neither male 50–64 year of age	1,479	0.3626394	0.6881734	0.02167	5.28878
Black female 50–64 year of age	1,479	0.763614	0.8955906	0.00168	5.75757
White female 50–64 year of age	1,479	6.101135	1.308835	1.8069	10.00725
Neither female 50–64 year of age	1,479	0.3780827	0.6964578	0.02227	5.79369
Black male over 64 years of age	1,479	0.5470208	0.6538146	0.000713	4.11721
White male over 64 years of age	1,479	4.851585	1.274724	0.9776	7.63618
Neither male over 64 years of age	1,479	0.2767989	0.5631171	0.01591	4.54086
Black female over 64 years of age	1,479	0.643306	0.7923862	0.000996	6.03449
White female over 64 years of age	1,479	5.451655	1.553272	0.8869	9.49874
Neither female over 64 years of age	1,479	0.2982582	0.6113198	0.01638	5.73696

Table A6.2 The probability that any year's crime rate is different from the crime rate during years 0–1 before the law was enacted, as shown in figures 10.1a–10.1i

	Murder	Probability that year's murder rate is different from years 0–1 before the law	Rape	Probability that year's rape rate is different from years 0–1 before the law	Robbery	Probability that year's robbery rate is different from years 0–1 before the law	Aggravated assault	Probability that year's aggravated assault rate is different from years 0–1 before the law
Greater than or equal to 10 years before law	0.797		1.417		0.888		0.711	
8–9 years before law	0.770		1.473		0.931		0.695	
6–7 years before law	0.770		1.460		0.948		0.702	
4–5 years before law	0.799		1.486		0.920		0.679	
2–3 years before law	0.792		1.463		0.921		0.672	
0–1 years before law	0.795		1.476		0.945		0.646	
1–2 years after law	0.735	56.9%	1.404	43.8%	0.946	96.0%	0.528	21.6%
3–4 years after law	0.716	45.4%	1.306	6.9%	0.931	94.8%	0.510	15.9%
5–6 years after law	0.637	13.5%	1.300	5.8%	0.903	77.4%	0.459	5.0%
7–8 years after law	0.623	10.0%	1.320	9.4%	0.947	95.6%	0.547	29.9%
9–10 years after law	0.614	9.6%	1.356	21.3%	0.927	92.3%	0.542	29.2%
11–12 years after law	0.597	7.3%	1.287	5.7%	0.896	74.4%	0.558	38.1%
13–14 years after law	0.537	2.2%	1.341	19.0%	0.842	47.9%	0.525	23.6%
15 or more years after the law	0.482	0.6%	1.358	25.5%	0.835	44.8%	0.521	22.4%

Table A6.3 Regression for murder that corresponds to the estimates shown in figure 10.1b

Variable	Coefficient	Absolute *t*-statistic	Probability
Arrest rate for murder	–0.00047	2.86	0.004
Execution rate	–3.020	2.71	0.007
Prisoners as a percentage of the population	–7.312	1.92	0.055
Greater than or equal to 10 years before law	0.797	4.16	0
8–9 years before law	0.770	4	0
6–7 years before law	0.770	3.98	0
4–5 years before law	0.799	4.1	0
2–3 years before law	0.792	4.04	0
0–1 years before law	0.795	4.07	0
1–2 years after law	0.735	4.13	0
3–4 years after law	0.716	4.02	0
5–6 years after law	0.637	3.55	0
7–8 years after law	0.623	3.47	0.001
9–10 years after law	0.614	3.35	0.001
11–12 years after law	0.597	3.15	0.002
13–14 years after law	0.537	2.81	0.005
15 or more years after the law	0.482	2.53	0.011
Training hours required	–0.022	1.74	0.082
Training hours required squared	0.001	1.02	0.309
Training hours required >8	–0.014	0.47	0.636
Permit duration in years	–0.015	1.07	0.284
Age required for permit	0.018	3.22	0.001
Permit fees	0.002	1.83	0.067
Permit fees squared	0.000001	0.2	0.839
One-gun-a-month rule	0.001	0.03	0.976
Neighboring state has one-gun-a-month rule	0.167	5.13	0
Assault weapons ban	0.004	0.11	0.91
Castle Doctrine	–0.088	2.73	0.006
Saturday night special	0.303	3.67	0
Regulations on private transfers	0.020	0.88	0.378
State population	0.000	1.2	0.23
Unemployment rate	–0.032	5.23	0
Poverty rate	–0.002	1.36	0.174
Real per capita income	0.000	0.59	0.555
Real per capita unemployment insurance payments	0.001	2.42	0.016
Real per capita income maintenance payments	0.000	1.95	0.051

Table A6.3 (*continued*)

Variable	Coefficient	Absolute *t*-statistic	Probability
Percentage of the population by sex, race, and age:			
Black male 10–19 years of age	0.685	1.29	0.199
White male 10–19 years of age	–0.405	2.78	0.006
Neither male 10–19 years of age	2.362	3.1	0.002
Black female 10–19 years of age	–0.836	1.64	0.1
White female 10–19 years of age	0.383	2.69	0.007
Neither female 10–19 years of age	–1.880	2.52	0.012
Black male 20–29 years of age	0.462	1.07	0.284
White male 20–29 years of age	0.446	3.66	0
Neither male 20–29 years of age	0.879	1.06	0.291
Black female 20–29 years of age	–0.345	0.83	0.405
White female 20–29 years of age	–0.491	4.05	0
Neither female 20–29 years of age	–0.821	0.98	0.328
Black male 30–39 years of age	–1.199	2.28	0.023
White male 30–39 years of age	–0.511	3.01	0.003
Neither male 30–39 years of age	2.767	3.09	0.002
Black female 30–39 years of age	0.935	1.91	0.057
White female 30–39 years of age	0.597	3.54	0
Neither female 30–39 years of age	–3.145	3.48	0.001
Black male 40–49 years of age	0.847	1.45	0.148
White male 40–49 years of age	0.402	2.44	0.015
Neither male 40–49 years of age	–3.866	4.24	0
Black female 40–49 years of age	–0.561	1.11	0.269
White female 40–49 years of age	–0.412	2.49	0.013
Neither female 40–49 years of age	3.962	4.32	0
Black male 50–64 year of age	2.055	4.91	0
White male 50–64 year of age	–0.020	0.16	0.87
Neither male 50–64 year of age	–0.697	0.94	0.348
Black female 50–64 year of age	–1.617	4.64	0
White female 50–64 year of age	0.080	0.67	0.503
Neither female 50–64 year of age	–0.196	0.26	0.792
Black male over 64 years of age	–0.589	2.14	0.033
White male over 64 years of age	–0.172	3.25	0.001
Neither male over 64 years of age	1.118	2.86	0.004
Black female over 64 years of age	0.756	3.54	0
White female over 64 years of age	–0.008	0.17	0.864
Neither female over 64 years of age	–0.406	1.2	0.231

APPENDIX SEVEN Using Gun Magazine Sales as a Proxy for Gun Ownership

Table A7.1 examines whether changes in gun magazine sales are related to changes in gun ownership rates. Changes in the sales of the six gun magazines are related to the gun ownership rate in a state.[1] Information on gun ownership rates is from the National Opinion Research Corporation's General Social Survey. Survey data was readily available from 1977 to 1998, though they are not available for every year and the sample size is relatively small.[2] While I have used the larger CBS News General Election Exit Poll or the Voter News Survey in the past,[3] I will use the General Social Survey here because Duggan refers to it.[4] Two different measures of gun ownership were derived from General Social Survey: a simple rate at which people own guns and the rate at which households owned guns.[5]

The regressions in table A7.1 attempt to account for the average differences in gun ownership across states and any national changes in gun ownership rates across years. What the table shows is that the gun magazines that most closely proxy the survey data are the two NRA publications, *American Hunter* and *American Rifleman*, and *Handguns* magazine. For these three magazines, increasing magazine sales by 1 percent is associated with an increased gun ownership rate of anywhere from 0.34 to 0.52 percent.

Guns and Ammo is positively related to the survey data, but the relationship is not statistically

significant and is only about a third to a half as large as for the three most closely related magazines. Duggan provides a similar analysis using only *Guns and Ammo* and claims to provide a significant positive relationship between survey data and magazine sales, but while he uses the data at the state level, he weights the polling data by regional and not state-level demographic characteristics. Of the six magazines, *Guns and Ammo* ranked fourth in its ability to explain changes in the survey data, and its effect was never statistically different from zero.

So do increases in either gun magazine sales or survey data precede changes in murder? To answer this I added the sales of the different gun magazines into the crime regressions reported earlier in this book. This allows us to account for the impact that other factors have on murder rates. These include the arrest rate for murder, the death penalty execution rate, the population density, the unemployment rate, the poverty rate, per capita income, per capita welfare payments, and detailed demographic information on the share of the population by age, sex, and race.[6]

The results are reported in table A7.1. If more sales of a gun magazine lead in a year or two to higher murder rates, it appears to occur only for the fourth largest magazine, *Guns and Ammo*, where a 1 percent increase in magazine sales increases murder rates by 0.24 percent the following year and by 0.17 percent two years later. What is puzzling with these results is that handguns are used to commit most murders (indeed, that is the reason that Duggan claims to focus on *Guns and Ammo*). Yet, the relationship between the two purely handgun magazines and murder rates is essentially zero, with coefficients that are less than 18 percent of the size of the *Guns and Ammo* coefficients in three of the four cases. Almost the same results are obtained when homicide or firearm homicide data are used. *Guns and Ammo* magazine is the only magazine that ever implies a statistically significant relationship for both previous years of sales.

Table A7.1 Effect of gun magazine sales on murder rate

Name	Average annual national sales from 1990 to 1999	Percent change in the rate that guns are owned in households from increasing magazine sales one year earlier by 1 percent	Percent change in murder rate from increasing magazine sales one year earlier by 1 percent	Percent change in murder rate from increasing magazine sales two years earlier by 1 percent
Guns and Ammo	147,110	0.28%	0.25%*	0.17%**
American Handgunner	1,027,854	0.19%	0.04%	0.03%
Handguns	1,328,805	0.50%***	0.10%	0.002%
American Hunter	569,108	0.58%*	0.19%	–0.31%***
American Rifleman	148,308	0.79%*	0.32%	–0.12%
North American Hunter	766,326	0.10%	–0.11%	–0.08%

* The result is significant at the 1 percent level for a two-tailed *t*-test
** The result is significant at the 5 percent level for a two-tailed *t*-test
*** The result is significant at the 10 percent level for a two-tailed *t*-test

NOTES

CHAPTER ONE

1. ABC News/Washington Post Poll, April 21–24, 2009; Graduate Institute of International Studies, *Small Arms Survey* (London: Cambridge University Press, 2007); and U.S. Department of Commerce, Bureau of the Census, *Statistical Abstract of the United States* (Washington, DC: U.S. Government Printing Office, 2009). And discussions of older data are available in Gary Kleck, *Targeting Guns* (Hawthorne, NY: Aldine de Gruyter Publishers, 1997); and David B. Kopel, *Guns: Who Should Have Them?* (Amherst, NY: Prometheus Books, 1995), pp. 260–61, 300–301. The estimates on the number of guns are very sensitive to the rate at which guns are assumed to wear out. Higher depreciation rates produce a lower estimated current stock. About a third of all guns are handguns.

A recent poll by the *Dallas Morning News* indicated that "52 percent of the respondents said they or a member of their household own a gun. That response is consistent with Texas Polls dating to 1985 that found more than half of Texans surveyed own guns.

"In the latest poll, of those who said they owned a gun, 43 percent said they had two to five guns; 28 percent said they had one; and 19 percent said they had more than five guns. And of the gun owners polled, 65 percent said they had some type of shooting instruction." See Sylvia Moreno, "Concealed-Gun Law Alters Habits of Some Texans, Poll Finds Supporters, Foes Disagree About What That Means," *Dallas Morning News,* Nov. 3, 1996, p. 45A. The number of people owning guns is examined in more detail in chapter 3.

2. For example, in Chicago 59 percent of police officers report never having had to fire their guns. See Andrew Martin, "73% of Chicago Cops Have Been Attacked While Doing Their Job," *Chicago Tribune,* June 17, 1997, p. A3.

3. Dawn Lewis of Texans Against Gun Violence provided a typical reaction from gun-control advocates to the grand jury decision not to charge Gordon Hale. She said, "We are appalled. This law is doing what we expected, causing senseless death." Mark Potok, a Texan, said that the concealed-gun law saved his life. "I did what I thought I had to do," (*USA Today,* Mar. 22, 1996, p. 3A). For a more recent

evaluation of the Texas experience, see "Few Problems Reported After Allowing Concealed Handguns, Officers Say," *Fort Worth Star-Telegram,* July 16, 1996. By the end of December 1996, more than 120,000 permits had been issued in Texas.

4. Japan Economic Newswire, "U.S. Jury Clears Man Who Shot Japanese Student," *Kyodo News Service,* May 24, 1993; and Lori Sharn, "Violence Shoots Holes in USA's Tourist Image," *USA Today,* Sept. 9, 1993, p. 2A.

5. Gary Kleck, *Point Blank: Guns and Violence in America* (Hawthorne, NY: Aldine de Gruyter Publishers, 1991).

6. John R. Lott, Jr., "Now That the Brady Law Is Law, You Are Not Any Safer Than Before," *Philadelphia Inquirer,* Feb. 1, 1994, p. A9. For a more detailed breakdown of police shootings in the larger U.S. cities, see William A. Geller and Michael S. Scott, *Deadly Force: What We Know* (Washington, DC: Police Executive Research Forum, 1992).

7. "Mexican Woman Who Killed Would-Be Rapist to Turn to Activism," *Associated Press Newswire,* Feb. 12, 1997, dateline Mexico City.

8. For many examples of how guns have prevented rapes from occurring, see Paxton Quigley, *Armed and Female* (New York: St. Martin's, 1989).

9. Newspaper stories abound. Examples of pizza deliverymen defending themselves can be found in the *Chicago Tribune,* May 22, 1997, p. 1; *Baltimore Sun,* Aug. 9, 1996, p. B1; *Tampa Tribune,* Dec. 27, 1996, p. A1; and *Los Angeles Times,* Jan. 28, 1997, p. B1. Another recent example involved a pizza deliveryman in New Paltz, NY (*Middletown (New York) Times Herald Record,* Jan. 25, 1997). Examples of thwarted carjackings (*Little Rock Democrat-Gazette,* Aug. 3, 1996) and robberies at automatic teller machines (*York (Pennsylvania) Daily Record,* April 25, 1996) are also common.

For a case in which a gun was merely brandished to stop an armed street robbery, see the *Annapolis Capitol,* Aug. 7, 1996. Other examples of street robberies that were foiled by law-abiding citizens using concealed handguns include the case of Francisco Castellano, who was shot in the chest during an attempted street robbery by two perpetrators but was able to draw his own handgun and fire back. Castellano's actions caused the robbers to flee the scene (Corey Dada and Ivonne Perez, "Armed Robbery Botched as Restaurateur Shoots Back," *Miami Herald,* Aug. 3, 1996, p. B6.) The following story gives another example: "Curtis Smalls was standing outside the USF&G building when he was attacked by two thugs. They knocked him down, robbed, and stabbed him. Mr. Smalls pulled a .38-caliber revolver and shot both attackers, who were later charged with this attack and two other robberies and are suspects in at least 15 more robberies." This story was described in "Gun Laws Render Us Self-Defenseless," *Baltimore Sun,* Sept. 27, 1996. See also Charles Strouse, "Attacker Killed by His Victim," *Fort Lauderdale (Florida) Sun-Sentinel,* Sept. 16, 1997, p. 4B; Henry Pierson Curtis, "Bicyclist Kills Man Who Tried to Rob Him," *Orlando Sentinel,* Sept. 19, 1997, p. D3; and *Florence (Alabama) Times Daily,* Dec. 27, 1996, for other examples. Examples of foiled carjackings can be found in "Guns and Carjacking: This Is My Car," *Economist,* Sept. 20, 1997. Many other types of robberies have been foiled by people carrying concealed handguns. In at least one case, citizens carrying concealed handguns in Jacksonville, Florida may have saved a restaurant waitress from being shot ("Pistol-Packing Seniors in Florida Wound Robber," Reuter Information Service, Sept. 24, 1997, 6:15 P.M. EDT). For another example, see Clea Benson, "Wounded Barmaid Kills Gunman in Holdup," *Philadelphia Inquirer,* Jan. 23, 1997, p. R1.

10. Stories involving defensive uses of guns in the home are featured even more prominently. For example, four intruders forced their way into the home of two elderly women, struggled with them, and demanded their car keys. The attack stopped only after one of the women brandished her handgun ("Pistol-Packing Grandmas Honored by Sheriff," *Associated Press Newswire,* Feb. 16, 1997 2:30 P.M. EST, dateline Moses Lake, WA). In another case a twenty-three-year-old burglar "pummeled" a 92-year-old man and "ransack[ed]" his house. The burglar left only after the elderly man reached his gun ("Burglar Puts 92-Year-Old in the Gun Closet and Is Shot," *New York Times,* Sept. 7, 1995, p. A16). Although the defensive

use of guns in the home is interesting, my focus in this book is on the effects of allowing citizens to carry concealed handguns.

11. Not all news stories of defensive uses involve shots being fired. For example, the *Arizona Republic* reported the following: "In January 1995, a permit-holder who lives in Scottsdale pulled a handgun from a shoulder holster and scared off two men armed with aluminum baseball bats who attempted to rob him near 77th Street and East McDowell Road. No shots were fired." ("In Arizona, High Numbers of Concealed-Weapon Permit Holders Are Found in the Suburbs," *Arizona Republic,* Mar. 17, 1996.)

12. "Mom Saves Self and Child with Handgun," *Atlanta Constitution,* Nov. 12, 1996, p. E2.

13. See *Los Angeles Times,* Jan. 28, 1997, p. B1. Similarly, Pete Shields, Handgun Control, Inc.'s founder, wrote that "the best defense against injury is to put up no defense—give them what they want or run. This may not be macho, but it can keep you alive." See Pete Shields, *Guns Don't Die, People Do* (New York: Arbor, 1981).

14. Problems exist with the National Crime Victimization Survey both because of its nonrepresentative sample (for example, it weights urban and minority populations too heavily) and because it fails to adjust for the fact that many people do not admit to a law-enforcement agency that they used a gun, even defensively; such problems make it difficult to rely too heavily on these estimates. Unfortunately, this survey is the only source of evidence on the way the probability of significant injury varies with the level and type of resistance.

15. Lawrence Southwick, Jr., "Self-Defense with Guns: The Consequences," *Journal of Criminal Justice*, vol. 8 (2000), tables 5 and 6; see also Kleck, *Point Blank*.

16. For example, see David B. Kopel, *The Samurai, the Mountie, and the Cowboy* (Amherst, NY: Prometheus, 1992), p. 155; and John R. Lott, Jr., "Now That the Brady Law Is Law, You Are Not Any Safer Than Before," *Philadelphia Inquirer,* Feb. 1, 1994, p. A9.

17. James D. Wright and Peter Rossi, *Armed and Considered Dangerous: A Survey of Felons and Their Firearms* (Hawthorne, NY: Aldine de Gruyter Publishers, 1986).

Examples of anecdotes in which people successfully defend themselves from burglaries with guns are quite common. For example, see "Burglar Puts 92-Year-Old in the Gun Closet and Is Shot," *New York Times,* Sept. 7, 1995, p. A16. George F. Will, in "Are We 'a Nation of Cowards'?" *Newsweek,* Nov. 15, 1993, discusses more generally the benefits produced from an armed citizenry.

18. See Wright and Rossi, *Armed and Considered Dangerous,* p. 150.

19. Ibid., p. 151.

20. *Baltimore Sun,* Oct. 26, 1991; referred to in Don Kates and Dan Polsby, "Of Genocide and Disarmament," *Journal of Criminal Law and Criminology* 86 (Fall 1995): 252.

21. Rebecca Trounson, "Anxiety, Anger Surround Return of Young Survivors," *Los Angeles Times,* Mar. 14, 1997, p. A1.

22. It is possible that both terrorists and citizens are worse off because of the switch to bombings if shootings would have involved targeted attacks against fewer citizens.

23. David Firestone, "Political Memo: Gun Issue Gives Mayor Self-Defense on Crime," *New York Times,* Mar. 7, 1997, p. B1.

24. Using an on-line retrieval search, it is easy to find many news articles and letters to the editor that repeat this common claim. For example, one letter to the *Newark Star-Ledger* (Oct. 12, 1996) stated that "over half the firearm homicides are committed not by criminals but by friends, family members, and lovers—people with no criminal record."

25. The sum of these percentages does not equal precisely 100 percent because fractions of a percent were rounded to the nearest whole percent.

26. Captain James Mulvihill recently testified before the U.S. Senate that "the greater L.A. area suffers under the weight of more than 1,250 known street gangs, whose membership numbers approximately 150,000. These gangs are responsible for nearly 7,000 homicides over the last 10 years, and injury to thousands of other people." (Prepared testimony of Captain

James Mulvihill, commander of the Safe Streets Bureau for Sheriff Block of Los Angeles County before the Senate Judiciary Committee, Apr. 23, 1997.)

27. I would like to thank Kathy O'Connell of the Illinois Criminal Justice Information Authority for taking the time to provide me with such a detailed breakdown of these data.

28. Many such murders also end up in the "undetermined relationship" category.

Probably the best known study of who kills whom is by Daly and Wilson. They examined nonaccidental homicide data for Detroit in 1972. In contrast to my emphasis here, however, they focused exclusively on trying to explain the composition of murders when relatives killed relatives. Of the total of 690 murders committed in Detroit in 1972, 243 (47.8 percent) involved unrelated acquaintances, 138 (27.2 percent) involved strangers, and 127 (25 percent) involved relatives. Of this last category, 32 (4.6 percent) involved blood relatives, and 80 (11.6 percent) victims were spouses (36 women killed by their husbands, and 44 men killed by their wives). The percentage of Chicago's murders involving relatives in 1972 was very similar (25.2 percent), though by the 1990–95 period the percentage of murders involving relatives had fallen to 12.6 percent (7.2 percent involving spouses). For the information about Detroit, see Martin Daly and Margo Wilson, *Homicide* (Hawthorne, NY: Aldine de Gruyter Publishers, 1988).

29. Kathy O'Connell of the Illinois Criminal Justice Information Authority provided these data.

30. See also Daniel D. Polsby, "From the Hip: The Intellectual Argument in Favor of Restrictive Gun Control Is Collapsing. So How Come the Political Strength of Its Advocates Is Increasing?" *National Review* (Mar. 24, 1997): 35–36.

31. In these seventy-five largest counties in 1988, 77 percent of murder arrestees and 78 percent of defendants in murder prosecutions had criminal histories, with over 13 percent of murders being committed by minors, who by definition cannot have criminal records. This implies that 89 percent of those arrested for murders must be adults with criminal records, with 90 percent of those being prosecuted. See Bureau of Justice Statistics Special Reports, "Murder in Large Urban Counties, 1988," (Washington, DC: U.S. Department of Justice, 1993), and "Murder in Families" (Washington, DC: U.S. Department of Justice, 1994); see also Don B. Kates and Dan Polsby, "The Background of Murders," Northwestern University Law School working paper (1997).

32. The average victim had 9.5 prior arraignments, while the average offender had 9.7. David M. Kennedy, Anne M. Piehl, and Anthony A. Braga, "Youth Violence in Boston: Gun Markets, Serious Youth Offenders, and a Use-Reduction Strategy," *Law and Contemporary Problems* 59 (Winter 1996): 147–96.

33. The relationship between age and sex and who commits murders holds across other countries such as Canada; see Daly and Wilson, *Homicide,* pp. 168–70.

34. James Q. Wilson and Richard J. Herrnstein, *Crime and Human Nature,* (New York: Simon and Schuster, 1985), p. 177. Wilson and Herrnstein also discuss in chapter 3 evidence linking criminality to physical characteristics. The surveys that they summarize find evidence that criminality is more likely among those who are shorter and more muscular.

35. Ibid., pp. 204–7; see also Michael K. Block and Vernon E. Gerety, "Some Experimental Evidence on the Differences between Student and Prisoner Reactions to Monetary Penalties," *Journal of Legal Studies* 24 (Jan. 1995): 123–138.

36. John J. Dilulio, Jr., "The Question of Black Crime," *The Public Interest* 117 (Fall 1994): 3–24; and "White Lies About Black Crime," *The Public Interest* 118 (Winter 1995): 30–44.

37. While there are many sources of misinformation on the deaths that arise from handguns, some stories attempt to clarify claims. For example, a *Nando Times* (www.nando.com) news story (Oct. 26, 1996) reported that "during a campaign visit here this week, President Clinton met with the widow of a police officer killed in the line of duty and later during a political rally cited his death as a reason to outlaw armor-piercing bullets. What he did

not tell his audience, however, was that the officer died in an auto accident, not from gunfire. . . . Neither a bulletproof vest nor a ban on 'cop-killer bullets,' however, would have saved Officer Jerome Harrison Seaberry Sr., 35. He was responding to a radio call for backup on Christmas night last year when 'he lost control of his vehicle, going too fast . . . hit a tree head-on, and the vehicle burst into flames,' said Lake Charles Police Chief Sam Ivey. Armor-piercing bullets, Ivey said, 'had nothing to do with it.'"

38. National Center for Injury Prevention, *Injury Mortality Statistics* (Atlanta: Centers for Disease Control, 1999).

39. Editorial, "The Story of a Gun and a Kid," *Washington Times,* May 22, 1997, p. A18.

40. Joyce Price, "Heston Attacks Trigger-Lock Proposal: Actor Begins Role as NRA Executive," *Washington Times,* May 19, 1997, p. A4.

41. Currently, the impact of gun locks is difficult to test simply because no state requires them. Seven states (California, Connecticut, Florida, Hawaii, Minnesota, New Jersey, and North Carolina) and the District of Columbia have laws regarding proper storage, but these laws do not mandate a particular method of storage.

42. W. Kip Viscusi, "The Lulling Effect: The Impact of Child-Resistant Packaging on Aspirin and Analgesic Ingestions," *American Economic Review* (May, 1984): 324–27.

43. The Department of Justice's National Institute of Justice recently released a government-funded study entitled "Guns in America: National Survey on Private Ownership and Use of Firearms," by Philip Cook and Jens Ludwig. The study used poll evidence from 2,568 adults in 1994 to claim that "20 percent of all gun-owning households had an unlocked, loaded gun at the time of the survey. The report cited the accidental deaths of 185 children under the age of 14, and many times that number of accidental shootings. For each death, there are several accidental shootings that cause serious injuries." Fifty percent of respondents were said to have stored an unloaded gun that was unlocked. The Justice Department's press release quoted Attorney General Janet Reno as claiming that "these results show how dangerous unlocked guns are to children. That's why we must pass the child-safety-lock provision in the President's Anti-Gang and Youth Violence Act of 1997, now before Congress. A locked gun can avoid a family tragedy." Ignoring problems with the survey itself, several problems exist with these conclusions. First, the report does not show that those 20 percent of gun-owning households with "unlocked, loaded" guns were responsible for the 185 firearm deaths of children. We would be interested to know if the 20 percent of households included children. Second, the report only concentrates on the costs, while ignoring any possible benefits. One question that might be useful in considering benefits is this: Where did those with unlocked, loaded guns tend to live? For example, were they more likely to live in urban, high-crime areas? (See Department of Justice, *PR Newswire,* May 5, 1997.)

Unfortunately, despite issuing press releases and talking to the press about their findings, neither the Department of Justice, nor professors Cook or Ludwig, nor the Police Foundation, which oversaw the government grant, have made any attempt to release their data at least by August 1997.

44. U.S. Department of Commerce, Bureau of the Census, *Statistical Abstract of the United States* (Washington, DC: U.S. Government Printing Office, 1995). A common claim I will discuss later is that "more than half of all firearm deaths occur in the home where the firearm is kept." As noted in the text, since one-half of all firearm deaths are suicides, this should come as no surprise.

45. Editorial, *Cincinnati Enquirer,* Jan. 23, 1996, p. A8. Others share this belief. "It's common sense," says Doug Weil, research director at the Center to Prevent Handgun Violence, in Washington. "The more guns people are carrying, the more likely it is that ordinary confrontations will escalate into violent confrontations" (William Tucker, "Maybe You Should Carry a Handgun," the *Weekly Standard,* Dec. 16, 1996, p. 30).

46. For these arguments, see P. J. Cook, "The Role of Firearms in Violent Crime," in M. E.

Wolfgang and N. A. Werner, eds., *Criminal Violence* (Newbury, NJ: Sage Publishers, 1982); and Franklin Zimring, "The Medium Is the Message: Firearm Caliber as a Determinant of Death from Assault," *Journal of Legal Studies* 1 (1972): 97–124.

47. P. J. Cook, "The Technology of Personal Violence," *Crime and Justice: Annual Review of Research* 14 (1991): 57, 56 n. 4. Cook reported 82,000 defensive uses for an earlier period. The irony of Cook's position here is that his earlier work argued that the National Crime Victimization Survey radically underreports other violence-related events, including domestic violence, rapes, and gunshot woundings linked to criminal acts; see Gary Kleck, *Targeting Guns* (Hawthorne, NY: Aldine de Gruyter Publishers, 1997).

It is easy to find people who argue that concealed handguns will have no deterrent effect. H. Richard Uviller writes that "more handguns lawfully in civilian hands will not reduce deaths from bullets and cannot stop the predators from enforcing their criminal demands and expressing their lethal purposes with the most effective tool they can get their hands on." See H. Richard Uviller, *Virtual Justice: The Flawed Prosecution of Crime in America* (New Haven: Yale University Press, 1996), p. 95.

48. For instance, the University of Chicago's National Opinion Research Center states that reported gun ownership rates are much lower in urban areas. In the nation's twelve largest cities, just 18 percent of all households report owning a gun. Women in rural areas appear to own guns at about three times the rate that women in the twelve largest cities do. For a discussion about how these numbers vary between urban and rural areas generally or for women across areas, see James A. Davis and Tom W. Smith, *General Social Surveys, 1972–1993: Cumulative Codebook* (Chicago: National Opinion Research Center, 1993); and Tom W. Smith and Robert J. Smith, "Changes in Firearm Ownership Among Women, 1980–1994," *Journal of Criminal Law and Criminology* 86 (Fall 1995): 133–49. This issue is discussed further in chapter 3.

49. Gary Kleck provides an excellent discussion of the methodological weaknesses in the National Crime Victimization Survey. As an example, he writes, "Unfortunately, 88 percent of the violent crimes reported to the [National Crime Victimization Survey] in 1992 were committed away from the victim's home. Thus, by the time the self-protection question is asked, almost all the [respondents] who in fact had used a gun for self-protection know that they had already admitted that the incident occurred in a place where it would be a crime for them to have possessed a gun" (see Kleck, *Targeting Guns*).

50. Still another survey deals more directly with the number of lives potentially saved by defensive gun uses. It reports that potential victims believe that each year, 400,000 people "almost certainly" saved a life by using a gun, though even the researchers providing this estimate believe that the number is too high. See Gary Kleck and Marc Gertz, "Armed Resistance to Crime: The Prevalence and Nature of Self-Defense with a Gun," *Journal of Criminal Law and Criminology* 86 (Fall 1995): 150, 153, 180, 180–2; see also Gary Kleck, "Critique of Cook/Ludwig Paper," undated manuscript, Dept. of Criminology, Florida State University). Recent evidence confirms other numbers from Kleck's and Gertz's study. For example, Annest et al. estimate that 99,025 people sought medical treatment for nonfatal firearm woundings. When one considers that many criminals will not seek treatment for wounds and that not all wounds require medical treatment, Kleck's and Gertz's estimate of 200,000 woundings seems somewhat plausible, though even Kleck and Gertz believe that this is undoubtedly too high, given the very high level of marksmanship that this implies for those firing the guns. Even if the true number of times that criminals are wounded is much smaller, however, this still implies that criminals face a very real expected cost when they attack armed civilians. For discussions of the defensive use of guns, see J. L. Annest et al., "National Estimates of Nonfatal, Firearm-Related Injuries: Beyond the Tip of the Iceberg," *Journal of the American Medical Association* (June 14, 1995): 1749–54; and Lawrence Southwick, Jr., "Self-Defense with Guns: The Consequences," *Managerial and Decision Economics* (forthcoming).

51. Information from telephone call to Susan Harrell, Administrator, Bureau of License

Issuance for the state of Florida in Tallahassee. David Kopel writes that "in Florida as a whole, 315,000 permits had been issued by December 31, 1995. Only five had been revoked because the permit holder committed a violent crime with a gun." See David Kopel, "The Untold Triumph of Concealed-Carry Permits," *Policy Review* 78 (July–Aug. 1996); see also Stan Schellpeper, "Case for a Handgun-Carry Law," *Omaha World-Herald,* Feb. 6, 1997, p. 27; and Clayton E. Cramer and David B. Kopel, "'Shall Issue': The New Wave of Concealed-Handgun Permit Laws," *Tennessee Law Review* 62 (Spring 1995): 679, 691. An expanded version of this last article is available from the Independence Institute, 14142 Denver West Parkway, Suite 185, Golden, Colorado, 80401–3134.

52. Cramer and Kopel, "New Wave of Concealed-Handgun Permit Laws," pp. 691–92.

53. Bob Barnhart, "Concealed-Handgun Licensing in Multnomah County," mimeo (Intelligence/Concealed Handgun Unit: Multnomah County, Oct. 1994).

54. See *Richmond Times Dispatch,* Jan. 16, 1997.

55. Schellpeper, "Case for a Handgun-Carry Law," p. 27.

56. "Packin' and More Peaceful," *Las Vegas Review-Journal,* Aug. 5, 1996, p. 6B.

57. Kentucky State Police Trooper Jan Wuchner is also quoted as saying that he has "heard nothing around the state related to crime with a gun committed by permit holders. There has been nothing like that that I've been informed of." See Terry Flynn, "Gun-Toting Kentuckians Hold Their Fire," *Cincinnati Enquirer,* June 16, 1997, p. A1.

58. Lee Anderson, "North Carolina's Guns," *Chattanooga Free Press,* May 31, 1997, p. A4.

59. Lawrence Messina, "Gun-Permit Seekers Not the Criminal Type," *Charleston Gazette,* July 28, 1997, p. C1.

60. This is the incident discussed in note 3 that occurred during the beginning of 1996 in Texas. As for citizens with concealed handgun permits coming to the aid of police officers see the end of note 68.

61. Peter Hermann, "Unarmed Resident Slain by Intruder; Victim's Rifle Taken by Authorities," *Baltimore Sun,* Sept. 19, 1996, p. B1.

62. Christi Parsons and Andrew Martin, "Bead Drawn on Gun Law," *Chicago Tribune,* May 22, 1997, p. 1; the article includes a long list of such cases, not all of which ended with the charges being dropped. For example,

> In Chicago, two motorists, both U.S. Marine Recruiters, were charged with felonies for allegedly having guns in their car when stopped by police for a minor traffic violation. State Rep. Joel Brunsvold (D-Milan) said a downstate woman who kept an assembled rifle in her car to shoot rodents on her farm was pulled over and charged with a felony, as if she had been planning a drive-by shooting. And in March, Chicago Bears defensive end Alonzo Spellman was charged with a felony after volunteering to a police officer during a traffic stop that he had a handgun inside his car.

63. Stephen Singular, *Talked to Death* (New York: Beech Tree Books, 1987), p. 142. In several other tragic cases people have carried concealed handguns because of death threats, only to be arrested by the police for carrying them; see, for example, Kristi Wright, "Executive Decision," *Omaha World-Herald,* June 8, 1997, p. 1E.

64. A recent case in Oklahoma illustrates how a gun allowed an elderly woman to defend herself:

> An 83-year-old woman proved her aim was good Tuesday morning as she shot a burglar trying to get inside her home. Delia Mae Wiggins's home has been burglarized four times. She was beaten by a burglar in November. And she wasn't going to let it happen again. When she heard someone trying to break into her home at about 5 A.M., Wiggins said she grabbed a gun that had been loaded for nine years but never fired. She told police an intruder removed her window-unit air conditioner to enter her home. She said she warned the intruder she was armed. Then she pulled

> the trigger, hitting the intruder in the thigh. The man backed out the window and fled. (Robert Medley, "83-Year-Old Woman Shoots Fifth Burglar to Try to Victimize Her," *Oklahoma City Daily Oklahoman,* May 21, 1997.

This case also illustrates another point, because it involves a crime where the perpetrator would have been classified as knowing the intended victim. The attacker had just a few days earlier "mowed a lawn at a rental property for her."

65. Kristi O'Brien, "Concealed-Gun Legislation Bottled Up Again," *Copley News Service* (Apr. 15, 1997).

66. As Lon Cripps, the police chief in Langsberg, Montcalm County, Michigan, said in discussing concealed handguns, "There comes a time when you have to take responsibility for your own life. Police officers just aren't always going to be there" (*Detroit News,* June 14, 1996).

67. States where less than 10 percent of the members responded to the poll were excluded from the polling numbers reported by the National Association of Chiefs of Police.

68. Recent legislative testimony during 1997 provides similar evidence. In testifying before the Kansas House of Representatives on behalf of the Kansas State Lodge of the Fraternal Order of Police, Joseph T. Gimar said, "We . . . continue our support of the [right-to-carry] legislation with the belief that the citizens of Kansas will use it responsibly. . . . I have gone to great lengths to speak to as many national [Fraternal Order of Police] members as possible, many in jurisdictions that have concealed-carry statutes, but [I] have been unable to find any that were in opposition to their statutes." (For this and other quotations by law-enforcement officers, see Gary K. Hayzlett, "Kansans Should Get to Carry and Conceal Arms," *Kansas City Star,* Mar. 21, 1997.)

Many stories involve armed citizens, some with licensed concealed handguns, who have come to the aid of police officers who are being attacked. For example,

> Shapiro was arrested April 9 after punching and kicking Howey police Officer David Kiss in the face and mouth during a State Road 48 traffic stop, which also involved his wife, Susan Jane Shapiro.
>
> The melee didn't break up until a Mission Inn employee who was passing by shot Mark Shapiro in the back of his left knee.
>
> The passer-by, Vincent McCarthy, 46, of Eustis, had a permit to carry his .25-caliber automatic pistol and will not be charged, Lake sheriff's authorities said.
>
> The Howey Town Council earlier this week commended McCarthy for coming to the aid of Kiss. (Linda Chong, "Man Gets House Arrest in Law Officer's Beating," *Orlando Sentinel Tribune,* May 16, 1992, p. 8)

69. Related stories can be found in the *Alva (Oklahoma) Review Courier,* Jan. 8, 1995; the *Tuscaloosa News,* Jan. 12, 1995; and the *Houston Post,* Jan. 22, 1994; see "Gun-Control Survey" *Law Enforcement Technology* (July–Aug. 1991), pp. 14–15.

Police officers are well aware that off-duty officers have often been able to thwart crimes because they were armed. News stories on such cases are easy to find; see, for example, Deborah Hastings, "Girl Killed in California During Stop for Ice Cream on Parents' Anniversary," *Associated Press,* June 18, 1997, dateline Los Angeles, 02:50 A.M. EDT).

70. See Richard Connelly, "Handgun Law's First Year Belies Fears of 'Blood in the Streets,'" *Texas Lawyer,* Dec. 9, 1996, p. 2.

71. See the *Florida Times-Union,* May 9, 1988, and *Palm Beach Post,* July 26, 1988.

72. Flynn, "Gun-Toting Kentuckians Hold Their Fire," p. A1.

73. However, other polls, such as one done by the Johns Hopkins Center for Gun Policy and Research, a group that I will discuss again in chapter 7, argue that people favor more restrictions on gun ownership and claim that 82 percent favored mandatory registration of all handguns (Larry Bivens, "Most Want Child-Proof Handguns, Poll Shows," *Detroit News,* Mar. 14, 1997, p. A5).

74. Zogby International, "Strong Majority of Independents, Democrats, and Obama Voters Support Right to Carry a Firearm," PR Web, Aug. 4, 2009. Tom Smith, "1996 National Gun-Policy Survey of the National Opinion Research Center: Research Findings," (Chicago: National Opinion Research Center, Mar. 1997), p. 21.

75. Lydia Saad, "Before Recent Shootings, Gun-Control Support Was Fading," Gallup poll, Apr. 8, 2009. Smith, "1996 National Gun Policy Survey," pp. 8–9. The survey did include overwhelmingly positive responses to many questions on additional safety regulations for guns. I believe that many of these responses would have been significantly altered if the questions had been posed in terms of the trade-off between safety benefits and estimates of their costs, or if terms describing dangers to children had been eliminated (especially, as already noted in the text, since the number of children harmed by gun accidents is probably much smaller than most people believe).

76. Ibid., p. 13. The other major deciding factor for people's views on gun control appears to be whether they trust government. Those who do trust government are much more in favor of gun control.

77. Erika Schwarz (the first runner-up in the 1997 Miss America Pageant) decided to obtain a gun after a gunman stole her car when she pulled into her driveway. "It's about time they allow citizens to protect themselves. I don't advocate taking the law in your own hand. But in a situation where you're cooped up in a car or house and somebody wants to harm you, this is a good law." Erika Schwarz said that after a carjacking she had been afraid to drive at night. (Guy Coates, "Beauty Gets Ready to Shoot Carjackers " *Chattanooga Free Press,* Aug. 14, 1997, p. B7). Similar stories are told by others who were motivated to obtain firearms training. A recent *Wall Street Journal* story discussed the reasons given by fourteen people who enrolled in a self-defense class run by Smith & Wesson: "The budget analyst had a knife held to her throat in a crowded Manhattan bar. Ms. Denman awoke 18 months ago in her rural home to find a masked, armed burglar at the foot of her bed. He'd kicked in her deadbolted door, and shot at her several times before fleeing. She dialed 911, and then waited 45 minutes for help to arrive." See Caitlin Kelly, "Gun Control," *Wall Street Journal,* Sept. 12, 1997, p. A20.

78. "Georgia Lawmakers Quietly Vote Themselves the Right to Carry Weapons," *Associated Press,* dateline Atlanta, Mar. 19, 1996, 11:09 P.M. EST.

79. According to Larry Mason of the Association of California Deputy District Attorneys, "The association is . . . glad prosecutors have been permitted to protect themselves and that they can continue to do so for their own peace of mind and well-being" (quoted in Greg Krikorian, "Lungren Rules Prosecutors Can Carry Guns to Offices," *Los Angeles Times,* July 25, 1997, p. B1).

The Fraternal Order of Police has also strongly supported legislation that would allow current or retired police officers to carry concealed handguns with them wherever they travel within the United States. (Prepared testimony of Bernard H. Teodorski, National Vice President, Fraternal Order of Police, before the House Committee on the Judiciary, Subcommittee on Crime *(Federal News Service,* July 22, 1997.)

80. *Los Angeles Times,* Jan. 4, 1996, p. E1; Louis Graham, "Officials, Celebs Become Gun-Toting Sheriff's Deputies in Long Tradition," *Memphis Commercial Appeal,* Oct. 3, 1994, pp. 1A, B7; and Clayton B. Cramer and David B. Kopel, "'Shall Issue': New Wave of Concealed-Handgun Permit Laws" (Independence Institute: Golden Colorado, Oct. 17, 1994).

81. See note 77 above.

82. See Adriel Bettelheim, "Campbell Gunning for Concealed-Weapon Proposal," *Denver Post,* June 8, 1997, p. A31.

83. Gary Marx and Janan Hanna, "Boy Called Unfit for Murder Trial," *Chicago Tribune,* Jan. 18, 1995, p. 3; and Joan Beck, "The Murder of Children," *St. Louis Post-Dispatch,* Oct. 24, 1994, p. B19.

84. Maggi Martin, "Symphony of Life Ended Too Quickly for Musician: Grieving

Friends Say Man Stabbed in Lakewood Had Much to Give," *Cleveland Plain Dealer,* July 12, 1995, p. 1A.

85. John Stevenson, "Jurors Begin Deliberating Stroud's Fate," *Durham (North Carolina) Herald-Sun,* Feb. 9, 1995, p. C1.

86. In 1992, only three states did not allow insanity as a defense (Idaho, Montana, and Utah), but even in these states, insanity can be used in determining whether a person had intent.

87. See Dan Kahan and Martha Nussbaum, "Two Conceptions of Emotion in Criminal Law," *Columbia Law Review* 96 (Mar. 1996): 269–374.

88. *Model Penal Code* § 2100.3(1)(b) (1980).

89. See Kahan and Nussbaum, "Emotion in Criminal Law," *Columbia Law Review:* 315–17.

90. *Bullock v United States,* 122 F2d 214 (DC Cir 1941).

91. Kahan and Nussbaum, "Emotion in Criminal Law," *Columbia Law Review:* 325.

92. Anne Lamoy, "Murder Rate in KCK Lowest Since 1991," *Kansas City Star,* Jan. 1, 1997, p. C1.

93. John H. Kagel, Raymond C. Battalio, Howard Rachlin, and Leonard Green, "Demand Curves for Animal Consumers," *Quarterly Journal of Economics* 96 (Feb. 1981): 1–16; John H. Kagel, Raymond C. Battalio, Howard Rachlin, and Leonard Green, "Experimental Studies of Consumer Demand Behavior Using Laboratory Animals," *Economic Inquiry* 13 (Jan. 1975): 22–38; Raymond C. Battalio, John H. Kagel, and Owen R. Phillips, "Optimal Prices and Animal Consumers in Congested Markets," *Economic Inquiry* 24 (Apr. 1986): 181–93; Todd Sandler, "Optimal Prices and Animal Consumers in Congested Markets: A Comment," *Economic Inquiry* 25 (Oct. 1987): 715–20; Raymond C. Battalio, John H. Kagel, and Owen R. Phillips, "Optimal Prices and Animal Consumers in Congested Markets: A Reply," *Economic Inquiry* 25 (Oct. 1987): 721–22; and Raymond C. Battalio, John H. Kagel, Howard Rachlin, and Leonard Green, "Commodity Choice Behavior with Pigeons as Subjects ," *Journal of Political Economy* 84 (Feb. 1981): 116–51.

94. William M. Landes, "An Economic Study of U.S. Aircraft Hijacking, 1961–1976," *Journal of Law and Economics* 21 (Apr. 1978): 1–29.

95. Alfred Blumstein and Daniel Nagin, "The Deterrent Effect of Legal Sanctions on Draft Evasion," *Stanford University Law Review* 28 (1977): 241–76.

96. For a particularly well-done piece that uses data from another country, see Kenneth Wolpin, "An Economic Analysis of Crime and Punishment in England and Wales, 1894–1967," *Journal of Political Economy* 86 (1978): 815–40. For a recent survey of papers in this area, see Isaac Ehrlich, "Crime, Punishment, and the Market for Offenses," *Journal of Economic Perspectives* 10 (Winter 1996): 43–67.

97. Alfred Blumstein, Jacqueline Cohen, and Daniel Nagin, eds., *Deterrence and Incapacitation: Estimating the Effects of Criminal Sanctions on Crime Rates* (Washington, DC: National Academy of Sciences, 1978), pp. 4, 7. Economists have responded to this report; see Isaac Ehrlich and Randall Mark, "Fear of Deterrence: A Critical Evaluation of the 'Report of the Panel on Research on Deterrent and Incapacitation Effects,'" *Journal of Legal Studies* 6 (June 1977): 293–316.

98. Wallace P. Mullin, "Will Gun Buyback Programs Increase the Quantity of Guns?" Michigan State University working paper (Mar. 1997), and Martha R. Plotkin, ed., *Under Fire: Gun Buy-Backs, Exchanges, and Amnesty Programs* (Washington, DC: Police Executive Research Forum, 1996).

CHAPTER TWO

1. The Supreme Court Justices would not uphold broad protections for gun ownership "if they thought blood would flow in the streets." This point was made by Professor Daniel Polsby in a talk given at the University of Chicago, February 20, 1997. As he points out, the Supreme Court would not have allowed the publication of the Pentagon Papers, despite the

arguments about the freedom of the press, if it had posed a severe military risk to the United States. It is not the role of this book to debate the purpose of the Second Amendment. However, the argument that the Second Amendment implies broad protection of gun ownership seems quite strong. William Van Alstyne argues that the reference to a "well-regulated Militia" refers to the "ordinary citizen" and that it was emphatically not an allusion to "regular armed soldiers." It was ordinary citizens who were to bring their own arms to form an army when the Republic was in danger. The amendment was viewed as the ultimate limit on a government's turning against the will of the people. See William Van Alstyne, "The Second Amendment Right to Arms," *Duke Law Review* 43 (Apr. 1994): 1236–55.

2. The opposite of endogenous is exogenous. An exogenous change in something is an independent change, not a response to something else. In reality, almost everything is to some extent related to something else, so the distinction between exogenous and endogenous is a matter of degree. Since models and statistical methods must put a limit on how much to include, some variables will always be treated as "exogenously given" rather than dependent on other variables. For the social sciences, this is a constant headache. Virtually any study is open to the criticism that "if variable *X* depends upon variable *Y*, your results are not necessarily valid." In general, larger studies that rely on more data have better chances of reliably incorporating more relationships. Part of the process of doing research is determining which relationships may raise important concerns for readers and then attempting to test for those concerns.

3. With purely cross-sectional data, if one recognizes that differences may exist in crime rates even after all the demographic and criminal-punishment variables are accounted for, there are simply not enough observations to take these regional differences into account. One cannot control for more variables than one has observations to explain.

The problem with time-series data is the same. Time-series studies typically assume that crime follows a particular type of time trend (for example, they may simply assume that crime rises at a constant rate over time, or they may assume more complicated growth rates involving squared or cubic relationships). Yet almost any crime pattern over time is possible, and, as with cross-sectional data, unexplained differences over time will persist even after all the demographic and criminal-punishment variables are accounted for. Ideally, one could allow each year to have a different effect, but with time-series data we would again find that we had more variables with which to explain changes than we had observations to explain.

4. Gary Kleck and E. Britt Patterson, "The Impact of Gun Control and Gun-Ownership Levels on Violence Rates," *Journal of Quantitative Criminology* 9 (1993): 249–87.

5. David McDowall, Colin Loftin, and Brian Wiersema, "Easing Concealed Firearm Laws: Effects on Homicide in Three States," *Journal of Criminal Law and Criminology* 86 (Fall 1995): 193–206.

6. Arthur L. Kellermann, et al., "Gun Ownership as a Risk Factor for Homicide in the Home," *New England Journal of Medicine* (Oct. 7, 1993): 1084–91.

7. Ibid., p. 1084.

8. The interesting letter that provoked this response from Kellermann et al. was written by students in a graduate statistics class at St. Louis University. See the *New England Journal of Medicine* (Feb. 3, 1994): 366, 368. The estimated rate at which defensive gun uses result in the death of the criminal is derived by comparing the estimated number of defensive gun uses with the number of justifiable homicides. The justifiable-homicide number is obviously an underestimate, and it implies that the actual rate of criminal deaths from defensive gun uses is somewhat higher than reported in the text, but it could be several times higher and not affect the overall statement.

9. Recent attempts to relate the crime rate to the prison population concern me. Besides difficulties in relating the total prison population to any particular type of crime, I think it is problematic to compare a stock (the prison population) with a flow (the crime rate). See,

for example, Steven Levitt, "The Effect of Prison Population Size on Crime Rates: Evidence from Prison Overcrowding Litigation," *Quarterly Journal of Economics* 111 (1996): 144–67.

10. Gary S. Becker, "Crime and Punishment: An Economic Approach," *Journal of Political Economy* 76 (Mar./Apr. 1968): 169–217. See also, for example, Isaac Ehrlich, "Participation in Illegitimate Activities: A Theoretical and Empirical Investigation," *Journal of Political Economy* 81 (1973): 521–65; Michael K. Block and John Heineke, "A Labor Theoretical Analysis of Criminal Choice," *American Economic Review* 65 (June 1975): 314–25; William M. Landes, "An Economic Study of U.S. Aircraft Hijacking, 1961–1976," *Journal of Law and Economics* 21 (Apr. 1978): 1–29.; John R. Lott, Jr., "Juvenile Delinquency and Education: A Comparison of Public and Private Provision," *International Review of Law and Economics* 7 (Dec. 1987): 163–75; James Andreoni, "Criminal Deterrence in the Reduce Form: A New Perspective on Ehrlich's Seminal Study" *Economic Inquiry* 33 (July 1995): 476–83; Morgan O. Reynolds, "Crime and Punishment in America," (Dallas: National Center for Policy Analysis, June 1995); and Levitt, "Effect of Prison Population Size on Crime Rates."

11. John R. Lott, Jr., "Do We Punish High-Income Criminals Too Heavily?" *Economic Inquiry* 30 (Oct. 1992): 583–608.

12. John R. Lott, Jr., "The Effect of Conviction on the Legitimate Income of Criminals," *Economics Letters* 34 (Dec. 1990): 381–85; John R. Lott, Jr., "An Attempt at Measuring the Total Monetary Penalty from Drug Convictions: The Importance of an Individual's Reputation," *Journal of Legal Studies* 21 (Jan. 1992): 159–87.

13. This approach is also known as controlling for "fixed effects," where a separate dummy variable is used to account for each county.

14. James Q. Wilson and George L. Kelling, "Making Neighborhoods Safe," *Atlantic Monthly,* Feb. 1989, and "Broken Windows," *Atlantic Monthly,* Mar. 1982.

15. Arson was excluded because of a large number of inconsistencies in the data and the small number of counties reporting this measure.

16. Robbery includes street robbery, commercial robbery, service station robbery, convenience store robbery, residence robbery, and bank robbery. (See also the discussion of burglary regarding why the inclusion of residence robbery creates difficulty with this broad measure.) After I wrote the original paper, two different commentators attempted to argue that "If 'shall-issue' [a synonym for "nondiscretionary"] concealed-carrying laws really deter criminals from undertaking street crimes, then it is only reasonable to expect the laws to have an impact on robberies. Robbery takes place between strangers on the street. A high percentage of homicide and rape, on the other hand, occurs inside a home—where concealed-weapons laws should have no impact. These findings strongly suggest that something else—not new concealed-carry laws—is responsible for the reduction in crime observed by the authors." See, for example, Doug Weil, "Response to John Lott's Study on the Impact of 'Carry-Concealed' Laws on Crime Rates," *U.S. Newswire,* Aug. 8,1996. The curious aspect of the emphasis on robbery over other crimes like murder and rape is that if robbery is the most obvious crime to be affected by gun-control laws, why have virtually no gun-control studies examined robberies? In fact, Kleck's literature survey only notes one previous gun-control study that examined the issue of robberies ("Guns and Violence: An Interpretive Review of the Field," Social Pathology 1 [Jan. 1995]: 12–47). More important, given that the FBI includes many categories of robberies besides those that "take place between strangers on the street," it is not obvious why this category should exhibit the greatest sensitivity to concealed-handgun laws.

17. "NRA poll: Salespeople No. 1 for Permit Applications," *Dallas Morning News,* Apr. 19, 1996, p. 32A.

18. For example, see Arnold S. Linsky, Murray A. Strauss, and Ronet Bachman-Prehn, "Social Stress, Legitimate Violence, and Gun Availability," Paper presented at the annual meetings of the Society for the Study of Social Problems, 1988; and Clayton E. Cramer and

David B. Kopel, "'Shall Issue': The New Wave of Concealed-Handgun Permit Laws," *Tennessee Law Review* 62 (Spring 1995): 680–91.

19. Among those who made this comment to David Mustard and me were Bob Barnhart, Manager of the Intelligence/Concealed Handgun United of Multnomah County, Oregon; Mike Woodward, of the Oregon Law Enforcement Data System; Joe Vincent of the Washington Department of Licensing Firearms Unit; Alan Krug, who provided us with the Pennsylvania Permit data; and Susan Harrell of the Florida Department of State Concealed Weapons Division. Evidence for this point with respect to Virginia was obtained from Eric Lipton, "Virginians Get Ready to Conceal Arms: State's New Weapon Law Brings a Flood of Inquiries," *Washington Post,* June 28, 1995, p. Al, who notes that "analysts say the new law, which drops the requirement that prospective gun carriers show a 'demonstrated need' to be armed, likely won't make much of a difference in rural areas, where judges have long issued permits to most people who applied for them. But in urban areas such as Northern Virginia—where judges granted few permits because few residents could justify a need for them—the number of concealed weapon permits issued is expected to soar. In Fairfax, for example, a county of more than 850,000 people, only 10 now have permits." See also Cramer and Kopel, "New Wave of Concealed-Handgun Permit Laws," pp. 679–758.

20. For example, see Kleck and Patterson, "Impact of Gun Control and Gun-Ownership Levels on Violence Rates."

21. The sex ratios in Alaska are quite large. For example, white males outnumber white females in the 20–29 age range by 19 percent, while the difference for the United States as a whole is 3 percent. The same ratio for the 30–39 age range is 12 percent in Alaska and 1 percent nationally. Yet the greatest differences occur for blacks. In Alaska black males outnumber black females in the 20–29 age range by 40 percent, while in the rest of the United States the reverse is true, with black females outnumbering (nonincarcerated) black males by 7 percent.

22. While no reliable data are available on this question, a couple of polls indicate that the number of otherwise law-abiding citizens who carry concealed handguns may be substantial. The results of a recent Oklahoma poll showed that up to 6 percent of Oklahoma residents already carry concealed handguns either on their persons or in their cars; see Michael Smith, "Many Permits to Go to Lawbreakers," *Tulsa World,* May 5, 1996, p. A15. The margin of error in the poll was 3.5 percent, which is substantial, given the small value with which this error is compared.

23. Sam Peltzman, "The Effects of Automobile Safety Regulation," *Journal of Political Economy* 83 (Aug. 1975): 677–725.

24. Steven Peterson, George Hoffer, and Edward Millner, "Are Drivers of Air-Bag-Equipped Cars More Aggressive? A Test of the Offsetting-Behavior Hypothesis," *Journal of Law and Economics* 38 (Oct. 1995): 251–64.

25. Kieran Murray, "NRA Taps into Anger of Mid-American Gunlovers," *Reuters Newswire,* dateline Dallas, Apr. 21, 1996.

26. At least since the work of Isaac Ehrlich, economists have also realized that potential biases exist from using the offense rate as both the variable that one is seeking to explain and as the denominator in determining the arrest rate. To see this, suppose that mistakes are made in measuring the crime rate (and mistakes are certainly made) because of recording inaccuracies or simply because citizens may change the rates at which they report crime over time. Accidentally recording a crime rate that is too high will result in our recording an arrest rate that is too low, since the arrest rate is the total number of arrests divided by the total number of crimes. The converse is also true: When too low a crime rate is recorded, the arrest rate that we observe will be too high. Obviously, this problem will make it appear that a negative relationship exists between arrest rates and crime even if no relationship exists. There is also the concern that increasing crime rates may lower arrest

rates if the same resources are being asked to do more work. See Isaac Ehrlich, "Participation in Illegitimate Activities: A Theoretical and Empirical Investigation," *Journal of Political Economy* 81 (1973): 548–53.

CHAPTER THREE

1. The 1988 poll's margin of error was 1.1 percent, while that of the 1996 poll was 2.2 percent.

2. In order to obtain the rate at which people in the general population owned guns, I weighted the respondents' answers to give less weight to groups that were overrepresented among voters compared to their share in the overall population, and to give greater weight to those groups that were underrepresented. Twenty-four categories of personal characteristics were used to compute these weightings: white males and females, and black males and females, aged 18–29; neither black nor white males and females 18–29; white males and females, and black males and females 30–44; neither black nor white males and females 30–44; white males and females, and black males and females 45–59; neither black nor white males and females 45–59; white males and females, and black males and females over 59; neither black nor white males and females over 59.

3. This argument has been made explicitly in the press many times. See, for example, Scott Baldauf, "As Crime Shrinks, Security Is Still Growth Industry," *Christian Science Monitor,* Oct. 2, 1996, p. 1.

4. Alix M. Freedman, "Tinier, Deadlier Pocket Pistols Are in Vogue," *Wall Street Journal,* Sept. 12, 1996, P. B1.

5. The primary concern here is that letting people check those parts of a list that apply will result in fewer positive responses than asking people to answer individual questions about each item. As one way of checking the importance of this concern, I examined whether other questions that changed in a similar way between the two polls experienced a change in the same direction as that shown for gun ownership. The two questions that I looked at—regarding marriage and whether children less than 18 lived with the respondent—moved in the opposite direction. Relatively more people indicated these responses in the 1988 poll when the questions were presented in a list than did so when they were presented with separate questions about these characteristics. I have also done extensive research using other questions involving marriage and children under 18 living with the respondent that were part of a "check as many as apply" question. That research provides extremely strong evidence that these questions were answered consistently between 1988 and 1996. See John R. Lott, Jr. and Larry W. Kenny, "How Dramatically Did Women's Suffrage Change the Size and Scope of Government?" University of Chicago School of Law working paper (1997). The relative differences in gun ownership across groups is also consistent with recent work using other polls by Edward Glaeser and Spencer Glendon, "Who Owns Guns?" *American Economic Review* 88 (May 1998).

The empirical work that will be done later will allow us to adjust for the changes in the reported level of gun ownership that might result from the change in this question.

6. I appreciate Tom Smith's taking the time to talk to me about these issues on May 30, 1997.

7. Gun owners within each of the twenty-four categories listed in note 2 above may have particular characteristics that cause them to vote at rates that differ from the rates at which other people vote. One would hope that some of that difference would be accounted for in the detailed demographic characteristics, but there is a good chance that this may not occur. Several attempts were made to see how large this effect might be by asking, for example, whether gun owners were more or less likely not to have voted in previous elections. This question has also been broken down to account for those who are old enough to have voted previously. For 1988, the difference in gun ownership between those who were voting for the first time and those who had voted previously was 3 percent (23.2 percent of those vot-

ing for the first time and 26.2 percent of those who were not owned guns). Limiting this question to people who were 30 years of age or older produced an even smaller difference: 28.9 percent of first-time voters owned guns versus 27.5 percent of those who had voted previously. Similarly, for the question of whether voters in 1988 had also voted in 1984, the difference was also 3 percent (23 percent of those who did not vote in 1984 and 26.4 percent of those who did owned guns).

Because most people voted, a 13 percent increase in the proportion of the general population owning guns would require an even greater drop in gun ownership among those who didn't vote in order for gun ownership to have remained constant. For some groups, such as women, for whom gun ownership among voters increased by over 70 percent, the increase is so large and the percent of women voting so high that an 80 percent drop in gun ownership among nonvoting women would have been required for gun ownership among women to have remained constant.

8. Indeed, making this adjustment produces a number that is much closer to that found in other polls of the general population, such as the National Opinion Research Center's 1996 National Gun-Policy Survey, which finds that 42 percent of the general adult population owns guns.

9. The data are available from the ICPSR at the University of Michigan as ICPSR 4181. The National Election Pool General Election Exit Polls, 2004, National Data National Election Pool, Edison Media Research Mitofsky International, March 2005.

10. Richard Morin, "Surveying the Damage," *Washington Post*, November 21, 2004, p. B1. Evidence that the underlying vote data did not involve fraud is provided by Kevin Hassett and John R. Lott, Jr., "Voting Technology and Voter Fraud: A Test Using Exit Poll Data," American Enterprise Institute working paper, February 2005.

11. Evaluation of Edison/Mitofsky Election System 2004 prepared by Edison Media Research and Mitofsky International for the National Election Pool, January 19, 2005 (http://abcnews.go.com/images/Politics/EvaluationofEdisonMitofskyElectionSystem.pdf).

12. The initial exit poll survey results that were reported on election day used a weighting that "for the national exit poll overstated the proportion of women in the electorate." That problem was fixed in the weightings that were released after that date. Weightings for the share of voters who were Republican or conservative could also have been introduced to rectify the skewness in the survey, but this was not done.

13. The previous peak in murder rates occurred at the end of Prohibition in the early 1930s, with the peak of 9.7 murders per 100,000 people being reached in 1933. The 1996 murder rate of 7.3 murders per 100,000 people seems tame by comparison. Indeed many people, such as Milton Friedman, have argued that much of the change in murder rates over time has been driven by the country's war on drugs and its earlier war on alcohol. Even the gradual increase in murder rates leading up to the Nineteenth Amendment's adoption in 1991 corresponds with passage of individual state laws. Kansas, Maine, and North Dakota enacted prohibition laws between 1880 and 1890. Five states enacted prohibition in 1907–1909, followed by twelve more between 1912 and 1915 and another twelve between 1916 and 1918. Obviously, all this points to the importance of other factors in the murder rate, and that is part of the reason why I include a measure of drug prices in my estimates to explain why crime rates change over time. See Ernest H. Cherrington, *The Evolution of Prohibition in the United States of America* (Westerville, OH: Tem-Press, 1920); Edward B. Dunford, *The History of the Temperance Movement* (Washington, DC: Tem-Press, 1943); D. Leigh Colvin, *Prohibition in the United States,* (New York: George H. Doran, 1926); as well as state statutes (as a check).

14. While I will follow Cramer and Kopel's definition of what constitutes a "shall-issue" or a "do-issue" state (see "'Shall Issue': The New Wave of Concealed-Handgun Permit Laws," *Tennessee Law Review* 62 [Spring 1995]), one commentator has suggested that it is not appropriate to include Maine in these categories (Stephen P. Teret, "Critical Comments on a Paper by Lott and Mustard," School of Hygiene and Public Health, Johns Hopkins University,

mimeo, Aug. 7, 1996). Neither defining Maine so that the "shall-issue" dummy equals zero nor removing Maine from the data set alters the findings shown in this book.

15. While the intent of the 1988 legislation in Virginia was clearly to institute a "shall-issue" law, the law was not equally implemented in all counties in the state. To deal with this problem, I reran the regressions reported in this paper with the "shall-issue" dummy equal to both 1 and 0 for Virginia.

16. I rely on Cramer and Kopel for this list of states. Some states, known as "do-issue" states, are also included in Cramer and Kopel's list of "shall-issue" states, though these authors argue that for all practical purposes these two groups of states are identical. See Cramer and Kopel, "New Wave of Concealed-Handgun Permit Laws," pp. 679–91.

17. The Oregon counties providing permit data were Benton, Clackamas, Columbia, Coos, Curry, Deschutes, Douglas, Gilliam, Hood River, Jackson, Jefferson, Josephine, Klamath, Lane, Lincoln, Linn, Malheur, Marion, Morrow, Multnomah, Polk, Tillamook, Umatilla, Washington and Yamhill.

18. In economics jargon I would say that I am interacting the sentence length with year-dummy variables.

19. These variables are referred to as county fixed-effects, where a separate dummy variable is set equal to 1 for each individual county.

20. See appendix 4 for the list and summary statistics.

21. For example, see James Q. Wilson and Richard J. Herrnstein, *Crime and Human Nature* (New York: Simon and Schuster, 1985), pp. 126–47.

22. However, the effect of an unusually large percentage of young males in the population may be mitigated because those most vulnerable to crime may be more likely to take actions to protect themselves. Depending upon how responsive victims are to these threats, the coefficient for a variable like the percent of young males in the population could be zero even when the group in question poses a large criminal threat.

23. Edward L. Glaeser and Bruce Sacerdote, "Why Is There More Crime in Cities?" Harvard University working paper, Nov. 14, 1995.

24. For a discussion of the relationship between income and crime, see John R. Lott, Jr., "A Transaction-Costs Explanation for Why the Poor Are More Likely to Commit Crime" *Journal of Legal Studies* 19 (Jan. 1990): 243–45.

25. A brief survey of the laws, excluding the changes in the rules regarding permits, reveals the following: Alabama made no significant changes in these laws during the period. Connecticut law gradually changed its wording from "criminal use" to "criminal possession" from 1986 to 1994. Florida has the most extensive description of penalties; the same basic law (790.161) persists throughout the years. An additional law (790.07) appeared only in 1986. In Georgia, a law (16-11-106) that does not appear in the 1986 edition appears in the 1989 and 1994 editions. The law involves possession of a firearm during commission of a crime and specifies the associated penalties. Because this legal change might have occurred at the same time as the 1989 changes in the rules regarding permits, I used a Lexis search to check the legislative history of 16-11-106 and found that the laws were last changed in 1987, two years before the permit rules were changed (*Official Code of Georgia, Annotated,* at 16-11-106 [1996]). Idaho has made no significant changes over time. In Indiana and Maine no significant changes occurred in these laws during the period. In Mississippi, Law 97-37-1 talks explicitly about penalties. It appears in the 1986 version but not in the 1989 or the 1994 versions. Montana enacted some changes in punishments related to unauthorized carrying of concealed weapons, but no changes in the punishment for using a weapon in a crime. New Hampshire, North Dakota, Oregon, Pennsylvania, and Washington made no significant changes in these laws during period. In South Dakota, Law 22-14-13, which specifies penalties for commission of a felony while armed, appears in 1986 but not 1989. In Vermont, Section 4005, which outlines the penalties for carrying a gun when committing a felony, appears in

1986 but not in 1989 or 1994. Virginia and Washington made no significant changes in these laws during the period. West Virginia had Law 67-7-12 on the books in 1994, but not in the earlier versions. It involves punishment for endangerment with firearms. Removing Georgia from the sample, which was the only state that enacted changes in its gun laws near the year that the "shall-issue" law went into affect, eliminates the chance that the other changes in gun laws might affect my results and does not appreciably alter those results.

26. Thomas B. Marvell and Carlisle E. Moody, "The Impact of Enhanced Prison Terms for Felonies Committed with Guns," *Criminology* 33 (May 1995): 247, 258–61.

27. Marvell and Moody's findings (see note 22 above) show that the shortest time period between these sentencing enhancements and changes in concealed-weapon laws is seven years (Pennsylvania). Twenty-six states passed their enhancement laws prior to the beginning of my sample period, and only four states passed such laws after 1981. Maine, which implemented its concealed-handgun law in 1985, passed its sentencing-enhancement laws in 1971.

28. The states that had waiting periods prior to the beginning of the sample are Alabama, California, Connecticut, Illinois, Maryland, Minnesota, New Jersey, North Carolina, Pennsylvania, Rhode Island, South Dakota, Washington, and Wisconsin. The District of Columbia also had a waiting period prior to the beginning of my sample. The states that adopted this rule during the sample period are Hawaii, Indiana, Iowa, Missouri, Oregon, and Virginia.

CHAPTER FOUR

1. More precisely, it is the percentage of a one-standard-deviation change in the crime rate that can be explained by a one-standard-deviation change in the endogenous variable.

2. All the results are reported for the higher threshold required with a two-tailed t-test.

3. One possible concern with these initial results arises from my use of an aggregate public-policy variable (state right-to-carry laws) on county-level data. See Bruce C. Greenwald, "A General Analysis of the Bias in the Estimated Standard Errors of Least Squares Coefficients," *Journal of Econometrics* 22 (Aug. 1983): 323–38; and Brent R. Moulton, "An Illustration of a Pitfall in Estimating the Effects of Aggregate Variables on Micro Units," *Review of Economics and Statistics* 72 (1990): 334. Moulton writes, "If disturbances are correlated within the groupings that are used to merge aggregate with micro data, however, then even small levels of correlation can cause the standard errors from the ordinary least squares (OLS) to be seriously biased downward." Yet this should not really be a concern here because of my use of dummy variables for all the counties, which is equivalent to using state dummies as well as county dummies for all but one of the counties within each state. Using these dummy variables thus allows us to control for any disturbances that are correlated within any individual state. The regressions discussed in table 4.2 reestimate the specifications shown in table 4.1 but also include state dummies that are interacted with a time trend. This should thus not only control for any disturbances that are correlated with the states, but also for any disturbances that are correlated within a state over time. Finally, while right-to-carry laws are almost always statewide laws, there is one exception. Pennsylvania partially exempted its largest county (Philadelphia) from the law when it was passed in 1989, and it remained exempt from the law during the rest of the sample period. However, permits granted in the counties surrounding Philadelphia were valid for use in the city.

4. However, the increase in the number of property crimes is larger than the decrease in the number of robberies.

5. While I adopt the classifications used by Cramer and Kopel in "'Shall Issue': The New Wave of Concealed-Handgun Permit Laws," Tennessee Law Review 62 (Spring 1995), some are more convinced by other classifications of states (for example, see Doug Weil, "Response to John Lott's Study on the Impact of 'Carry-Concealed' Laws on Crime Rates," *U.S. Newswire,* Aug. 8, 1996; and Stephen P. Teret, "Critical Comments on a Paper by Lott and Mustard," School

of Hygiene and Public Health, Johns Hopkins University, mimeo, Aug. 7, 1996). Setting the "shall-issue" dummy for Maine to zero and rerunning the regressions shown in table 4.1 results in the "shall-issue" coefficient equaling –3% for violent crimes, –8% for murder, –6% for rape, –4.5% for aggravated assault, –1% for robbery, 3% for property crimes, 8.1% for automobile theft, 0.4% for burglary, and 3% for larceny. Similarly, setting the "shall-issue" dummy for Virginia to zero results in the "shall-issue" coefficient equaling –4% for violent crimes, –9% for murder, –5% for rape, –5% for aggravated assault, –0.11% for robbery, 3% for property crimes, 9% for automobile theft, 2% for burglary, and 3% for larceny. As a final test, dropping both Maine and Virginia from the data set results in the "shall-issue" coefficient equaling –2% for violent crimes, –10% for murder, –6% for rape, –3% for aggravated assault, 0.6% for robbery, 3.6% for property crimes, 10% for automobile theft, 2% for burglary, and 4% for larceny.

6. This information is obtained from Mortality Detail Records provided by the U.S. Department of Health and Human Services.

7. This assumption is implausible for many reasons. One reason is that accidental handgun deaths occur in states without concealed-handgun laws.

8. Given the possible relationship between drug prices and crime, I reran the regressions in table 4.1 and included an additional variable for cocaine prices. One argument linking drug prices and crime is that if the demand for drugs is inelastic and if people commit crimes in order to finance their habits, higher drug prices might lead to increased levels of crime. Using the Drug Enforcement Administration's STRIDE data set from 1977 to 1992 (with the exceptions of 1988 and 1989), Michael Grossman, Frank J. Chaloupka, and Charles C. Brown, ("The Demand for Cocaine by Young Adults: A Rational Addiction Approach," NBER working paper, July 1996), estimate the price of cocaine as a function of its purity, weight, year dummies, year dummies interacted with eight regional dummies, and individual city dummies. There are two problems with this measure of predicted prices: (1) it removes observations during a couple of important years during which changes were occurring in concealed-handgun laws, and (2) the predicted values that I obtained ignored the city-level observations. The reduced number of observations provides an important reason why I do not include this variable in the regressions shown in table 4. 1. However, the primary impact of including this new variable is to make the "shall-issue" coefficients in the violent-crime regressions even more negative and more significant (for example, the coefficient for the violent-crime regression becomes –7.5%, –10% for the murder regression, –7.7% for rape, and –11% for aggravated assault, with all of them significant at more than the 0.01 level). Only for the burglary regression does the "shall-issue" coefficient change appreciably: it becomes negative and insignificant. The variable for drug prices itself is negatively related to murders and rapes and positively and significantly related, at least at the 0.01 level for a one-tailed t-test, to all the other categories of crime. I would like to thank Michael Grossman for providing me with the original regressions on drug prices from his paper.

9. In contrast, if we had instead inquired what difference it would make in crime rates if either all states or no states adopted right-to-carry concealed-handgun laws, the case of all states adopting concealed-handgun laws would have produced 2,000 fewer murders; 5,700 fewer rapes; 79,000 fewer aggravated assaults; and 14,900 fewer robberies. In contrast, property crimes would have risen by 336,410.

10. Ted R. Miller, Mark A. Cohen, and Brian Wiersema, *Victim Costs and Consequences: A New Look* (Washington, DC: National Institute of Justice, Feb. 1996).

11. See Sam Peltzman, "The Effects of Automobile Safety Regulation," *Journal of Political Economy* 83 (Aug. 1975): 677–725.

12. To be more precise, a one-standard-deviation change in the probability of arrest accounts for 3 to 11 percent of a one-standard-deviation change in the various crime rates.

13. Translating this into statistical terms, a one-standard-deviation change in the percentage of the population that is black, male, and between 10 and 19 years of age explains 22 percent of the ups and downs in the crime rate.

14. This is particularly observed when there are more black females between the ages of 20 and 39, more white females between the ages of 10 and 39 and over 65, and females of other races between 20 and 29.

15. In other words, the second number shows how a one-standard-deviation change in an explanatory variable explains a certain percent of a one-standard-deviation change in the various crime rates.

16. While I believe that such variables as the arrest rate should be included in any regressions on crime, one concern with the results reported in the various tables is over whether the relationship between the "shall-issue" variable and the crime rates occurs even when all the other variables are not controlled for. Using weighted least squares and reporting only the "shall-issue" coefficients, I estimated the following regression coefficients.

How do average crime rates differ among states with and without nondiscretionary laws?

Crime rates	Crime rates in states with nondiscretionary concealed-handgun laws compared to those without the law (regressing the crime rate only on the variable for the law)	Crime rates in states with nondiscretionary concealed-handgun laws compared to those without the law after adjusting for national trends (regressing the crime rate on the variable for the law and year-dummy variables)
Violent crimes	–40%	–57%
Murder	–48	–52
Rape	–16	–28
Aggravated assault	–38	–57
Robbery	–62	–75
Property crime	–17	–20
Auto theft	–31	–43
Burglary	–28	–24
Larceny	–11	–15

Note: The only factors included are the presence of the law and/or year-specific effects. All these differences are statistically significant at least at the 1 percent level for a two-tailed *t*-test. To calculate these percentages, I used the approximation 100 [exp(coefficient) – 1].

17. The time-trend variable ranges from 1 to 16: for the first year in the sample, it equals 1; for the last year, it is 16.

18. Other differences arise in the other control variables, such as those relating to the portion of the population of a certain race, sex, and age. For example, the percent of black males in the population between 10 and 19 is no longer statistically significant.

19. If the task instead had been to determine the difference in crime rates when either all states or no states adopt the right-to-carry handgun laws, the case of all states adopting concealed-handgun laws would have produced 2,048 fewer murders, 6,618 fewer rapes, 129,114 fewer aggravated assaults, and 86,459 fewer robberies. Non-arson property crimes also would have fallen by 511,940.

20. Generally, aggregation is frowned on in statistics anyway, as it reduces the amount of information yielded by the data set. Lumping data together into a group cannot yield any new information that did not exist before; it only reduces the richness of the data.

21. Eric Rasmusen, "Stigma and Self-Fulfilling Expectations of Criminality," *Journal of Law and Economics* 39 (Oct. 1996): 519–44.

22. In January 1996, women held 118,728 permits in Washington and 17,930 permits in Oregon. The time-series data available for Oregon during the sample period even indicate that 17.6 percent of all permit holders were women in 1991. The Washington state data were obtained from Joe Vincent of the Department of Licensing Firearms Unit in Olympia,

Washington. The Oregon state data were obtained from Mike Woodward of the Law Enforcement Data System, Department of State Police, Salem, Oregon. Recent evidence from Texas indicates that about 28 percent of applicants were women ("NRA poll: Sales people No. 1 for Permit Applications," *Dallas Morning News,* Apr. 19, 1996, p. 32A).

23. For an interesting discussion of the benefits to women of owning guns, see Paxton Quigley, *Armed and Female* (New York: E. P. Dutton, 1989).

24. Unpublished information obtained by Kleck and Gertz in their 1995 National Self-Defense Survey implies that women were as likely as men to use handguns in self-defense in or near their homes (defined as in the yard, carport, apartment hall, street adjacent to home, detached garage, etc.), but that women were less than half as likely to use a gun in self-defense away from home. See Gary Kleck and Marc Gertz, "Armed Resistance to Crime: The Prevalence and Nature of Self-Defense with a Gun," *Journal of Criminal Law and Criminology* 86 (Fall 1995): 249–87.

25. Counties with real personal income of about $15,000 in real 1983 dollars experienced 8 percent drops in murder, while mean-income counties experienced a 5.5 percent drop.

26. Lori Montgomery, "More Blacks Say Guns Are Answer to Urban Violence," *Houston Chronicle,* July 9, 1995, p. Al. This article argues that while the opposition to guns in the black community is strong, more people are coming to understand the benefits of self-protection.

27. For an excellent overview of the role of race in gun control, see Robert J. Cottrol and Raymond T. Diamond, "The Second Amendment: Toward an Afro-Americanist Reconsideration," *Georgetown Law Review* 80 (Dec. 1991): 309.

28. See William Van Alstyne, "The Second Amendment Right to Arms," *Duke Law Review* 43 (Apr. 1994): 1236–55. In slave states prior to the Civil War, the freedoms guaranteed under the Bill of Rights were regularly restricted by states because of the fear that free reign might lead to an insurrection. As Akhil Reed Amar writes, "In a society that saw itself under siege after Nat Turner's rebellion, access to firearms had to be strictly restricted, especially to free blacks." See Akhil Reed Amar, "The Bill of Rights and the Fourteenth Amendment," *Yale Law Journal* 101 (Apr. 1992): 1193.

29. *Associated Press Newswire,* May 9, 1997, 4:37 P.M. EDT. As the *Washington Times* recently noted, this story "comes at an awkward time for the administration, since President Clinton has spent the last week or two berating Republicans for failing to include in anti-crime legislation a provision requiring that child safety locks be sold with guns to keep children from hurting themselves" (Editorial, "The Story of a Gun and a Kid," *Washington Times,* May 22, 1997, p. A18).

30. The conversation took place on March 18, 1997, though regrettably I have misplaced the note containing the representative's name.

31. John Carpenter, "Six Other States Have Same Law," *Chicago Sun-Times,* Mar. 11, 1997, p. 8.

32. John J. Dilulio, Jr., "The Question of Black Crime," *The Public Interest* 117 (Fall 1994): 3–24. Similar concerns about the inability of minorities to rely on the police was also expressed to me by Assemblyman Rod Wright (D-Los Angeles) during testimony before the California Assembly's Public Safety Committee on November 18, 1997.

33. One additional minor change is made in two of the earlier specifications. In order to avoid any artificial collinearity either between violent crime and robbery or between property crimes and burglary, violent crimes net of robbery and property crimes net of burglary are used as the endogenous variables when robbery or burglary are controlled for.

34. The Pearson correlation coefficient between robbery and the other crime categories ranges between .49 and .80, and all are so statistically significant that a negative correlation would only appear randomly once out of every ten thousand times. For burglary, the correlations range from 0.45 to 0.68, and they are also equally statistically significant.

35. All the results in tables 4.1 and 4.4 as well as the regressions related to both parts of figure 4.1 were reestimated to deal with the concerns raised in chapter 3 over the "noise" in arrest rates arising from the timing of offenses and arrests and the possibility of multiple

offenders. I reran all the regressions in this section by limiting the sample to those counties with populations over 10,000, over 100,000, and then over 200,000 people. The more the sample was restricted to larger-population counties, the stronger and more statistically significant was the relationship between concealed-handgun laws and the previously reported effects on crime. This is consistent with the evidence reported in figure 4. 1. The arrest-rate results also tended to be stronger and more significant. I further reestimated all the regressions by redefining the arrest rate as the number of arrests over the last three years divided by the total number of offenses over the last three years. Despite the reduced sample size, the results remained similar to those already reported.

36. More formally, by using restricted least squares, we can test whether constraining the coefficients for the period before the law produces results that yield the same pattern after the passage of the law. Using both the time-trend and the time-trend-squared relationships, the F-tests reject the hypothesis that the before and after relationships are the same, at least at the 10 percent level, for all the crime categories except aggravated assault and larceny, for which the F-tests are only significant at the 20 percent level. Using only the time-trend relationship, the F-tests reject the hypothesis in all the cases.

37. The main exception was West Virginia, which showed large drops in murder but not in other crime categories.

38. See Thomas B. Marvell and Carlisle E. Moody, "The Impact of Enhanced Prison Terms for Felonies Committed with Guns," *Criminology* 33 (May 1995): 259–60.

39. I should note, however, that the "nondiscretionary" coefficients for robbery in the county-level regressions and for property crimes using the state levels are no longer statistically significant.

40. Toni Heinzl, "Police Groups Oppose Concealed-Weapons Bill," *Omaha World-Herald,* Mar. 18, 1997, p. 9SF.

41. A simple dummy variable is used for whether the limit was 18 or 21 years of age.

42. Here is one example: "Mrs. Elmasri, a Wisconsin woman whose estranged husband had threatened her and her children, called a firearms instructor for advice on how to buy a gun for self-defense. She was advised that, under Wisconsin's progressive handgun law, she would have to wait 48 hours so that the police could perform the required background check.

"Twenty-four hours later, . . . Mrs. Elmasri's husband murdered the defenseless woman and her two children" (William P. Cheshire, "Gun Laws No Answer for Crime," *Arizona Republic,* Jan. 10, 1993, p. C1.) Other examples can be found in David B. Kopel, "Background Checks and Waiting Periods," in *Guns: Who Should Have Them,* ed. David B. Kopel (Amherst, NY: Prometheus Books, 1995.) Other examples tell of women who successfully evaded these restrictions to obtain guns.

> In September 1990, mail carrier Catherine Latta of Charlotte, N. C, went to the police to obtain permission to buy a handgun. Her ex-boyfriend had previously robbed her, assaulted her several times, and raped her. The clerk at the sheriff's office informed her that processing a gun permit would take two to four weeks. "I told her I'd be dead by then," Latta recalled.
>
> That afternoon, Latta bought an illegal $20 semiautomatic pistol on the street. Five hours later, her ex-boyfriend attacked her outside her house. She shot him dead. The county prosecutor decided not to prosecute Latta for either the self-defense homicide or the illegal gun. (Quoted from David B. Kopel, "Guns and Crime: Does Restricting Firearms Really Reduce Violence?" *San Diego Union-Tribune,* May 9, 1993, p. G4.)

For another example where a woman's ability to defend herself would have been impaired by a waiting period, see "Waiting Period Law Might Have Cost Mother's Life," *USA Today,* May 27, 1994, p. 10A.

43. Quoted in David Armstrong, "Cities' Crime Moves to Suburbs," *Boston Globe,* May 19, 1997, pp. 1 and B6.

CHAPTER FIVE

1. While county-level data were provided in the *Supplementary Homicide Reports,* matching these county observations with those used in the *Uniform Crime Reports* proved unusually difficult. A unique county identifier was used in the *Supplementary Homicide Reports* that was not consistent across years. In addition, some caution is necessary in using both the Mortality Detail Records and the *Supplementary Homicide Reports,* since the murder rates reported in both sources have relatively low correlations of less than .7 with the murder rates reported in the *Uniform Crime Reports*. This is especially surprising for the supplementary reports, which are derived from the *Uniform Crime Reports*. See U.S. Department of Justice, FBI staff, *Uniform Crime Reports* (Washington, DC: U.S. Govt. Printing Office) for the years 1977 to 1992.

2. Indeed, the average age of permit holders is frequently in the mid- to late forties (see, for example, "NRA poll: Salespeople No. 1 for Permit Applications," *Dallas Morning News,* Apr. 19, 1996, p. 32A.) In Kentucky the average age of permit holders is about fifty (see Terry Flynn, "Gun-Toting Kentuckians Hold Their Fire," *Cincinnati Enquirer,* June 16, 1997, p. Al).

3. This is the significance for a two-tailed t-test.

4. Similar breakdowns for deaths and injuries are explored in much more depth in a paper that I have written with William Landes; see William Landes and John R. Lott, Jr., "Mass Public Shootings, Bombings, and Right-to-Carry Concealed-Handgun Laws," University of Chicago working paper, 1997.

5. A second change was also made. Because of the large number of observations noting no deaths or injuries from mass public shootings in a given year, I used a statistical technique known as Tobit that is particularly well suited to this situation.

6. The results shown below provide the estimates for the simple linear time trends before and after the adoption of the law. They demonstrate that for each year leading up to the passage of the law, total deaths or injuries from mass public shootings rose by 1.5 more per 10 million people and that after the passage of the law, total deaths or injuries fell by 4 more per 10 million people. The difference in these two trends is statistically significant at the 1 percent level for a two-tailed t-test. It is interesting to note that higher murder arrest rates, although they deter murderers, do not seem to deter perpetrators of mass public shootings.

Linear time trends for deaths and injuries from mass public shootings before and after adoption of concealed-handgun law

	Total deaths and injuries per 100,000 population
Average annual change for years after adoption of the law	–0.04***
Average annual change for years before adoption of the law	0.015***
Arrest rate for murder	–0.0003

***Statistically significant at least at the 10 percent level for a two-tailed *t*-test
Note: numbers are negative; years furthest beyond adoption are the largest

7. See appendix 4 for the means and standard deviations of the variables used in these regressions.

8. Again, this is stating that a one-standard-deviation change in arrest rates explains more than 15 percent of a one-standard-deviation change in crime rates.

9. Running the regressions for all Pennsylvania counties (not just those with more than 200,000 people) produced similar signs for the coefficient for the change in concealed-

handgun permits, though the coefficients were no longer statistically significant for violent crimes, rape, and aggravated assault. Alan Krug, who provided us with the Pennsylvania handgun-permit data, told us that one reason for the large increase in concealed-handgun permits in some rural counties was that people used the guns for hunting. He told us that the number of permits issued in these low-population, rural counties tended to increase most sharply in the fall around hunting season. If people were in fact getting large numbers of permits in low-population counties (which already have extremely low crime rates) for some reason other than crime, it would be more difficult to pick up the deterrent effect of concealed handguns on crime that was occurring in the larger counties.

10. A one-standard-deviation change in conviction rates explains 4 to 20 percent of a one-standard-deviation change in the corresponding crime rates.

11. I reran these regressions using the natural logs of the arrest and conviction rates, and I consistently found statistically larger and even economically more important effects for the arrest rates than for the conviction rates.

12. For example, see Dan M. Kahan, "What Do Alternative Sanctions Mean?" *University of Chicago Law Review* 63 (1996): 591–653.

13. See John R. Lott, Jr., "The Effect of Conviction on the Legitimate Income of Criminals," *Economics Letters* 34 (Dec. 1990): 381–85; John R. Lott, Jr., "An Attempt at Measuring the Total Monetary Penalty from Drug Convictions: The Importance of an Individual's Reputation," *Journal of Legal Studies* 21 (Jan. 1992): 159–87; John R. Lott, Jr., "Do We Punish High-Income Criminals Too Heavily?" *Economic Inquiry* 30 (Oct. 1992): 583–608.

14. Put differently, six of the specifications imply that a one-standard-deviation change in the number of concealed-handgun permits explains at least 8 percent of a one-standard-deviation change in the corresponding crime rates.

15. Philip Heymann, a former deputy attorney general in the Clinton administration and currently a law professor at Harvard University, wrote, "None of this [the drop in crime rates] is the result of . . . the Brady Act (for most guns were never bought by youth from licensed gun dealers)." See "The Limits of Federal Crime-Fighting," *Washington Post,* Jan. 5, 1997, p. C7.

16. For a discussion of externalities (both benefits and costs) from crime, see Kermit Daniel and John R. Lott, Jr., "Should Criminal Penalties Include Third-Party Avoidance Costs?" *Journal of Legal Studies* 24 (June 1995): 523–34.

17. Alix M. Freedman, "Tinier, Deadlier Pocket Pistols Are in Vogue," *Wall Street Journal,* Sept. 12, 1996, pp. B1, B16.

18. One hundred and eighty-two million people lived in states without these laws in 1991, so the regressions would have also implied nine more accidental deaths from handguns in that year.

19. Given the very small number of accidental deaths from handguns in the United States, the rate of such deaths in the vast majority of counties is zero, and the last two columns of table 5.6 again use Tobit regressions to deal with this problem. Limitations in statistical packages, however, prevented me from being able to control for all the county dummies, and I opted to rerun these regressions with only state dummy variables.

20. For example, see Nicholas D. Kristof, "Guns: One Nation Bars, the Other Requires," *New York Times,* Mar. 10, 1996, sec. 4, p. 3. For some evidence on international gun ownership rates see Munday and Stevenson, *Guns and Violence* (1996): 30.

21. See Ian Ayres and Steven Levitt, "Measuring Positive Externalities from Unobservable Victim Precaution: An Empirical Analysis of Lojack" NBER working paper 5928 (1997); and John Donohue and Peter Siegelman, "Is the United States at the Optimal Rate of Crime?" *Journal of Legal Studies* 27 (Jan. 1998).

22. See notes 12 and 13 above.

CHAPTER SIX

1. Isaac Ehrlich, "Participation in Illegitimate Activities: A Theoretical and Empirical Investigation," *Journal of Political Economy* 81 (1973): 548–51. Except for the political variables, my specification accords fairly closely with at least the spirit of Ehrlich's specification, though some of my variables, like the demographic breakdowns, are much more detailed, and I have a few other measures that were not available to him.

2. See also Robert E. McCormick and Robert Tollison, "Crime on the Court," *Journal of Political Economy* 92 (Apr. 1984): 223–35, for a novel article testing the endogeneity of the "arrest rate" in the context of basketball penalties.

3. These last two variables are measured at the state level.

4. Phil Cook suggested this addition to me. In a sense, this is similar to Ehrlich's specification, except that the current crime rate is broken down into its lagged value and the change between the current and previous periods. See Ehrlich, "Participation in Illegitimate Activities," p. 557.

5. The natural logs of the rates for violent crime and property crime were used.

6. These estimates are known as two-stage least squares.

7. Ehrlich raises the concern that the types of two-stage, least-squares estimates discussed above might still be affected by spurious correlation if the measurement errors for the crime rate were serially correlated over time. To account for this, I reestimated the first-stage regressions predicting the arrest rate without the lagged crime rate, which made the estimated results for the nondiscretionary law dummy even more negative and more statistically significant than those already shown. See Ehrlich, "Participation in Illegitimate Activities" p. 552 n. 46.

8. Still another approach would be to estimate what are known as Tobit regressions, but unfortunately no statistical package is available that allows me both to control for all the different county dummy variables and to use the Tobit procedure.

CHAPTER SEVEN

1. The Violence Policy Center grew out of the National Coalition to Ban Handguns.

2. Douglas Weil, the research director for Handgun Control, Inc., has publicly disagreed with the claim that most gun-control advocates initially refused to comment on my study. In a letter to the *Washington Times*, Weil wrote,

> The *Washington Times* editorial ("Armed and Safer," Aug. 14) is misinformed and misguided. The *Times* falsely claims that gun-control proponents "initially refused to read" John Lott's and David Mustard's study of the impact of laws regarding the right to carry concealed guns, and that I attacked the researchers' motivations rather than challenge the study "on the merits." This charge is untrue.
>
> One look at the study would prove the *Times* wrong. On the title page of the study, several pro-gun-control researchers are credited for their comments "on the merits" of the study. Included in this list are David McDowall, a criminologist at the University of Maryland; Philip Cook, an economist at Duke University; and myself, research director for the Center to Prevent Handgun Violence.
>
> Upon reviewing the study, I found Mr. Lott's methodology to be seriously flawed. I told Mr. Lott that his study did not adequately control for the whole range of ways that state and local governments attempt to lower the crime rate. In Oregon, for example, the same legislation that made it easier to carry a concealed handgun included one of the toughest new handgun-purchase laws in the country—a 15-day waiting period and fingerprint-background check on all purchases. . . .
>
> I gladly shared my critique of this study with Mr. Lott and will now reiterate it here; as someone fully credentialed to evaluate Mr. Lott's and Mr. Mustard's work,

I would have recommended that the paper be rejected. (See Douglas Weil, "A Few Thoughts on the Study of Handgun Violence and Gun Control," *Washington Times,* Aug. 22, 1996, p. A16.)

While it is true that I thanked Mr. Weil in my paper for a comment that he made, his single comment was nothing like what his letter to the *Times* claimed. Before he explained his concerns to the press, he and I had no discussions about whether I had controlled for "ways that state and local governments attempt to lower the crime rate," possibly because my study not only controls for arrest and conviction rates, prison sentences, the number of police officers and police payroll, but also waiting periods and criminal penalties for using a gun in the commission of a crime.

Mr. Weil's sole comment to me came after two previous telephone calls over a month and a half in which Mr. Weil had said that he was too busy to give me any comments. His sole comment on August 1 was that he was upset that I had cited a study by a professor, Gary Kleck, with whom Weil disagreed. I attempted to meet this unusual but minor criticism by rewriting the relevant sentence on the first page in a further attempt to dispassionately state the alternative hypotheses.

Mr. Weil's claims are particularly difficult to understand in light of a conversation that I had with him on August 5. After hearing him discuss my paper on the news, I called him to say how surprised I was to hear about his telling the press that the paper was "fundamentally flawed" when the only comment that he had given me was on the reference to Kleck. Mr. Weil then immediately demanded to know whether it was true that I had thanked him for giving comments on the paper. He had heard from people in the news media who had seen a draft with his name listed among those thanked. (On August 1, I had added his name to the list of people who had given comments, and when the news of the paper suddenly broke on August 2 with the story in *USA Today,* it was this new version that had been faxed to the news media.) He wanted to know if I was trying to "embarrass" him with others in the gun-control community, and he insisted that he had not given me any comments. I said that I had only done it to be nice, and I mentioned the concern that he raised about the reference to Kleck. Weil then demanded that I "immediately remove [his] name" from the paper.

3. This was not my only experience with Ms. Glick. On August 8, 1996, six days after the events of August 2 described above, I appeared with her on MSNBC. After I tried to make an introductory statement setting out my findings, Ms. Glick attacked me for having my study funded by "gun manufacturers." She claimed that I was a "shill" for the gun manufactures and that it was important that I be properly identified as not being an objective academic. She also claimed that there were many serious problems with the paper. Referring to the study, she asserted that it was a fraud.

I responded by saying that these were very serious charges and that if she had some evidence, she should say what it was. I told her that I didn't think she had any such evidence, and that if she didn't, we should talk about the issues involved in the study.

At this point the moderator broke in and said to Ms. Glick that he agreed that these were very serious charges, and he asked her what evidence she had for her statements. Glick responded by saying that she had lots of evidence and that it was quite obvious to her that this study had been done to benefit gun manufacturers.

The moderator then asked her to comment further on her claim that there were serious problems with the study, and she stated that one only had to go to page 2 before finding a problem. Her concern was that I had used data for Florida that was a year and a half old. The moderator then asked her why this was a problem, since I couldn't be expected to use data that was, say, as recent as last week. Ms. Glick responded by saying that a lot of things could have changed since the most recent data were available. I then mentioned that I had obtained more recent data since the study had been written and that the pattern

of people not using permitted guns improperly had held true from October 1987 to December 31, 1995.

A more recent exchange that I had with the Violence Policy Center's President, Josh Sugarmann, on MSNBC on February 24, 1997, involved the same accusations.

4. Douglas Weil, from the Center to Prevent Handgun Violence, a division of Handgun Control, wrote the following to the *Washington Times:* "Given that Mr. Lott has published 70 papers in peer-reviewed journals, it is curious that he has chosen a law review for his research on concealed-gun-carrying laws" (*Washington Times,* Aug. 22, 1996, p. A16).

5. Scott Harris, "To Build a Better America, Pack Heat," *Los Angeles Times,* Jan. 9, 1997, p. B1. In many ways, my study was indeed fortunate for the coverage that it received. It appears that no other study documenting the ability of guns to deter crime has received the same level of coverage. MediaWatch, a conservative organization tracking the content of television news programs, reviewed every gun-control story on four evening shows (ABC's *World News Tonight,* CBS's *Evening News,* CNN's *The World Today,* and NBC's *Nightly News*) and three morning broadcasts (ABC's *Good Morning America,* CBS's *This Morning,* and NBC's *Today*) from July 1, 1995 through June 30, 1997. MediaWatch categorized news stories in the following way: "Analysts counted the number of pro- and anti-gun-control statements by reporters in each story. Pieces with a disparity of greater than 1.5 to 1 were categorized as either for or against gun control. Stories closer than the ratio were deemed neutral. Among statements recorded as pro-gun control: violent crime occurs because of guns, not criminals, and gun control prevents crime. Categorized as arguments against gun control: gun control would not reduce crime; that criminals, not guns are the problem; Americans have a constitutional right to keep and bear arms; right-to-carry concealed weapons laws caused a drop in crime." MediaWatch concluded that "in 244 gun policy stories, those favoring gun control outnumbered stories opposing gun control by 157 to 10, or a ratio of almost 16 to 1 (77 were neutral). Talking heads were slightly more balanced: gun-control advocates outnumbered gun-rights spokesmen 165 to 110 (40 were neutral)." The news coverage of my study apparently accounted for 4 of the 10 "anti-gun control" news reports. (*Networks Use First Amendment Rights to Promote Opponents of Second Amendment Rights: Gun Rights Forces Outgunned on TV,* MediaWatch, July 1997.)

6. One of the unfortunate consequences of such attacks is the anger that they generate among the audience. For example, after Congressman Schumer's letter to the *Wall Street Journal,* I received dozens of angry telephone calls denouncing me for publishing my *Wall Street Journal* op-ed piece on concealed-handgun laws without first publicly stating that the research had been paid for by gun manufacturers. Other letters from the Violence Policy Center making these funding claims produced similar results.

Understandably, given the seriousness of the charges, this matter has been brought up by legislators in every state in which I have testified before the state legislature. Other politicians have also taken up these charges. Minnesota State Rep. Wes Skoglund (DFL-Minneapolis) provided one of the milder statements of these charges in the *Minneapolis Star Tribune* (Mar. 29, 1997, p. A13): "Betterman [a Minnesota state representative] uses a much-publicized study by John Lott Jr., of the University of Chicago, to back up her claims about the benefits of her radical gun-carry law. . . . But what no one has told you about Lott's study is that it has been found to be inaccurate and flawed. And Betterman didn't tell you that the study was funded by the Olin Foundation, which was created by the founder of Winchester Arms."

7. I telephoned Ms. Rand to ask her what evidence she had for her claim that the study was "the product of gun-industry funding" and reminded her that the public relations office at the University of Chicago had already explained the funding issue to her boss, Josh Sugarmann, but Ms. Rand hung up on me within about a minute.

8. Alex Rodriquez, "Gun Debate Flares; Study: Concealed Weapons Deter Crime," *Chicago Sun-Times*, Aug. 9, 1996, p. 2. Kotowski made his remark at a press conference organized by the Violence Policy Center, whose president, Josh Sugarmann, had been clearly told by the

press office at the University of Chicago on August 6 that these charges were not true (as the letter by William E. Simon shown later will explain). Catherine Behan in the press office spent an hour trying to explain to him how funding works at universities.

9. *Chicago Tribune*, Aug. 15, 1996.

10. "Study: Concealed Guns Deterring Violent Crime," *Austin American Statesman,* Aug. 9, 1996, p. A12.

11. The brief correction ran in the *Austin American Statesman,* Aug. 10, 1996.

12. As Mr. Simon mentions, one journalist who looked into these charges was Stephen Chapman of the *Chicago Tribune.* One part of his article that is particularly relevant follows:

> Another problem is that the [Olin] foundation didn't (1) choose Lott as a fellow, (2) give him money, or (3) approve his topic. It made a grant to the law school's law and economics program (one of many grants it makes to top universities around the country). A committee at the law school then awarded the fellowship to Lott, one of many applicants in a highly competitive process.
>
> Even the committee had nothing to do with his choice of topics. The fellowship was to allow Lott—a prolific scholar who has published some 75 academic articles—to do research on whatever subject he chose. . . .
>
> To accept their conspiracy theory, you have to believe the following: A company that derives a small share of its earnings from sporting ammunition somehow prevailed on an independent family foundation to funnel money to a scholar who was willing to risk his academic reputation (and, since he does not yet have tenure, his future employment) by fudging data to serve the interests of the firearms lobby—and one of the premier research universities in the world cooperated in the fraud. (See Stephen Chapman, "A Gun Study and a Conspiracy Theory," *Chicago Tribune,* Aug. 15, 1996, p. 31.)

13. A Gannett Newswire story quoted a spokeswoman for the Coalition to Stop Gun Violence who made similar statements: "But Katcher said the study . . . was funded by the Olin Foundation, which has strong ties to the gun industry. The study has 'been proven by a series of well-known, well-respected researchers to be inaccurate, false, junk science,' she said." (Dennis Camire, "Legislation before Congress Would Allow Concealed Weapons Nationwide," Gannett News Service, June 6, 1997.)

14. John R. Lott, Jr., "Should the Wealthy Be Able to 'Buy Justice'?" *Journal of Political Economy* 95 (Dec. 1987): 1307.

15. "Notebook," *The New Republic,* Apr. 14, 1997, p. 10.

16. After much effort, Randy was eventually able to get Cynthia Henry Thielen, a Hawaiian State Representative, to participate in the radio program.

17. Richard Morin, "Unconventional Wisdom: New Facts and Hot Stats from the Social Sciences," *Washington Post,* Mar. 23, 1997, p. C5.

18. It is surely not uncommon for academics to write letters to their local newspapers or to national or international publications, and indeed such letters were also written (see, for example, *The Economist,* Dec. 7, 1996, p. 8). But to track down the letters of everyday citizens to local newspapers and send replies is unusual.

19. The *Springfield State Journal-Register,* Nov. 26, 1996. Steven Teret, director of the Center for Gun Policy and Research wrote dozens of letters to newspapers across the country. They usually began with statements like the following: "Recently in a letter to the editor dated October 19, Kurt Amebury cited the work of two University of Chicago professors" (*Orlando Sentinel,* Nov. 16, 1996, p. A18); "Recently the *Dispatch* published a letter to the editor citing the work of two researchers" (*Columbus Dispatch,* Nov. 16, 1996, p. A11); "The *State Journal-Register* Oct. 28 published two letters citing research by the University of Chicago's John Lott" (*Springfield State Journal-Register,* Nov. 13, 1996, p. 6); or "A recent letter to the editor . . ." (*Buffalo News,* Nov. 17, 1996, p. H3). In late November, I asked Stephen Teret how many newspapers he had

sent letters to. He would not give me an exact count, but he said "dozens" and then listed the names of some major newspapers to which they had written. It is curious that none of the effort put into responding to my paper by the Center has gone into writing a comment for submission to the *Journal of Legal Studies,* where my original paper was published. Nor has the Center prepared a response for any other scholarly journal.

20. My opinion piece appeared in the *Omaha World-Herald,* Mar. 9, 1997, p. B9.

21. *Virginia Code Annotated,* § 18.2–3088 (1988).

22. This discussion relies on conversations with Clayton Cramer.

23. This point is similar to the "broken-window" argument made by Wilson and Kelling; see James Q. Wilson and George L. Kelling, "Making Neighborhoods Safe," *Atlantic Monthly,* Feb. 1989.

24. Some robberies also involve rape. While I am not taking a stand on whether rape or robbery is the primary motivation for the attack, there might be cases where robbery was the primary motive.

25. Information obtained from Kathy O'Connell at the Illinois Criminal Justice Information Authority.

26. For example, see Douglas Weil, "A Few Thoughts on the Study of Handgun Violence and Gun Control," *Washington Times,* Aug. 22, 1996, p. A16.

27. The durability of these initial false claims about Florida's crime rates can be seen in more recent popular publications. For example, William Tucker, writing in the *Weekly Standard,* claims that "Florida crime rates remained level from 1988 to 1990, then took a big dive. As with all social phenomena, though, it is difficult to isolate cause and effect." See William Tucker, "Maybe You Should Carry a Handgun," *Weekly Standard,* Dec. 16, 1996, p. 30.

28. In an attempt to facilitate Black's and Nagin's research, I provided them not only with all the data that they used but also computer files containing the regressions, in order to facilitate the replication of each of my regressions. It was thus very easy for them to try all possible permutations of my regressions, doing such things as excluding one state at a time or excluding data based on other criteria.

29. Dan Black and Dan Nagin, "Do 'Right-to-Carry' Laws Deter Violent Crime?" Carnegie-Mellon University working paper, Dec. 18, 1996, p. 5.

30. In addition, because the regressions use individual county dummy variables, so that they are really measuring changes in crime rates relative to each county's mean, one need not be concerned with the possibility that the average crime rates for the years that are farthest beyond the adoption of the concealed-handgun laws are being pulled down by relatively low crime rates in some states.

31. Ian Ayres and Steven Levitt, "Measuring Positive Externalities from Unobservable Victim Precaution: An Empirical Analysis of Lojack," NBER working paper 5928 (1997). The main issue with their empirical estimates, however, is whether they might be overestimating the impact from Lojack because they do not control for any other responses to higher auto-theft rates. For example, while higher auto-theft rates might trigger implementation of Lojack, they might also increase purchases of other antitheft devices like The Club. In addition, the political support for altering the distribution of police resources among different types of crimes might also change. Unfortunately, neither Ayres and Levitt nor Lojack has made the information on the number of Lojacks installed available to other researchers. My attempts to replicate their results with dummy variables have found insignificant effects.

32. Ultimately, however, the levels of significance that I have tested for are the final arbiters in deciding whether one has enough data, and the results presented here are quite statistically significant.

33. Daniel W. Webster, "The Claims That Right-to-Carry Laws Reduce Violent Crime Are Unsubstantiated," The Johns Hopkins Center for Gun Policy and Research, copy obtained March 6, 1997, p. 5.

34. Jens Ludwig, "Do Permissive Concealed-Carry Laws Reduce Violent Crime?" Georgetown University working paper (Oct. 8, 1996), p. 12.

35. "Battered Woman Found Not Guilty for Shooting Her Husband Five Times," *San Francisco Examiner,* Apr. 9, 1997.

36. In Chicago from 1990 to 1995, 383 murders (or 7.2 percent of all murders) were committed by a spouse.

37. For a detailed discussion of how Black's and Nagin's arguments have changed over time, see my paper entitled "'If at First You Don't Succeed . . .': The Perils of Data Mining When There Is a Paper (and Video) Trail: The Concealed-Handgun Debate," *Journal of Legal Studies* 27 (January 1998), forthcoming.

38. Black and Nagin, "Do 'Right-to-Carry' Laws Deter Violent Crime?" Carnegie-Mellon working paper, version of December 18, p. 5, n. 4.

39. The December 18, 1996, version of their paper included a footnote admitting this point:

> Lott and Mustard weight their regression by the county's population, and smaller counties are much more likely to have missing data than larger counties. When we weight the data by population, the frequencies of missing data are 11.7% for homicides, 5.6% for rapes, 2.8% for assaults, and 5% for robberies.

In discussing the sample comprising only counties with more than 100,000 people, they write in the same paper that "the (weighted) frequencies of missing arrest ratios are 1.9% for homicides, 0.9% for rapes, 1.5% for assaults, and 0.9% for robberies."

40. For rape, 82 percent of the counties are deleted to reduce the weighted frequencies of missing data from 5.6 to 0.9 percent. Finally, for robbery (the only other category that they examine), 82 percent of the observations are removed to reduce the weighted missing data from 5 to 0.9 percent.

41. The reluctance of gun-control advocates to share their data is quite widespread. In May 1997 I tried to obtain data from the Police Foundation about a study that they had recently released by Philip Cook and Jens Ludwig, but after many telephone calls I was told by Earl Hamilton on May 27, "Well, lots of other researchers like Arthur Kellermann do not release their data." I responded by saying that was true, but that it was not something other researchers approved of, nor did it give people much confidence in his results.

42. See William Alan Bartley, Mark Cohen, and Luke Froeb, "The Effect of Concealed-Weapon Laws: Estimating Misspecification Uncertainty," Vanderbilt University working paper (1997).

CHAPTER EIGHT

1. Allison Thompson, "Robber Gets Outgunned on Westside," *Jacksonville (Florida) Times-Union,* Sept. 24, 1997, p. B1.

2. Craig Jarvis, "Pizza Worker's Husband Shoots Masked Bandit," *Raleigh News and Observer,* Dec. 11, 1996, p. B3.

3. Other work that I have done indicates that while hiring certain types of police officers can be quite effective in reducing crime rates, the net benefit from hiring an additional police officer is about a quarter of the benefit from spending an equivalent amount on concealed handguns. See John R. Lott, Jr., "Does a Helping Hand Put Others At Risk? Affirmative Action, Police Departments, and Crime," University of Chicago working paper (July 1997).

4. The cost of public prisons runs about twice this rate; see Mike Flaherty, "Prisons for Profit; Can Texas System Work for Wisconsin's Overflowing System," *Wisconsin State Journal,* Feb. 16, 1997, p. Al.

5. Fox Butterfield, "Serious Crime Decreased for Fifth Year in a Row," *New York Times,* Jan. 5, 1997, p. 10.

6. Michael Fumento, "Are We Winning the Fight Against Crime?" *Investor's Business Daily,* Feb. 5, 1997, p. A34.

7. Yet there never was much controversy over this issue: when Congress debated the law, no one, not even the National Rifle Association, opposed background checks. The dispute was over a five-day waiting period versus an "instant check."

8. Fumento, "Fight Against Crime," p. A34.

9. After the Supreme Court decision, Arkansas completely stopped the background checks, while Ohio has essentially gutted the rules by making background checks voluntary. In addition, as "Ohio Deputy Attorney General Mark Weaver said, the responsibility for conducting background checks rests with counties and cities in most states—rather than with statewide agencies—and . . . 'hundreds of counties' stopped doing checks after the Supreme Court ruling." (Joe Stumpe, "Arkansas Won't Touch Gun Checks 'Unwarranted,' Chief Cop Says," *Arkansas Democrat-Gazette,* July 29, 1997, p. 1A.

10. Bureau of Alcohol, Tobacco, and Firearms, *A Progress Report: Gun-Dealer Licensing and Illegal Gun Trafficking,* Washington, DC: Department of the Treasury, Bureau of Alcohol, Tobacco, and Firearms (Jan. 1997). John Lott, "A Fair Shot: New Legislation Claims to Ease the Unfair Burden on Gun-Stores," *National Review Online*, July 26, 2006; and Michael Doyle, "Number of U.S. Gun Dealers Has Plunged," *Roanoke Times*, Sept. 20, 2009.

11. Many other restrictions on gun use have prevailed during the last couple of years, even some that appear fairly trivial. For example, in 1996 alone thirteen states voted on initiatives to restrict hunting. The initiatives were successful in eleven of the states. Congressman Steve Largent from Tulsa, Oklahoma, claims that the new rules are "part of a national effort to erode our ability to hunt. . . . It wasn't a local effort. It was a national effort." Not only were the initiatives strongly supported by animal rights activists, but they also received strong support from gun-control advocates. It is probably not lost on gun-control advocates that support for gun control seems to be strongest among those who grew up in households without guns and that making hunting less attractive is one long-term way to alter support for these initiatives. See Janet Pearson, "A 'Fair Chase': Keep the Sport in Hunting" *Tulsa World,* Nov. 17, 1996, p. G1.

12. For most government agencies that try to obtain higher funding, exaggerating the problems helps justify such higher funding. Michael Fitzgerald, a spokesman for the BATF in Chicago, is quoted as saying that 1 percent of federal license holders are estimated to be illegally running guns. "If that figure is accurate, the reduction of . . . dealers should eliminate a substantial number of traffickers." See Jim Adams, "Number of Licenses Falls Dramatically: Crime Law Puts Squeeze on Gun Dealers; Zoning Can Be Used to Keep Gun Sales Out of Private Homes," *Louisville Courier-Journal,* Mar. 20, 1997, p. A1.

13. During the last few years, the BATF has been much more aggressive in harassing law-abiding gun owners and retailers. A recent study using 1995 data, by Jim Couch and William Shughart, claims not only that the BATF refers dramatically more criminal fire arm violations to prosecutors in states that have more National Rifle Association members, but that Clinton's own U.S. attorneys have declined to prosecute a much greater percentage of the cases referred to them in these states. They estimate that 54 percent of the variation across states in the BATF's criminal referrals is explained simply by the number of NRA members in a state, and that about a quarter of these higher requests for prosecutions are declined by U.S. attorneys. See Jim F. Couch and William F. Shughart I, "Crime, Gun Control, and the BATF: The Political Economy of Law Enforcement," University of Mississippi working paper presented at the March, 1997, Public Choice Meetings in San Francisco.

14. I cannot end, however, without at least mentioning several excellent law-review articles on the issue of what was intended in the Second Amendment: see Nelson Lund, "The Second Amendment, Political Liberty, and the Right to Self-Preservation," *Alabama Law Review* 33 (1988): 103–47; Robert J. Cottrol and Raymond T. Diamond, "The Fifth Auxiliary Right," *Yale Law Journal* 104 (1995): 309–42; Don B. Kates, "Handgun Prohibition and

the Original Meaning of the Second Amendment," *University of Michigan Law Review* 82 (1983): 204–68; William Van Alstyne, "The Second Amendment Right to Arms," *Duke Law Review* 43 (Apr. 1994): 1236–55; and Sanford Levinson, "The Embarrassing Second Amendment," *Yale Law Journal* 99 (Dec. 1989): 637–89. Legal scholars seem to be in general agreement on the way the Second Amendment's use of the word *militia* is so completely misinterpreted in current discussions of what the amendment means. The only twentieth-century case in which the Supreme Court directly interpreted the Second Amendment was *United States v. Miller,* 307 US 174 (1939). The court was quite clear that historical sources "showed plainly enough that the Militia comprised all males physically capable of acting in concert for the common defense." The court accepted "the common view . . . that adequate defense of the country and laws could be secured through the Militia—citizens primarily, soldiers on occasion."

The framers of the Constitution were also very clear on this issue. James Madison wrote in the Federalist papers that if a standing army threatened citizens' liberties, it would be opposed by "a militia amounting to near a half-million citizens with arms in their hands"; see Clinton Rossiter, ed., *The Federalist* no. 46 (1961): 299. An excellent discussion of this and related issues is presented by David L. Franklin and Heather L. O'Farrell in their University of Chicago Moot Court brief on *Printz and Mack v United States,* Apr. 18, 1997.

CHAPTER NINE

1. Dates were established by doing a Nexis search. During 1996, Kentucky, Louisiana, and South Carolina enacted "shall-issue" laws. However, these did not go into effect until extremely late in the year. Louisiana did not even start issuing applications until the end of September (Lisa Roland, "Applications for Concealed Handgun Permits to Be Issued This Week," *Gannett News Service,* Sept. 20, 1999). In Kentucky, permits were also not issued until the very end of the year (Michael Quinlan, "Concealed Guns: Permits Will Take Time, Law Will Go into Effect Tomorrow," *Louisville Courier-Journal,* Sept. 30, 1996, p. A1). South Carolina's law went into effect August 22, 1996, but its permitting process also took a couple of months to start actually issuing permits (Kathy Steele, "Women with Guns on Rise," *Augusta (GA) Chronicle,* Apr. 11, 1997, p. B2).

2. While I believe the much more interesting question is how crime rates change before and after the adoption of right-to-carry laws, the states with right-to-carry laws in effect for at least one year in 1996 had an average violent crime rate of 446.6 per 100,000 people, while the states with more restrictive "may-issue" rules had a violent crime rate of 592.6, and states banning concealed handguns a rate of 789.7. The main reason for not focusing on these numbers is simply that it ignores whether these states tended to be the lowest-crime-rate states even before they adopted right-to-carry laws. One method that partially accounts for this concern is to examine the cross-sectional data using the demographic, poverty, income, and other variables that have been employed throughout the book. After controlling for these other factors, the presence of a right-to-carry law implies a violent crime rate 15 percent lower than the absence of a law implies, and the effect is quite statistically significant, with a t-statistic that is significant at better than the .01 percent level for a two-tailed t-test.

3. David Hemenway, "Book Review of *More Guns, Less Crime," New England Journal of Medicine,* Dec. 31, 1998, pp. 2029–30.

4. Jens Ludwig, "Concealed-Gun-Carrying Law and Violent Crime: Evidence from State Panel Data," *International Review of Law and Economics* 18 (Sept. 1998): 239–54.

5. The Northeast includes Connecticut, Delaware, the District of Columbia, Maine, Maryland, Massachusetts, New Hampshire, New Jersey, New York, Pennsylvania, Rhode Island, and Vermont; the South includes Alabama, Arkansas, Florida, Georgia, Louisiana, Mississippi, Missouri, North Carolina, Oklahoma, South Carolina, Tennessee, Texas, and Virginia; the Midwest includes Illinois, Indiana, Iowa, Kansas, Kentucky, Michigan, Minnesota, Nebraska, North Dakota, Ohio, South Dakota, West Virginia, and Wisconsin; the

Rocky Mountains include Arizona, Colorado, Idaho, Montana, Nevada, New Mexico, Utah, and Wyoming; and the Pacific states include Alaska, California, Hawaii, Oregon, and Washington.

6. Because of the criticism that it is unrealistic to use a simple dummy variable, I have decided to focus from the beginning on the more realistic approach that examines the before- and after-law trends in crime rates.

7. The results using the old specifications also continue to be very similar.

8. As another test of the sensitivity of the results, I also reestimated the before-and-after trends by limiting them to ten years before and after the adoption of the right-to-carry laws. The results equivalent to table 9.1 are –3.1 percent for violent crime, –0.8 percent for murder, –2.0 percent for rape, –2.6 percent for robbery, –3.3 percent for aggravated assault, and –0.4 percent for property crime. All the violent-crime category results are significant at least at the .01 percent level except for murder, which is significant at the 4 percent level.

9. See also figures 7.7–7.9.

10. Glenn Puit, "Survey: Gun Sales Increasing since Grocery Store Shooting," *Las Vegas Review-Journal,* June 24, 1999, p. 4A; and "Gun Sales up 30 Percent This Year," *Associated Press Newswire,* dateline San Francisco, Aug. 28, 1999. The *Las Vegas Review-Journal* article mentions that "Firearms instructors also said they have seen a jump in the number of people wanting to know the requirements to carry a concealed weapon. And, Las Vegas police have seen an increase in requests for concealed weapons permits in recent weeks." The Associated Press story mentions that "Others say recent crime stories in the news, from the shooting rampage at a Los Angeles Jewish day camp to the tourist killings in Yosemite National Park, have motivated gun buyers."

11. The average murder rate for states over this period is 7.57 per 100,000; for rapes, 33.8; for aggravated assaults, 282.4; and for robberies, 161.8. A 4 percent change in murders is 0.3 per 100,000, a 7 percent change in rape is 2.4 per 100,000, a 5 percent change in aggravated assaults is 14.1 per 100,000, and a 13 percent change in robberies is 21 per 100,000. By contrast, a one-percentage-point increase in the population with permits is 1,000 per 100,000.

12. While small, lightweight guns are available and new materials have also made it possible to make lighter guns, most handguns weigh about the same as a laptop computer. Carrying them around requires some significant inconvenience.

13. More precisely, I replaced the predicted percentage of the population with permits with the predicted percentage of the population with permits divided by the permit fee. This is the same as the interactions done earlier looking at the percentage with permits multiplied by county demographics.

14. Ideally, one would also want to use the expected variation in permit rates across counties (though those data were not available at the time that I put these results together), but since I am examining all counties in the state, the state permitting rates at least allow us to rank the relative impact of right-to-carry laws across states.

15. The different drafts of their paper also went through different specifications.

16. Edward E. Leamer, "Let's Take the Con Out of Econometrics," *American Economic Review* 173 (Mar. 1983): 31–43; and Walter S. McManus, "Estimates of the Deterrent Effect of Capital Punishment: The Importance of the Researcher's Prior Beliefs," *Journal of Political Economy* 93 (Feb. 1985): 417–25.

17. I also included a tenth variable that examined the percentage of the adult population that was in prison, but there were sufficient theoretical objections to including this that I have decided not to report these results in the text. The major theoretical problem is that this variable is a "stock" while the crime rate is a "flow." In other words, the prison population is created by the number of people who are convicted and sentenced over many years and not just how harsh the current sentences are. In fact, if tough sentencing in the past makes it more likely that current criminals will not be sentenced to prison terms as long as those of past criminals (e.g., because of a takeover of the prison system by the courts),

it is possible that there might even be a negative relationship between the prison population and the current toughness of the system. The bottom line is that past punishment is only roughly related to current punishment, particularly when average state differences are already being taken into account through fixed effects and when regional yearly fixed effects have also been added.

18. In a powerful piece, Isaac Ehrlich and Zhiqiang Liu show that classic economics papers concerning the law of demand, production theory, and investment theory would fail this test (Isaac Ehrlich and Zhiqiang Liu, "Sensitivity Analyses of the Deterrence Hypothesis: Let's Keep the Econ in Econometrics," *Journal of Law and Economics* 42 [Apr. 1999]: 455–88). Because of this strong bias toward not finding "true" relationships, Leamer and McManus have dropped off the 10 percent most extreme values on both ends of their estimates when they have reported their results. Yet even this does not protect most studies from having their results determined to be "fragile" by this test.

19. One problem from excluding the arrest rate was never clearly made in the first edition of this book. The reason using the arrest rate forces some county observations to be dropped is that when the number of crimes is zero, the arrest rate is "undefined." Including counties with zero crime rates biases the results toward not finding an effect because crime rates cannot fall below zero. Since these counties already have a zero crime rate, the passage of the right-to-carry law can produce no benefit. The more counties with zero crime rates that are included, the more the estimated benefit from the law will move toward zero.

My work with Steve Bronars also examined whether replacing the crime-specific arrest rates with the overall violent-crime or property-crime arrest rates altered the results, and we found that it had no impact on the results. There are few counties which have no violent crimes of any type, so there are few missing observations for the violent-crime arrest rate (Stephen G. Bronars and John R. Lott, Jr., "Criminal Deterrence, Geographic Spillovers, and Right-to-Carry Laws," *American Economic Review* 88 (May 1998): 475–79).

20. While I find it difficult to believe that anyone would argue that demographic factors are not important in explaining crime rates, I did try a couple of specification tests. Paring the demographic variables down to the percentage of the population that is black, the percentage of the population that is white, the percentage of the population that is male, and the percentage of the population in the six different age classifications leaves the results essentially unchanged. Eliminating the demographic variables entirely reduces the estimated drop in violent-crime rates from right-to-carry laws by at most one percentage point.

21. The way that the county-level data were compiled was changed in 1994. Prior to that time those jurisdictions within a county which provided data for fewer than six months were estimated to have the same offense rates as the rest of the county. From 1994 onward, the imputation method was applied only to counties with less than three months of data. For jurisdictions with at least six months of data prior to 1994 and at least three months of data after that time, the jurisdiction was calculated to have $12/N$ offenses, where N is the number of months reported.

Because of concerns that this might affect estimates using data after 1993, I reran the regressions reported in table 9.1 by including a variable for the change in a county's crime rate between 1993 and 1994. This change variable was included for the 1994–1996 observations to account for the relative differences that this change in measurement might have had across different counties. The results are similar to those already reported. The annual difference in the trends in violent-crime rates before and after the passage of a right-to-carry law are –1.4 percent for murder, –2.94 percent for rape, –2.8 percent for robbery, and –3.12 percent for aggravated assault. All the results are significant at better than the .01 percent level with F-tests of 17.36, 83.33, 87.38, and 87.31, respectively.

22. These data draw on research that I am currently conducting with Kevin Cremin. Kevin collected all the data used here on policing policies.

23. "[The] problem-solving effort began essentially as directed patrol operations designed to identify patterns of offending or known offenders and to deploy police to catch the offenders. All gradually evolved into quite different efforts that involved activities other than arrest and agencies other than the police. The attack on burglaries in the housing projects involved surveying tenants, cleaning the projects, creating a multiagency task force to deal with particular problems in the housing projects, and organizing the tenants not only to undertake block watches but also to make demands on city agencies. The attack on thefts from cars eventually involved the inclusion of police officers in the design of new parking lots to make them less vulnerable to theft. The attack on prostitution and robbery involved enhanced code enforcement against hotels and bars that provided the meeting places for prostitutes and their customers as well as decoy operations" (Christopher Slobogin, "Why Liberals Should Chuck the Exclusionary Rule," *University of Illinois Law Review* 99 (1999): 363.

24. The data on community-oriented policing, problem-oriented policing, and the broken-windows strategy were primarily obtained by using the Westlaw "News" database. For community policing, the search took the form [name of city] & "community policing" & DA(BEF 1/1/1997) & DA(AFT 1/1/1975). For problem-oriented policing, the search took the form ("Problem Solving Policing" or "Problem-Solving Policing" or "Problem Oriented Policing" or "Problem-Oriented Policing") & DA(AFT 1/1/1975) & DA(BEF 1/1/1997). Finally, for the broken-windows strategy, the search consisted of "Broken Window" & Crime & DA(AFT 1/1/1975) & DA(BEF 1/1/1997) AND NOT "Broken Windows." Other sources were also investigated. For community policing, the sources included Robert C. Trojanowicz and Hazel A. Harden, "The Status of Contemporary Community Policing Programs," National Center for Community Policing, 1985; Washington State University, Division of Governmental Studies and Services (DGSS), surveys of police administrators conducted at three-year intervals between 1978 and 1994; Anna Sampson, "National Survey of Community Policing Strategies, 1992–93"; and Robert C. Trojanowicz et al., "Community Policing: A Survey of Police Departments in the United States," 1994. However, the only one of these studies which identifies the cities is the 1985 Trojanowicz and Harden study. The authors of the other studies were unwilling to identify the cities in their samples. For the broken-windows strategy, George Kelling's book was also used to identify additional cities (George L. Kelling, *Fixing Broken Windows: Restoring Order and Reducing Crime in Our Communities* [New York: Free Press, 1998]).

25. John R. Lott, Jr., "Does a Helping Hand Put Others at Risk? Affirmative Action, Police Departments, and Crime," *Economic Inquiry* (forthcoming).

26. For example, policing policies may have changed because of concerns about future crime rates. Not adopting the change might have resulted in even more crime.

27. Bartholomew Sullivan, "Students Recall 'Unreal' Rampage," *Commercial Appeal*, June 11, 1998, p. A1.

28. Lance Gay, "New Gun Measure Wouldn't Have Halted School Tragedies," *Cleveland Plain Dealer*, May 30, 1999, p. 19A.

29. Pam Belluck and Jodi Wilgoren, "Shattered Lives—a Special Report: Caring Parents, No Answers, in Columbine Killers' Pasts," *New York Times*, June 29, 1999, p. A1; and Virginia Culver, "Pastor Comforts Gunman's Family," *Arizona Republic*, May 1, 1999, p. D7.

30. Evelyn Larrubia, Ted Rohrlich, and Andrew Blankstein, "Suspect Scouted 3 Prominent L.A. Jewish Sites as Targets," *Los Angeles Times*, Aug. 13, 1999, p. 1.

31. An earlier attempt by Congress to pass this law was never really enforced and was struck down by the Supreme Court in 1995. The 1995 law put in simple "boiler plate" language requiring that prosecutors make a finding that the gun or parts of the gun had been involved in interstate commerce.

32. These results are available at http://ssrn.com/abstract=272929. If the variance doesn't equal the mean, the appropriate test is to use a negative binomial, which no longer requires this assumption. Redoing the results presented in this chapter with a negative binomial

produces results extremely similar to those that will be reported. For example, redoing the right-to-carry estimates for table 6.6 with a negative binomial produces

Murders in multiple-victim public shootings: 71%, z = 2.496, significant at the 1.3% level

Injuries in multiple-victim public shootings: 83%, z = 3.414, significant at the 0.1% level

Attempted or actual bombings: 67%, z = 3.821, significant at the 0.1% level

33. To illustrate, let the probability that a single individual is carrying a concealed handgun equal .10. Assume further that there are 10 individuals in a public place. Then the probability that at least one of them is armed is $1 - .9^{10}$, or about .65.

34. *Baltimore Sun*, Apr. 30, 1999.

35. Greg Pierce, "Professional Viewpoint," *Washington Times*, Sept. 3, 1999, p. A5.

36. Even so-called smart locks, which are activated by one's fingerprint or by a special ring with a computer, pose several types of risks. With locks activated by fingerprints, a spouse would be unable to use the gun to come to the other person's rescue if the gun were coded for the other person. The person must also correctly position the finger on the fingerprint reader. Small differences in the angle of the finger may leave the gun inoperable even for the designated user.

37. This discussion is based upon research that I am currently doing with John Whitley.

38. Peter Cummings, David C. Grossman, Frederick P. Rivara, and Thomas D. Koepsell, "State Gun Safe Storage Laws and Child Mortality Due to Firearms," *Journal of the American Medical Association* 278 (Oct. 1, 1997): 1084–86.

39. U.S. General Accounting Office, "Accidental Shootings: Many Deaths and Injuries Caused by Firearms Could Be Prevented" (Washington, DC: U.S. General Accounting Office, Mar. 1991).

40. An article in the *Journal of the American Medical Association* does not control for any other factors but claims that 23 percent of the accidental gun deaths for children under fifteen would have been prevented by these storage rules. In 1996, this would have amounted to thirty-two lives if the laws had been in effect for the entire country. One obvious mistake that this article made was that it made no attempt to account for the normal downward trend in accidental gun deaths that would have continued to at least some extent even without these safe-storage laws. Since no other variables were being controlled for, all of the drop was being attributed to the new law (Cummings et al., "State Gun Safe Storage Laws").

41. As of this writing, the Violence Policy Center still has a section of its Web site entitled "Funder of the Lott CCW Study Has Links to the Gun Industry" at http://www.vpc.org/fact_sht/lottlink.htm.

42. M. W. Guzy, "Soft Logic on Hard Facts on Guns," *St. Louis Post-Dispatch*, July 22, 1998, p. B7.

43. Shelley Kiel [state senator in Nebraska], "Some Gun Restrictions Needed," *Omaha World-Herald*, July 11, 1998, p. 11.

44. Kevin Beck, "Conceal Carry," *St. Louis Post-Dispatch*, Aug. 12, 1998.

45. Minnesota Representative Wesley Skoglund on PBS's *Almanac*, Sept. 26, 1998.

46. Take for example a June 21, 1999, discussion between two people on alt.fan.cecil-adams:

"Dutch Courage": hey, did you know Lott's study was funded by a gun manufacturer? I did. That's a little suspicious, don't you think?

"Shawn Wilson": Actually, it wasn't.

"Dutch Courage": You're right, it was a foundation founded by the owner of a gun company, which is now an ammunition company, and further the foundation

> has large holdings in this company, and several of the directors of this foundation are men with standing within the company which shares the name. So much for his reputation as an honest scholar and academic reputation, eh?

47. Linnet Myers, "Go Ahead . . . Make Her Day," *Chicago Tribune*, May 2, 1999, p. C12. See also Diane Carman, "Gun-Bill Premise Is Bogus," *Denver Post*, Mar. 23, 1999, p. B1: "While gun-control activists have criticized Lott's work because it is funded in part through a grant from the Olin Foundation, which was founded by the largest manufacturer of ammunition in the U.S., [Jens] Ludwig argues that the debate about the grant money 'only distracts people. The study fails on its merits.'"

48. This quote is from the Web site of Handgun Control, Inc. (http://www.handgun-control.org/lott.htm). The Violence Policy Center's claim that I believe that "increases in the percent of minority police officers increase crime rates" can be found at http://www.vpc.org/fact_sht/wholott.htm. Of course, the Violence Policy Center fails to mention the rest of the abstract in question, which points out that the paper (Lott, "Does a Helping Hand Put Others at Risk?") will investigate "whether these increases in crime are due to changes in the quality of all new police officers or just minority officers."

49. The previous footnote provides references for this claim on gun-control Web sites. Similar statements were made by Luis Tolley, the western regional director for Handgun Control, Inc., at a debate that I participated in at Claremont College, and Tom Diaz, an analyst for the Violence Policy Center, has made this claim a couple of times when we appeared on radio shows together.

50. Lott, "Does a Helping Hand Put Others at Risk?"

51. The selective quoting was obviously a well-orchestrated campaign, with newspaper editorials also getting involved in repeating the statements by Handgun Control. Consider the following editorial attack on me: "In May 1998, for instance, he published the following in a police research journal: 'Increasing black officers' share of the police force by one percentage point increases murders by four percent, the violent crimes by seven percent, and property crimes by eight percent. . . . More black and female officers are also associated with declines in both the arrest and conviction rates'" (Editorial, "A Lott More Guns," *St. Louis Post-Dispatch*, Mar. 23, 1999, p. B6). They failed to quote some other sentences in this same piece, such as "Not all black officers nor all white officers nor all officers of any other race are of the same quality. Some black officers are undoubtedly better at reducing crime than most potential white officers, and some white officers are probably better than most potential black officers. The question is how to select those officers who will do the best job. There is the possibility that choosing applicants by race or sex could work against hiring the best officers available. . . . One must be very clear about what is happening, however. The large impact of more black officers indicates that more than just the quality of new minority recruits or new minority promotions are affected. Indeed, changing tests to employ a greater percentage of blacks appears to make it more difficult to screen out lower-quality candidates generally, including whites and other racial groups" (John R. Lott, Jr., "Who Is Really Hurt by Affirmative Action?" *Subject to Debate*, May 1998, pp. 1, 3).

52. William F. Shughart II, "More Guns, Less Crime: Understanding Crime and Gun Control Laws: Review," *Southern Economic Journal* 65, no. 4 (Apr. 1, 1999): 978.

53. Bruce L. Benson, "Review of *More Guns, Less Crime*," *Public Choice* 100 (Sept. 1999), nos. 3–4: 309.

54. Stan Liebowitz, "Handgun Argument Is Loaded," *Dallas Morning News*, June 21, 1998.

55. Nelson Lund, "Gunning Down Crime: The Statistics of Concealed Weapons," *Weekly Standard*, June 1, 1998.

56. Joanne Eisen and Paul Gallant, "Scientific Proof That Gun Control Increases the Cost of Crime," *Shield*, Summer 1998, p. 42.

57. I really don't take most threats very seriously, and I believe that it is just people blowing off steam. The worst threats usually come over the telephone, though I did have some regular writers from Canada who would express the hope that someone would get a gun and kill either me or my family members. The one e-mail threat that was forwarded to me by one of the editors at the University of Chicago Press gives some idea of the types of comments I received:

> Pass along the word, to that soulless weasel and absolutely irresponsible chickenshit John M. Lott that he better change his name and get some plastic surgery because his days of [obscenities deleted] of the NRA's [obscenities deleted] will be quickly coming to a crashing close if he keeps trying to pass off unethical, and second rate statistics with his pseudoscience rhetorical sylogisms.
>
> My point—someone is going to become very angered by the view of this imbecile, and is going to get a concealed hand-gun permit and find where he lives and make a point. I won't lose sleep knowing that one more moron is dead, but I feel that he should be warned none-the-less. Also, if John Lott had any integrity he'd make it possible to reach him. Since the little scatmuncher is playing hide and seek by having no-available e-mail adress, whoever reads this please forward this too him. This is not a threat, just a warning.
>
> Sometimes when views of cretins like this are expressed I think "love it or leave it," and man, if our scholars get any stupider and any more immoral than Mr. Lott I'm out of this shit house. I nearly packed my bags.

58. Matt Bai, "Is He the Smoking Gun?" *Newsweek*, Jan. 25, 1999, Business section.

59. "According to the Federal Bureau of Investigation's *Uniform Crime Report*, from 1992 to 1997, states which made it easier for citizens to carry concealed handguns had a significantly smaller drop in their crime rates than states which chose not to loosen their concealed weapons laws" (Brian Morton [associate director of communications for Handgun Control and the Center to Prevent Handgun Violence], "John Lott's Gun Research Doesn't Hold Up to Review," *Fort Wayne Journal Gazette*, Aug. 15, 1999, p. 3C).

Even when others would state that the FBI indeed did not produce these claims, Handgun Control's press release was put on the same footing as my research. Consider the following: "The Center to Prevent Handgun Violence did a 1999 analysis of crime statistics that came to a conclusion opposite of Mr. Lott's, and their study (like his) is open to review by experts in many fields" (Molly Ivins, "More Guns, Less Crime? Are You Sure?" *Fort Worth Star-Telegram*, Aug. 15, 1999). For clarification, the Center to Prevent Handgun Violence is part of Handgun Control, and Sarah Brady serves as the head of both organizations. Many similar statements were made by the media in Missouri during the debate over the concealed-handgun law.

60. For example, a December 1998 press release on children and gun violence had South Carolina and Colorado ranking similarly in terms of how liberal their right-to-carry laws were, but by January 1999, in a press release examining the change between 1992 and 1997, Colorado was listed as having a more restrictive law than South Carolina. The only motivation that I can conjecture for the change was that it helped get them the different results that they wanted.

61. "In stark contrast, a review of the national Uniformed Crime Reporting data, which is compiled by the FBI each year from state and local law enforcement agencies, indicates that the violent crime rate has fallen in all states by an average of 19 percent from 1992–97" (Richard Cook, "Don't Buy the Pro-Gun Arguments," *Kansas City Star*, Mar. 11, 1999, p. B7).

62. Peter Squires, "Review of More Guns, Less Crime," *British Journal of Criminology* 39, no. 2 (Spring 1999): 318–20.

63. My book does not even cite this quotation, though I mentioned it in an earlier re-

search paper because it was "quite relevant" to the debate over concealed handguns: it illustrates both the possibility of deterrence and the fears about the possible disasters that such laws could lead to.

Still other recent discussions in medical journals continue claiming that the nondiscretionary concealed-handgun laws for "several counties . . . were misclassified" and that the National Academy of Sciences deemed it inappropriate to account for arrest rates when researchers tried to explain changes in crime (see Arthur Kellermann and Sheryl Heron, "Firearms and Family Violence," *Emergency Medicine Clinics of North America*, Aug. 1999, pp. 699–708). Of course, responses 4 and 9 on pages 132–33 and 142 in this book addressed the first concern and page 18 discussed the second one.

64. http://www.handguncontrol.org/gunowner/statflaw.htm.

65. Doug Weil, Handgun Control's research director, provided the only response that I know of to my research on the Brady law by claiming that "Since John's data does not cover the years following implementation of the Brady Act, it's hard to know how he can claim to have studied the impact of the Brady law on crime rates or criminal access to guns" ("More Guns, Less Crime?: A Debate between John Lott, Author of *More Guns, Less Crime*, and Douglas Weil, Research Director of Handgun Control, Inc.," an online debate sponsored by *Time* magazine, transcript from July 1, 1998.) In fact, my book examined data up through 1994, the first year that the Brady law was in effect.

66. Romesh Ratnesar, "Should You Carry A Gun? A New Study Argues for Concealed Weapons," *Time*, July 6, 1998, p. 48.

67. I responded by saying that he was doing more than simply reporting these statements as claims when he used phrases like "Lott dropped" or "the book does not account." More importantly, readers were likely to believe that he had looked at the material and that he would not print something, even if the critics claimed it was true, unless it was true. Again, he emphasized that his role was that of a reporter and not to take sides in the debate.

I had called Romesh in part to tell him that I planned to send in a letter clarifying these points, and *Time* magazine did print a letter. Undoubtedly he played some role in guaranteeing that the letter was published, but it seems doubtful that the letter carried the same weight as a statement by the reporter about whether he could verify if the claims made against me were true. The letter in *Time* magazine was printed in the Aug. 3, 1998, issue under the heading "More about Concealed Weapons." It read:

> While your piece "Should You Carry a Gun?" [July 6] was generally favorable toward my new book, *More Guns, Less Crime*, it contained seriously misleading statements. Despite accusations by some critics, my study on the effect that carrying concealed weapons has on crime absolutely did not ignore "counties that had no reported murders or assaults for a given year." In contrast to the tiny samples in previous work by others, I used data on all the counties in the U.S. that were available when I did the study on the years from 1977 to 1994. It is likewise false that I did "not account for fluctuating factors like poverty levels and police techniques." Among the factors I included in the analysis were poverty, income, unemployment, arrest and conviction rates, the number of police officers and police expenditures per capita, as well as the impact that the prevention of less serious crimes has on more serious ones.

68. Ivins, "More Guns, Less Crime? Are You Sure?"

69. Tom Teepen, "A Modest Proposal: Let's Arm the Teachers," *Atlanta Journal and Constitution*, Sunday, May 17, 1998, p. 2G.

70. The following letter of mine appeared in the *Atlanta Journal and Constitution*, May 24, 1998, p. 6B:

> Tom Teepen's column "A modest proposal: Let's arm the teachers," Perspective, May 17), an attack on my new book "More Guns, Less Crime" (University of Chicago

Press), contained misleading information. He claimed that "Lott can't fairly compare 1988 and 1996 exit polls on gun ownership, as he does, because the questions were asked differently." Yet on pages 36–37 in my book, I point out this fact and discuss in detail what impact this has on estimates of changing gun ownership.

Citing a paper in the *Journal of Legal Studies*, Teepen claimed that I make a "fundamental gaffe" by failing to consider other anti-crime variables. My book provides the first systematic national evidence and examines the crime, accidental gun death, and suicide rates for all 3,054 counties in the United States by year from 1977 to 1994. No other study on crime has attempted to account for anywhere near as many different factors that could have affected crime rates over time. Unlike the Centers for Disease Control and Prevention's claim that homes with guns were "more likely to experience suicide," or have "a member of the family killed by another member or by an acquaintance," I did not focus on data from only one or a few cities for only one year. There is no evidence that these claims are correct.

Obviously, bad things can happen with guns, but guns also prevent bad things from happening to people. The evidence in my book indicates that many more lives are saved than lost from gun ownership.

71. An editor at the *Fort Worth Star-Telegraph*, Bob Davis, was very helpful, and he took the time to read my book to evaluate whether a mistake had been made. He printed a response by me in his newspaper, and he asked Creators Syndicate, which distributes Ms. Ivins's commentary, to make the response available to other newspapers around the country that carried Ms. Ivins's column. Unfortunately, despite repeated promises by Creators to do so, they never followed through on this.

72. Let me just give a couple of other examples.

> Even John Lott admits that 58 percent of homicides are committed either by family members or friends and acquaintances, not criminals. (Richard Scribner, [director of the Injury Control Research Center], "More Guns Don't Mean a Safer Society," *New Orleans Times-Picayune*, Apr. 28, 1999, p. B6)

> Dr. Lott's own analysis accounts for only about 10 percent of why some crime rates have fallen. We need to explain the other 90 percent before concluding that the "best" social policy is to carry more handguns. (Shela Van Ness, "More Guns, Less Crime? This Isn't Just a 'Good Guy' vs. 'Bad Guy' Issue," *Chattanooga Times / Chattanooga Free Press*, May 9, 1999, p. H1)

For the first point, not only do I not "admit" this, but my book points out that this claim is extremely misleading because the term "acquaintances" primarily includes rival gang members killing each other or drug buyers and drug sellers killing each other. As to the second point, the estimates shown in this book explain about 80–95 percent of the variation in crime rates.

73. The *Chronicle of Higher Education* noted that the opposition to my book also showed up in the University of Chicago Press, this book's publisher. The *Chronicle* reported that "The book also caused a mini-revolt at Chicago, where salespeople initially blanched at the prospect of pitching it to bookstores. Some cited personal views about guns; others thought that the book would alienate booksellers" (Christopher Shea, "'More Guns, Less Crime': A Scholar's Thesis Inflames Debate over Weapons Control," *Chronicle of Higher Education*, June 5, 1998, p. A14).

74. In this case, the dummy must be interpreted as whether the law raised or lowered the crime rate as quickly as the quadratic time trend would predict.

75. This example is taken from David D. Friedman's Web site, www.best.com/~ddfr/Lott_v_Teret/Lott_Mustard_Controversy.html.

76. Virtually identical complaints have been posted on the Handgun Control, Inc., Web

site, where Handgun Control writes: "To this day, John Lott has failed to provide any statistical evidence of his own that counters Black and Nagin's finding that Lott's conclusions are inappropriately attributed to changes in concealed-carry laws. Until Lott can do this, it is inappropriate for him to continue to claim that allowing more people to carry concealed handguns causes a drop in crime."

77. Dan A. Black and Daniel S. Nagin, "Do Right-to-Carry Laws Deter Violent Crime?" *Journal of Legal Studies* 27 (Jan. 1998): p. 213.

78. What is mystifying to me is how others have also continued to make this claim. Hashem Dezhbakhsh and Paul H. Rubin claim that "We believe that Lott and Mustard's findings are suspect, mainly because of the way they parameterize and measure the effect of permissive handgun laws on crime. They model the effect as a shift in the intercept of the linear crime equation they estimate at the county level. This approach is predicated on two assumptions: (i) all behavioral (response) parameters of this equation (slope coefficients) are fixed (unaffected by the law), and (ii) the effect of the law on crime is identical across counties" (Hashem Dezbakhsh and Paul H. Rubin, "Lives Saved or Lives Lost? The Effects of Concealed-Handgun Laws on Crime," *American Economic Review Papers and Proceedings*, May 1998, p. 468).

79. http://www.best.com/-ddfr/Lott_v_Teret/Friedman_on_B_and_N.html. A great deal of debate about my research and other gun-related research takes place on the Internet in discussion groups such as talk.politics.guns or on Web sites such as David Friedman's, which allows for a very detailed discussion of the issues. The give and take also allows people to ferret out the weaknesses and strengths of different arguments.

80. Benson, "Review of *More Guns, Less Crime*" p. 312.

81. An example of one of the other criticisms is by Ayres and Donohue where they write that "the ultimate criticism of Lott will be that the model is too flawed to provide any information on the effect of the law. . . . One of the strongest results to emerge from Lott's book is that shall issue laws, as he models them, lead to higher property crime. If you don't believe this, then you cannot endorse any of Lott's findings. But, to believe that property crime rose you must believe that the rate of robbery fell, because the only reason that more concealed handguns would cause property crime to go up is that some other money-generating activity became less available or less attractive. One would hardly expect that someone desiring to beat up an individul would instead decide to steal a car if the assaultive option were foreclosed. But since the robbery results are arguably weak, it is hard to tell a convincing story that would explain the alleged shift from violent crime to property crime that the Lott model attributes to shall issue laws" (Ian Ayres and John J. Donohue III, "Nondiscretionary Concealed Weapons Laws: A Case Study of Statistics, Standards of Proof, and Public Policy," *American Law and Economics Review* 1, nos. 1–2 (Fall 1999): 436–70.

82. The "recidivism" referred to by Ayres and Donohue is actually not a good measure for what they are discussing, since recidivism refers to whether criminals keep on committing a crime after they have been punished by the legal system.

83. Ayres and Donohue raise another issue that should be discussed at least briefly, and that is the use of the percentage of a state's population that is in prison as an enforcement variable. They find that including this variable strengthens the results, but while the variable provides some information, there are some important theoretical problems with it. One problem is that the prison population and the crime rate are simply in different units. The prison population measures a "stock," while the crime rate represents a "flow." The simplest comparison is between the amount of water in a bathtub (a stock) and the rate at which water is flowing into the bathtub (a flow). The amount of water in the bathtub is only loosely related to the current flow into it because it depends upon not only flows in previous periods but also the rate at which water is flowing out of it. A second problem is that I have focused on county-level data because of the heterogeneity in law enforcement across counties within a state, and this variable is available only at the state level.

84. For example, Sarah Brady, "Q: Would New Requirements for Gun Buyers Save lives? Yes: Stop Deadly, Unregulated Sales to Minors, at Gun Shows and on the Internet," *Insight,* June 21, 1999, p. 24; or "More Guns, Less Crime? A Debate between John Lott and Douglas Weil."

85. Gary Kleck, *Targeting Guns: Firearms and Their Control* (Hawthorne, NY: Aldine de Gruyter Publishers, 1997), p. 371.

86. This is true whether one uses the 430,000 instances in 1997 in which crimes with guns were reported to police in the *Uniform Crime Report* or the number that is about twice as large from the National Crime Victimization Survey.

87. Frank Main, "Taxpayers Pay Big Part of Gunshot Victims' Bills," *Chicago Sun-Times,* Aug. 4, 1999, p. 30.

88. Another survey by gun-control advocates claims that "four million legal handgun owners sometimes carried guns for protection 'in connection with work.' Two-thirds of those who carried handguns said they kept them in their vehicles, while the others said they sometimes carried them. . . . The researchers said about 56 percent of those who carried handguns outside of work did so fewer than 30 days per year, while 22 percent said they rarely left home without a gun" (Will Hacker, "Majority of Owners Cite Security Concerns," *South Bend Tribune,* June 29, 1997, p. A6).

89. Janelle Hartman, "Assailant Gets Shot by Victim," *Eugene (OR) Register Guard,* Mar. 11, 1998, p. 1.

90. Nicole Marshall, "Concealed Gun Carrier Subdues Suspect: Man Reportedly Had Snatched Toddler," *Tulsa World,* Jan. 31, 1998.

91. Frank J. Murray, "Arizona Gun Owner's Courage Led to Scary Arrests," *Washington Times,* May 2, 1999, p. C8.

92. Laurie Mason, "Customer Stops Would-Be Robber," *Bucks County Courier Times,* Dec. 13, 1998, p. 7C.

93. Edward W. Lempinen, "Robber Shot Dead," *Newsday,* Aug. 3, 1999, p. A3; "Concealed Carry Permit Pays Off," *Local Cincinnati-Northern Kentucky TV 9 Evening News,* Aug. 19, 1999; Tom Jackman and Maria Glod, "A Glimmer of Hope, Then Violent Death," *Washington Post,* June 21, 1999, p. B1; "Carjacking Suspect Critically Wounded," *Arizona Republic,* June 5, 1999, p. B2; and Joe Brogan, "Rent Collector Shoots, Kills Riviera Robber," *Palm Beach Post,* Jan. 14, 1999, p. B1.

A case from the end of 1998 that deserves some mention involved an eighty-one-year-old Chicago native who defended himself by illegally carrying a concealed handgun—a gun that he wasn't even allowed to own legally in Chicago, let alone carry with him.

> In the pre-dawn hours Tuesday, 81-year-old Bruno Kosinski looked like an easy mark for a robbery. Kosinski, a frail man with thinning white hair who shuffles his feet as he walks slightly hunched over, was getting into his car in Ukrainian Village when he felt something wet on his head. In a few brief moments, two teenagers allegedly squirted pepper spray in his face, pushed him to the ground, took his wallet and, still unsatisfied, threatened to kill him, police said. Kosinski did something authorities said was rare: The 5-foot-5 elderly man used a concealed handgun he carries in his pants. Without saying a word, he got to his feet and fired once. . . . Kosinski, admitting he illegally carried a concealed handgun, was unapologetic. "I don't feel at all sorry that it happened," said Kosinski. . . ."The least that I could do was defend myself." (Bechetta Jackson and Todd Lightly, "Aged Hold-Up Victim Shoots Teen Suspect," *Chicago Tribune,* Dec. 8, 1998, sec. 2, p. 8)

94. Mene Tekel Upharsin, "Homeowner Grabs Gun for Self-Defense, Assists Police in Capture of Escaped Murderer," *Associated Press Newswire,* Aug. 21, 1999, 8:37 EDT. What would have become a multiple-victim public shooting at a business in July 1999 was stopped by a person with a concealed handgun ("Gunman Turns Weapon on Gun Store Employees,

Is Wounded in Shootout," *St. Louis Post-Dispatch* (from Reuters), July 7, 1999, p. A11; and see also Thomas Sowell, "Why Does Media Hide Benefits of Arming Citizens?" *Bergen County (NJ) Record,* July 19, 1999, p. L3). Typical is the story of a Greenville, North Carolina, restaurant owner who prevented a robbery with a gun that he carried with him all the time "in the small of his back" (Travis Fain, "Man Shot, Killed in Attempted Robbery," *Daily Reflector,* June 2, 1999, p. B1). Other articles on prevented robberies from June 1999 can be found in the *Providence Journal,* June 18, 1999; the *Prescott (AZ) Daily Courier,* June 13, 1999; the *Augusta (GA) Chronicle,* June 16, 1999; and the *Aiken (SC) Standard,* June 2, 1999.

95. "Man Charged in Robbery Hurt in Intimidation Bid," *Buffalo News,* Jan. 16, 1999; "Cops: Woman Shot by Man She Tried to Rob," *Orlando Sentinel* (from Associated Press), Jan. 10, 1999, p. B4; Seth Muller, "Homeowner Fires .357 at Night Burglar," *Martinsburg (WV) Journal,* Mar. 2, 1999; Valerie Bauertein, "Woman Kills an Intruder in Her Home," *Winston-Salem Journal,* Mar. 2, 1999, p. A1; "Woman Shoots Golf Stalker to Death during Attack," *Palm Beach Post* (from Associated Press), May 12, 1999, p. All; Kirk Swauger, "Shooter's Brother: Break-in Not First," *Johnstown (PA) Tribune-Democrat,* Apr. 10, 1999, p. Al; Bill Blair, "West End Man Slays Intruder," *Johnstown (PA) Tribune-Democrat,* Apr. 9, 1999, p. Al; Bill Hanna, "Robbery Victim Shoots Suspect," *Fort Worth Star-Telegram,* June 15, 1999; Mark Duncan, "Hall of Fame Cowboy Stems Tragedy at Ranch: Family Survives Knife Attack," *Yavapai County (AZ) Daily Courier,* June 13,1999, p. Al; Dan Richardson, "Armed Homeowner Drives Off Intruder," *Valley News (VT),* July 2,1999; Heather Romero, "Intruder Is Wounded As Shots Fly 'All Over,'" *Arizona Daily Star,* July 3, 1999, p 1B; Beena A. Hyatt, "Intruder Is Killed in Home," *Chattanooga Times,* July 22, 1999, p. Bl; Kate Folmar and Luise Roug, "Late-Night Intruder Gets More Than He Bargained For," *Los Angeles Times,* Aug. 2, 1999, Orange County edition, p. Bl; and "Homeowner Shoots Man Climbing into Window," *Knoxville (TN) Knox-News Sentinel,* Aug. 1, 1999.

96. While I find the claims greatly exaggerated, another recent study has come out claiming that sixty-four children under the age of two die every year from sleeping with their parents (Shari Roan, "Baby's First Year: Dangerous to Doze with Baby Alongside?" *Los Angeles Times,* Oct. 4, 1999, p. S1).

97. Brenda Rodriguez, "Notes Begin to Tell Story of Rampage in Atlanta: Killer Wanted to Exact Revenge," *Dallas Morning News,* July 31, 1999, p. 1A.

98. Rhonda Cook, "To the Rescue: Salesman Grabs Gun, Prevents Tragedy," *Atlanta Journal and Constitution,* Aug. 3, 1999, 1B; Lyda Longa and David Pendered, "Armed Patient Shot in Grady," *Atlanta Journal and Constitution,* Aug. 4, 1999, p. 1B; Hannity and Colmes, Fox News Network, Aug. 11, 1999 (21:30 EST); and "Armed Georgia Defenders Thwart Two Gunmen," *Washington Times* (from Reuters), Aug. 4, 1999, p. A9.

99. Carol Robinson, "Gunman Opens Fire at Alabama Business," *New Orleans Times-Picayune,* Aug. 6, 1999 p. A17.

100. Editorial, "Lethal Weapon," *Daily News of Los Angeles,* May 6, 1999, p. N20.

101. Elaine Gale, "Grieving Mother Haunted by Crash Scene," *Los Angeles Times,* May 6, 1999, p. Al.

102. A Nexis search of news stories for the one week after both incidents indicates that Buford Furrow was mentioned in the news about five times as often as Steve Abrams, and that while news accounts of Furrow tended to be full-feature news stories, virtually all of the mentions of Mr. Abrams were fairly minor recounts of the Associated Press story that ran on him. Later in the day it was discovered that Furrow had killed a U.S. Post Office worker, but the initial news coverage was based upon the attack at the community center.

Other writers have done an excellent job of pointing out these biases (Sowell, "Why Does Media Hide Benefits of Arming Citizens?" p. L3; Jeff Jacoby, "Media Bias Revealed by Crimes That Go Unnoticed," *San Jose Mercury News,* Aug. 24, 1999).

103. This total includes 427 incendiary bombings. Eleven deaths and 29 injuries were classified as "noncriminal" (Bureau of Alcohol, Tobacco, and Firearms, "Arson and Explosives: Incidents Report, 1997" [Department of the Treasury, 1999]). For an example of a recent

knife attack that injured several people on an Amtrak train see Editorial, "Speak Up," *Dayton Daily News,* Sept. 2, 1999.

104. For example, CNN's *Late Edition with Wolf Blitzer,* May 2, 1999, 12:00 A.M. EST.

105. Tom Diaz, *Making a Killing: The Business of Guns in America* (New York: New Press, 1999).

106. Ruth Teichroeb, "Hearing Today for Boy Expelled over Squirt Gun," *Seattle Post-Intelligencer,* Sept. 22, 1998, p. B1; Mike Martindale, "OU Acts after Police Take Youth into Custody after Call," *Detroit News,* Aug. 13, 1999; Pete Falcone, "Student Expelled for Toting BB Gun," *Bloomington (IL) Pantagraph,* May 27, 1999, p. A2; Cathy Cummins, "Expulsion Law's Author Says Schools Have Gone Too Far," *Rocky Mountain News,* Feb. 23, 1998, p. A4; and "Howitzer Picture Cut From Yearbook," *Associated Press Newswire,* Oct. 28, 1999, 3:16 EDT. These different incidents were said to violate schools' "zero tolerance" policy.

107. This information on the number of words in different gun-control laws was compiled by Alan Korwin (Alan Korwin, *The California Gun Owner's Guide* [Phoenix: Bloomfield Press, 1999], *The Texas Gun Owner's Guide* [Phoenix: Bloomfield Press, 1998]; and *Gun Laws of America* [Phoenix: Bloomfield Press, 1997]).

108. Terry L. Anderson, Charles W. Baird, Randy E. Barnett, et al. [letter signed by 290 academics], "Disarming Good People," *Washington Times,* June 16, 1999, p. A17. The correct number of 294 signatories was noted in John R. Lott, Jr., "More Gun Controls? They Haven't Worked in the Past," *Wall Street Journal,* June 17, 1999, p. A26.

109. Dale Anema, "A Father at Columbine High," *American Enterprise,* Sept./Oct. 1999, pp. 48–50.

CHAPTER TEN

1. Matt Bai, "The Gun Crowd's Guru," *Newsweek*, Mar. 12, 2001.

2. John Donohue, "The Final Bullet in the Body of the More Guns, Less Crime Hypothesis," *Criminology and Public Policy* 2, no. 3 (2003): 397–410. In an interview for the *Wichita Eagle* (Kansas), Donohue would claim in 2003, "It borders on fraud for anyone to try to make the case that there is a drop in crime," and "Lott's earlier work failed to account for the peak and subsequent decline in violent crime related to the advent of crack cocaine in the 1980s." *Wichita Eagle*, Oct. 12, 2003.

3. John J. Donohue, "Can You Believe Econometric Evaluations of Law, Policy, and Medicine," address given at the University of Virginia Law School, Oct. 24, 2008. The paper was said to contain a discussion that Donohue has in a forthcoming book.

4. See the section "Fewer Guns, More Crime" for a detailed discussion of claims by Steven Levitt regarding these claims.

5. Jordy Yager, "New Rule Prompts Fears of Guns at Inauguration," *Hill*, Dec. 27, 2008 (http://thehill.com/leading-the-news/new-rule-prompts-fears-of-guns-at-inauguration-2008-12-27.html).

6. Rep. Rob Bishop, "Telling the Second Amendment to Take a Hike," *Human Events*, Jan. 15, 2009 (http://www.humanevents.com/article.php?id=30284&page=1).

7. There is some debate about whether Iowa should be classified as a right-to-carry state or as a may-issue state, but for the empirical work in this book it isn't relevant, because it did not change its law during the period that I have examined since 1977. For convenience, I will classify Iowa as being more restrictive than some do and classify it as a may-issue state.

8. Still, even if Alabama's concealed-permit rate for the entire state is as low as the lowest urban county for which numbers are readily available (and urban counties tend to have lower rates of permits than rural areas), it would mean that more than 300,000 people in Alabama have permits—possibly the highest rate of any state in the country.

9. Information obtained from State and Local Affairs Department for the National Rifle Association's Institute for Legislative Action (July 28, 2008) and confirmed again on March 3, 2009.

10. "Well-Armed Lawmakers," *State* (Columbia, SC), Mar. 8, 2008 (http://www.feedzilla.com/news-archive/usa/2008-03-08-columbia.html).

11. Andy Sher, "1 in 4 State Legislators Holds Gun Permit," *Chattanooga Times Free Press*, Mar. 1, 2009 (http://timesfreepress.com/news/2009/mar/01/1-4-state-legislators-holds-gun-permit/).

12. Joanne Kimberlin, "Tech Massacre Only Heated Up the Gun Debate in Virginia," *Virginian-Pilot*, Mar. 2, 2008 (http://hamptonroads.com/2008/03/tech-massacre-only-heated-gun-debate-virginia).

13. Phillip Morris, "Run-In Changes Lawmaker's Stance," *Cleveland Plain Dealer*, May 15, 2007, p. B1.

14. Richard Stenger, "Armed Pilots Offer Added Protection, New Danger," CNN.com, Sept. 26, 2001.

15. John R. Lott, Jr., "P.C. Air Security: When Will Our Pilots Be Armed?" *National Review Online*, Sept. 2, 2003.

16. I worked with several of the pilots' unions over helping let pilots again carry guns. I did not accept any payments from them for the work that I did.

17. Serena Parker, "Arming Airline Pilots," *Voice of America News*, Sept. 4, 2003. See also John R. Lott, Jr., "Marshals Are Good, but Armed Pilots Are Better," *Wall Street Journal Europe*, Jan. 2, 2004 (http://online.wsj.com/article/SB107299581523057500.html?mod=opinion%255Feurope%255Fcommentaries).

18. Based on conversations with Tracy Price and Bob Lambert with the Airline Pilots Security Alliance and union representatives from Southwest and American Airlines.

19. Based on conversations both with pilots' union officials and with officials from the Transportation Security Administration during 2002 and 2003.

20. John R. Lott, Jr., "Arming of Pilots Is Way Overdue," *Los Angeles Times*, Apr. 14, 2003.

21. Ben Dubose, "Pilot's Gun Goes Off on US Airways Flight," *Los Angeles Times*, Tuesday, Mar. 25, 2008.

22. See a press release from the Airline Pilots Security Alliance, "Blame Shifts to TSA in Pilot's Gun Mishap," Mar. 27, 2008 (http://secure-skies.org/doc/PRESS%20RELEASE%20-%20BLAME%20SHIFTS%20TO%20TSA%20IN%20PILOTS%20GUN%20MISHAP.pdf).

23. For example, John Donohue debating at the Contemporary Club in Charlottesville, Virginia, on October 22, 2008. After I described the gun going off in the cockpit, Donohue said: "John made the exact point that I made, I said that the gun went off in the cockpit and basically John [Lott]'s attitude, the NRA attitude, is sort of like NASA's view when they are bringing down the spaceships. They crash them into the earth and most of the time they don't hit anybody because there is a lot of ocean there." Quote from recording of the debate.

24. John R. Lott, Jr., "Letting Teachers Pack Guns Will Make America's Schools Safer," *Los Angeles Times*, July 13, 2003.

25. While I will discuss some of the problems with the Brady Campaign listing of incidents with permit holders, it is still interesting to note that their list does not contain a single example involving a school. See "CCW License Holders: 'Law-Abiding Citizens?'" on the Brady Campaign Web site, accessed Jan. 10, 2009 (http://www.bradycampaign.org/facts/research/?page=incident&menu=gvr). My own extensive research as well as calls to the NEA and the AFT confirm this.

26. "Do Guns Reduce Crime?" debate on National Public Radio, Nov. 5, 2008 (broadcast available at http://www.npr.org/templates/story/story.php?storyId=96409853).

27. Ibid.

28. I also talked with Philip Ward, the assistant managing editor at the *Sun-Sentinel*. When I noted that none of the stories by O'Matz and Maines had any examples of permit holders committing crimes in their stories he said: "Well, I would say that answers your question." See also Megan O'Matz and John Maines, "Investigation Reveals Criminal Pasts of

Those Toting Guns," *South Florida Sun-Sentinel*, Jan. 28, 2007. Megan O'Matz can be reached at momatz@sun-sentinel.com and John Maines at jmaines@sun-sentinel.com.

29. This is based on a series of e-mail exchanges during December 2008 and January 2009 and telephone conversations with Sally Kestin and Philip Ward on February 17, 2009.

30. "Texas Concealed Handgun Law and Selected Statutes, 2007–2008," Texas Department of Public Safety, Austin, Oct. 2007 (http://www.txdps.state.tx.us/ftp/forms/LS-16.pdf).

31. Katie Brown, "Permit Changes Opposed: Blind Man Who Passed Defends Tests," *Bismarck Tribune*, Nov. 25, 2005.

32. Information obtained from Donna J. Street, administrative supervisor, Arizona Department of Public Safety, Concealed Weapons Permit Unit, P.O. Box 6488, Phoenix, AZ 85005; phone: 602-223-2704.

33. Jaime Holguin, "Arizona Gunman Had Threatened School," CBSNews.com, Oct. 29, 2002 (http://www.cbsnews.com/stories/2002/10/30/national/main527553.shtml).

34. Division of Licensing, Florida Department of Agriculture and Consumer Services, "Concealed Weapon/Firearm Summary Report, October 1, 1987–November 30, 2008" (http://licgweb.doacs.state.fl.us/stats/cw_monthly.html).

35. When asked about the July 1997 murder case involving Joseph Corcoran, Berkey said that case had been improperly reported as his having a permitted concealed handgun.

36. Jim Hannah, "Shooting Stirs Vigilante Justice Fears," *Cincinnati Enquirer*, Jan. 7, 2002, p. B1.

37. Information on the number of active permits is available from Dawson Bell, "Michigan Sees Fewer Gun Deaths—with More Permits," *Detroit Free Press*, Jan. 6, 2008, p. A1. Michigan State Police, "Concealed Pistol Licensure Annual Report, July 1, 2006 to June 30, 2007" (http://www.michigan.gov/documents/msp/CCW_Annual_Report_2006-2007_228850_7.pdf).

38. See the various issues of the Concealed Pistol Licensure Annual Report for Michigan that are available at http://www.michigan.gov/msp/0,1607,7-123-1591_3503_4654-77621-,00.html.

39. This information was supplied by Matt Connor with the Missouri Department of Revenue, and it covers the year up until December 11, 2008. Seventeen permits were revoked due to a protection order issued by a court, 78 were revoked due to disqualifying convictions, and one is just listed as being revoked by a court. In addition, two permits were suspended.

40. Ted Richardson, Montana Department of Justice, 406-444-2800. I talked to him during December 2008.

41. All quotes here are from a telephone conversation that I had with Rosemary Ruby during January 2009.

42. "North Carolina Concealed Handgun Permit Statistics by County, 12/01/1995 thru 12/31/2008" (http://sbi2.jus.state.nc.us/crp/public/other/conceal/Dec%2031_2008%20Data.pdf).

43. All quotes here are from a telephone conversation that I had with Tamara Road during January 2009.

44. For example, in Johnston County, the information was provided to me by Angie Butts, an administrative assistant.

45. John Futty, "Few Concealed-Carry Permits Revoked, Records Show," *Columbus Dispatch*, Aug. 13, 2006 (http://www.dispatch.com/live/contentbe/dispatch/2006/08/13/20060813-C6-00.html).

46. Attorney General of the State of Ohio, "Ohio's Concealed Handgun Law: 2008 Report to the Governor and General Assembly," Mar. 1, 2008 (http://www.ag.state.oh.us/le/prevention/concealcarry/docs/07_cc_annual_rpt.pdf).

47. "Gun Law Marks Its First Year," *Cincinnati Post*, Apr. 9, 2005, p. A1.

48. Pennsylvania State Police. "Firearm Record Information by County for 2007."

49. Pennsylvania State Police, "Firearm Record Information by County" for 2003 to 2006.

50. Conversations with Pennsylvania State Police Public Information Office press secretary Jack Lewis and Lt. Annette Quinn, Jan. 16, 2009.

51. Pennsylvania State Polic, "Firearm Record Information by County for 2007."

52. Conversations with Kim Stolfer, chairman of Firearm Owners Against Crime, Jan. 19, 2009.

53. Telephone conversation with Stolfer, Jan. 19, 2009.

54. In 2006, there were 61,539 convictions for misdemeanors and felonies among those without a concealed-handgun permit ("Conviction Rates for Concealed Handgun License Holders, Reporting Period: 01/01/2006–12/31/2006," Texas Department of Public Safety, http://www.txdps.state.tx.us/administration/crime_records/chl/ConvictionRatesReport2006.pdf). The adult population was 16,925,604.

55. This information is available in "Conviction Rates for Concealed Handgun License Holders, Reporting Period: 01/01/2006–12/31/2006," Texas Department of Public Safety, http://www.txdps.state.tx.us/administration/crime_records/chl/ConvictionRatesReport2006.pdf; "Conviction Rates for Concealed Handgun License Holders, Reporting Period: 01/01/2005–12/31/2005," Texas Department of Public Safety, http://www.txdps.state.tx.us/administration/crime_records/chl/ConvictionRatesReport2005.pdf; "Conviction Rates for Concealed Handgun License Holders, Reporting Period: 01/01/2004–12/31/2004," Texas Department of Public Safety, http://www.txdps.state.tx.us/administration/crime_records/chl/ConvictionRatesReport2004.pdf; "Conviction Rates for Concealed Handgun License Holders, Reporting Period: 01/01/2003–12/31/2003," Texas Department of Public Safety, http://www.txdps.state.tx.us/administration/crime_records/chl/ConvictionRatesReport2003.pdf; and "Conviction Rates for Concealed Handgun License Holders, Reporting Period: 01/01/2002–12/31/2002," Texas Department of Public Safety, http://www.txdps.state.tx.us/administration/crime_records/chl/ConvictionRatesReport2002.pdf.

56. Over the four years from 2005 to 2008, the rate at which new permits were denied was 1.3 percent. The reasons given for not issuing a permit include an alcohol violation, a protective order, that the person was wanted, a firearm offense, being a threat to oneself or another, domestic violence, moral turpitude, abuse of a controlled substance, conviction of a felony, and other. Thirty-eight percent of the denials were due to alcohol violations. Fourteen percent were due to moral turpitude. Thirteen percent were due to felonies.

One quote on the issuance of concealed-handgun permits in Utah has gotten some attention. A March 22, 2002, article in the *Salt Lake Tribune* quotes Joyce Carter, the then supervisor of the Bureau of Criminal Identification firearms section, "It's an extremely small percentage that have been revoked. But they're not all straight, law-abiding citizens. . . . I would hazard a fairly educated guess that better than 50 percent of the applicants have a criminal history of some kind." But there seems to be some debate whether the reporter accurately quoted Carter. Regarding the 50 percent claim, Carter told me on January 6, 2009, that "I am going to say that is wrong. I don't remember making that statement or even where or when I would have made it, but I have heard others quote it back to me." Nicole Starks, who replaced Carter as supervisor, said that "I don't think that it is 50 percent." There is at least one other mistake in the story. Clark Aposhian said that the reporter also confused arrests for murder with convictions. The article claimed that four permit holders had committed murder, and the actual number was two. See Dan Harrie, "Crimes Trigger Revocation of 584 Concealed-Weapon Permits," *Salt Lake Tribune*, Mar. 25, 2002, p. A1.

57. The officer had gotten a concealed-handgun permit so that he didn't have to go through the background checks when he bought a gun. Information provided by Clark Aposhian.

58. Chris Lynch, records analyst, Wyoming Division of Criminal Investigations, clynch@dci.wyo.gov.

59. I did talk to some county offices in Colorado. In 2006, 95 revocations occurred for arrest, but there is no available information on how many of those revocations resulted in conviction. Revocations for arrest account for about 0.35 percent of active permits. The counties that I contacted were very helpful but were unable to provide me with information on convictions. Laurie Thomas, the concealed-handgun permit coordinator for El Paso County, told me that they didn't follow up to see if the arrests resulted in conviction, though she did say that none of the 24 revocations in 2006 resulted in those permit holders applying for their permits to be reinstated. Two of the 24 revocations involved prohibited use of a firearm, but Ms. Thomas told me that those types of cases usually "simply involve, I say simply because it doesn't involve the use of the gun, being drunk or intoxicated . . . while in possession of a firearm." However, she was not sure of the actual facts of those two cases. Ms. Thomas was available at 719-520-7249. See also http://www.rmgo.org/faq/CCW%20Permits%20by%20county.pdf.

60. Interestingly, Texas has the most murders by permit holders, and it also has one of the strictest concealed handgun laws—having the longest training and retraining requirements, among the highest fees for permits, and strictest restrictions on who can get permits. The Texas number is obtained by comparing Texas Department of Public Safety numbers with news reports to get more detail on each case, though this might overestimate the total because cases in the DPS conviction numbers which could not be confirmed as involving guns were assumed to involve guns. The numbers were based on calls to those state agencies that collect permit records as well as their annual reports on permit holders discussed earlier, reports from the Brady Campaign and the Violence Policy Center, and news reports. "Signature Grand Shooting an Apparent Murder-Suicide," NBC 6, Miami, FL, Apr. 29, 2003 (http://www.nbc6.net/news/2166227/detail.html); "South Florida Couple, Dog Dead in Apparent Murder-Suicide," *Orlando News*, Jan. 12, 2004 (http://www.clickorlando.com/news/2758332/detail.html); Louis Mockewich in Philadelphia (Associated Press, "Man Convicted of Shooting Neighbor over Snow Shoveling," Jan. 27, 2000); Jamie Cokes in Pittsburgh ("Local News, Homicide Conviction," *Pittsburgh Post-Gazette*, Nov. 8, 2002, p. B4); William Manies in Knoxville, Tennessee (Don Jacobs, "Judge Retains Gun Permit despite State Law; Statute Relies on Applicants' Honesty," *Knoxville News-Sentinel*, Oct. 1, 2000, p. B1; the crime occurred in December 1999); Shirley Henson in Alabama (Jay Reeves, "Woman Sentenced to 13 Years for 'Road Rage' Killing," Associated Press, Dec. 4, 2000; the crime occurred in November 1999); Scott Stone in North Carolina (Betsy Blaney, "Custody Loss Prompts Separation Violence," *Chicago Tribune*, Nov. 7, 1999, p. N8; the murder actually took place in Texas); John Corcoran in Indiana (Niki Kelly, "Corcoran Execution Is July 21; Killed 4 Men inside City House in '97; Federal Appeal Planned," *Fort Wayne (IN) Journal Gazette*, June 16, 2005, p. 1C); Geraldine Beasley in Ohio was convicted of voluntary manslaughter (Associated Press, "Woman Accused of Shooting Beggar Had Her Concealed Weapon Permit," *Bryan Times,* Aug. 13, 2007); Terrance Hough, Jr., in Ohio was convicted of murder (Associated Press, "Firefighter Charged in 3 Killings at Party," *Columbus Dispatch,* July 7, 2007); David Ragsdale in Utah (Janice Peterson, "David Ragsdale Gets 20 Years to Life in Prison in Wife's Murder," *Daily Herald,* Jan. 30, 2009); Jason K. Hamilton in Maine was involved in a murder-suicide (Taryn Brodwater, Bill Morlin, and Amy Cannata, "Shooter Linked to Ayrans," *Spokesman-Review,* May 23, 2007); Aaron P. Jackson in Virginia was involved in a murder-suicide (Nick Miroff, "Four Dead in Murder, Suicide in Va.," *Washington Post,* May 7, 2008); Ashford Thompson killed Joshua Miktarian in Twinsburg Township, Ohio, in July (Macollvie Jean-Francois, Brian Haas, Andrew Tran, and Rachel Hatzipangos, "Police Report: Federal Agent's Death Due to Road Rage," *South Florida Sun-Sentinel*, Aug. 7, 2008; and Karen Farkas, "Man Indicted in Killing," *Cleveland Plain Dealer*, July 22, 2008, p. B2).

As this book was going to press, the Violence Policy Center released a report entitled "CCW Total Killed," claiming that from May 2007 through December 2009, permit holders had killed 107 people. Thirty-six of the sixty-two cases involved instances where someone

was charged and not convicted of murder. Take Reginald Royal's case, where the media report, "Royals saw the victim had a gun in his pocket. He saw it because he displayed it" and that he was confronted by four men who threatened him (Michelle Washington, "Waterside Shooting Was Self-Defense, Accused Man Says," *Virginian-Pilot,* March 29, 2009). Other cases included instances where there was no evidence that a permit holder had committed the crime (indeed, the police did not know who had committed it), cases that never made it to court because they obviously involved self-defense (e.g., the case of Charles Podany, who obviously used his gun to protect himself from a man who was beating him; Jessica Vander Velde, "Man Shot after Speeding Confrontation," *St. Petersburg Times,* March 2, 2008), and cases where no prison time was imposed even when there was a conviction (e.g., Willie Donaldson's case, which involved self-defense). Even a quick examination indicates that many of these cases are exactly the self-defense type of cases that legislators wanted to let occur. Instead of being counted as a cost of right-to-carry laws, they should be counted as a benefit.

61. Associated Press, "Jury Sent Home, to Return and Continue Deliberations," Associated Press Newswire, Sept. 28, 1999.

62. Associated Press, "Man Killed After Argument," Associated Press Newswire, Feb. 26, 1999.

63. Associated Press, "Wallace Found Guilty of Negligent Homicide," Associated Press Newswire, Sept. 29, 1999.

64. Associated Press, "Man Killed After Argument."

65. Associated Press, "Jury Sent Home, to Return and Continue Deliberations"; Associated Press, "Wallace Found Guilty of Negligent Homicide."

66. Deon Hampton, "Charge Filed in Road-Rage Homicide: A Former Security Guard Who Said He Feared for His Life Was Not Justified in Shooting Another Man, the D.A. Says," *Tulsa World*, Sept. 27, 2007.

67. Bill Braun, "Plea Deal Reached in Fatal Shooting," *Tulsa World*, May 30, 2008, A1.

68. Mike Glenn, "Longshoreman Charged in Fatal Eastside Shooting," *Houston Chronicle*, Apr. 25, 2006, p. B3.

69. Identity of person engaging in the attack based on discussion with Brian Rogers of the *Houston Chronicle*.

70. Brian Rogers, "Convicted Killer Ordered to Serve 10 Years' Probation; Attorney Says Man Was Defending Himself at the Time of Fatal Shooting," *Houston Chronicle*, July 22, 2008.

71. See "Comments Opposing Proposed Rule to Allow the Carrying of Loaded, Hidden Firearms in National Park Areas and National Wildlife Refuges" from the Brady Campaign Web site, accessed Jan. 10, 2009 (http://www.bradycampaign.org/xshare/pdf/fedleg/national-park-comments.pdf).

72. Associated Press, "Public-Safety Department Lists 23 Concealed-Gun Incidents; House Panel Tracking Law's Effects, Enforcement," *Dallas Morning News*, July 17, 1996.

73. Ralph Winingham, "Although Cleared of Murder Charge, Memories Remain," *San Antonio Express-News*, Sept. 7, 1997, p. 29A.

74. "CCW License Holders: 'Law-Abiding Citizens?'" on the Brady Campaign Web site, accessed Jan. 10, 2009 (http://www.bradycampaign.org/facts/research/?page=incident&menu=gvr).

75. Bill Braun, "Man Cleared in Fatal Shooting: Slaying Case Is First Involving a Legally Concealed Weapon," *Tulsa World*, Feb. 12, 1999, p. 17.

76. William C. Rempel and Lianne Hart, "Murder Case Puts New Focus on Texas' Concealed Gun Law," *Los Angeles Times*, Nov. 2, 2000, p. A5.

77. Both the Brady Campaign and the Violence Policy Center have tracked cases—the Brady Campaign up through the beginning of 2007 and the Violence Policy Center up through the end of 1998. Their Web sites were last checked on January 10, 2009. The Brady Campaign's list of crimes by permit holder is at http://www.bradycampaign.org/xshare/

pdf/reports/no-gun-left-behind.pdf and http://www.bradycampaign.org/facts/research/?page=incident&menu=gvr. The list for the Violence Policy Center is available at http://www.vpc.org/Studies/ltk2cont.htm.

78. In 2007, that equals 0.017 murders per 100,000 permit holders. By contrast, for the United States has a whole, 4.47 adults were arrested for committing murder out of every 100,000 adults. "Expanded Homicide Data Table 3," in "Crime in the United States 2007," FBI *Uniform Crime Reports* (http://www.fbi.gov/ucr/cius2007/offenses/expanded_information/data/shrtable_03.html). These calculations assume an adult population in the United States of 227 million in 2007.

79. The other cases are these:

Thibodaux, Louisiana (Jan. 1, 2009)—"Police said a city man was shot and wounded Thursday night while trying to rob another at gunpoint near a Canal Boulevard convenience store. . . . No charges have been filed against [the armed victim]" (John DeSantis, "Police Say Victim Shot Robbery Suspect," *Thibodaux Daily Comment*, Jan. 2, 2009; http://www.dailycomet.com/article/20090102/ARTICLES/901029977/1212?Title=Police_say_victim_shot_robbery_suspect).

Anniston, Alabama (Dec. 24, 2008)—"A would-be robber was shot dead overnight Wednesday and his alleged accomplice was wounded after they attempted to commit a robbery at a gas station near Anniston. Calhoun County Sheriff Larry Amerson said . . . he acted in self-defense" (Associated Press, "Robbery Suspect Shot Dead, Alleged Accomplice Wounded," *Huntsville Times*, Dec. 25, 2008; http://blog.al.com/breaking/2008/12/robbery_suspect_shot_dead_alle.html).

Orlando, Florida (Dec. 23, 2008)—Three men attempted to rob a woman in a mall parking lot. Two of the men were wrestling with the woman. A permit holder shot one of the suspects in the leg ("Police: Bystander Shoots Robber in Mall Parking Lot," WESH.com, Thursday, Dec. 23, 2008; http://www.msnbc.msn.com/id/28380721/).

80. "Police: Man Shoots, Kills Suspect in Hammond Robbery Attempt," *Northwest Indiana Times*, Jan. 12, 2009; http://nwitimes.com/articles/2009/01/12/news/top_news/docce29b62cf028c9ef8625753c00081c82.txt).

81. In this second follow-up story they mention that the individual who fired his gun defensively had a concealed-handgun permit. "Police: Statements Support Self Defense Claim in Fatal Shooting," *Northwest Indiana Times*, Jan. 12, 2009; http://www.thetimesonline.com/articles/2009/01/12/updates/breaking_news/doc496ba228600bb151954649.txt).

82. "Victim Kills Robbery Suspect at Car Wash," WFTV.com, Jan. 9, 2009 (http://www.wftv.com/news/18451573/detail.html).

83. "Police: Man Shoots, Kills Armed Robber at Car Wash," WESH.com (Orlando), Jan. 10, 2009 (http://www.wesh.com/news/18451432/detail.html).

84. "2 Men Sought After Customer Saves Clerk from Robber," WESH.com (Orlando), Jan. 7, 2009 (http://www.msnbc.msn.com/id/28518530/).

85. "West Park Homeowner Kills Robber," *Florida Today*, Dec. 26, 2008 (http://www.floridatoday.com/article/20081226/BREAKINGNEWS/81226044/1006/NEWS01).

86. "Dog Walker Kills Armed Teen Robber, Police Say," MyFoxDFW, Dec. 18, 2008 (http://www.myfoxdfw.com/myfox/pages/News/Detail?contentId=8100189&version=5&locale=EN-US&layoutCode=TSTY&pageId=3.2.1).

87. Jared Broyles, "Concealed Carry Permit Comes in Handy for Woman in Fort Smith," KFSM.com, Channel 5 News, Dec. 18, 2008 (http://www.kfsm.com/Global/story.asp?S=9541680).

88. Minnesota's permit system was prevented from issuing more permits from July 2004 to June 2005.

89. The means and standard deviations for the state-level data are shown in appendix table 10A.1.

90. Change.gov, the Office of the President-Elect, under Agenda, under Urban Policy,

under the headline "The Obama-Biden Plan" (http://change.gov/agenda/urbanpolicy_agenda/).

91. One could add up the list of places where permitted concealed handguns are prohibited, and I have done that in some of my research, but it is also somewhat arbitrary, because it assumes that all prohibited places are equally important to permit holders. Grambsch is a public health researcher who estimates that later adoption of right-to-carry laws gives states reduced benefits, but despite my suggestion to her, there is no attempt to account for later states having more restrictive laws. Patricia Grambsch, "Regression to the Mean, Murder Rates, and Shall-Issue Laws," *American Statistician* 62, no. 4 (2008): 289–95.

92. "Bill Would Allow Prosecutors Guns in Court," KCTV Channel 5 News, Jan. 23, 2009 (http://www.kctv5.com/news/18546809/detail.html). Gorman isn't alone. Reno County prosecutor Thomas Stanton says, "We act to insure those who violate the laws of Kansas receive consequences for their actions. This can lead to prosecutors being targets of violent acts perpetrated by defendants, their families and their friends." Stanton said that he would carry a concealed firearm, but the prohibitions on where he can carry a gun makes it impracticable. Tim Carpenter, "Concealed-Carry Bill Would Let Prosecutors Be Armed in Court," *Topeka Capital-Journal*, Jan. 21, 2009 (http://cjonline.com/stories/012109/sta_379727801.shtml).

93. Nancy Badertscher, "Georgia Law: Guns Allowed in More Places: 79% More Apply for Firearms Permit," *Atlanta Journal-Constitution*, Feb. 1, 2009 (http://www.ajc.com/services/content/printedition/2009/02/01/gunpermits02011.html).

94. For Florida's information see http://licgweb.doacs.state.fl.us/stats/cw_active.html. The explosion in Utah's issuing of permits in 2008 was due to out-of-state residents (http://www.des.utah.gov/bci/documents/2008Q4.pdf).

95. Following Donohue, "The Final Bullet in the Body of the More Guns, Less Crime Hypothesis," and F. Plassmann and John Whitley, "Confirming 'More Guns, Less Crime,'" *Stanford Law Review*, 2003, pp. 1313–69, I will do the year-by-year affects by two-year intervals.

96. The drops are statistically significant at better than the 10 percent level by years 5 and 6 after the law was put in place. These levels of significance are calculated using robust standard errors. By years 11 and 12, the drop is statistically significant at the 5 percent level, and by years 15 or more later, the drop is significant at better than the 1 percent level. A breakdown of the year-by-year levels of significance is shown in appendix 10.2.

97. The drop in violent crime is statistically significant at 7 percent level by years 3 and 4 after the law.

98. The drop is statistically significant at the 2 percent level by years 3 and 4 after the law.

99. The estimated benefits from reduced property crimes shown in table 10.6 depend a lot upon what set of estimates are used. For example, if the change in average crime rates before and after the law had been used, property crimes would have been associated with an equally large increase in victimization costs, reducing the total gain to the states by about one-third.

100. Ian Ayres and John Donohue, "Nondiscretionary Concealed Weapons Laws: A Case Study of Statistics, Standards of Proof, and Public Policy," *American Law and Economics Review* 1 (1999): 436; William Bartley and Mark Cohen, "The Effect of Concealed Weapons Laws: An Extreme Bound Analysis," *Economic Inquiry* 36 (Apr. 1998): 259; Daniel Black and Daniel Nagin, "Do Right-to-Carry Laws Deter Violent Crime?" *Journal of Legal Studies* 27 (1998): 209; Stephen Bronars and John R. Lott, "Criminal Deterrence, Geographic Spillovers, and Right-to-Carry Laws," *American Economic Review* 88 (1998): 475.

101. F. Plassmann and T. N. Tideman, "Does the Right to Carry Concealed Handguns Deter Countable Crimes? Only a Count Analysis Can Say," *Journal of Law and Economics* 44 (2001): 771–98.

102. C. E. Moody, "Testing for the Effects of Concealed Weapons Laws: Specification Errors and Robustness," *Journal of Law and Economics* 44, no. 2, pt. 2 (2001): 799–813.

103. Carlisle Moody and Thomas Marvell, "The Debate on Shall-Issue Laws," *Econ Journal Watch*, Sept. 2008, pp. 269–93.

104. E. Helland and A. Tabarrok, "Using Placebo Laws to Test 'More Guns, Less Crime,'" *Advances in Economic Analysis and Policy* 4, no. 1 (2004).

105. D. E. Olsen and M. D. Maltz, "Right-to-Carry Concealed Weapons Laws and Homicide in Large U.S. Counties: The Effect on Weapons Types, Victim Characteristics, and Victim-Offender Relationships," *Journal of Law and Economics*44 (2001): 747–70.

106. B. L. Benson and B. D. Mast, "Privately Produced General Deterrence," *Journal of Law and Economics* 44 (2001): 725–46.

107. D. B. Mustard, "The Impact of Gun Laws on Police Deaths," *Journal of Law and Economics* 44 (2001): 635–57.

108. James Q. Wilson, "Dissent," in *Firearms and Violence: A Critical Review*, ed. Charles F. Wellford, John V. Pepper, and Carol V. Petrie (Washington, DC: National Academies Press, 2005), appendix A.

109. John R. Lott, Jr., and John Whitley, "Safe Storage Gun Laws: Accidental Deaths, Suicides, and Crime," *Journal of Law and Economics* 44, no. 2, pt. 2 (Oct. 2001): 659–89.

110. The only study that tried to retest my results for the Brady Act was by Jens Ludwig and Philip Cook, and they also found that the law had no statistical impact on murder rates or overall accidental gun deaths or suicides. Their study did not examine the one crime category for which I found an increase in crime, rapes. Even though they concede that the Brady Act had no effect on total suicides, they claim that it reduced suicides for those over age 55. A closer look at narrower age groupings contradicts the pattern that they predict. The reduced incidence of firearm suicides for persons over 54 is overwhelmingly driven by the change for just those from ages 55 to 64, but this subcategory has the lowest suicide rate for those over age 54 and has the highest gun ownership rate. The different age groups experienced apparently random increases and decreases in firearm suicides after enactment of the law: the groups aged 35–44 years, 45–54 years, and older than age 85 all show increases in firearm suicides after the Brady Act. (See Jens Ludwig and Philip Cook, "Homicide and Suicide Rates Associated with Implementation of the Brady Handgun Violence Prevention Act," *Journal of the American Medical Association*, Aug. 2, 2000, pp. 585–91; and John R. Lott, Jr., "Impact of the Brady Act on Homicide and Suicide Rates," *Journal of the American Medical Association*, Dec. 6, 2000, p. 2718.)

111. In fact, there is frequently much more variation in crime rates or other individual characteristics across counties within a state than there is across states. For example, 80 percent of the counties in the United States have zero murders in any given year, and even the states with the highest murder rates contain many counties without any murders.

112. To obtain the level of statistical significance for his table 12, column 2, one must divide the coefficients by the reported standard errors. When that is done, four of the five violent crime rates indeed show a statistically significant reduction after the passage of the right-to-carry law. The *t*-statistics for these coefficients when they are corrected are at least 2.3 for a two-tailed *t*-test. See Mark Duggan, "More Guns, More Crime," *Journal of Political Economy* 109 (2001): 1110, table 12.

113. Two other points need to be made. First, Duggan provides no evidence that the adjustments that he makes are appropriate (indeed, my original paper with Mustard discussed these adjustments). Second, examining the before-and-after trends produce extremely statistically significant results. Duggan chose only to report the results for the before-and-after averages.

114. Of the two significant positive coefficients, one by Black and Nagin includes separate nonlinear time trends for each state (see chapter 9, critique 2, "Does it make sense for

nonlinear time trends for each state?" for a discussion of this). The one significant result from Duggan uses differences even though he doesn't do any tests for whether this is the appropriate specification. (In fact, Moody, "Testing for the Effects of Concealed Weapons Laws," p. 805, tests for unit roots and finds that county crime rates are stationary.)

There is one paper by Dezhbakhsh and Rubin that is critical of my work, but I have not included it in table 10.7, because it is a shorter version of the exact same empirical work that is already included in the list. They also do not investigate the differences in crime rates before and after right-to-carry laws are adopted (Hashem Dezhbakhsh and Paul H. Rubin, "Lives Saved or Lives Lost? The Effects of Concealed-Handgun Laws on Crime," *American Economic Review Papers and Proceedings* 88 (May 1998): 468–74). This has been discussed in other parts of this book.

115. Black and Nagin mark only the levels of statistical significance at the 5 percent level. There are a number of negative coefficients with statistical significance between the 5 and 10 percent levels.

116. For example, Ian Ayres and John J. Donohue III, "Shooting Down the More Guns, Less Crime Hypothesis" (draft version available from author), p. 20, write: "Note that for a number of the violent crime categories, very large negative estimated coefficients are found on some of the dummies for more than 6 years after passage. As noted, only a small portion of the entire array of shall issue states contribute to these estimates, thereby allowing a substantial drop in crime in an early passing state (whether caused by the shall issue law or not) to have a disproportionate effect in estimating a post-passage dummy or linear trend."

117. Steven Levitt, "Understanding Why Crime Fell in the 1990s: Four Factors That Explain the Decline and Six That Do Not," *Journal of Economic Perspectives* 18 (Winter 2004): 175.

118. Grant Duwe, Tom Kovandzic, and Carl Moody, "The Impact of Right-to-Carry Concealed Firearm Laws on Mass Public Shootings," *Homicide Studies*, Nov. 1, 2002.

119. John R. Lott, Jr., and William M. Landes, "Acts of Terror with Guns: Multiple Victim Shootings," in *The Bias Against Guns* (Washington, DC: Regnery, 2003), chapter 6.

120. It was also nice of Maltz and Targonski to state: "We should note that the authors of these studies were not aware of the extent of the problems with the data they used. In fact, they went to great lengths to secure the best possible data and conferred with many people (including an author of this paper) prior to performing their analysis" (Michael D. Maltz and Joseph Targonski, "A Note on the Use of County-Level UCR Data," *Journal of Quantitative Criminology*, Sept. 2002, p. 298.)

121. John R. Lott, Jr., and John Whitley, "Measurement Error in County-Level UCR Data," *Journal of Quantitative Criminology* 19, no. 2 (June 2003): 185–98.

122. The city-level data also answer another related objection. A referee for this book argued that right-to-carry laws "required local police agencies to perform fingerprinting of carry permit (CP) applicants, as well as other administrative tasks in processing applications, it placed a strain on agencies with less manpower relative to the demand for their services. This caused the agencies to withdraw personnel who compiled the (voluntary) UCR statistics and reassign them to the CP-related tasks mandated by the state legislature." While such a bias might affect county-level data, there is no bias that this creates in either city- or state-level data. Since all three types of data imply the same change in violent crime rates, it provides evidence against the claim that the results are driven by a bias in the county data. If this is a problem, eliminating the counties with the most missing data should cause the results to become less negative, but that is not the case.

123. See Duggan, "More Guns, More Crime," p. 1086.

124. There is also work that finds that magazine sales did not precede increases in murder rates. Carlisle E. Moody and Thomas B. Marvell, "Guns and Crime," *Southern Economic Journal* 71 (2005): 720–36.

125. Based on a telephone call with Mr. Johnson on December 5, 2001.

126. Ayres and Donohue, "Shooting Down the More Guns, Less Crime Hypothesis," p. 1296.

127. http://www.npr.org/templates/story/story.php?storyId=96409853.

128. See Roland Fryer, Paul Heaton, Steven Levitt, and Kevin Murphy, "Measuring the Impact of Crack Cocaine," *Social Science Research Network*, May 2005 (http://ssrn.com/abstract=720405).

129. Charles F. Wellford, John V. Pepper, and Carol V. Petrie, eds., *Firearms and Violence: A Critical Review* (Washington, DC: National Research Council of the National Academies and National Academies Press, 2004), p. 2.

130. "Do Guns Reduce Crime?"

131. Wellford, Pepper, and Petrie, *Firearms and Violence*, p. 2.

132. E-mail correspondence from James Q. Wilson to me, November 25, 2008. In e-mail correspondence with me on January 6, 2009, John Donohue did not accept this response from Wilson.

133. Wilson, "Dissent," p. 269.

134. Charles C. Branas, Therese S. Richmond, Dennis P. Culhane, Thomas R. Ten Have, and Douglas J. Wiebe, "Investigating the Link between Gun Possession and Gun Assault," *American Journal of Public Health* 99, no. 11 (Nov. 2009): 1–7.

135. Gary Kleck and Miriam A. Delone, "Victim Resistance and Offender Weapon Effects in Robbery," *Journal of Quantitative Criminology* 9 (1993): 55–81; Lawrence Southwick, "Self-Defense with Guns," *Journal of Criminal Justice* 28 (2000): 351–370; and Jongyeon Tark and Gary Kleck, "Resisting Crime," *Criminology* 42 (2004): 861–909.

136. The complete text of Levitt's letter is available at http://johnrlott.blogspot.com/2007/08/steve-levitts-correction-letter.html.

137. David Glenn, "Unusual Agreement Means Settlement May Be Near in 'Lott v. Levitt,'" *Chronicle of Higher Education*, July 27, 2007.

138. Unfortunately, the *Stanford Law Review*'s correction piece hasn't stopped Ayres and Donohue from continuing to make this claim even six years later. In a new paper that they released in 2009 just two days before I finished writing this book, Ayres and Donohue made the claim again, writing: "After we pointed out that our corrections of Lott's errors had rendered statistically insignificant all of the Lott, Plassmann, and Whitley results, Lott removed his name from the reply to Ayres and Donohue (2003a), which then was published—with the errors uncorrected—as Plassmann and Whitley (2003)."

Or take this example from another piece by Donohue that was published after the *Stanford Law Review* correction: "Lott acted somewhat incongruously in dropping his name from his reply to my work with Ian Ayres on the eve of publication. Ayres and I showed that virtually every regression in Lott's reply to our paper in the *Stanford Law Review* was incorrect due to Lott's coding errors." Donohue, "The Final Bullet in the Body of the More Guns, Less Crime Hypothesis," pp. 397–410.

139. A copy of the *Stanford Law Review* "Clarification" can be found at http://johnrlott.tripod.com/clarification.pdf.

140. See http://www.law.cornell.edu/supct/pdf/07-290P.ZD1.

141. Vivian S. Chu, "District of Columbia v. Heller: The Supreme Court and the Second Amendment," Congressional Research Service report for Congress, Jan. 15, 2009.

142. *National Rifle Association, et al. v. City of Chicago* and *National Rifle Association, et al. v. Village of Oak Park*, filed December 4, 2008, in the United States District Court for the Northern District of Illinois, Eastern Division, *Case 1:08-cv-03697* 4, 2008 (http://www.chicagoguncase.com/wp-content/uploads/2008/12/dist_ct_nra_opinion.pdf).

143. Kim Murphy, "Proposed Seattle Gun Ban May Draw New Fire," *Los Angeles Times*, Nov. 25, 2008.

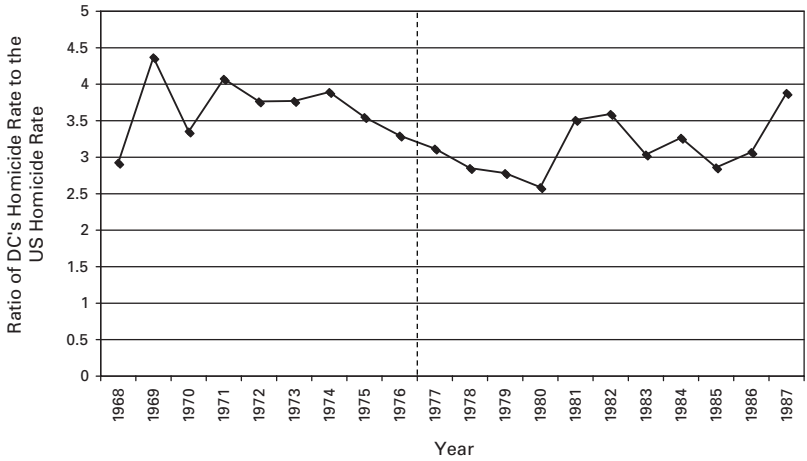
144. Suzanne Pardington, "Oregon Universities' Gun Ban Faces Legal Test," *Oregonian*, Feb. 20, 2009 (http://www.oregonlive.com/news/index.ssf/2009/02/oregon_universities_

gun_ban_fa.html); Windsor Genova, "Rifle Association Suit Seeks End to Chicago, San Francisco Gun Bans," *Gant Daily*, June 27, 2008 (http://www.gantdaily.com/news/35/ARTICLE/24122/2008-06-27.html).

145. DC's brief and various amici point to murder as the violent crime category that was reduced after the DC handgun ban went into effect during February 1977. No other violent crime rate is pointed to.

146. John R. Lott, Jr., "Does a Helping Hand Put Others at Risk? Affirmative Action, Police Departments, and Crime," *Economic Inquiry* 38, no. 2 (Apr. 2000): 239–77.

147. Colin Loftin, David McDowall, Brian Wiersema, and Talbert J. Cottey, "Effects of Restrictive Licensing of Handguns on Homicide and Suicide in the District of Columbia," *New England Journal of Medicine* 325 (1991): 1615.



Comparison of homicide rates in DC and the United States for the years 1968–1987

148. James Alan Fox and David McDowall, "Brief of Professors of Criminal Justice as Amici Curiae in Support of the Petitioners," submitted to the Supreme Court of the United States in *District of Columbia v. Heller*, no. 07-290, Jan. 11, 2008 (http://www.gurapossessky.com/news/parker/documents/07-290tsacprof.ofcrim.just.pdf).

149. The debate between John Donohue and myself took place at the Contemporary Club in Charlottesville, Virginia, on October 22, 2008.

150. The average ratio from 1968 to 1976 was 219 percent compared to the average rate of 257 percent from 1977 to 1987.

151. Gary Kleck compares the changes in DC's murder rate to the changes in Baltimore's.

152. "Do Guns Reduce Crime?"

153. See http://www.law.cornell.edu/supct/pdf/07-290P.ZD1.

154. The Chicago Handgun Freeze of 1982 stopped the registration of handguns after April 10, 1982, but a lawsuit was immediately filed by Jerome Sklar, an individual who had moved to Chicago from Skokie, Illinois, on April 15. The case *Sklar v. Byrne* (*Federal Supplement*, vol. 556, p. 736 [Northern District of Illinois, 1983]) did not have a judgment entered until February 1983, and enforcement of Chicago's ordinances did not begin until that date. The Seventh Circuit didn't reach its decision in *Quilici v. Village of Morton Grove* upholding Morton Grove's ban on handguns until December 6, 1982. Richard Pearson, the executive director of the Illinois State Rifle Association, said that Chicago didn't really enforce the ban until the be-

ginning of 1983, when these legal cases had run their course. Chicago's registration rules had been in effect since 1968. I interviewed Victor Quilici, the plaintiff in *Quilici v. Village of Morton Grove*, on January 9, 2009. I also interviewed Kenneth Denberg, the lawyer who had filed Sklar's case in District Court, on January 14, 2009, but he was unable to add anything to this discussion (312-704-3000). See Illinois Citizens for Handgun Control, "Sample Firearms Regulations in Illinois Municipalities—Four Types of Local Ordinances in Effect in Illinois in 1983 and the Judicial Opinions Regarding Their Constitutionality," National Institute of Justice, 1983 (http://www.ncjrs.gov/App/Publications/abstract.aspx?ID=96896). I also talked to Richard Pearson, the head of the Illinois State Rifle Association, by telephone on January 9, 2009.

155. The FBI's UCR had data on cities with more than 10,000 people between 1974 and 2005.

156. The adjacent counties in Illinois are Lake, Will, DuPage, Kane, and McHenry. In Indiana, there is also Lake County.

157. David Leppard, "Ministers 'Covered Up' Gun Crime," *London Times*, Aug. 26, 2007.

158. David Kopel, *The Samurai, the Mountie, and the Cowboy* (Amherst, NY: Prometheus Books, 1992), pp. 73–78.

159. Don B. Kates and Gary A. Mauser, "Would Banning Firearms Reduce Murder and Suicide: A Review of International Evidence," *Harvard Journal of Law and Public Policy* 30 (Spring 2007): 651–94.

160. For Anguilla, Antigua and Barbuda, Dominica, Grenada, Montserrat, Saint Kitts and Nevis, Saint Lucia, and Saint Vincent and the Grenadines, the average murder rate rose to 19.9 per 100,000 people in 2007. The murder rate in Trinidad and Tobago rose to a historical high of 30.6. That same year, Jamaica's murder rate was at 58. "A Caribbean Crime Wave," *Economist*, Mar. 20, 2008 (http://www.economist.com/displayStory.cfm?story_id=10903343&fsrc=RSS).

161. Jamaica's crime data were obtained from a variety of sources. Its murder data from 1960 to 1967 were obtained from Terry Lacey, *Violence and Politics in Jamaica, 1960–70* (Manchester: Manchester University Press, 1977). Professor Gary Mauser obtained the data from 1970 to 2000 from a Professor A. Francis in Jamaica and the data from 2001 to 20006 from the Statistical Institute of Jamaica (http://www.statinja.com/stats.html). Jamaica's population estimates were obtained from NationMaster.com (http://www.nationmaster.com/graph/peo_pop-people-population&date=1975).

162. "While Firearms Seized at Border on Rise, Majority of Guns Caught from Vacationers, not Smugglers," *Global Travel Industry News*, May 18, 2008 (http://www.eturbonews.com/2554/while-firearms-seized-border-rise-majority-gu). The article originally appeared in the *Toronto Sun*.

163. National Gang Intelligence Center, U.S. Department of Justice, "National Gang Threat Assessment 2009," Jan. 2009 (http://www.usdoj.gov/ndic/pubs32/32146/index.htm). See also Kevin Johnson, "FBI: Burgeoning Gangs Behind up to 80% of U.S. Crime," *USA Today*, Jan. 29, 2009, p. A1 (http://www.usatoday.com/news/nation/2009-01-29-ms13_N.htm).

164. Loftin et al.'s suicide analysis (like his homicide analysis) looked only at raw numbers, not rates, and therefore failed to account for DC's decline in population. By ignoring rates, Loftin implied a somewhat larger drop in suicides than actually occurred. A graph showing the number of suicides instead of the suicide rate can be found in Loftin et al., "Effects of Restrictive Licensing in Handguns," p. 1620.

165. D. M. Cutler, E. L. Glaeser, and K. E. Norberg, "Explaining the Rise in Youth Suicide," in Risky Behavior Among Youths: An Economic Analysis, ed. J. Gruber (Chicago: University of Chicago Press, 2001), pp. 219–69.

166. Jerome Taylor, "Mumbai Photographer: I Wish I'd Had a Gun, Not a Camera; Armed Police Would Not Fire Back," *Belfast Telegraph* (Northern Ireland), Nov. 29, 2008 (http://www.belfasttelegraph.co.uk/news/world-news/article14086308.ece).

167. Yaroslav Trofimov, Geeta Anad, Peter Wonacott, and Matthew Rosenberg, "India Security Faulted as Survivors Tell of Terror," *Wall Street Journal*, Dec. 1, 2008, p. A1 (http://online.wsj.com/article/SB122809281744967855.html).

168. Abhijeet Singh, "Gun Ownership in India" (http://www.abhijeetsingh.com/arms/india/).

169. In 2002, there were about 6.5 million people living in Israel. Of those, 81 percent were Jews and 63 percent were over 20 years of age (http://www.jafi.org.il/agenda/2001/english/wk3–1/14.asp and http://www.un.org/Depts/unsd/social/youth.htm). Arabs were restricted from obtaining concealed-handgun permits. At the time of this writing Israel had 340,000 handgun permit holders, though they were planning on issuing 40,000 more permits ("Army Issuing 40,000 Handgun Permits," IsraelNN.com, Mar. 6, 2002 [http://www.israelnationalnews.com/news.php3?id=19628]). Related information can be found at http://www.jpost.com/Editions/2002/03/06/LatestNews/LatestNews.44715.html, though this second story appears to include only a portion of all the permit holders.

170. See Lott and Landes's discussion in "Acts of Terror with Guns" for examples.

171. Steven Gray, "How the NIU Massacre Happened," *Time*, Feb. 16, 2008 (http://www.time.com/time/nation/article/0,8599,1714069,00.html?cnn=yes).

172. "Worst U.S. Shooting Ever Kills 33 on Va. Campus: 15 Others Wounded as Panic Grips Virginia Tech for 2½ Hours," MSNBC.com, Apr. 17, 2007 (http://www.msnbc.msn.com/id/18134671/).

173. Associated Press, "Gunman Who Killed 5 at Missouri Council Meeting Left Suicide Note 'Truth Will Come Out,'" FoxNews.com, Feb. 8, 2008 (http://www.foxnews.com/story/0,2933,329767,00.html).

174. http://www.foxnews.com/video2/player06.html?020808/020808_studiob_thornton&Studio_B&%26%2392%3B%26%2339%3BNot%20Right%20or%20Wrong%26%2392%3B%26%2339%3B&%26%2392%3B%26%2339%3BNot%20Right%20or%20Wrong%26%2392%3B%26%2339%3B&US&-1&News&335&&&new.

175. John Keenan, "Few Merchants See Need to Post No-Weapons Signs," *Omaha World-Herald*, Mar. 28, 2007 (http://4.bp.blogspot.com/_2SW2_lbrxgY/R1gZwy1WaWI/AAAAAAAAAE8/a6HSha-Arys/s1600-h/Picture+5.png).

176. A picture of the warning signs at the mall can be seen in John R. Lott, Jr., "Evidence that Trolley Square Mall in Utah Occurred in a 'Gun Free Zone,'" Johnrlott.blogspot.com, Feb. 15, 2007 (http://johnrlott.blogspot.com/2007/02/proof-that-trolley-square-mall-in-utah.html).

177. Associated Press, "5 Dead in Illinois Mall Shooting; Suspect Still on the Loose," FoxNews.com, Feb. 3, 2008 (http://www.foxnews.com/story/0,2933,327908,00.html).

178. An article in the *Omaha World-Herald* noted: "Signs banning guns from privately owned businesses haven't exactly popped up all over the Omaha area." Keenan, "Few Merchants See Need to Post No-Weapons Signs."

179. See Lott and Landes, "Acts of Terror with Guns."

180. Ibid.

181. Trofimov et al., "India Security Faulted as Survivors Tell of Terror."

182. "18 Dead in German School Shooting," BBC News, Apr. 26, 2002 (http://news.bbc.co.uk/2/hi/europe/1952869.stm); Nick Allen, "Finland School Shooting: Gunman Planned Massacre for Six Years," *London Telegraph*, Sept. 24, 2008 (http://www.telegraph.co.uk/news/worldnews/europe/finland/3071235/Finland-school-shooting-Gunman-planned-massacre-for-six-years.html); "'Grudge' Behind Swiss Gun Massacre," CNN, Sept. 27, 2001 (http://archives.cnn.com/2001/WORLD/europe/09/27/switzerland.shooting/index.html); and John Lichfield, "Gunman with a Grudge Kills Four in French Shooting Spree," *Independent*, Oct. 30, 2001 (http://www.independent.co.uk/news/world/europe/gunman-with-a-grudge-kills-four-in-french-shooting-spree-633112.html). Another attack at a German school in

2006 wounded eleven people ("I Loathe People: Ex-pupil Wounds 11 in German School Shooting," *Spiegel Online International*, Nov. 20, 2006 (http://www.spiegel.de/international/0,1518,449492,00.html)).

183. "Greek Gunman Dies After Shooting," BBC, Apr. 10, 2009 (http://news.bbc.co.uk/2/hi/europe/7993401.stm); "One Killed in Dutch Café Shooting," BBC, Apr. 11, 2009 (http://news.bbc.co.uk/2/hi/europe/7994731.stm); and David Nowak, "Moscow Policeman Opens Fire in Public, Kills 3," Associated Press, Apr. 27, 2009.

184. Marcella Bombardieri, "Campus Police Renew Call to Carry Arms," *Boston Globe*, Apr. 28, 2007 (http://www.boston.com/news/local/articles/2007/04/28/campus_police_renew_call_to_carry_arms/).

185. Scott Buhrmaster, "Should Campus Cops Carry Guns? One College President Says No," Policeone.com, Nov. 19, 2003.

186. Fred Abraham, "No One Benefits from Arming of UNI Safety Officers," *Cedar Falls Courier*, May 6, 2007 (http://www.wcfcourier.com/articles/2007/05/06/columnists/guest_column/c904ad59385a60bd862572d1006016b0.txt).

187. "Safety and Security on the Virginia Tech Campus," Division of Student Affairs, Virginia Tech University, as posted on May 30, 2007 (http://www.dsa.vt.edu/family/Safetylisting.php). Since that time the campus police force has expanded by a third.

188. William M. Landes, "An Economic Study of U.S. Airline Hijacking, 1961–1976," *Journal of Law and Economics* 21 (Apr. 1978): 1–32.

189. Ibid.

190. "Ohio Trainer Makes the Case for Single-Officer Entry Against Active Killers," Policeone.com, May 14, 2008 (http://www.policeone.com/active-shooter/articles/1695125-Ohio-trainer-makes-the-case-for-single-officer-entry-against-active-killers/). The article says: "'Time is our worst adversary in dealing with active killers,' Borsch told *Force Science News*. 'We're racing what I call "the Stopwatch of Death." Victims are often added to the toll every several seconds.'" Brendan Keefe, "When Seconds Count: Stopping Active Killers," WCPO.com, Nov. 21, 2008 (http://www.wcpo.com/news/local/story/When-Seconds-Count-Stopping-Active-Killers/_yls0jTxAkK8QJR1NKbePA.cspx). Keefe's news article notes: "Based on the Virginia Tech data, top tactics training facilities determined the first officer on scene should make entry immediately with an aggressive attack on the shooter. Every minute the officer waits for back-up, another three or more people could die. In other words, while it was once considered suicide for a lone officer to take on an active killer, it is now considered statistical homicide for him not to do so."

191. P. Solomon Banda, "Year After Attack, Colorado Church Guard Reflects," Policeone.com, Dec. 8, 2008 (http://www.policeone.com/patrol-issues/articles/1763667-Year-after-attack-Colo-church-guard-reflects/). Jeanne Assam had prior experience as a police officer.

192. "Armed Citizens Stop Memphis Street Shooter," WBIR.com, Mar. 8, 2007 (http://www.wbir.com/news/local/story.aspx?storyid=43109). A copy of the article can also be found at http://johnrlott.blogspot.com/2007/03/two-concealed-handgun-permit-holders.html.

193. Josh White, "Law School Shooter Pleads Guilty, Former Student Avoids Death Penalty in Deal on Va. Slayings," *Washington Post*, Feb. 28, 2004, p. B3. "Mississippi v. Luke Woodham," Court TV archives (http://www.courttv.com/archive/trials/woodham/).

194. "Counselors' Heroism Foils Terror Attack in Gush Etzion Seminary High School," Jan. 25, 2008 (http://web.israelinsider.com/Articles/Security/12596.htm). Tzippe Barrow, "Terrorists Target Israeli Students," CBNNews.com, Jan. 25, 2008 (http://www.cbn.com/CBNnews/310078.aspx). News Agencies, "Jerusalem Yeshiva Student: I Shot the Terrorist Twice in the Head," Haaretz.com, July 7, 2008 (http://www.haaretz.com/hasen/spages/961703.html).

195. A recent segment on ABC's *20/20* mentioned this as a possibility, but the reporters were not able to identify a single example of this either ("If I Only Had a Gun," http://abcnews.go.com/2020/story?id=7298996&page=1).

196. Joshua Molina, "Utah Students Hide Guns, Head to Class," CNN.com, Feb. 21, 2008 (http://www.cnn.com/2008/US/02/20/cnnu.guns/index.html). See also the chart "Guns on Campus" (http://www.cnn.com/2008/US/02/20/cnnu.guns/index.html#cnnSTCOther1).

197. Discussions with faculty members at a number of schools indicate that the rules are somewhat more flexible for them (e.g., Robert G. Hansen at the Tuck Business School and Charlotte Twight in the economics department at Boise State University, who consulted their faculty handbooks).

198. For example, the University of Colorado Board of Regents enacted a weapons control policy in 1994 (*Students for Concealed Carry on Campus v. The Regents of the University of Colorado*, District Court, El Paso County, State of Colorado, Case Number 2008CV6492, Motion to Dismiss,).

199. See the conclusion in Christopher Koper, Daniel Woods, and Jeffrey Roth, "An Updated Assessment of the Federal Assault Weapons Ban: Impacts on Gun Markets and Gun Violence, 1994–2003," report to the National Institute of Justice, United States Department of Justice, June 2004.

200. John R. Lott, Jr., "Candidates Confused on Gun Ban," Fox News, Wednesday, Nov. 19, 2003.

201. Ibid.

202. For a very useful reference see David B. Kopel, "Should Gun Shows Be Outlawed? McCain Bill Does Much More than Impose Background Checks" (issue paper no. 1-2002, Independence Institute, Golden, CO, Jan. 23, 2002) (http://www.davekopel.com/2A/IP/gunshows2.htm).

203. Caroline Wolf Harlow, "Firearm Use by Offenders," Bureau of Justice Statistics, U.S. Department of Justice, Nov. 2001. An earlier study using the same survey data found slightly higher rates of criminal guns acquired from gun shows (1.7 percent) or flea markets (1.7 percent), but a discussion with Ms. Harlow indicated that the later study had used a "cleaned up" version of the survey data. Apparently there had been several coding and other errors in the original version of the data. The earlier study was John Scalia, "Federal Firearm Offenders, 1992–98," Bureau of Justice Statistics, U.S. Department of Justice, June 2000.

204. James D. Wright and Peter Rossi, *Armed and Considered Dangerous: A Survey of Felons and Their Firearms* (Hawthorne, NY: Aldine de Gruyter Publishers, 1986).

205. Lott and Landes, *The Bias Against Guns*, chapter 8.

206. Surprisingly, the Bureau of Alcohol, Tobacco, and Firearms used the same source when examining 1998 and claimed that there were 4,442 such shows, when the total given to me by Bruce Wolberg, advertising manager for the Gun Show Calendar, was 2,630. The differences across some states was substantial and affected the rankings. For example, Texas is 213 instead of the BATF's 472. Pennsylvania is now 138 instead of 250, Florida 178 instead of 224, and Illinois 103 instead of 203.

Back in 2002 after repeated attempts, James Knowles, my research assistant at the time, finally got John D'Angelo at the BATF to answer questions about these discrepancies. James told me that, according to D'Angelo, "it would be better to use our own numbers than the numbers from the report, unless I hear otherwise from him. He asks what we expect from him in response to our question. He explained that he could not find the exact documentation for the manual counts and said that he will look into it a little further but that he is not optimistic that he will find it. He said that he would call back," but he never did. D'Angelo's telephone number is 202-927-8500.

207. Mark Duggan, Randi Hjalmarsson, and Brian Jacob, "The Effect of Gun Shows on Gun-Related Deaths: Evidence from California and Texas" (National Bureau of Economic Research working paper, Oct. 2008).

208. "Colorado's 'Make My Day' Law Eyed in Home Shooting," *Taiwan News*, Jan. 1, 2009; and Catherine Ellsworth, "Colorado Home Owner Could Avoid Murder Charges Under 'Make My Day' Law," *London Telegraph*, Jan. 3, 2009.

209. "New Details in Springs Shooting," KRDO.com, Jan. 4, 2009 (http://www.krdo.com/Global/story.asp?S=9604233).

210. Sue McMillin and Bill Vogrin, "Mistaken for Thief, Man Gunned Down," *Colorado Springs Gazette*, Dec. 29, 2008 (http://www.gazette.com/articles/police_45478_article.html/kennedy_house.html).

211. "Do Guns Reduce Crime?" Rosenkranz Foundation/Intelligence Squared US, Oct. 28, 2008.

APPENDIX ONE

1. Although this jargon may appear overwhelming, it is actually fairly simple. Consider the following example. Suppose we wish to present findings that height and SAT scores are correlated among college-bound students. Instead of reporting that an additional inch is related to an increase in test scores of so many points, we can compare standard-deviation changes, which would be equivalent to reporting the results as comparisons of changes in percentile height with percentile changes in the SAT scores.

2. To phrase this in terms of the earlier discussion of standard deviations, with a symmetric distribution, there is a 32 percent probability that a variable will take on a value that is more than one standard deviation different from its mean, and only a five percent probability that it will be more than two standard deviations away from the mean.

APPENDIX THREE

1. U.S. Department of Justice, *Crime in the United States, 1994* (Washington, DC: U.S. Department of Justice, 1994.) I also wish to thank Tom Bailey of the FBI and Jeff Maurer of the Department of Health and Human Services for answering questions concerning the data used in this paper.

2. The Inter-University Consortium for Political and Social Research number for this data set was 6387, and the principle investigator was James Alan Fox of Northeastern University College of Criminal Justice.

3. Dropping the zero crime values from the sample made the "shall-issue" coefficients larger and more significant, but doing the same thing for the accident-rate regressions did not alter "shall-issue" coefficients. (See also the discussion at the end of the section headed "Using County and State Data for the United States" in chapter 4).

4. For further descriptions of the procedures for calculating intercensus estimates of population, see ICPSR (8384): U.S. Department of Commerce, Bureau of the Census, *Intercensal Estimates of the Population of Counties by Age, Sex, and Race (United States), 1970–1980* (Ann Arbor, MI: ICPSR, Winter 1985). See also Bureau of the Census, *Methodology for Experimental Estimates of the Population of Counties by Age and Sex: July 1, 1975,* Current Population Reports, series P-23, no. 103, and *Census of Population, 1980: County Population by Age, Sex, Race, and Spanish Origin (Preliminary OMB-Consistent Modified Race).*

5. U.S. Department of Commerce, Bureau of the Census, *Methodology for Experimental Estimates of the Population of Counties by Age and Sex: July 1, 1975,* Current Population Reports, series P-23, no. 103; see also Bureau of the Census, *Census of Population, 1980: County Population by Age, Sex, Race, and Spanish Origin (Preliminary OMB-Consistent Modified Race)*, pp. 19–23.

6. U.S. Department of Commerce, *Statistical Abstract of the United States,* 114th ed., table 746, p. 487.

7. Thomas B. Marvell and Carlisle E. Moody, "The Impact of Enhanced Prison Terms for Felonies Committed with Guns," *Criminology* 33 (May 1995): 259–60.

APPENDIX SEVEN

1. The regression is natural log of gun ownership given by General Social Survey on the natural log of per capita magazine sales and state and year fixed effects

2. The survey was not conducted every year. Initial years when the gun questions were not asked are 1972, 1975, 1978, 1983, and 1986. Beginning in 1988, the gun questions were asked every year but of only two-thirds of the total survey sample. There was no funding for surveys in 1979 and 1981, and beginning in 1994 the survey was switched to biennial (even years). The survey data are also weighted by the demographics in each individual state. Over the entire period, "owngun" was "refuse to answer" for 156 out of the total 24,855 observations with a response to that variable.

3. See chapter 3.

4. Compared to other surveys such as the CBS General Election Exit Poll, with over 36,000 observations in 1988, and the Voter News Service Poll, with over 3,400 people surveyed in 1996, the General Social Survey only surveys 899–1,973 in any given year. While the General Social Survey will not provide a very accurate picture of gun ownership in any given state in a year, the much larger number of years over which the survey is provided allows us to investigate trends.

5. The household rate was calculated by assuming that married women owned guns at the same rate as married men of the same race and age grouping.

6. Using weighted least squares where the weight was the state population, I estimated

$$\ln(\text{murder rate}) = a_0 + b_1 \ln(\text{magazine sales for the preceding year}) + b_2 \ln(\text{magazine sales two years previously}) + b_3 \text{ arrest rate for murder} + b_4 \text{ death penalty execution rate} + b_5 \text{ state population} + b_6 \text{ state population squared} + b_7 \text{ unemployment rate} + b_8 \text{ poverty rate} + b_9 \text{ real per capita income} + b_{10} \text{ real per capita unemployment insurance payments} + b_{11} \text{ real per capita welfare payments} + b_{12} \text{ real per capita retirement payments} + b_{13} \text{ 36 different demographic variables that measure the percentage of the state population in different age, sex, and race divisions} + \text{state fixed effects} + \text{year fixed effects}$$

To deal with the endogeneity issues involved in using the arrest rate for murder in explaining the murder rate, I also tried using the arrest rate for violent crime, and the results were virtually identical. Removing the arrest rate entirely also produced similar results.

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INDEX

Note: Page numbers followed by t *indicate tables; page numbers followed by* f *indicate figures.*

CURRENT EVENTS

"A compelling book with enough hard evidence that even politicians may have to stop and pay attention. *More Guns, Less Crime* is an exhaustive analysis of the effect of gun possession on crime rates."—James Bovard, *Wall Street Journal*

On its initial publication in 1998, John R. Lott's *More Guns, Less Crime* drew both lavish praise and heated criticism. More than a decade later, it continues to play a key role in ongoing arguments over gun-control laws: despite all the attacks by gun-control advocates, no one has ever been able to refute Lott's simple, startling conclusion that more guns mean less crime. Relying on the most rigorously comprehensive data analysis ever conducted on crime statistics and right-to-carry laws, the book directly challenges common perceptions about the relationship of guns, crime, and violence. For this third edition, Lott draws on an additional ten years of data—including provocative analysis of the effects of gun bans in Chicago and Washington, DC—that brings the book fully up to date and further bolsters its central contention.

"John Lott documents how far 'politically correct' vested interests are willing to go to denigrate anyone who dares disagree with them. Lott has done us all a service by his thorough, thoughtful, scholarly approach to a highly controversial issue."—Milton Friedman

"Lott's pro-gun argument has to be examined on the merits, and its chief merit is lots of data. . . . If you still disagree with Lott, at least you will know what will be required to rebut a case that looks pretty near bulletproof."—Peter Coy, *Business Week*

"By providing strong empirical evidence that yet another liberal policy is a cause of the very evil it purports to cure, he has permanently changed the terms of debate on gun control. . . . A model of the meticulous application of economics and statistics to law and policy."
—John O. McGinnis, *National Review*

John R. Lott, Jr., is the author of five books, including *Freedomnomics* and *Are Predatory Commitments Credible? Who Should the Courts Believe?* the latter also published by the University of Chicago Press.

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Criminal Use of Assault Weapons and High-Capacity Semiautomatic Firearms: an Updated Examination of Local and National Sources

Christopher S. Koper · William D. Johnson · Jordan L. Nichols · Ambrozine Ayers · Natalie Mullins

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Abstract Policies restricting semiautomatic assault weapons and large-capacity ammunition magazines are intended to reduce gunshot victimizations by limiting the stock of semiautomatic firearms with large ammunition capacities and other military-style features conducive to criminal use. The federal government banned such weaponry from 1994 to 2004, and a few states currently impose similar restrictions. Recent debates concerning these weapons have highlighted their use in mass shootings, but there has been little examination of their use in gun crime more generally since the expiration of the federal ban. This study investigates current levels of criminal activity with assault weapons and other high-capacity semiautomatics in the USA using several local and national data sources including the following: (1) guns recovered by police in ten large cities, (2) guns reported by police to federal authorities for investigative tracing, (3) guns used in murders of police, and (4) guns used in mass murders. Results suggest assault weapons (primarily assault-type rifles) account for 2–12% of guns used in crime in general (most estimates suggest less than 7%) and 13–16% of guns used in murders of police. Assault weapons and other high-capacity semiautomatics together generally account for 22 to 36% of crime guns, with some estimates upwards of 40% for cases involving serious violence including murders of police. Assault weapons and other high-capacity semiautomatics appear to be used in a higher share of firearm mass murders (up to 57% in total), though data on this issue are very limited. Trend analyses also indicate that high-capacity semiautomatics have grown from 33 to 112% as a share of crime guns since the expiration of the federal ban—a trend that has coincided with recent growth in shootings nationwide. Further research seems warranted on how these weapons affect injuries and deaths from gun violence and how their regulation may impact public health.

Keywords Firearms · Assault weapons · Violence

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Introduction

Firearm violence imposes a significant burden on public health in the USA. From 2010 through 2012, the nation experienced an annual average of 11,256 firearm homicides and 48,534 non-fatal assault-related gunshot victimizations that cost society nearly $22 billion a year in lifetime medical and work-related costs [1]. One type of policy response to reduce gun violence involves restricting or mandating design changes in particular types of firearms that are considered to be especially dangerous and/or attractive for criminal use.

Restrictions on assault weapons (AWs) represent one particularly controversial and highly contested form of such legislation that has featured prominently in gun policy debates in recent decades. In general, AW laws

restrict manufacturing, sales, and ownership of semiautomatic firearms with large ammunition capacities and other military-style features that appear useful in military and criminal applications but unnecessary in shooting sports or self-defense [2]. Examples of such features include pistol grips on rifles, flash hiders, folding rifle stocks, threaded barrels for attaching silencers, and barrel shrouds on pistols. AW laws also commonly include restrictions on large-capacity magazines (LCMs), which are typically defined as ammunition feeding devices holding more than ten rounds of ammunition (some laws have higher limits). LCM restrictions are arguably the most important components of AW laws in that they also apply to the larger class of high-capacity semiautomatic firearms without military-style features. In the broadest sense, AW-LCM laws are thus intended to reduce gunshot victimizations by limiting the stock of semiautomatic firearms with large ammunition capacities and other features conducive to criminal use. The federal government enacted a national ban on AWs and LCMs in 1994 but allowed it to expire in 2004. Currently, eight states and the District of Columbia have AW and/or LCM restrictions, as do some additional localities [3].

Recent discussion and debates concerning these weapons have largely focused on their use in mass shootings. However, there has been little examination of the use of AWs and LCMs in gun crime more generally since the expiration of the federal ban. Studies conducted around the time of the federal ban found that AWs accounted for up to 8% of guns used in crime (generally between 1 and 6% and averaging around 2%) and that the broader class of firearms equipped with LCMs (including AWs and other semiautomatic firearms equipped with LCMs) accounted for up to a quarter [2, 4–12]. Criminal use of such weaponry declined during the years of the federal ban [2, 13, 14], but trends since then have only been examined in the state of Virginia, where LCM use rose following the ban's expiration [14]. Semiautomatic weapons with LCMs and/or other military-style features are common among models produced in the contemporary gun market [15, 16], but precise estimates of their production and ownership are unavailable. Growth in the use of such weapons could have important implications for public health as these weapons tend to produce more lethal and injurious outcomes when used in gun violence [2, 17]. This study provides an updated examination of the AW issue by investigating current levels of criminal activity with AWs and other LCM firearms as measured in a variety of national and local data sources.

Data and Methods

There is no national data source that can be used to count the numbers of homicides, non-fatal shootings, or other crimes committed with AWs and other LCM firearms. Therefore, criminal use of these weapons was approximated by examining and triangulating across several local and national data sources on guns used in different types of crimes.

Local Data Sources

The local-level analyses are based on guns recovered by police over multiple years (defined below) in a convenience sample of ten cities including Hartford (CT), Rochester (NY), Syracuse (NY), Baltimore (MD), Richmond (VA), Minneapolis (MN), Milwaukee (WI), Kansas City (MO), Seattle (WA), and Sacramento (CA). Large cities were selected for the analysis (these cities range in size from roughly 124,000 to 684,500) due to the concentration of gun violence in urban areas [18, 19]. Patterns and trends in these particular cities may not be indicative of those elsewhere; further, some (Baltimore, Hartford, Rochester, Syracuse, and Sacramento) are covered by state AW and LCM restrictions that were in effect during all or portions of the study period (this study does not attempt to evaluate the implementation and effects of these laws or variations therein). Nonetheless, these cities constitute a geographically diverse set of ban and non-ban locations, thus strengthening generalizations. The data were obtained from law enforcement authorities in these jurisdictions except where otherwise noted. Information available in most of the police databases included the type, make, model, and caliber of each confiscated firearm; the date when it was recovered; and the type of crime with which it was associated.

Guns recovered by police (often referred to as "crime guns") are the only readily available data with which to study patterns and trends in the types of guns used in crime across jurisdictions, and they are commonly used in research on gun markets, gun violence, and gun policy [2, 9, 20–37]. Guns confiscated by police include guns recovered in violent crime investigations as well as those recovered in connection with weapon offenses

(illegal possession, carrying, and discharges), drug violations, property crimes, and other incidents. These samples thus represent guns known to have been used in violence as well as guns possessed and/or carried by criminal and otherwise high-risk persons. As others have noted, they represent a sample from the population of guns that are at greatest risk of misuse [24] and thereby provide a probable sample of guns used to commit crimes [21]. As caveats, nonetheless, it should be noted that police do not recover all guns used and possessed illegally, and it is possible that the types of guns they confiscate differ from those of unrecovered guns linked to illegal possessors and users. The analyses highlighted below are based on all confiscated firearms in the study jurisdictions. Additional analyses conducted with just those guns clearly connected to a violent offense, which represented at least 13 to 19% of guns across the cities, produced very similar results except where noted (separate offense-type analyses could not be conducted with the Syracuse and Rochester gun data or the Richmond LCM data).

National Data Sources

National-level analyses were conducted using three data sources and compilations. The first consists of information on firearms recovered by law enforcement agencies throughout the nation and reported to the federal Bureau of Alcohol, Tobacco, Firearms, and Explosives (ATF) for investigative tracing of their sale histories. Guns reported to ATF provide a national sample of crime guns numbering in the hundreds of thousands annually (predominantly from urban jurisdictions), but they do not constitute a statistically representative sample for the nation given that gun tracing is voluntary (agencies trace guns as needed for specific investigations and/or analysis of illegal gun markets) and varies between agencies and over time [24, 27, 38–40]. Further, publicly available data on traced guns are limited to aggregate figures on basic types and calibers of the weapons, thus limiting the analyses that could be conducted as described below. The other national data sources included information on guns used in murders of police officers and mass murder incidents. Prior research has shown that AWs and LCM firearms are used in a higher share of these crimes, due presumably to their lethality and attractiveness to the types of offenders who commit these offenses [2, 4], and this has been a prominent issue in the AW debate. Information on firearms used in murders of police, including the type, make, model, and caliber of each weapon, was obtained from the Federal Bureau of Investigation (FBI), which compiles these data from reports by police agencies throughout the country. Information on firearms used in mass murder shooting incidents was collected from lists and reports compiled by several organizations since there is no single official data source that regularly provides detailed and comprehensive information on mass murders and the guns used in these incidents [41–50]. Consistent with many prior studies of this issue, firearm mass murders were defined as incidents in which four or more people were murdered with a firearm, not including the death of the shooter if applicable and irrespective of the number of additional victims shot but not killed. This increased the number of sources that could be used to gather information. As described below, however, detailed weapon information could not be found in public sources for many of the cases.

Methods

There is no universal definition of an AW that applies across current and past AW laws. For example, the expired federal ban and some current state laws define AWs as having two military-style features, whereas other state bans and a recent (2013) proposal for a new federal ban use a one feature criterion [2, 51]. For this study, AWs were defined based on the weapons that have most commonly been identified as such based on the old federal ban, current state laws, and the recently proposed federal ban. This list included more than 200 make-model combinations covered by either of the federal lists (2004 and 2013) or at least two of the state laws. Based on preliminary analyses showing that most recovered AWs are assault rifles (as opposed to assault pistols or assault shotguns), an additional ceiling estimate of AW use was calculated based on the prevalence of semiautomatic rifles. This was also done to compensate for imprecision in the AW estimates (due, for example, to missing or partial gun model data, lack of information about the specific features or configurations of the weapons that could affect their AW status, and possible omissions from the operational AW list).

Use of guns with LCMs could only be measured precisely for the Syracuse, Baltimore, and Richmond analyses, which are based on data sources having an indicator for magazine capacity (which is typically

missing from police gun databases), and some of the mass murder incidents. For most analyses, use of LCM firearms was approximated based on recoveries of semiautomatics that are commonly manufactured and sold with LCMs, referred to below as LCM-compatible firearms. Identification of these models was based on gun catalogs (such as the *Blue Book of Gun Values* and *Gun Digest*) and examination of gun manufacturers' websites. This method likely overstates LCM use to some degree since many LCM compatible firearms can also be equipped with smaller magazines. As a rough guide, inspection of all recoveries of a small number of LCM-compatible handgun models in the Baltimore data revealed that approximately four of five were equipped with LCMs. Conversely, LCM use can also be undercounted for guns that were missing complete model information or equipped with aftermarket LCMs, which are available for some guns not sold with LCMs at retail. LCM use was not estimated for Rochester and Sacramento since New York and California have had longstanding restrictions on magazines with more than ten rounds (hence, it seems less likely that LCM-compatible guns recovered in those jurisdictions were actually equipped with LCMs).

Data were collected from 2014 through 2016. Current estimates of AW and LCM use were developed using the most recent 2–3 years of data from the local police databases and ATF data. Data spanning the most recent 5–6 years were used to generate contemporary estimates of AW and LCM use in murders of police and mass murders due to the rarity of these events. As described below, some data sources were also used to estimate trends in the use of semiautomatic rifles and LCM firearms since the expiration of the federal ban. Reported figures highlight AWs and LCM firearms as a share of crime guns in order to control for differences in the volume of gun crime and overall gun recoveries between places and over time. Other noteworthy aspects of the data and analyses are discussed below.

Results

Local Analyses

Results of the local analyses are presented in Table 1. For each site, estimates are based on data spanning different portions of the 2011–2014 period. The number of guns analyzed ranged from 281 in Syracuse to 4994 in Kansas City and totaled 21,551 across all data sources.

Estimates of the prevalence of AWs among crime guns ranged from a low of 2.4% in Baltimore to a high of 8.5% in Syracuse. Assault rifles (e.g., variations of the AR-15 or AK-47) accounted for the majority of AWs in all sites and more than three-quarters in all but one (Richmond). The remaining AWs consisted entirely (or nearly so) of assault pistols (e.g., the TEC-9 or TEC-22). The share of crime guns consisting of semiautomatic rifles of any sort is also displayed in Table 1 for localities that had gun databases with gun-type designations (i.e., handgun/rifle/shotgun, semiautomatic/non-semiautomatic). These estimates ranged from a low of 4.1% in Hartford to 12.4% in Rochester but were less than 9% for most cities. (The Milwaukee estimate is based on the percentage of crime guns that were rifles of any sort as semiautomatic/non-semiautomatic designations were unavailable.) As noted, the semiautomatic rifle estimates, which include both AW-type and non-AW-type rifles, provide a likely ceiling for estimates of AW prevalence.

The percentage of crime guns clearly equipped with an LCM (including AWs and other high-capacity semiautomatics, most of which are pistols) was 16.5% in Baltimore during the 2012–2014 period, but this figure rose to 21.5% for guns that were connected to a violent crime. These findings are similar to those from a recent news report (involving a separate and independent analysis of Baltimore data) indicating that 18.4% of guns recovered in Baltimore had LCMs for the period of 2010 through 2016 [52]. In Richmond, 22% of crime guns were equipped with LCMs during 2008 and 2009 based on data collected by the Virginia State Police and initially reported by *The Washington Post* [14] (the *Post's* reported figures have been reanalyzed here to focus on the most recent available years and to assess trends). Crime guns were least likely to be equipped with LCMs in Syracuse (14.6%), where New York State LCM restrictions have been in effect since the early 2000s.

For the other sites, the prevalence of LCM-compatible guns ranged from 22.2% in Hartford to 36.2% in both Kansas City and Seattle, with the majority of the estimates (3 of 5) higher than one-third. In most of these cities, the prevalence of LCM guns was similar whether focusing on all guns or those connected to a violent crime. In Hartford, however, 30% of violent crime guns were LCM compatible in contrast to 22.2% for all guns. Further, a supplemental analysis of guns linked to assault-

Table 1 Prevalence of assault weapons, semiautomatic rifles, and semiautomatics with large-capacity magazines among guns recovered by police: estimates for selected cities and years

Location and sample	Assault weapons as % of guns	Semiautomatic rifles as % of guns	Semiautomatics with large-capacity magazines as % of guns
Hartford, CT (2011–2012, *N* = 854)	2.6%	4.1%	22.2% overall, 30% for guns linked to violent crime
Rochester, NY (2012–July 2014, *N* = 1687)	4.9%	12.4%	Not estimated
Syracuse, NY (2012–May 2014, *N* = 281)	8.5%	12.1%	14.6%
Baltimore, MD (2012–Sep. 2014, *N* = 4680)	2.4%	5.4%	16.5% overall, 21.5% for guns linked to violent crime
Richmond, VA (AW analysis: 2012–2013, *N* = 1180) (LCM analysis: 2008–2009, *N* = 1960)	2.7%	Not estimated	22.0%
Minneapolis, MN (2012–Aug. 2014, *N* = 2178)	3.4%	6.4%	25.1% overall, 46.3% for guns linked to shootings
Milwaukee, WI (Jul. 2013–Jun. 2014, *N* = 1868)	4.6%	< 9.4%	35.5%
Kansas City, MO (2012–Aug. 2014, *N* = 4994)	6.1%	6.3%	36.2%
Seattle, WA (2012–July 2014, *N* = 596 guns linked to violent crimes or weapons violations)	6.4%	7.9%	36.2%
Sacramento, CA (Aug. 2013–Jul. 2014, *N* = 1273)	6.0%	Not estimated	Not estimated

Estimates are based on general gun recovery samples except where noted. Estimates were similar for guns known to have been connected to violent crimes except where noted. Large-capacity magazine (LCM) estimates for Syracuse, Baltimore, and Richmond are based on known LCM recoveries (the Richmond estimates are based on Virginia State Police data initially reported by *The Washington Post*). Other LCM estimates are based on recoveries of LCM compatible firearm models. The Milwaukee semiautomatic rifle estimate is based on the prevalence of all rifles

related shootings in Minneapolis (using gunshot victimization data provided by Minneapolis police) revealed that 46.3% were LCM compatible, though this was based on a small sample (*n* = 80 guns).

National Analyses

Results of the national analyses are presented in Table 2. AW prevalence was approximated in the national ATF tracing data for 2012 and 2013 (*n* = 481,632) based on traces of guns in calibers .223, 5.56, and 7.62 mm. These are common calibers for AW-type semiautomatic rifles, though not all firearms in these calibers are AWs, and not all AWs fall into these calibers. This method nonetheless yielded an estimate of 5%, which is within the range of estimates provided by the local analyses. Further estimates of semiautomatic rifles and LCM firearms were not possible given the limitations of published tracing data.

Guns used in murders of police were analyzed for the years 2009 through 2013 (*n* = 219, excluding cases involving the officers' own weapons, which are often LCM firearms). AWs accounted for an estimated 13.2% of the firearms used in these crimes overall and varied between 8 and 18% from year to year. Virtually all of the AWs (97%) were assault rifles. Semiautomatic rifles overall accounted for 15.5% of the firearms used in these cases and ranged from 5 to 23% annually. LCM-compatible firearms more generally constituted 40.6% of the murder weapons, ranging from 35 to 48% annually.

AW and LCM use in firearm mass murders was examined for a sample of 145 incidents that occurred from 2009 through 2015 but could only be estimated within broad ranges due to high levels of missing weapons data in public accounts. AWs were used in at least 10.3% of these incidents. However, only 42 incidents had sufficiently detailed weapon information to make a definitive determination regarding AW use; among these cases, 35.7% involved AW use. All but one AW case involved an assault rifle. (A separate estimate for semiautomatic rifle use is not presented because only two additional cases clearly involved a semiautomatic rifle with an unclear or non-AW designation.) LCM firearms overall were involved in at least 18.6% of the incidents based on cases that involved clear possession of LCMs, AWs, or other LCM-compatible models. Although many additional cases involved semiautomatic firearms, an LCM coding could

Table 2 Prevalence of assault weapons, semiautomatic rifles, and semiautomatics with large-capacity magazines among national samples of guns recovered by police, guns used in murders of police, and guns used in mass murders

Data source and sample	Assault weapons as % of guns	Semiautomatic rifles as % of guns	Semiautomatics with large-capacity magazines as % of guns
Federal Bureau of Alcohol, Tobacco, Firearms, and Explosives (ATF): guns recovered by police and reported to ATF for investigative tracing (2012–2013, $N = 481{,}632$)	5%	Not estimated	Not estimated
Federal Bureau of Investigation: guns used in murders of police (2009–2013, $N = 219$)	13.2%	15.5%	40.6%
Public reports of firearm mass murders (4+ killed) (2009–2015, $N = 145$)	10.3–35.7%	Not estimated	18.6–57.4%

Assault weapon estimate for ATF data is based on reported firearms in calibers .223, 5.56, and 7.62 mm. LCM estimates are based on recoveries of LCM compatible firearm models in the FBI data and recoveries of both LCMs and LCM compatible firearms in the mass murder data

only be made for 47 cases, 57.4% of which involved an LCM firearm. The identified AW and LCM cases typically occurred in public locations (80%) and resulted in more than twice as many people shot on average as did other incidents (13.7 victims on average for AW-LCM cases versus 5.2 for other cases; t test p level < 0.01).

Trend Analyses

Trends in the use of AWs and LCM firearms since the end of the federal AW ban or the early post-ban years were also estimated using selected data sources that had sufficiently detailed weapon information and spanned the period of interest. First, trends in recoveries of semiautomatic rifles were used to approximate trends in crime with AWs using the FBI national data on police murders (2003–2013) and data from the following cities and time periods: Baltimore (2004–2014), Rochester (2004–2014), Syracuse (2004–2014), Milwaukee (2006–2014, based on all rifles), Seattle (2008–2014), Minneapolis (2006–2014), and Kansas City (2008–2014). In summary, these analyses (not shown) revealed little evidence of upward trends in the use of semiautomatic rifles across sites.

Second, trends in crimes with LCM firearms were estimated based on guns used in murders of police (2003–2013) as well as guns recovered in Baltimore (2004–2014), Richmond (2003–2009), and Minneapolis (2006–2014). Table 3 shows changes over time in the percentage of guns that were LCM firearms using the earliest and latest years of each data source. In relative terms, the prevalence of LCM firearms increased from 33 to 49% in the Baltimore, Minneapolis, and national (FBI) data (note that Maryland restricted LCMs with more than 20 rounds throughout this period and extended these restrictions to LCMs with more than 10 rounds in late 2013). The largest increase occurred in Richmond, where LCM firearms increased 111.5%, rising from 10.4% of recovered guns in 2003–2004 (the final years of the federal AW ban) to 22% in 2008–2009. Similar trends have also been reported for the state of Virginia overall [14]. All of these changes were statistically significant ($p < 0.05$) based on chi-square tests of the equality of proportions.

Discussion

Subject to caveats noted above, this examination of several national and local data sources suggests that AWs are used in between 2 and 9% of gun crimes in general with most estimates being less than 7%. Upper bound estimates of AW use based on semiautomatic rifles range from 4 to 12% in most data sources and are typically less than 9%. These estimates are broadly similar to those generated in the early 1990s prior to the federal AW ban [2], though they are perhaps somewhat higher on average. However, comparisons of these estimates with others should be made cautiously, as operational definitions of an AW have varied across studies and estimates presented here are based on the most contemporary definitions of AWs. One clearly notable

Table 3 Changes in prevalence of semiautomatics with LCMs: estimates for selected local and national data sources and time frames, 2003–2014

Data source/location	LCM firearm prevalence: early time period	LCM firearm prevalence: late time period	Change in LCM firearm prevalence
Baltimore crime guns	11.1% (2004, 2006, $N = 5369$ total firearms)	16.5% (2012–Sep. 2014, $N = 4381$ total firearms)	+ 48.6%**
Richmond, VA crime guns	10.4% (2003–2004, $N = 2413$ total firearms)	22.0% (2008–2009, $N = 1960$ total firearms)	+ 111.5%**
Minneapolis crime guns	16.8% (2006–2007, $N = 2564$ total firearms)	25.1% (2012–Aug. 2014, $N = 2178$ total firearms)	+ 49.4%**
National (FBI): guns used in murders of police	30.4% (2003–2007, $N = 224$ total firearms)	40.6% (2009–2013, $N = 219$ total firearms)	+ 33.6%*

Change in proportions statistically significant at $p < 0.05$ (*) or $p < 0.01$ (**)

Estimates are based on general gun recovery samples except where noted. LCM estimates for Baltimore and Richmond are based on known LCM recoveries (the Richmond estimates are based on Virginia State Police data initially reported by *The Washington Post*). The early period estimate for Baltimore excludes the year 2005 due to an unusually large number of guns appearing that year within the buyback/turn-in/safekeeping category. Other LCM estimates are based on recoveries of LCM compatible firearm models

recent change is that assault rifles, rather than assault pistols, now account for a substantial majority of AWs used in crime in contrast to prior estimates [2]. This implies an increase over time in the average lethality of AWs used in violence.

LCM firearms, which include AWs as well as other high-capacity semiautomatics, appear to account for 22 to 36% of crime guns in most places, with some estimates upwards of 40% for cases involving serious violence. These estimates are comparable to or higher than earlier estimates of LCM use. However, the higher-end estimates may overstate LCM use somewhat as most are based on measurement of LCM-compatible guns that may not all have been equipped with LCMs.

Consistent with prior research, this study also finds that AWs and LCM firearms are more heavily represented among guns used in murders of police and mass murders. AWs account for 13–16% of guns used in murders of police, while LCM weapons overall account for about 41% of these weapons. Estimates for firearm mass murders are very imprecise due to lack of data on the guns and magazines used in these cases, but available information suggests that AWs and other high-capacity semiautomatics are involved in as many as 57% of such incidents. Further, they are particularly prominent in public mass shootings and those resulting in the highest casualty counts.

Importantly, trend analyses suggest that LCM firearms have grown substantially as a share of crime guns since the expiration of the federal ban on AWs and LCMs. This implies possible increases in the level of gunfire and injury per gun attack during this time. Consistent with this inference, national statistics from the Centers for Disease Control and Prevention (CDC) and the FBI show that the ratio of gun homicides and assaultive non-fatal shootings to overall reported violent gun crimes (homicides, assaults, and robberies) rose from an average of 0.163 for 2003–2005 to an average of 0.21 for 2010–2012 (calculated from CDC [53] and FBI [54] data). This change was driven by non-fatal shootings, which have been trending upward since the early 2000s and recently reached their highest levels since 1995 [1]. The findings presented in this study suggest the possibility that greater use of high-capacity semiautomatics has contributed to this upward trend in shootings.

Further study would seem warranted on LCM use trends with additional jurisdictions and data sources. Research on this issue could be facilitated by more systematic efforts to collect detailed information on crime guns and magazines in local police databases as well as through national data collection systems like the Supplemental Homicide Reports and the National Violent Death Reporting System. Study of these weapons is also hampered by lack of public data on production of LCMs and LCM-compatible firearms. The need for better data on this issue may become more pressing if there continue to be significant changes in the lethality of commercially available firearms.

Additional research is also needed to quantify the effects that LCM use has on injuries and deaths from gun attacks—and by extension on the costs to society

from gun violence. Research suggests that gunfire attacks involving semiautomatics produce more lethal and injurious outcomes [2, 10, 17, 55] and that 4–5% of assault-related gunshot victims are wounded in attacks involving more than ten shots fired [2]. However, such evidence is extremely limited at present. Studies of this issue, combined with evaluation research on the effects of current state and local LCM laws, could provide additional insights into the efficacy of expanding LCM restrictions at the local, state, and/or national levels. Research illuminating the public health and safety benefits of AW-LCM restrictions could also inform the courts as they continue to adjudicate recent challenges to the constitutionality of these statutes. Although this study does not directly evaluate any AW-LCM law, it provides further evidence that the federal ban curbed the spread of high-capacity semiautomatic weapons when it was in place and, in so doing, may have had preventive effects on gunshot victimizations.

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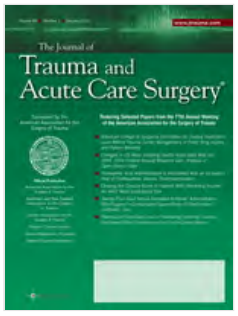
EXHIBIT "12"



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Changes in US mass shooting deaths associated with the 1994-2004 federal assault weapons ban: Analysis of open-source data

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New York, New York

Abstract

BACKGROUND: A federal assault weapons ban has been proposed as a way to reduce mass shootings in the United States. The Federal Assault Weapons Ban of 1994 made the manufacture and civilian use of a defined set of automatic and semiautomatic weapons and large capacity magazines illegal. The ban expired in 2004. The period from 1994 to 2004 serves as a single-arm pre-post observational study to assess the effectiveness of this policy intervention.

METHODS: Mass shooting data for 1981 to 2017 were obtained from three well-documented, referenced, and open-source sets of data, based on media reports. We calculated the yearly rates of mass shooting fatalities as a proportion of total firearm homicide deaths and per US population. We compared the 1994 to 2004 federal ban period to non-ban periods, using simple linear regression models for rates and a Poison model for counts with a year variable to control for trend. The relative effects of the ban period were estimated with odds ratios.

RESULTS: Assault rifles accounted for 430 or 85.8% of the total 501 mass-shooting fatalities reported (95% confidence interval, 82.8-88.9) in 44 mass-shooting incidents. Mass shootings in the United States accounted for an increasing proportion of all firearm-related homicides (coefficient for year, 0.7; $p = 0.0003$), with increment in year alone capturing over a third of the overall variance in the data (adjusted $R^2 = 0.3$). In

a linear regression model controlling for yearly trend, the federal ban period was associated with a statistically significant 9 fewer mass shooting related deaths per 10,000 firearm homicides (p = 0.03). Mass-shooting fatalities were 70% less likely to occur during the federal ban period (relative rate, 0.30; 95% confidence interval, 0.22-0.39).

CONCLUSION: Mass-shooting related homicides in the United States were reduced during the years of the federal assault weapons ban of 1994 to 2004.

LEVEL OF EVIDENCE: Observational, level II/IV.

Increases in firearm-related injuries, particularly mass-shooting related fatalities, in the United States have contributed to a polarizing and sometimes contentious debate over gun ownership and limiting weapons characterized as assault weapons.[1,2] Despite the increasing sense that there is an epidemic of indiscriminate firearm violence in our schools and public spaces, there is a paucity of public health evidence on the topic. Among a number of recommendations, a federal Assault Weapons Ban (AWB) has been proposed as a way to prevent and control mass shootings in the United States. In this article, we assess evidence for the effectiveness of such a ban in preventing or controlling mass-shooting homicides in the United States.

While mass shootings occur in other industrialized nations, the United States is particularly prone to these crimes. In a recent 30-year period, the United States had double the number of mass-shooting incidents than the next 24 industrialized nations combined.[3] Any public perception of recent increases in the number of these events is borne out by analysis of available data.[4] By one measure, there have been more deaths due to mass shootings in the United States in the past 18 years than in the entire 20th century.[5] While there is some debate about the role of mental illness in mass shootings,[6-8] many high-profile recent mass shootings (Aurora, CO; Roseburg, OR; San Bernadino, CA; Newtown, CT; Orlando; Las Vegas; Sutherland Springs, TX) have been characterized by the use of semiautomatic assault rifles,[9] leading some to advocate for restrictions on the manufacture and sale of these weapons.

While survey results indicate that researchers in criminology, law and public health rank an assault weapons ban as one of the most effective measures to prevent mass shootings, and that 67% of the US general population support such a ban,[10] the existing evidence on banning assault weapons is scant and sometimes contradictory. Most evidence is related to the Federal AWB of 1994, which made illegal the manufacture and use by civilians of a defined set of automatic and semiautomatic weapons and large capacity magazines. Formally known as "The Public Safety and Recreational Firearms Use Protection Act", the AWB was part of the broader "Violent Crime Control and Law Enforcement Act of 1994. The ban lasted 10 years, expiring in 2004 when the US Congress declined to renew it.

In a study soon following the implementation of the 1994 ban, researchers reported a 55% decrease in the recovery of assault weapons by the Baltimore City Police in the first 6 months of 1995, indicating a statistically significant 29 fewer such firearms in the population.[11] In a 2009 study based on ICD9 external cause of injury codes for patients younger than 18 years in the United States, 11 states with assault and large-capacity magazine bans, as well as other firearm laws, were compared with 33 states without such restrictions. The incidence of firearm injuries per 1,000 total traumatic injuries was significantly lower in states with restrictive laws, 2.2 compared with 5.9.[12] In contrast, a comprehensive 2001 evaluation of the AWB itself concluded that there was "no evidence of reductions in multiple-victim gun homicides or multiple-gunshot wound victimizations". The authors cautioned their results should be "interpreted cautiously" because of the short period since the ban's inception, and that future assessments were warranted.[13] More recent studies, while not primarily addressing the US Federal AWB have found results generally consistent with its effectiveness in preventing mass-shooting fatalities.[14,15]

We believe sufficient time has passed and enough data have accumulated to treat the period from 1994 to 2004 as a naturalistic pre-post observational comparison period for the association of the AWB with changes in mass-shootings in the United States. Because there is no authoritative source or registry, or even a widely agreed upon definition for these incidents, we obtained data from three open source references and restricted our analyses to only those incidents confirmed by all three sources. We assess evidence for the potential effectiveness of such a ban in preventing and controlling mass-shooting homicides in the United States. We hypothesized that the implementation of the Federal AWB contributed to a reduction in mass shooting deaths as measured by the number and rate of mass shooting fatalities before, during, and after the federal AWB.

METHODS

Mass incident shooting data were obtained from three independent, well-documented and referenced online sources: Mother Jones Magazine, the Los Angeles Times and Stanford University.[16-18] These sources have each been the basis for a number of previous studies.[19-26] Data from the three online open-source references were combined. Analyses were restricted to incidents reported by all three sources. Entries were further restricted to those for which four or more fatalities (not including the shooter) were reported, which meets the strictest definition of mass shootings as defined by the Federal Bureau of Investigation.[27,28] Yearly homicide data were obtained from the US Centers for Disease Control and Prevention Web-based Injury Statistics Query and Reporting System (WISQARS) an online database of fatal and nonfatal injury.[29] Because 2017 data were not yet available in the WISQARS system, data for firearm-related homicide data for that year were obtained from a separate online source.[30]

A variable was created to indicate the 1994 to 2004 period as the federal ban period. We attempted to identify incidents involving assault weapons. An assault weapon has been defined as semiautomatic rifle that incorporates military-style features such as pistol grips, folding stocks, and high-capacity detachable magazines.[31] In this study, assault weapons were identified using the text search terms "AK," "AR," "MCX," "assault," "assault," or "semiautomatic" in a text field for weapon details. These terms were based on descriptions of the federal assault ban legislative language.[32] The total number of mass shooting fatalities and injuries were aggregated by year and merged with the yearly firearm homicide data.

The rate of mass shooting fatalities per 10,000 firearm homicide deaths was calculated. For the years covered by the data sources, we calculated (1) the total and yearly number of mass-shooting incidents that met the strictest criteria and were confirmed by all three sources, (2) the number of all weapon (assault and nonassault weapons) mass-shooting fatalities, and (3) the case-fatality ratio of all-weapon mass-shooting fatalities per 100 total mass-shooting fatalities and injuries. The yearly case-fatality ratio was plotted with overlying Loess line for trend and standard error limits. We also plotted the yearly rate of mass shooting fatalities per 10,000 firearm-related homicides with an overlying simple linear model with year as the predictor for (1) the total period, and (2) for preban, ban, and postban periods.

We evaluated assumptions of normality and linearity of the data using graphical methods such as density plots and Q-Q normal plots as well as summary statistics. We tested the hypothesis that the federal ban period was associated with a decrease in the number and rate of mass-shooting fatalities in the United States with a multiple linear regression model, with total homicide-based mass-shooting fatality rate as the outcome variable, a dichotomous indicator variable for the federal ban period as the predictor variable, and year as a control variable for trend over time. We calculated the relative risk of mass shooting fatalities during the federal ban period compared to nonban periods by using the "epitab" function of the R "epitools" package. This estimate is based on the ratio of the fatality rate during the ban period divided by the fatality rate during the nonban period. All results are presented with two-sided *p* values with a significance level of 0.05 and/or 95% confidence intervals (CI). We conducted subgroup analysis with data restricted to incidents in which an assault-type weapon was explicitly noted.

We conducted analyses to test the sensitivity of our results to the choice of denominator with linear regression models controlling for trend with yearly rates based on (1) CDC WISQARS homicide data ending in 2016, (2) extrapolated CDC WISQARS homicide data for 2017, and (3) population denominator-based rates. We tested the robustness of our underlying modeling assumptions with an alternate mixed-effects generalized linear model of yearly mass shooting fatality counts with an observation-level random effect to account for overdispersion.

The study was determined to be exempt as nonidentifiable data. The study data and analytic code are available for download at
http://www.injuryepi.org/styled-2/
.

RESULTS

The three data sources listed incidents ranging in number from 51 (LA Times) to 335 (Stanford) and in dates from 1966 (Stanford) to 2018 (LA Times). There were a total of 51 reported cases of mass shootings between 1981 and 2017 confirmed by all three sources. Forty-four of these incidents met the strictest criteria for mass shootings (4 or more killed), totaling 501 all-weapon fatalities. In total 1,460 persons were injured or killed over the 37-year period, for a total case-fatality ratio of 34.3% (95% CI, 31.9-36.8). The overall rate of mass shooting fatalities per 10,000 firearm-related homicides was 10.2 (95% CI, 9.4-11.2). There was an increase in the all-weapon yearly number of mass-shooting fatalities in the United States during the study period, (Fig. 1) and evidence of a decrease in case fatality in the post-2010 period (Fig. 2). Incidents in which weapons were characterized as assault rifles accounted for 430 or 85.8% of mass-shooting fatalities (95% CI, 82.8-88.9). Weapons characterized as assault rifles accounted for *all* mass-shooting fatalities in 15 (62.5%) of the 24 (95% CI, 42.6-78.9) years for which a mass-shooting incident was reported, accounting for a total of 230 fatalities in those years.

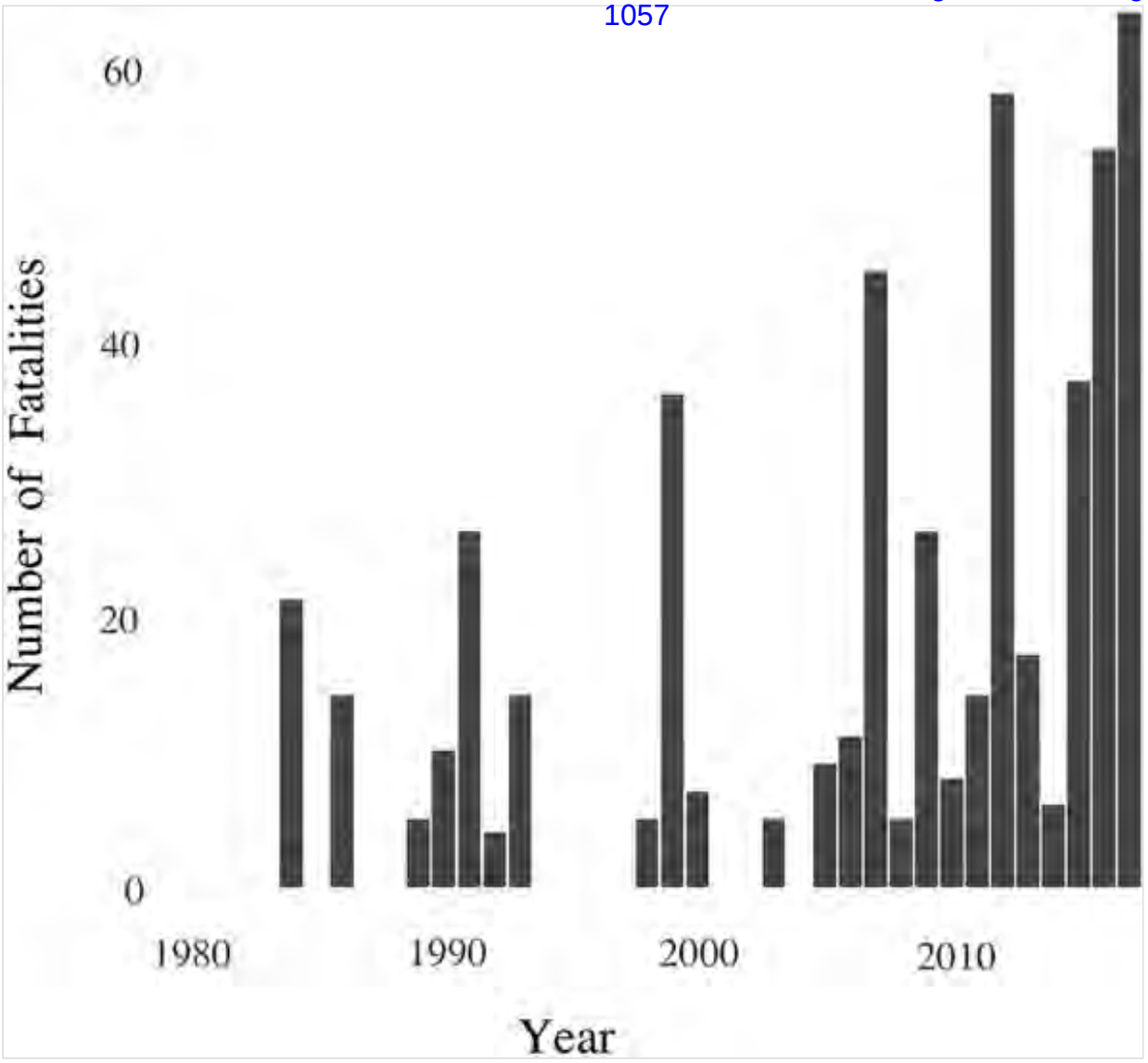

Figure 1. Mass shooting deaths. United States 1981-2017.

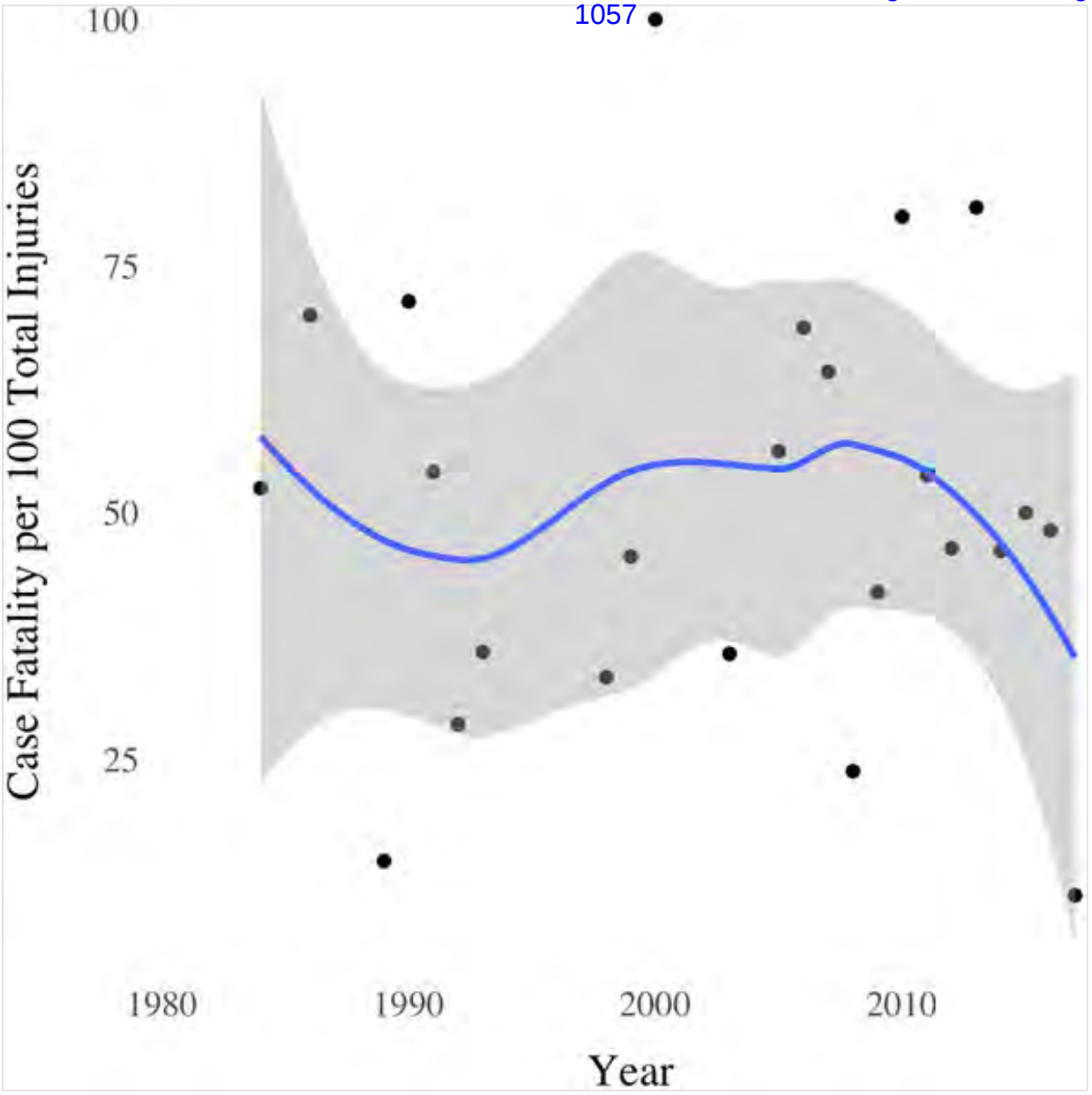

Figure 2. Case fatality per 100 total mass-shooting injuries with loess smoothing line for trend and standard error bounds. United States 1981-2017.

Between 1981 and 2017, mass shootings in the United States accounted for an increasing proportion of all firearm-related homicides, with increment in year accounting for nearly 32% of the overall variance in the data. During the years in which the AWB was in effect, this slope decreased, with an increase in the slope of yearly mass-shooting homicides in the postban period (Fig. 3). A similar pattern was evident in data restricted to those incidents characterized as involving assault weapons (Fig. 4).

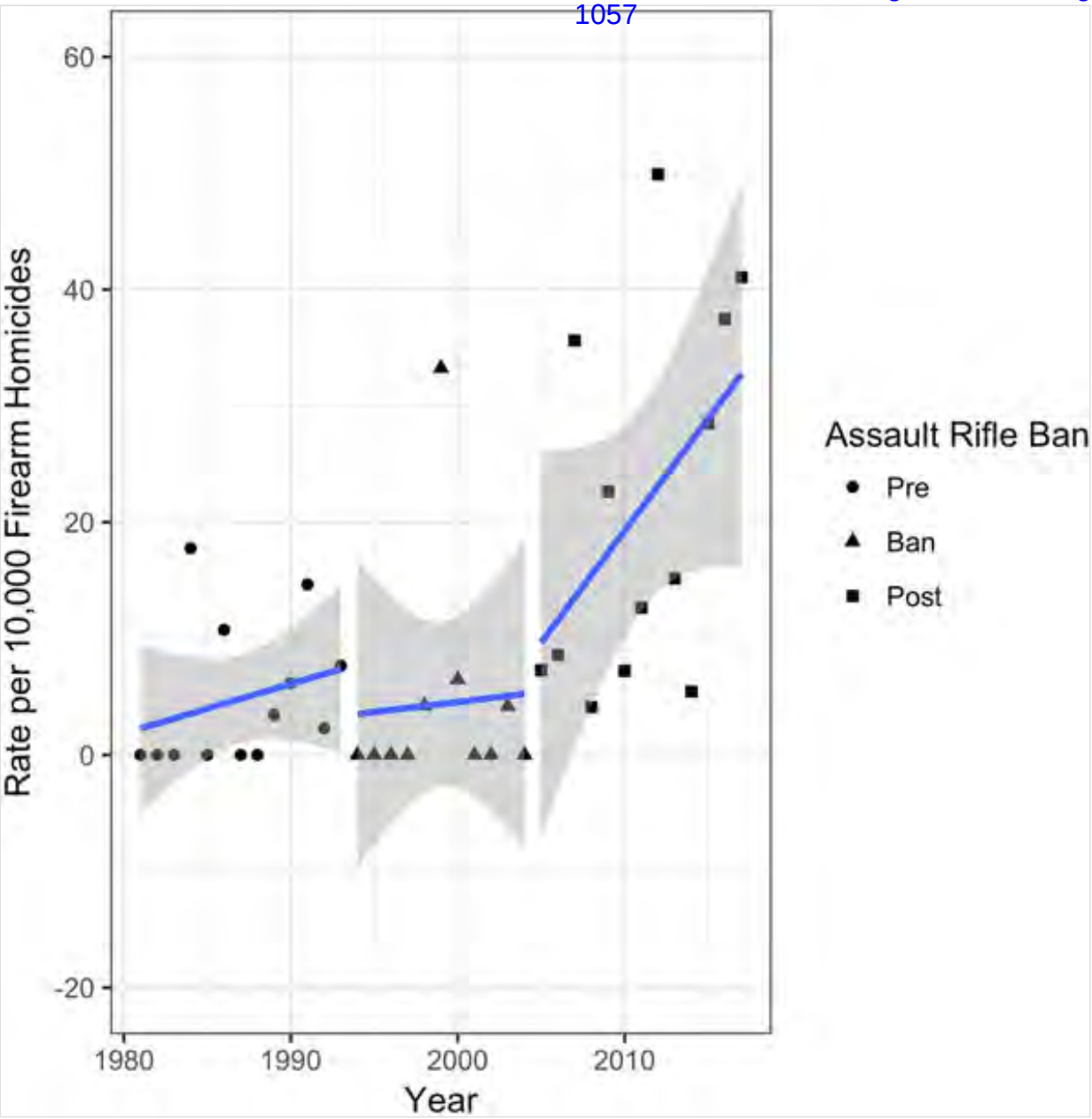

Figure 3. Mass shooting deaths per 10,000 firearm-related homicides with linear trends for preban, ban, and postban periods. United States 1981-2017.

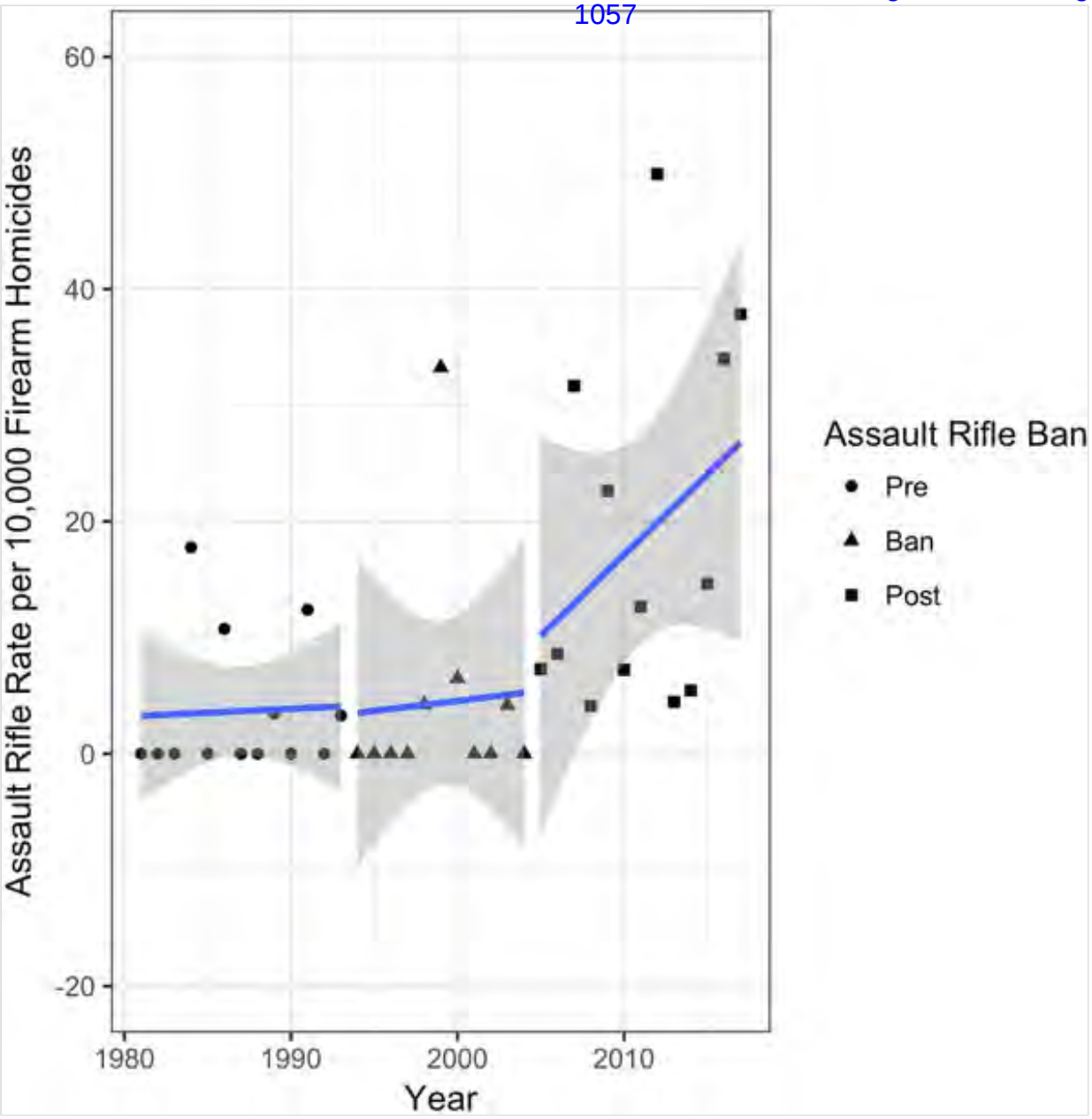

Figure 4. Mass-shooting shooting deaths per 10,000 firearm-related homicides restricted to incidents involving assault weapons with linear trends for preban, ban, and postban periods. United States 1981-2017.

In a linear regression model controlling for yearly trend, the federal ban period was associated with a statistically significant 9 fewer mass shooting-related deaths per 10,000 firearm homicides per year (Table 1). The model indicated that year and federal ban period alone accounted for nearly 40% of all the variation in the data (adjusted R^2 = 0.37). A subanalysis restricted to just those incidents characterized by the use of an assault weapon indicated that seven preventable deaths during the ban period were due to assault weapons alone (Table 2).

Variable	Estimate	Std. Error	*t*	*p*
(Intercept)	−1409.4	333.0	−4.2	0.0002
Year	0.7	0.2	4.3	0.0001
Ban Period	−8.6	3.9	−2.2	0.03

TABLE 1 Linear Regression Effect of 1994-2004 Federal Assault Weapon Ban on Mass-Shooting Deaths per 10,000 Firearm Homicides, United States, 1981-2017

Variable	Estimate	Std. Error	*t*	*p*
(Intercept)	−1219.7	333.9	−3.7	0.0009
Year	0.6	0.2	3.7	0.0008
Ban	−6.7	3.9	−1.7	0.09

TABLE 2 Linear Regression Effect of 1994-2004 Federal Assault Weapon Ban on Mass-Shooting Deaths Characterized by Use of Assault Weapon per 10,000 Firearm Homicides, United States, 1981-2017

The risk of mass shooting fatalities during the federal van period was 53 per 140,515 total firearm homicides compared with 448 per 348,528 during the nonban periods, for a risk ratio of 0.30 (95% CI, 0.22-0.39). The calculated risk ratio for the association of the federal ban period with mass-shooting fatalities as a proportion of all firearm-related homicides was 0.29 (95% CI, 0.22-0.29), indicating that mass shooting fatalities were 70% less likely to occur during the federal ban period.

The results of our sensitivity analyses were consistent with our main analyses for total mass shooting fatalities. In a linear regression analysis controlling for yearly trend and restricted to the period ending in 2016 using just CDC WISQARS homicide data as the denominator, the effect of ban period was associated with a statistically significant eight fewer mass shooting related deaths per 10,000 firearm homicides per year (coefficient for ban period, 8.0; $p = 0.05$). In a similar model using extrapolated CDC WISQARS homicide data for 2017 instead of Online Gun Violence Archive data as the denominator, the effect of ban period was associated with a statistically significant 9 fewer mass shooting related deaths per 10,000 firearm homicides per year (coefficient for ban period, 8.6; $p = 0.03$). A model based on the total yearly US population as the denominator, the effect of ban period was associated with a statistically significant 0.4 fewer mass shooting related deaths per 10,000,000 population (coefficient for ban period, 0.4; $p = 0.02$).

The results of a mixed-effects generalized linear Poisson model of yearly mass shooting fatality counts with an observation-level random effect to account for overdispersion were very similar whether the offset variable was the number of total firearm deaths or the population size. In either case, the assault weapons ban period was associated with an approximately 85% reduction in mass shooting fatalities (Table 3).

	Homicide Offset		Population Offset	
Variable	Estimate	95% CI	Estimate	95% CI
Year	0.6	0.2	3.7	0.0008
Ban	−6.7	3.9	−1.7	0.09

Effect of 1994–2004 federal assault weapon ban on mass-shooting death counts. United States, 1981–20017.

TABLE 3 Exponentiated Coefficients Generalized Linear Poisson Model

DISCUSSION

Recently, 75% of members of the American College of Surgeons Committee on Trauma endorsed restrictions to "civilian access to assault rifles (magazine fed, semiautomatic, i.e., AR-15),"33 and 76% of the Board of Governors were in favor of a limit to "[horizontal ellipsis] civilian access to ammunition designed for military or law enforcement use (that is, armor piercing, large magazine capacity)."34 In 2015, the American College of Surgeons joined seven of the largest most prestigious professional health organizations in the United States and the American Bar Association to call for "restricting the manufacture and sale of military-style assault weapons and large-capacity magazines for civilian use."35 This analysis adds evidence to support these recommendations.

No observational epidemiologic study can answer the question whether the 1994 US federal assault ban was causally related to preventing mass-shooting homicides. However, this study adds to the evidence by narrowly focusing our question on the potential effect of a national assault weapon ban on mass shootings as measured through the lens of case fatality. While the data are amenable to a number of additional analyses, such as stratification by location (e.g. school vs. nonschool) or by characterization of large-capacity magazines versus non large-capacity magazine, we chose to focus only on year of occurrence and total number of fatalities. In this way, we relied on the least subjective aspects of the published reports. We believe our results support the conclusion that the ban period was associated with fewer overall mass-shooting homicides. These results are also consistent with a similar study of the effect of a 1996 ban on assault type weapons in Australia after which mass-shooting fatalities dropped to zero.36

While the absolute effects of our regression analyses appears modest (7 to 9 fewer deaths per 10,000 firearm-homicides), it must be interpreted in the context of the overall number of such fatalities, which ranges from none to 60 in any given year in our data. However, if our linear regression estimate of 9 fewer mass shooting-related deaths per 10,000 homicides is correct, an assault weapons ban would have prevented 314 of the 448 or 70% of the mass shooting deaths during the nonban periods under study. Notably, this estimate is roughly consistent with our odds ratio estimate and Poisson model results.

Our results add to the documentation that mass shooting-related homicides are indeed increasing, most rapidly in the postban period, and that these incidents are frequently associated with weapons characterized as assault rifles by the language of the 1994 AWB. We did not find an increase in the case fatality ratio of mass-shooting deaths to mass-shooting injuries. This might at first seem counterintuitive and paradoxical. The destructive effect of these weapons is unequivocal. They are engineered to cause maximum tissue damage rapidly to the greatest number of targets. However, it may be that the use of these kinds of weapons results in indiscriminate injury with additional rounds more likely to injure more people increasing the denominator in a case-fatality ratio. By contrast, the use of nonassault weapons may result in more precise targeting of victims. It is also possible that improvements in trauma care are driving down case fatality.37 Also, it is worth noting that in absolute terms, there were many more fatalities outside the ban period and that survivable injury comes with its own physical, emotional, and economic costs, which have been estimated at US $32,237 per hospital admission.38

Despite US federal funding restrictions on firearm-related research dating to 1996,39,40 there is a small but growing number of analyses of mass shooting violence in the United States. Many articles have focused on the mental health aspects of these incidents,41-43 or on social effects like increased firearm acquisition following mass shootings.44,45 However, fewer studies have taken a strictly public health or clinical approach. Among these, an autopsy-based study of the incidence and severity of mass-shooting casualties concluded the wound patterns differed sufficiently from combat injuries to require new management strategies, indicating there is much to be learned from a systematic epidemiological perspective.46 Recently, there have been calls to remove such funding restrictions from both academics and elected officials from across the political spectrum.47,48

Our choice of data and analytic approach may reasonably be debated. We chose to base our analyses on the yearly rate of mass shooting fatalities per 10,000 overall firearm homicides. This is not a population-based risk estimate, but is in fact a risk as commonly used in the epidemiologic literature which is essentially a probability statement, that is, the number of events that occurred over the number of times that event could occur. It is the risk of a homicide occurring as a result of a mass shooting. It may be considered a strong assumption to build mass shooting death rates based on the overall firearm homicide rate. The demographics of most homicide victims may differ appreciably from those of mass shooting victims. We selected this approach from among a number of imperfect potential denominators, believing that basing the rates on the number of firearm-homicides partly controls for secular trends in overall homicides and firearm availability. Our sensitivity analyses indicate that our results were robust to most any choice of denominator. We chose linear regression as our primary model because it was straightforward, accessible to most readers, accounted for linear trends in the data, and returned results in the metric in which we were most interested, that is, changes in the rate of fatalities. Our comparative Poisson model results were essentially consistent with the primary model.

These analyses are subject to a number of additional limitations and caveats, primary among which is that there is no authoritative source of data on mass shooting, and any one source may be biased and incomplete. It was for this reason that we chose to combine three independent sources of data, each with its own strengths and weaknesses, and base our analyses only on those numbers that were verified by all three sources. We further restricted our analyses to only the number of fatalities and the year in which the incident occurred, and to the strictest definition of mass shootings as defined by the Federal Bureau of Investigation.27,28 Even with this approach, the data remain imprecise and subject to differing definitions. We attempted to compensate for this by framing our questions as precisely as possible, following the advice of the scientist and statistician John Tukey to pursue, "[horizontal ellipsis] an approximate answer to the right question ...(rather) than the exact answer to the wrong question..."

In this study, we failed to falsify the hypothesis that the AWB was associated with a decrease in mass shooting fatalities in the United States. However, it is important to note that our model did not include important and potentially confounding factors like state-level and local differences in assault weapon laws following the sun downing of the federal AWB. Additional analyses including such variables and using approaches like propensity score matching and regression discontinuity 49 with data further aggregated to state and local levels are necessary to test the strength and consistency of our results.

Federally referenced denominator data were not available for the last year of the study. We chose to use data from the Online Gun Violence Archive to account for firearm homicide in 2017. This resource is a nonpartisan not-for-profit group founded and maintained by a retired computer systems analyst and gun advocate.50 The alternative would have been to extrapolate from the CDC data, but the 15,593 firearm-related homicides reported by the Online Gun Violence Archive in 2017 was more consistent with the 14,415 reported by CDC in 2016 compared with the 11,599 predicted by an extrapolation and returned more conservative estimates of the increased rate of recent mass shootings. We note there were many years in which the number of mass-shooting fatalities is listed as zero. There were, in fact, fatalities and incidents in those years that could meet a definition of mass shooting, but they were not reported by all three sources, or did not meet the strict criteria we set for this analysis.

An assault weapon ban is not a panacea, nor do our analyses indicate that an assault weapon ban will result in fewer overall firearm-related homicides. It is important to recognize that suicides make up the majority of firearm-related deaths in the United States, accounting for 60.7% of 36,252 deaths from firearms in 2015.51 However, while this is a critically important issue in its own right, suicides differ fundamentally from mass-shootings, and are unlikely to be affected by an assault weapons ban. Also, compared with the 501 mass-shooting fatalities we counted, there were 489,043 firearm-related homicides in the United States. Public health efforts should be directed at reducing all gun violence and must be multipronged, including targeted initiatives to address mental illness and reducing access to weapons in those with a propensity for violence. However, taken in the context of the increase in mass shootings in the United States, these results support the conclusion that the federal AWB of 1994 to 2004 was effective in reducing mass shooting-related homicides in the United States, and we believe our results support a re-institution of the 1994 federal assault weapons ban as a way to prevent and control mass shooting fatalities in the United States.

DISCLOSURE

The authors have no conflicts of interest to declare.

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DISCUSSION

Ernest E. 'Gene' Moore, MD (Denver, Colorado): Thank you, Dr. Rotondo and Dr. Reilly. Can I please have the discussion video. [sounds of a gun shooting]. Well, that is the AR15 rifle. Literally, 30 potential lethal shots delivered within 10 seconds. Is this safe to have in our society?

I congratulate Dr. DiMaggio and his colleagues from NYU for their superb presentation on a very timely issue. The AAST has had a long-term interest in reducing gun violence in the United States, and has recently published our 14-point approach. Access to assault rifles is one of them. At a reductionist level, mass shootings are the net result of (1) a deranged person intending to kill random individuals in a populated area, and (2) the use of an assault rifle. Since we seem to be unable to identify the active shooter preemptively, we are left with the alternative solution of eliminating the weapon.

The presentation today provides evidence that a federal assault weapon ban can reduce mass shootings. According to our recent national trauma surgeon surveys, three-fourths of us in the audience, including me, would like to believe the analysis; but I think we need to consider some of the potential limitations.

Many of these issues relate to the fact that research support for gun violence control in the United States remains frustratingly suppressed and fundamentally inadequate. The general lack of information, low quality of data, and need to merge data sets from diverse sources - medical, coroner, police, legal, and behavioral - compounded by scarce funding and public controversy, undermine research to inform policy and enlighten the public. The fact that you had to compare three open-access databases to be certain that the reported mass shootings occurred underscores this deficiency.

Furthermore, there is no definition of a mass shooting, although you employed perhaps the most acceptable at the moment - the FBI's definition. Could you explain for us the rationale for this definition?

You present an analysis of 44 events with four or more deaths, including the shooter, from 1981 to 2017 - a 36-year period; whereas, others suggest a much higher incidence, such as Klaveras, who reported 69 shootings of six or more over the past 27 years.

Identifying all known mass shootings per year during a study period would be useful to appreciate the overall trends, as your data somewhat understates the magnitude of mass shootings in the United States.

You employed the Gun Violence Archive to estimate homicides in 2017. Why did you not use this source for mass shootings? The Archive has reported an alarming 261 mass shootings - defined as six or more shot - thus far in 2018. Nonetheless, in the sample you studied, assault rifles accounted for greater than 85 percent of the fatalities, and this is the key issue.

You have evaluated the impact of the federal assault rifle ban by analyzing the rate of mass shootings per 10,000 firearm homicide deaths per year to adjust for confounders. This would assume that the factors influencing mass shootings are the same as those for homicides, which seems very unlikely. You have idicated that you analyzed mass-shooting fatalities per population per year; perhaps you could elaborate more about this analysis.

Another confounder as acknowledged in the presentation is the impact of individual state limitations on magazine capacity. The first state to enforce these limitations was New Jersey in 1990, and now at least eight states and Washington, D.C., have these restrictions in effect. How can we distinguish the effects of this policy? And could this be a potential bridge to ultimately reestablish a national assault rifle ban?

You have also calculated the case fatality of all weapons in mass shootings per 100 total shootings, finding a decrease since 2010. While you conjecture this may be due to indiscriminate injury from assault rifles or possibly attributed to better trauma care, I am uncertain how this is relevant to the issue of banning assault rifles. The Las Vegas shooting is a cogent example of how these data may be misleading.

Finally, there is the issue of so-called falsification that could be addressed by examining other causes of trauma mortality during this time period.

In sum, this study adds to overwhelming evidence that assault rifles are an essential component in the dramatic escalation of mass shootings in the United States. While the scientific data to support a federal ban on civilian assault rifles is imperfect due to inadequate research support, I submit collectively the existing information argues strongly for enactment of this measure, and compliment the authors for their timely contribution.

Sheldon H. Teperman, MD (Bronx, New York): Dr. DiMaggio, your home institution, Bellevue, plays a seminal role in the trauma center safety of our nation.

In fact, right now, your trauma medical director is not present with us, but he is at home on guard for the U.N. General Assembly. But in New York, we don't see long-gun injuries. New York has the Safe Act, and there is an assault weapons ban. So why is it so important to America's trauma center - Bellevue - that we see a national ban on assault rifles?

Charles E. Lucas, MD (Detroit, Michigan): Thank you for your nice presentation. How many of these incidents occurred in an inner-city environment, where most of the victims that we treat have received multiple wounds which were purposely inflicted in order to compete competitively for the distribution of heroin and other drugs? Also, how many of the assailants were African-American?

Martin A. Croce, MD (Memphis, Tennessee): Thank you. I want to commend the authors for an excellent study, and really, not so much to ask any questions but I rise to put out a plea to the membership that this issue is a public health problem.

This is not a right versus left problem, this is not a Second Amendment problem. This is a public health problem.

And to quote Wayne Meredith at one of the recent Board meetings, 'Our primary goal is to reduce the number of bullet holes in people." So I implore the Membership to correct this dearth of research that is going on about gun violence in order to promote a public health approach, so that we can reduce the number of bullet holes in people.

Deborah A. Kuhls, MD (Las Vegas, Nevada): And to carry on that thought, I would urge the authors to incorporate the public health data from the CDC when it is available, because part of the methodological issues for this paper is that one data set was used for a certain period of time.

But for the last year, the CDC data was not used because it was not available, so I would urge you to not only do that analysis, but I would also urge the Journal of Trauma to consider an update to that article when that is available. Thank you.

Charles DiMaggio, MPH, PhD (New York, New York): Thank you very much for all these comments and questions.

Dr. Moore, so with regard to your observation about the reductionist approach to looking at this particular issue, that puts me in the mind very much of the traditional epidemiologic triad of agent, host, and environment, and if you break one link in that connection, you can break the transmission. In this case, we could call assault weapons one link, whether it's agent or host, we can decide.

With regards to the rationale for the definition, I think it's reflective of the lack of research in this area.

A case definition is an essential and critical first step in any epidemiologic investigation, and you can see that we are barely there. I think the FBI definition makes sense, I think it's the oldest one, I think it's informed by expert consensus.

And I think all the other definitions are based in some form on that, which is why we chose it. And I would urge that if we are going to be doing this research going forward, probably it would be best if we all had the consensus that that be the definition.

Why did we not use the Gun Violence Archive to estimate some of these results, and why are our numbers so much smaller than some of the other numbers? I have to agree, our numbers are very much an under-count.

We restricted our analysis to these three databases. And so the limiting factor was the one database. And I can tell you it was the LA Times - they had the fewest number. And if it wasn't in the LA Times, then the other databases didn't contribute to this data set.

We felt that the important aspect of this particular study was to demonstrate the relative effects, merits or associations with the assault weapon ban as opposed to documenting the absolute numbers.

So the Gun Archive, for example, defines mass shootings as four or more deaths or injuries. That really raises the number of deaths that can be included. We didn't include it, but I think going forward we absolutely should.

With regard to the analysis using population denominators, we agree, actually, that gun homicides are an imperfect denominator. We also felt that population was an imperfect denominator. And again, as we keep on circling around, it has to do with the data in this case.

We did feel that gun homicides captured something about gun availability and criminality in the United States, although homicides themselves differ very much from these mass shooting fatalities.

We do note that our population-based results essentially mirrored the gun homicide results, indicating that, at least for the relative effects and benefits of the assault weapons ban, the results are robust and invariant to the choice of denominator in this case.

Can we distinguish local effects, and could this possibly be a bridge to reestablishing an assault rifle ban? The short answer is yes and yes. We can distinguish local effects.

We took a very broad approach on this particular study as a first pass on the data. But, there are data sources (and even within the data sources we used) where you can tease out local, municipal and state policies.

Also, we can link our data to other sources that have those variables. There are statistical methods available that will not only account for those variables, but also allow us to measure or estimate in some way the contribution of local or regional variation in these policies to the overall effectiveness.

The issue of the case fatality rate is very interesting and challenging. I want to note that there was a paper in JAMA on September 11th - just a couple of weeks ago - looking at mass shooter fatalities, that came essentially to the same conclusion - that there has been this recent decrease.

In our paper, in this write-up, we look at three potential explanations, and one of them is, first of all, it's just a matter of denominator. These are indiscriminate weapons.

You have someone shooting at a large group of people, and there are going to be more injuries and more casualties, and it just inflates the denominator in this case.

The second thing is, the obverse of that, is single-fire weapons, guns, are very personal weapons. They're usually characterized by someone who knows who they want to kill. And finally, we feel that perhaps there may be some improvement by the folks in this room in treating these.

I'm going to close at this point, given the time constraints.

KEY WORDS: Firearms; mass-shootings; assault weapons; epidemiology

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IMAGE GALLERY

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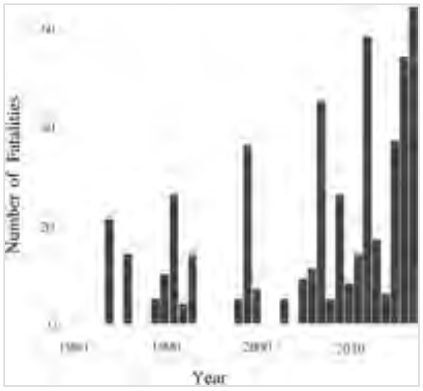


Figure 1

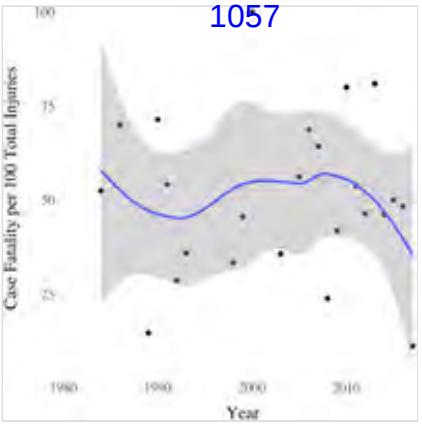


Figure 2

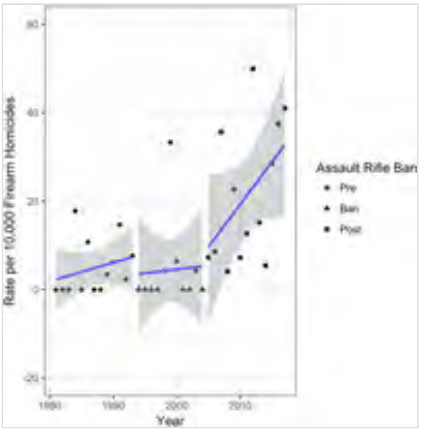


Figure 3

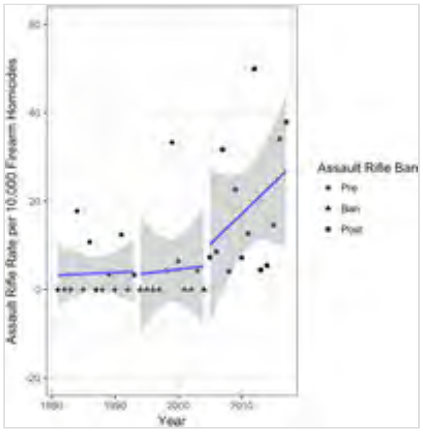


Figure 4

Variable	Estimate	Std. Error	t	p
(Intercept)	−1409.4	333.0	−4.2	0.0002
Year	0.7	0.2	4.1	0.0001
Ban Period	−9.6	3.9	−2.2	0.03

Table 1

Variable	Estimate	Std. Error	t	p
(Intercept)	−4219.7	333.9	−3.7	0.0009
Year	0.6	0.2	3.7	0.0008
Ban	−6.7	3.9	−1.7	0.09

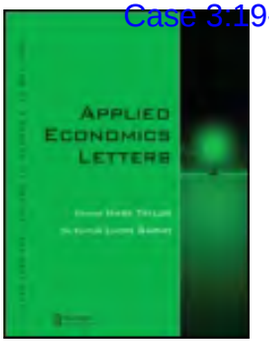
Table 2

	Homicide Offset		Population Offset	
Variable	Estimate	95% CI	Estimate	95% CI
Year	0.6	0.2	3.7	0.0008
Ban	−6.7	3.9	−1.7	0.09

Table 3

Back to Top

EXHIBIT "13"



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The impact of state and federal assault weapons bans on public mass shootings

Mark Gius

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The impact of state and federal assault weapons bans on public mass shootings

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The purpose of the present study is to determine the effects of federal and state assault weapons bans on public mass shootings. Using a Poisson effect model and data for the period 1982 to 2011, it was found that both state and federal assault weapons bans have statistically significant and negative effects on mass shooting fatalities but that only the federal assault weapons ban had a negative effect on mass shooting injuries. This study is one of the first studies that looks solely at the effects of assault weapons bans on public mass shootings.

Keywords: assault weapons ban; mass shootings

JEL Classification: K14; I12

I. Introduction

According to a recent report prepared by the Congressional Research Service (Bjelopera *et al.*, 2013), a public mass shooting has four distinct attributes:

(1) Occurred in a relatively public place.
(2) Involved four or more deaths – not including the shooter.
(3) Victims were selected randomly.
(4) Shooting was not a means to a criminal end, such as robbery or terrorism.

Examples of high-profile public mass shootings that fit this definition are Sandy Hook, Aurora, Fort Hood, Virginia Tech and Columbine. Many of the perpetrators in these mass shootings used multiple types of firearms. Contrary to popular belief, however, assault rifles were not the predominant type of weapon used in these types of crimes. In fact, according to a recent study, handguns were the most commonly used type of firearm in mass shootings (32.99% of mass shootings); rifles were used in only 8.25% of mass shootings (Huff-Corzine *et al.*, 2014). All data used in Huff-Corzine *et al*. (2014) is for the period 2001–2010.

Even though rifles are used in less than 10% of public mass shootings, one of the first pieces of legislation that comes up for consideration whenever there is a mass shooting is an assault weapons ban. For example, after the Sandy Hook shooting, there was a call for a revival of the 1994 federal assault weapons ban. This firearms ban was part of the Violent Crime Control and Law Enforcement Act of 1994 and outlawed semi-automatic weapons that had certain distinguishing features, such as pistol

grips, flash hiders and folding stocks (Koper, 2004). The ban was very narrow; only 118 gun models were banned under this law. In addition to banning certain types of guns, the 1994 law also prohibited large-capacity magazines, which held more than 10 rounds of ammunition. This prohibition affected many more types of guns than the assault weapons ban primarily because many semi-automatic weapons, including handguns, are capable of using large-capacity magazines.

The 1994 law had several loopholes and exemptions. All assault weapons and large-capacity magazines manufactured prior to the effective date of the ban were legal to own and transfer. In addition, only exact copies of the banned assault weapon models were banned; models without certain characteristics were still legal even though the rate of fire was the same. Finally, there was no prohibition against new, legal assault weapons being able to accept older, grandfathered large-capacity magazines. Hence, most new, legal models of assault rifles could use pre-ban large-capacity magazines. Given the above, the federal law was limited in its ability to affect firearm availability or crime.

Regarding state-level assault weapons bans, California was the first state to enact such a law in 1989. Several other states followed California's lead and enacted their own bans shortly thereafter (Connecticut, Hawaii and New Jersey), and then, in 1994, the federal ban was enacted. After the federal ban expired in 2004, all of the states that had bans prior to 1994 opted to continue with them.

Even though there have been numerous calls for assault weapons bans, both at the state and at the federal level, very little research has been conducted on the effects of these laws on mass shootings. Gius (2014), looking at data for the period 1980 to 2009, found that state-level assault weapons bans had no significant effects on gun-related murder rates, but that the federal assault weapons ban was associated with a 19% increase in gun-related murders. Chapman *et al.* (2006) examined the effects of Australia's 1996 gun law reforms on firearm-related homicides, including mass shootings, and found that, after enactment of the laws, there were declines in firearm-related homicides and suicides but no significant decrease in unintentional firearm deaths. It was also noted that there were 13 mass shooting incidents in Australia in the 18 years prior to the enactment of the stricter gun control measures but no mass shootings after passage of the laws. Koper (2004) looked at trends and correlations and concluded that the federal assault weapons ban's effect on gun-related violence was minimal at best. Duwe *et al.* (2002) examined the effects of right-to-carry laws on mass shootings. Using data for the period 1977 to 1999, the authors employed both Poisson and negative binomial models and found that right-to-carry laws had no statistically-significant effects on mass shootings. Finally, Lott and Landes (2000) looked at mass shooting incidents also for the period 1977 to 1997 and found that states that enacted right-to-carry laws had fewer mass shootings than states that did not enact such laws.

The purpose of the present study is to determine the effects of the federal and state assault weapons bans on public mass shootings. Using a Poisson, fixed-effect model and data for the period 1982 to 2011, it was found that both state and federal assault weapons bans had statistically significant and negative effects on mass shooting fatalities but that only the federal assault weapons ban had a negative effect on mass shooting injuries. This study is one of the first studies that looks solely at the effects of assault weapons bans on public mass shootings. Most prior studies examined the effects of other types of gun control measures on mass shootings (Lott and Landes, 2000; Duwe *et al.*, 2002; Chapman *et al.*, 2006) or the effects of assault weapons bans on much broader categories of crime (Koper, 2004; Gius, 2014).

II. Empirical Technique and Data

In order to determine whether assault weapons bans have any effects on public mass shootings, the following equation is estimated in the present study:

$$\begin{aligned} Y = {} & \alpha_0 + \alpha_1 \text{ state assault weapons ban} \\ & + \alpha_2 \text{ federal assault weapons ban} \\ & + \alpha_3 \text{ control variables} \\ & + \alpha_4 \text{ state fixed effects} \\ & + \alpha_5 \text{ year fixed effects} \end{aligned} \tag{1}$$

where Y is the number of deaths or injuries due to mass shootings. Control variables include the following: percentage of population that is black; population density; percentage of population that has a 4-year college degree; per capita median income; annual unemployment rate; percentage of population that is aged 18–24;

percentage of population that is aged 25–34 and per capita prison population. The state assault weapons ban variable is expressed as a dummy variable that equals one if the state has an assault weapons ban and zero otherwise. The federal assault weapons ban dummy variable equals one for the years 1995–2004.

All data are state level and were collected for the years 1982–2011. Socio-economic data were obtained from the *Statistical Abstract of the United States* and other relevant Census Bureau documents. Information on state-level assault weapons bans were obtained from Ludwig and Cook (2003), the Legal Community against Violence, the National Rifle Association and the US Bureau of Alcohol, Tobacco, Firearms and Explosives.

Data on mass shootings were obtained from the Mother Jones website and the *Supplementary Homicide Reports*, US Department of Justice. According to this data, there were 57 public mass shooting incidents from 1982 to 2011. For the assault weapons ban period (which includes the federal ban years and the years when states that had their own assault weapons bans), there were 24 public mass shootings; for the nonban period, there were 33 incidents. The average number of fatalities per mass shooting during the assault ban period was 7.5; during the nonban period, the average number of fatalities was 8.6.

III. Results and Concluding Remarks

A Poisson, two-way fixed-effect model, controlling for both state-specific and year-specific effects, was used to estimate the effects of state and federal assault weapons bans on public mass shootings. All observations were weighted by state population. Results are presented on Table 1.

These results indicate that fatalities due to mass shootings were lower during both the federal and state assault weapons ban periods. Although some prior research has shown either that assault weapons bans did not reduce crime or that they actually increased gun-related murder rates (Gius, 2014), the present study's focus on mass shootings shows the effectiveness of these gun control measures in reducing murders due to mass shootings. Regarding the injury regression, state-level assault weapons bans had no statistically-significant effects, but the federal ban had a significant and negative effect on mass shooting injuries.

It is important to note that these results are not unexpected. In 2012, for example, there were 72 fatalities due to mass public shootings. Of those 72, at least 30 were committed using a rifle. In the same year, there were 12 765 murders, of which only 322 were committed using a rifle. Rifles (assault weapons) are used much more frequently in mass shootings than they are in murders in general. Hence, any law that restricts access to rifles is likely to be much more effective in reducing mass shootings than it is in reducing murders in general.

Finally, it is important to note that mass shooting fatalities are a very small percentage of overall murders. Hence, even if a certain type of gun control measure was found to completely eliminate mass shootings (which assault weapons bans do not), the overall murder rate would decline by a very small

Table 1. Poisson fixed-effects regression results

Variable	Mass shooting deaths	Mass shooting injuries
State assault weapons ban	−0.59202 (−2.28)**	0.298 (1.16)
Federal assault weapons ban	−1.079 (−7.04)***	−1.733 (−10.10)***
Proportion of population that is black	65.66 (5.33)***	87.05 (6.20)***
Population density	−0.0177 (−2.73)***	−0.0542 (−7.18)***
Real per capita median income	0.000029 (0.48)	0.00021 (3.53)***
Proportion of population with college degree	1.66 (0.70)	−4.72 (−2.21)**
Unemployment rate	−0.0698 (−0.02)	−3.51 (−1.06)
Proportion of population >18 and <25	−55.21 (−5.94)***	−84.27 (−7.81)***
Proportion of population >24 and <35	−39.20 (−5.09)***	−20.59 (−2.65)***
Per capita prison population	−0.00362 (−4.62)***	−0.00067 (−0.85)
Log-likelihood	−1846.48	−2860.63

Notes: ** 1% < *p*-value < 5%; *** *p*-value < 1%.
Test statistics are in parentheses.
State and year fixed effects are not reported.

amount. Therefore, although the results of the present study indicate that assault weapons bans are effective in reducing mass shooting fatalities, their effects on the overall murder rate are probably minimal at best.

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EXHIBIT "14"

Homicides in Mexico and the Expiration of the U.S. Federal Assault Weapons Ban: A Difference-in-Discontinuities Approach

Luke E. Chicoine*

August 2016

Abstract

The year following the expiration of the U.S. Assault Weapons Ban (AWB), the homicide rate in Mexico increased for the first time in a decade. A difference-in-discontinuities model and a unique dataset are used to compare discontinuities generated by close mayoral elections on either side of the AWB expiration. The model finds a statistically significant increase in the firearm homicide rate following the expiration of the AWB. This effect is larger closer to the U.S.-Mexico border, is isolated to the timing of the expiration, and there is no evidence of a concurrent increase in non-firearm homicides or other violent crime.

JEL classification: F52, I18, K42

Keywords: Mexico; gun violence; homicides; assault weapons ban

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1 Introduction

After declining each of the ten years the U.S. Federal Assault Weapons Ban (AWB) was in place, the homicide rate in Mexico increased over 250 percent in the six years following the ban's expiration. Originally passed on September 13, 1994, as part of a larger anti-crime bill, the law had a ten-year sunset provision that was not extended by Congress. As a result, the law expired on September 13, 2004. The AWB prohibited the domestic sale and production of a large class of semi-automatic firearms and limited the size of a magazine to ten rounds. There were also a number of guns banned by name, including the AR-15 and AK-47. In Mexico, homicides committed with or without a firearm declined at similar rates prior to the AWB expiration. During the six years following the expiration of the AWB, the growth of homicides committed with firearms was more than four times larger than the growth of non-firearm homicides.

This paper examines whether the expiration of the AWB in the United States, and the increased availability of high powered firearms that accompanied the ban, contributed to the 2005 reversal of the homicide trend in Mexico. Using data from 2000 to 2006, estimates yield a 35 percent increase in the firearm homicide rate following the expiration of the AWB. This rules out the estimates being driven by President Calderon's 2007 deployment of federal troops, or 2007 changes in illicit narcotics prices. However, the estimates remain both quantitatively and qualitatively similar when the sample is expanded to include this period. The same model finds no evidence of a concurrent change in the non-firearm homicide rate, but an increase in the overall homicide rate in Mexico. Furthermore, there is no change in the underlying crime rate of six other major crimes and no evidence of changes in the illicit narcotics market that coincide with the expiration of the AWB. The increase in the firearm homicide rate is shown to be isolated to the timing of the expiration of the AWB, and the effect is found to be greater closer to U.S.-Mexico border crossings. These results rule out a secular increase in homicides and general criminal behavior, events that occurred at other points in time, and changes in the state of the narcotics market. The expiration of the AWB is isolated as the only plausible explanation of an increase in homicides in Mexico in 2005 that fits these criteria, only impacts homicides committed with a firearm, and has a larger impact closer to the United States.

These conclusions are generated by a difference-in-discontinuity model (DiRD), separately estimating a regression discontinuity (RD) model on either side of the expiration of the AWB. An RD framework, motivated by Dell (2015), is used to compare municipalities in which the incumbent mayoral party wins a close election, to those where the incumbent is defeated. In municipalities where the incumbent party is defeated the resulting dissolution of the status quo, and forthcoming policy shock, provides an environment in which friction between criminal entities and law enforcement likely increases; when the incumbent party remains in power the status quo is maintained. Close elections provide a setting in which municipalities in

Mexico are randomly sorted across the election threshold. The RD model is estimated for the post-AWB period, where the status quo effect is combined with the availability of high powered firearms from the United States, and a separate RD model is estimated for the years the ban was in place. To measure the impact of the expiration of the AWB following an incumbent defeat, these RD models are then differenced in a DiRD framework. Differencing the two estimates removes the consistent status quo effect of an incumbent losing, and the resulting estimate is the effect of having the newly available firearms in the post-AWB period, versus their unavailability in the pre-expiration period. Available evidence also demonstrates that there is no change in the status quo effect within either the pre-expiration or the post-expiration period. Only when the comparison crosses the expiration of the AWB is there evidence of an increase in the firearm homicide rate.

A consistent complication in identifying the causes of the escalating homicide rate in Mexico that occurred over the past decade is the local spikes in violence due to conflict between drug trafficking organizations, and more recently, with the federal government. An important innovation of this model is that municipalities from every part of Mexico are sorted to either side of the election discontinuity, in both the pre and post-expiration states of the AWB. This means that any specific conflict, and local spike in violence, is unlikely to be driving these estimates. A municipality with a close mayoral election is equally likely to be on either side of the status quo election threshold, and there is no statistically significant difference in violence leading up to an election that would bias this sorting. This is an important contribution to the literature that rules out specific localized conflict as an explanation of the model's estimates.

Finally, this work informs the policy debate on whether access to high-powered firearms, such as those banned under the AWB, can lead to a higher homicide rate. The evidence presented in this paper finds that, at least in certain settings, access to these firearms does lead to increased rate of homicide. These findings also highlight the potential international ramifications of domestic policy in today's globalized world. Although the effect is found to be larger closer to the border, it is important to highlight that the effects found in this paper are not isolated to regions of Mexico that border the United States. This escalates the importance of a comprehensive evaluation of policy effects, that can extend well beyond a nation's borders.

The remainder of the paper is organized as follows. Background on homicide trends in Mexico, the prevalence of firearm trafficking across the U.S.-Mexico border, and the current literature are summarized in Section 2. In Section 3, the underlying identification strategy and estimating model are described, and election and municipal data used in the paper are outlined in Section 4. In Section 5, the relevant tests are also shown to demonstrate that the density of the observations and predetermined outcomes at the election discontinuity are consistent across the expiration of the AWB, and the key results of the paper are discussed in detail. Section 6 concludes.

2 Background and Literature Review

Time series data suggest that the expiration of the AWB is related to the homicide rate in Mexico. The national homicide rate, shown as fatalities per 10,000 people, from 1990 to 2010 is plotted in Figure 1.[1] Homicide rate data are available for the five years prior to the implementation of the AWB, from 1990 to 1994. During this period the homicide rate in Mexico increased nearly 23 percent to 1.69 homicides per 10,000 people. In the United States, legal ownership of every firearm possessed prior to the law being enacted was extended into the post-implementation period; therefore, there was no immediate change in the stock of available firearms between 1994 and 1995. However, over each of the next ten years, when the AWB was in place, the homicide rate in Mexico declined, possibly due to a deteriorating stock of high-powered firearms and large capacity magazines banned under the AWB. This trend reversed in the first year after the ban's expiration, 2005. Over the next six years the homicide rate increased more than 250 percent. The initial deployment of federal troops by President Calderon, aimed at directly confronting drug trafficking organizations in Mexico, coincides with the 2007 decline in the homicide rate.[2]

The main analysis of this paper focuses on the 2004 expiration of the AWB, and to ensure that the post-deployment conflict is not responsible for any of the study's conclusions, this analysis focuses on the period ending in 2006. Due to data limitations – firearm homicide data are first available in 1998 – it is not possible to examine the 1994 implementation of the law. A simple counterfactual analysis of the trend can be used to illustrate the impact of the AWB expiration. The homicide rate from 1995 to 2006 is plotted in Figure 2, the solid line. The dashed line is constructed using data from 1994 to 2004 to estimate an OLS slope, and that slope is used to predict the counterfactual levels for 2005 and 2006, assuming a continuation of the pre-expiration trend. The same is done to construct the dotted line, using only the more recent 2000 to 2004 data. The difference between the 2006 data and the forecasted level is between 19.6 percent (2000 forecast) and 29.8 percent (1995 forecast) of the 2004 homicide rate. The same exercise is repeated with data for homicides committed using firearms, shown in Figure 3. The difference between the firearm homicide rate data and the forecasted level in 2006 is between 29.5 percent (2000 forecast) and 39.5 percent (1998 forecast) of the 2004 firearm homicide rate. Even though the post-2007 spike in the violence is massive, this descriptive analysis highlights the significant change in the homicide rate following the expiration of the

[1] The range of the figure is cut at 1.75 to better focus on the relevant time period. The full plot can be seen in Figure A.1.

[2] Prior to the election of President Calderon, Mexican officials had already publicly expressed concern regarding the expiration of the AWB. In January of 2006, an official with Mexico's Federal Preventive Police stated that, "Assault rifles such as the AR-15 and the AK-47 are by far the most popular weapons imported into Mexico by the drug cartels" (Tobar, 2006). Within the first month of the Calderon administration, Mexico's Deputy Attorney General had said that, "Re-imposing the U.S. Assault Weapons Ban would go a long way towards stemming the violence along the border. These weapons come from your country, we know that for a fact." He also directly tied the post-AWB firearms to the increase in violence stating, "there is a direct relationship between the flow of these weapons and the explosion of violence" (Hawley and Solache, 2007). Finally, in a 2010 speech to the United States Congress, President Calderon directly stated his belief that the expiration of the AWB has contributed to the violence in Mexico, and he directly asked congress to reinstate the ban (Charles, 2010; Sheridan, 2010).

AWB. The estimates from this simple descriptive analysis, for both the overall homicide rate and the firearm homicide rate, are similar to the estimates found by the DiRD model later in the paper.

If the reversal of trends is due to the expiration of the AWB, the change in the trend at the expiration of the AWB should be most prevalent for firearm homicides. The time series of the firearm homicide and non-firearm homicide rates are compared in Figure 4. The two trends are similar prior to 2004, if anything the decline of the firearm homicide rate is slightly steeper. However, immediately following the expiration of the AWB, the two rates diverge. In the first two post-AWB years, the firearm homicide rate increased 18 percent, while the non-firearm homicide rate declined less than one percent.[3] As the level of conflict increased over the next few years, this divergence continued to grow. This descriptive analysis is by no means definitive, but does provide evidence of a potential relationship between the expiration of the AWB and both the homicide rate and the firearm homicide rate in Mexico that warrants further investigation.

For the relationship between the AWB and the homicide rate in Mexico to exist, firearms banned by the AWB must be moving from the U.S. to Mexico in the post-AWB period. Although data on this topic are not widely available, both the statistics and related literature suggest a connection. Importantly, gun laws in Mexico are extremely restrictive. Firearms can only be sold through a single retailer in Mexico City, and only a single, permitted, small caliber handgun can be kept at home (Kopel, 2013). The most common type of firearms seized by Mexican authorities and traced back to the United States are the AR-15 and AK-47 models (Goodman and Marizco, 2010), which were banned during the AWB. In 2008, 25 percent of all firearms traced backed to the United States were variants of the AR-15 and AK-47 (Chu and Krouse, 2010). The trafficking of the AK-47 from the United States is particularly important, it is one of the most widely available firearms in the world, and is used by non-state groups in every region of the world.[4] The fact that the AK-47 is being so heavily trafficked from the United States suggests that the U.S. is providing the lowest cost sources for this rifle.[5] Although comparable pre-expiration data do not exist, between 2005 and 2014, the U.S. and Mexico have confirmed that tens of thousands of illegal firearms were trafficked from the United States and into Mexico.[6] This includes nearly 50 percent of all firearms recovered between 2006 and 2012;

[3]The increased lethality of the post-AWB firearms could have led this divergence. Evidence shown in Appendix Table A.3 documents that the rate of firearm homicides increased relative to the level of illegal arms possession and non-firearm homicides following the expiration of the AWB. Additionally, like the overall time trend, the number of municipal months with a high level of firearm homicides, four, five or ten, saw an annual increase of between 15 and 25 percent in 2005. The largest annual increase in each category since 1998, the initial year of the dataset (Appendix Table A.9).

[4]Small Arms Survey's Weapon Identification Sheet for the Kalashnikov AK-47.

[5]An AK-47 purchased in the U.S. can be sold in Mexico at 300 to 400 percent of the original price, with an additional markup for post-AWB models (Goodman and Marizco, 2010). Analysis of pre-expiration firearm catalogs (from River Arms Inc., Olympic Arms Inc., and Arma Lite Inc.) found that pre-expiration rifles had to be combined with additional parts that required assembly to meet "pre-ban" specifications, and that pre-expiration consumer prices were roughly equivalent to "law enforcement" models and post-AWB consumer prices. Therefore, the AWB added an additional cost of both time and money to build a pre-expiration model equivalent to what was available in the post-AWB period.

[6]The exact number of firearms recovered in Mexico and traced back to the United States during this period is 90,654 These data are from the Government Accountability Office (2009), and the Firearm Tracing System released by the Bureau of Alcohol, Tobacco, Firearms and Explosives' (ATF) Office of Strategic Intelligence and Information.

when Mexican authorities confiscated a total of 154,943 firearms, 99,691 were submitted for tracing, and 68,161 were traced back to the United States (Scroeder, 2013). These firearms were seized from criminals, actually submitted for tracing, and then had enough recoverable information remaining to be successfully traced back the the U.S., likely setting a lower bound for the number and fraction of firearms trafficked from the United States. In fact, examining variation in licensed firearm dealers in the United States, relative to the size of their domestic local market, McDougal et al. (2015) estimated that the firearms successfully traced back to the United States from Mexico represent less than one-fifth of all firearms bought with intent to traffic to Mexico.

Recent literature provides the framework in which such a movement of firearms would be expected. In an international setting, DellaVigna and La Ferrara (2010) find that arms embargoes tend to increase stock prices of firearm manufactures in low accountability settings. Knight (2013) finds that crime guns in the United States flow from low regulation states to high regulation states, and that criminal firearm possession is higher in states exposed to weaker firearm laws in nearby states. In the context of the U.S. And Mexico border, a movement of firearms from the low regulation states (i.e. Texas and Arizona) to the high regulation area of Mexico is consistent with the findings in this literature. Previous work has also found a positive relationship between gun prevalence and homicides (Cook, 1983; Duggan, 2001; Miller et al., 2002; Cook and Ludwig, 2006), and a survey of the literature by Hepburn and Hemenway (2004) concludes that there is a consistent positive relationship between firearm prevalence and homicides.[7] However, instead of focusing on the relationship between homicides and firearm prevalence, this paper examines the relationship between homicides and the type of firearms available. The most closely related work is Dube et al. (2013) who also investigated the impact of the expiration of the AWB in Mexico. They examined the effect in areas close to the border by exploiting geographic variation in state level bans within the U.S., and found that municipalities in Mexico closer to Arizona and Texas border crossings had higher rates of homicide, relative to those near California border crossings. These findings are consistent with the conclusions from this paper. However, the current study comes to this conclusion without relying on geographic variation for identification, lessening the likelihood of localized spikes in violence generated by inter-cartel conflict driving the result. Furthermore, this paper finds evidence that the effect was more widespread than previously documented, and that it exists even without the inclusion of municipalities closest to the border.

[7] In contrast to this literature, other work finds increased access to firearms through gun shows, which generally have less regulated sales, does not increase firearm-related deaths (Duggan et al., 2011). Lott and Mustard (1997) find that less restrictive gun carrying laws decrease crime; however, other work on this topic has not found the same result (Ludwig, 1998; Ayres and Donohue III, 2003).

3 Identification

3.1 Identification Strategy

The identification model in this paper considers crime to be a function of two inputs: the existence of a status quo local government and the relative strength between criminal groups and law enforcement. Increased access to high-powered firearms (e.g. the AK-47 and AR-15) in the post-AWB period could grow the relative strength of criminal entities in Mexico, in comparison to the pre-expiration period. If these firearms are in fact more deadly, in times of disharmony the effect should be most dramatic on the directly affected crime: homicides committed with a firearm. The ideal experiment would be to randomly sort areas into two groups. A treatment group in which local policy is shocked, possibly leading to increased conflict, bringing relative strength into play, and a control group where a consistent status quo is maintained. A comparison of the "treatment" and "control" groups in the post-expiration period would yield an estimated effect of dissolving the status quo and in a state with high-relative power of the criminal entities (i.e. after introducing more powerful firearms). Repeating this experiment in the pre-expiration environment yields an estimated effect of dissolving of the status quo in a low-relative power state. Under the assumption that the effect of dissolving the status quo is consistent across time, differencing the two effects then isolates the impact of the increased relative power, in this case, due to the newly available firearms.

Using local election data from Mexico it is possible to construct the experiment described above. In close elections, Lee (2008) shows that as long as some component of the vote cannot be precisely controlled, observations, in this case municipalities, are randomly distributed across the election threshold. This random distribution across the precise election threshold sorts municipalities into the two groups described above. Municipalities in which nothing changes, where the incumbent mayor's party is reelected, and municipalities in which the status quo is dissolved, where the incumbent party is defeated. Previous work exploits this type of variation using local mayoral elections in Mexico. Dell (2015) used an RD model to link efforts to combat illicit trafficking to political policies PAN (Partido Accion Nacional) pursued during President Felipe Calderon's term in office, and finds that these policing efforts led to an increase in homicides.[8] Unlike Dell (2015), the proposed estimating model does not rely on any specific party policies, but instead focuses on the success or failure of the incumbent political party in local mayoral elections. The defeat of the incumbent part, and the dissolution of the status quo, creates an environment in which conflict is more likely to occur. This could be due to explicit policing policies of the new mayor, as in Dell (2015), or more benign changes such as the severing of any implicit or explicit understanding between the outgoing administration

[8] President Calderon was in office from December 1, 2006 to November 30, 2012. Estimates in Appendix Table A.10 demonstrate that the link between PAN elections and homicides does not exist prior to the election of President Calderon.

and local criminal groups.[9] Conflict does not necessarily lead to an increase in violence; if law enforcement is sufficiently strong relative to the local criminal entities, increased interactions between law enforcement and criminals could lead to a reduction in crime. As described below, the effect of an incumbent defeat on firearm homicides is tested using an RD model in both the post-expiration (status quo + post-AWB relative strength) and pre-expiration (status quo + pre-expiration relative strength) periods. These estimates are then differenced in a DiRD model to isolate the impact of the increased non-government strength following the expiration of the AWB. A necessary assumption of this model is that impact of the change in the status quo, an incumbent defeat, is consistent across time. Evidence that this assumption holds in the data is shown later in the paper by estimating the DiRD model within both the pre and post-expiration periods.

To ensure that estimates are not driven by changes in Mexican federal government policy the main analysis focuses on evaluating this identification strategy for the 2000 to 2006 period. Results are also shown to be consistent for the extended time period from 1999 to 2010; however, the 2000 to 2006 sample focuses on the period of a single administration, that of President Vicente Fox.

3.2 Estimating Model

The effect of an incumbent loss, and dissolution of the status quo, is estimated using an RD model. This model is estimated separately for both the pre-expiration and post-expiration periods. The crime rates in the newly elected mayor's first full year in office, denoted at time-period t, are matched to the previous year's election results, denoted $(t-1)$. The running variable for the RD model is the vote margin $\left(v_{ms(t-1)}\right)$, which is calculated as the vote share of the highest ranked challenger minus the incumbent party's vote share. The vote margin is positive if the challenger wins the election and negative if the incumbent party is reelected.[10] The RD variable of interest is $D_{mst} = 1\left[v_{ms(t-1)} > 0\right]$, where D_{mst} is equal to one when the incumbent is defeated, and zero otherwise, yielding a sharp discontinuity. The following equation describes the RD model

[9] It is likely that low rates of violent crime, such as murder, are in the best interest of both the government and local organized crime. Data from municipalities in Mexico show there is a negative relationship between a party's "years in power" in a municipality and firearm homicide rates. This could be due to either explicit communication networks, or an implicit understanding of acceptable behavior from both sides. The removal of the incumbent party would severe either of these relationships. Details of the analysis are included in appendix section A.3.

[10] Mexico is a multiparty democracy in which individual candidates cannot run for reelection; therefore, reelection following a coalition administration is defined using the following conditions. If the incumbent mayoral administration is a coalition, the vote share is assigned to the incumbent party using the following rules. If the PRI, PAN and/or PRD parties are part of the coalition, the incumbent share is defined as the largest vote share of these "tier 1" parties. If no "tier 1" parties are part of the incumbent coalition, the largest vote share of the "tier 2" parties is assigned (PVEM, PANAL, PT, Convergencia). If none of these parties are part of the incumbent coalition, the largest vote share is assigned, or the incumbent share is equal to zero if no incumbent party is running for reelection. In 98.84 percent of elections, this is equivalent to assigning the vote share to the largest vote share of any incumbent party, regardless of tier. In the one percent of cases that a smaller member of the incumbent coalition outperforms a larger member, it is unclear how to define incumbent performance, these cases are not included in the baseline analysis. Estimates defining incumbent vote share as simply the largest incumbent party vote share regardless of tier can be seen in panel (d) of Appendix Table A.4.

that is estimated separately for the pre and post expiration periods.

$$ln\left(y_{mst}\right) = \alpha + D_{mst}\theta + \sum_{p=1}^{P} v_{ms(t-1)}^{p} D_{mst}\gamma_p + \sum_{p=1}^{P} v_{ms(t-1)}^{p}\gamma_p + X_{mst}\pi + ln\left(y_{ms(t-1)}\right)\rho + \tau_{st} + \varepsilon_{mst}. \quad (1)$$

The dependent variable is the log of the crime rate in municipality m, in state s, and year t. The coefficient of interest is θ, which measures the effect of a close incumbent loss on crime rates. However, as described in the previous subsection this estimate contains two components: (i) a time-invariant destruction of the status quo, and (ii) the relative strength of criminal groups. The second factor, relative strength, is possibly affected by the expiration of the AWB due to increased access to more powerful firearms. The polynomial of the running variable, the vote margin $\left(v_{ms(t-1)}\right)$, is allowed to vary on either side of the discontinuity. X_{mst} is a vector of municipal level control variables for crime rates, demographics, and additional election and party information.[11] These elections range over a number of years; therefore, a set of state-specific year fixed effects (τ_{st}) are included. When these state-year fixed effects are incorporated into the model, only state-year cells with at least five observations are used in the estimation.[12] Although it is not possible to include municipal fixed effects, each municipality holds mayoral elections every three years and not every election is a "close" election, a lagged dependent variable, $ln\left(y_{ms(t-1)}\right)$, is included to take into account the municipal level environment in which the outcome of interest is determined.

To separate the effect of an incumbent loss estimated in the above equation from a change in crime levels coinciding with the expiration of the AWB, the RD model described in equation (1) is combined with a difference-in-difference model to construct a DiRD model.[13] This model compares the change at the discontinuity in the post-expiration $(P_t = 1\,[t{\geq}2005])$ period to the same change during the period when the law was in place.

$$\begin{aligned} ln\left(y_{mst}\right) = \alpha + P_t D_{mst}\beta + D_{mst}\theta + \sum_{p=1}^{P} v_{ms(t-1)}^{p} D_{mst}\gamma_p + \sum_{p=1}^{P} v_{ms(t-1)}^{p}\gamma_p \\ + P_t\left[\sum_{p=1}^{P} v_{ms(t-1)}^{p} D_{mst}\mu_p + \sum_{p=1}^{P} v_{ms(t-1)}^{p}\mu_p\right] + X_{mst}\pi + ln\left(y_{ms(t-1)}\right)\rho + \tau_{st} + \varepsilon_{mst}. \end{aligned} \quad (2)$$

The model closely builds on equation (1). The dependent variable is again the log of the crime rate in municipality m, in state s, and year t. The set of control variables and trends remain the same, with the

[11] Election variables include the total vote share of the winning party, an indicator of whether a coalition of parties won the election, an indicator if the election occurred in the last half of the year, two sets of indicator variables for each of the nine most common parties in this sample (one for whether the party won the election and the other for whether the party was an incumbent), and a set of indicators for each year the incumbent party had been in power at the time of the election. Municipal characteristics include the portion of population that is male age 20 to 49, the logged values of the population density, GDP per capita, municipal government income per capita, infant mortality rate, a cubic measure of distance to the border, vehicular mortality rate, narcotics crime rate, theft rate, illegal weapons possession rate, rape rate, assault and battery rate, and the property damage rate.

[12] Estimates including all available municipalities using year fixed effects can be found in Appendix Table A.4.

[13] Grembi et al. (2016), Lalive (2008), and Leonardi and Pica (2013) also utilize similar DiRD strategies.

addition of allowing the vote margin polynomial to also vary across time periods. The percent change in the crime rate following the expiration of the AWB, relative to the pre-expiration period, is captured by β, the coefficient on the interaction between the post-expiration indicator (P_t) and the defeat of an incumbent indicator (D_{mst}). The DiRD model differences out the effect of the loss of the status quo, which occurs in both the pre and post-expiration periods, yielding the change in the effect that occurs at the time of the AWB's expiration. Observations are weighted by a triangular kernel; the kernel is interacted with the municipal population for all logged crime rates.[14] Optimal bandwidths are calculated using a cross-validation (CV) procedure (Ludwig and Miller, 2007; Imbens and Lemieux, 2008) that is adjusted to accommodate the DiRD model.

4 Data

4.1 Election Data

This paper utilizes a unique dataset of every mayoral election in Mexico between 1995 and 2010. Election data for a number of years, especially for the earlier years of the sample, are compiled by the *Instituto de Mercadotecnia y Opinion*, and *Elecciones en Mexico* has assembled information for the most recent years. Data available from electoral institutes of each state are also used to cross-reference the other sources and to fill in missing information.[15] Local mayoral elections generally occur every three years. This pattern can be seen in the first two columns of panel (a) in Table 1. The number of mayoral elections held the previous year is shown in the first column, and the fraction of the total number of elections in this time period is shown in the second column. The years above the line at the center of the table are pre-expiration, and the years below are post-AWB. The rotation of municipal elections every three years is visible in these first two columns. The year in which elections are held is determined at the state level, the smallest fraction of the country holds elections prior to 2000, 2003, 2006, and 2009. A significantly larger number of municipalities hold elections in the other two periods. The range from 1999 to 2010 includes four full electoral rotations, two on either side of the AWB expiration. The next two columns show a similar distribution for close elections, defined as elections in which an incumbent party either wins or loses by less than seven percentage points, the CV optimal bandwidth. This subsample yields a similar pattern to the previous two columns. The fraction of municipalities from each year that fall below the seven percent cutoff is shown in the last column of panel (a). The range is relatively consistent over the entire period. The years with the largest fraction of close

[14] Estimates using alternative weighting methods, including a uniform weights are shown in Appendix Table A.4. The table also contains a set of estimates that includes an interaction between the control variables (X_{mst})and the post-AWB indicator (P_t).

[15] Information from the *Instituto de Mercadotecnia y Opinion* can be found at http://imocorp.com.mx; and at http://www.eleccionesenmexico.org.mx for *Elecciones en Mexico*.

elections are balanced on either side of the AWB's expiration, the last year pre-expiration, and the first year post-expiration.

Sorting across the discontinuity is determined by the success or failure of the party of the incumbent mayor. The number of times an incumbent party lost a mayoral election over the four election cycles, from 1999 to 2010, is shown in panel (b) of Table 1 for each of the 1,864 municipalities. The municipalities are distributed across all possible outcomes, from having no incumbent losses (252 municipalities) to always voting challengers into office (130 municipalities). The incumbent party is not reelected in nearly half of the elections, 49.79 percent of the time. The frequency with which incumbents lose elections ensures that a sufficient number of incumbent losses will be included in the sample. The distribution of the municipalities guarantees that incumbent losses are spread across most of the municipalities in the sample; over 85 percent of municipalities have at least one incumbent loss during this period.

Additionally, the municipalities in which incumbents are defeated are distributed throughout the country. Over the period of the study there were a number of confrontations between cartels that led to local spikes in violence; a study focusing on geographical variation may be susceptible to bias generated by these events. Allowing municipalities to be sorted across the election threshold yields a sample in which all regions of the country are represented on either side of the election cutoff. In the two election cycles prior to the AWB expiration (1999 - 2004) each of the 31 states in the sample include both municipalities in which the incumbent won a close election, and those in which the incumbent lost. In the two election cycles following the expiration of the AWB (2005 - 2010), 29 of the 31 states include both types of municipalities, the two states that do not meet these criteria (Federal District and Baja California Sur) have a total of three close elections between them in the post-expiration period. In every part of the country incumbents both won and lost close elections, both prior to, and following the expiration of the AWB. The distribution of these elections are shown in Figure 5, for both the pre-expiration (Figure 5a) and post-expiration periods (Figure 5b).[16]

4.2 Municipal Data

The main analysis of this paper consists of annual observations for every municipality outside of the state of Oaxaca, from 2000 to 2006. A total of 1,864 municipalities.[17] Population and mortality (homicide and

[16] Municipalities where incumbents won reelection by less than seven percent twice during the specified time period are filled in black, and dark gray municipalities denote a single close incumbent win. The checkered pattern denotes municipalities in which there were two close incumbent elections, one won by the incumbent and the other lost. The municipalities shaded in light gray are those in which the incumbent lost a single close election, and municipalities filled in white with a black outline saw two close incumbent defeats. The gray lines represent state borders, and blank areas saw no close elections during the time period.

[17] Municipal government structure and mayoral elections in the state of Oaxaca differ from other states in Mexico, and a number of control variables are missing for these municipalities. There are over twice as many municipalities in Oaxaca than any other state in Mexico. This leads to Oaxaca having the smallest average population per municipality with only about a

vehicle-caused deaths) data are from Mexico's National System of Health Information (SINAIS – *Sistema Nacional de Informacion en Salud*). Data for both firearm homicides and non-firearm homicides are also obtained from SINAIS, and are available beginning in 1998. Non-firearm homicides are defined as the difference between the total number of homicides and those committed with the use of a firearm. Crime data and municipal data for all other control variables are from Mexico's National Institute of Statistics and Geography (INEGI – *Instituto Nacional de Estadistica y Geografia*). To minimize any potential corruption in the judicial process, crime data from the beginning of the sample to 2008 are for the crimes of suspected criminals. After 2008, the definition used by INEGI changed to crimes of the accused criminals. These definitions do not require a conviction, only a documented crime, lessening the possibility of corruption skewing the data.[18]

Means of key variables, separated by incumbent election status and time-period, are included in Table 2 for the years 2000 to 2006. The variables shown are time-invariant, election outcomes, or once lagged to allow for validity tests of the DiRD model in the following section. All rates represent occurrences per 10,000 people, and log calculations take the natural log of the rate plus one to maintain a consistent measure of zero when there are no occurrences.[19] Variables included in the table, beginning at the top, are three separate homicide rates, the logged firearm homicide rate (the main outcome of interest), the logged (total) homicide rate, and the logged non-firearm homicide rate. Logged crime rates included are theft, narcotics crime, illegal arms possession, assault and battery, property damage, and rape. These crime rates are also used as control variables, and to demonstrate that the post-AWB increase is isolated to firearm homicides. The municipal characteristics shown are also included in the main model as control variables. Distance is calculated as the driving distance in kilometers to the nearest border crossing, and area is recorded as square kilometers. The selection of variables includes those that take into account municipal level well-being and investment, such as the infant mortality rate, per capita GDP, and municipal income (taxes) collected.[20] The vehicular mortality rate is one of the few causes of death that has an age profile similar to homicides, and mostly young men are involved in violent crime, therefore, the fraction of the population that is male

third of the population of the next smallest state. Due to these traits, municipalities from Oaxaca are not included in the analysis.

[18]During the pre-2008 period, the illegal arms possession variable is constructed as the sum of illegal arms possession and violations of federal firearms law. The two variables are recorded separately only before 2008; combining the two variables during this period creates a consistent variable that can be used when the time period of the sample is expanded. Estimates of the paper's main analysis are not sensitive to the substitution of violations of the federal firearms law in the 2000 to 2006 period. See Appendix Table A.3.

[19]The paper's main estimates remain qualitatively similar, and results for firearm homicides statistically significant, with alternative rate calculations (i.e. 5,000, 50,000 and 100,000). The base estimates use a logged crime rate per 10,000 to ensure that the model does not overweight smaller municipalities with one homicide. Estimates with alternative rate calculations and municipal population cutoffs can be found in Appendix Table A.4.

[20]Distances are calculated using Google Maps. The furthest municipality is Isla Mujeres, which is 2,306 kilometers from the border. Municipal level GDP data are available in five-year intervals, and state level data are available annually. Annual GDP estimates for municipalities were imputed using trends in the share of state GDP between the five-year intervals. Only expenditure data, not municipal income data, are available for the municipalities in the Federal District.

between 20 and 49 is included. Election statistics include the years an incumbent party has been in power, fraction of municipalities won by PAN, and the vote share of the candidates from the three largest parties, PAN, PRI, and PRD.

5 Results

5.1 Validity Tests

Hahn et al. (2001) outline key assumptions that must be met to ensure proper identification of an RD model. Grembi et al. (2016) extend the necessary assumptions to the DiRD framework. In a cross sectional RD model the key assumption is that time-invariant and predetermined characteristics are equivalent within a sufficiently small range of the running variable, $E\left[x_m | v_m = v_m + e\right] \approx \left[x_m | v_m = v_m - e\right]$, most importantly this assumption must hold in the range of values that brackets the discontinuity threshold. Expanding this to the DiRD model only requires that any difference that may exist be consistent in both the pre (0) and post-treatment (1) periods, $\{E\left[x_{m1} | v_{m1} = v_{m1} + e\right] - E\left[x_{m1} | v_{m1} = v_{m1} - e\right]\} \approx \{E\left[x_{m0} | v_{m0} = v_{m0} + e\right] - E\left[x_{m0} | v_{m0} = v_{m0} - e\right]\}$. If the RD assumptions are satisfied within each time period, it is easy to see that the DiRD criteria will also be met. While it remains unrealistic to definitively test the assumption that the characteristics of observations on either side of the cutoff are identical, it is possible to demonstrate that any differences in the observable characteristics of municipalities near the cutoff are consistent over time, if they exist at all. These differences are first examined using the sample means presented in Table 2. The DiRD model is used to test the same set of variables, and examine whether the variation in these characteristics at the election threshold remained consistent across the expiration of the AWB. Finally, the McCrary (2008) density test can be expanded to accommodate the DiRD framework to test if there is a difference in the pre and post expiration densities around the cutoff.

To initially test the balance of the sample across the discontinuity the difference in the means across the election threshold are calculated separately for the pre and the post-expiration time periods, and are shown along with the t-statistic of the difference in Table 2. It is important to note that the characteristics being tested are either time-invariant (distance and area), or determined prior to the newly elected mayor's first full year in office. Only three non-election municipal characteristics across either time period have a statistically significant difference. They are the post-expiration lagged firearm homicide rate, the lagged theft rate, and the lagged illegal arms rate. For each of these differences, the level of crime is lower for the municipalities where the incumbent lost; if this has any impact on the rate in the following year, it would bias the model away from finding a result. However, as described above this is not the key difference that needs

to be explored. The key statistic of interest to the DiRD model is the difference of the differences, shown at the right hand side of the table, again along with the t-statistic of the difference-in-difference calculation. Only one of the three variables that exhibited a difference in the post-AWB period is statistically different from the pre-expiration period. However, this difference suggests a relatively lower level of illegal arms possession following an incumbent defeat in the post-AWB period. Again, likely making it more difficult to find an impact of illegal arms during this period. The differences that occur more frequently during each of the time periods seem to be the election outcomes. PAN are more likely to win close elections as the challenger, but consistently so across both time periods. Although the vote shares demonstrate some statistically significant differences within each time period, these differences are also consistent across time as seen in the difference-in-difference calculations.

These tests can be complemented by using the DiRD framework, described in equation (2), to estimate the difference between the pre and post periods more precisely at the discontinuity threshold. When estimating the model using time-invariant characteristics as the outcome of interest, road distance to the nearest U.S. border crossing and area, the vector of control variables are not included. These estimates, obtained at the CV optimal bandwidth, can be seen in panel (a) of Table 3. The coefficient on distance is small and statistically insignificant, and after a single outlier is removed (Ocampo, Coahuila), there is no evidence of a statistically significant change in municipality area at the RD threshold. Estimates in panel (c) demonstrate that the election environment and outcomes are consistent over time, at the threshold. Similar to Table 2, there is no evidence that PAN is more likely to win close elections following the expiration of the AWB, and no evidence of a change in the vote share received on either side of the election threshold for any of the three major parties or in the winning party's vote share. Unlike the calculations in Table 2, the final estimate in panel (c) finds no statistically significant evidence of a change in the length of time that the incumbent party was in power. Importantly, this evidence reveals that the electoral environment remained consistent through the ban's expiration.

Estimates of once lagged, $(t-1)$, crime rates and municipal characteristics are shown in panel (d) and panel (e), respectively. The difference in the discontinuities for five of the six municipal crime rates are statistically insignificant. This includes the illegal weapons possession variable, which is statistically significant and negative in Table 2. The only crime variable that shows evidence of imbalance in the lagged period is assault and battery. However, this variable is small and statistically in significant in Table 2, and the post-AWB change in discontinuity will be shown to be small and statistically insignificant when using time t, year after the election, crime rates. This movement is counter to the evolution in firearm homicides, suggesting that the assault and battery rate is not a factor contributing to the change in firearm violence. Of the six municipal characteristics used as control variables in this study, only the logged tax rate and

population density yield statistically significant evidence of possible imbalance across the AWB expiration. However, like the estimates for area in panel (a), removing a single outlier again yields estimates that are small and statistically insignificant.[21]

Like the estimates in Table 2, the estimates for all three homicides rates, in panel (b) of Table 3, are small and statistically insignificant. Most importantly, there is no evidence of a difference in the discontinuities of the lagged firearm homicide rate, the key variable of interest. These results establish that all three lagged homicide rates were consistent on either side of the AWB expiration, at the election threshold. Together, the full set of baseline estimates shown in Table 2 and Table 3 demonstrates that any change in homicide rate following the expiration of the AWB cannot be explained by permanent characteristics of the municipality, the election environment in which the incumbent was removed from office, or observable characteristics of the municipality prior to the newly elected official taking charge.

Finally, the density of the municipalities around the election threshold is examined. The distribution of municipalities sorted by vote margin is shown in Figure 6, separately for (a) pre and (b) post-AWB expiration periods. Positive margins indicate an incumbent defeat. In both time periods the distribution of municipalities follows a similar pattern, both distributions have a steady increase through the threshold to the modal bin which is immediately to the right of the cutoff. With the distribution skewed away from incumbent party reelection it is unlikely that the group in power is manipulating the election; furthermore, the distributions yield comparable patterns, suggesting that the election dynamics are similar both before and after the expiration of the AWB. Whether there exists any difference in the density at the threshold between the two time periods can be more formally tested by comparing logged bin counts in a DiRD framework. The results across a number of bandwidths and bin sizes are shown in Table 4. Estimates across all bandwidth and bin size combinations yield small and statistically insignificant estimates.[22] This information can also be expressed by plotting the differences in the logged bin count (post - pre) across the election threshold. These plots can be seen in Figure 7 for the 20 percent bandwidth and all three bin sizes (0.01, 0.005, and 0.0025). These figures again show no evidence of a change in the distribution of municipalities around the election threshold.

[21] The coefficient estimate for the lagged Ln(Tax Rate) reduces to 0.187 with a standard error of (0.189) when Poza Rica de Hidalgo, Veracruz is removed from the estimate. Without Guadalajara, Jalisco the estimate for the lagged Ln(Population Density) reduces to 0.213 with a standard error of (0.445).

[22] The combinations of options shown in Table 4 are selected to cover the optimized bin size (0.007 ~ 0.01) and bandwidth (0.16 ~ 0.19) for the McCrary (2008) density test for the pre and post RD samples. Estimates shown are from a model with a quadratic polynomial to best match point estimates from the separate pre and post McCrary density tests. Estimates using a cubic polynomial again yield 16 statistically insignificant estimates.

5.2 Firearm Homicide Rate and the Expiration of the AWB

Before using equation (2) to estimate the effect of the expiration of the AWB on the firearm homicide rate, the RD model, estimated separately for the pre and post-expiration periods, can be used to illustrate how the logged firearm homicide rate changes across the election threshold. To do this, the residuals of the logged firearm homicide rate are extracted using the control variables and state-year fixed effects shown in equation (1). The residuals are plotted in Figure 8 on either side of the vote threshold with a cubic spline and 95 percent confidence bands. The CV optimized bandwidth (0.07) is shown with 35 bins on either side of the election threshold, and observations are weighted by the interaction between a triangular kernel and municipal population. The incumbent vote margin is plotted on the horizontal axis. Positive values denote a victory by a challenger, and negative values an incumbent victory. Prior to the expiration of the AWB, Figure 8(a), there is a large decline at the election threshold. This decline is the graphical representation of the coefficient, θ, on the RD indicator variable, D_{mst}, in equation (1). The same calculation is repeated for the post-expiration time period, shown in Figure 8(b); here the effect is less pronounced, but positive. A significant change from the pre-expiration period. The coefficient, β, on the DiRD estimator, $P_t D_{mst}$, from equation (2), calculates the difference between the post-expiration change and the pre-expiration change. This can be demonstrated by taking the difference, post minus pre, of each bin shown in Figures 8(a) and 8(b). The result is shown in Figure 8(c) for the 2000 to 2006 period, and in 8(d) for the extended 1999 to 2010 period. Each of these figures illustrates the sizable increase in the firearm homicide rate that followed the expiration of the AWB.

To quantify the magnitude of the effects seen in Figure 8, estimates using the natural log of the firearm homicide rate as the dependent variable are shown in Table 5. Each estimate is from a separate regression and number of observations in each regression is shown below the point estimate and standard error. Estimates in panel (a) use the CV optimal bandwidth, alternative bandwidths from 0.05 to 0.09 are shown in panel (b). Regression discontinuity estimates of θ, from equation (1), are shown for the pre-expiration in column (1). As seen in the previous figure, the estimates for the effect of an incumbent loss prior to the expiration of the ban is large and negative, across all bandwidths. This suggests that without the availability of the restricted firearms, the relative strength of the newly elected mayors enables them to overcome any adverse impact generated by the deterioration of the status quo. The same exercise is repeated for the post-AWB period, the RD estimates are shown in column (2). The point estimates are positive, but generally smaller in magnitude than the pre-expiration estimates. This matches the pattern seen in the residual plot. However, the correct comparison is not the value of the post-expiration estimate alone, but whether there was a change relative to the pre-expiration baseline.

Equation (2) is used to estimate the difference in the incumbency effect before and after the expiration of the AWB; the difference is captured by the equation's β coefficient. The DiRD estimates for equation (2)'s β coefficient, with the logged firearm homicide rate as the dependent variable, are shown in column (3) of Table 5. The estimates for the change in the firearm homicide rate following the ban's expiration are positive and statistically significant for each of the five bandwidths. At the CV optimal bandwidth, the model estimates an increase in the firearm homicide rate of about 35 percent following the expiration of the AWB.[23] This evidence is consistent with the hypothesis that the expiration of the AWB contributed to the reversal of the firearm homicide trend in Mexico that began in 2005. Furthermore, the relationship described by the estimates in Table 5 must exist if the theory that the increased availability of high powered firearms led to an increase in the firearm homicide rate is correct. Due to the central importance of this result to the paper's hypothesis, the following sections investigate alternative explanations of the increase in the firearm homicide rate, a more detailed analysis of the timing of the increase, and whether the distance to the U.S. border impacts the magnitude of the increase in firearm homicides.

5.3 Examining Alternative Explanations

The evidence that the increase in the firearm homicide rate coincided with the expiration of the AWB is shown in the previous section. However, this relationship alone is not enough to isolate the cause of the increase in firearm homicides to the expiration of the AWB. A central concern could be that the increase in firearm homicides identified in the previous section is driven by a larger secular trend affecting all homicide and crime rates. To investigate this, the DiRD model shown in equation (2) is estimated using the non-firearm homicide rate and a number of other crime rates, as the dependent variable. The estimates of the DiRD coefficient, β, for these alternative crime rates are shown in panel (a) of Table 6. The estimate for the logged non-firearm homicide rate is shown in column (2), and for comparison the analogous estimate using the logged firearm homicide rate can be found in column (1). Estimates for the overall homicide rate, both firearm and non-firearm, are shown in column (3), and the other six columns include estimates using the alternative crime rates as the dependent variable.

The most important takeaway from Table 6 is that there was no concurrent increase in the non-firearm

[23] Alternative model specifications are shown in Appendix Table A.4. The table includes the baseline estimates for comparison, and 19 alternatives, each estimated using the state-year fixed effects used throughout the paper as well as models with only year fixed effects, and a post-AWB indicator variable. Estimates are shown for alternative weights, definition of incumbency, samples restricted to larger municipalities, samples dropping municipalities with the largest change in narcotics related activity, samples dropping municipalities with the highest levels of firearm homicide rates to rule out the effect of outliers, samples using crime rates per 100,000, 50,000 and 5,000, alternative spline calculations, and estimates dropping the outlying municipalities from the baseline estimates. Furthermore, to ensure that no single municipality is driving the firearm homicide estimates, the model was estimated dropping one municipality at a time. Each point estimate remains statistically significant at the 95-percent confidence level, and ranges from 0.290 to 0.382. These results demonstrate the strength of the firearm homicide estimates, and rule out any outlying municipality driving the result.

homicide rate; this isolates the increase in homicides to only those committed with firearms. However, increased access to more deadly firearms could have simply substituted murders that would have happened in any state of the AWB from non-firearm homicides to homicides with a firearm. If this substitution occurred, the change in firearm homicides would be more a function of the individuals wielding the firearms, and not the actual firearms. However, there is no evidence of a decline in the non-firearm homicide rate. This removes the possibility that the increase in the firearm homicide rate was a simple substitution away from non-firearm homicides, and lends more evidence to the hypothesis that newly available assault weapons led to an increase in homicides that would not have otherwise occurred. This is directly seen in column (3), where the model estimates a large, roughly 30 percent, and statistically significant increase in the overall homicide rate.

Examining the crime rates from six of the most common non-homicide crimes yields insight into whether a secular trend in the general level of crime could be behind the surge in the post-AWB firearm homicide rate. Interestingly, estimates for illegal arms possession are small and statistically insignificant. This is consistent with the theory that the expiration of the AWB changed the type of firearms available and that these guns are more lethal; to impact the level of gun-violence the expiration need not affect the quantity of firearms available.[24] Estimates for the rate of theft, assault and battery, and rape are also small and statistically insignificant. The estimate for property damage is larger, but also statistically insignificant. As previously discussed, assault and battery is the only crime that is not balanced in the lagged period. However, as seen in Table 6, there was no increase in this violent crime that contemporaneously changed with the firearm homicide rate.

The only non-firearm crime that consistently shows evidence of change following the expiration of the AWB is the level of narcotics crime. The importance of this result is that it contains no evidence of a rise in narcotics related crime that could have caused the increase in firearm homicides following 2004. Possible explanations for this change could be a strategic shift away from municipalities where the incumbent party was defeated, much like the results found in Dell (2015); this type of movement of criminal organizations would likely put a downward pressure on violent activity. Alternatively, this result could also be due to the strengthening of these groups, relative to the local government, following the increase in their access to high powered firearms. In this case, the estimate would be additional evidence of a decline in the government's relative strength following the expiration of the AWB. While the narcotics crime result is interesting, examining its cause is largely outside the scope of this study. Of central importance is that there is no evidence that narcotics crime increased at the same time as the firearm homicide rate. Additionally,

[24] Evidence of this is shown in Appendix Table A.12, where it is documented that following the expiration of the AWB, the rate of firearm homicides increased relative to a given level of illegal arms possession.

evidence presented in later in this section shows that there was no change in the overall narcotics market that coincided with the expiration of the AWB, and that pre-expiration levels of narcotics crime are unrelated to the magnitude of the increase in the post-AWB firearm homicide rate. The estimates in Table 6 indicate that there was no general increase in crime rates, or non-firearm homicides, that coincided with the increase in the firearm homicide. Furthermore, the increase in the firearm homicide rate was not a substitution away from other types of homicides, but led to an increase in the overall rate of homicide.

Extending the model past 2006 is not entirely straightforward. The DiRD model used in this paper identifies the effect of pre and post-expiration discontinuities using the change in the party of a municipality's mayor, irrespective of which party is entering or leaving office. However, during Calderon's administration, Dell (2015) found that Calderon's PAN party begins to act more aggressively against drug trafficking organizations.[25] Therefore, expanding the model into the period of the Calderon administration makes identification, and possibly interpretation, more difficult. Expanding the sample to include the years from 1999 to 2010 allows two cycles of mayoral elections to occur on either side of the AWB expiration. Estimates for this time period are shown in panel (b) of Table 6.

The pattern of the estimates for this extended sample mirrors the results from the original 2000 to 2006 sample. The estimate on the logged firearm homicide rate is large, positive, and statistically significant. The estimate for the non-firearm homicide rate is close to zero and statistically insignificant; estimates for the homicide rate are large and positive, and statistically significant at the 90-percent confidence level. Four of the other crime rates show no evidence of an effect, the narcotics crime rate is again negative, and the effect on property damage is positive and statistically significant in this extended time period.

It is unclear exactly how much of these estimates are driven by the expiration of the AWB, and how much may be due to the aggressive tactics employed in the later years of the expanded sample. This aggressiveness was targeted at parts of the country in which cartel activity and violence increased to unacceptable levels; therefore, to attempt and correct for this, municipalities that were in the top decile of pre-expiration narcotics crime are removed from the sample and the model re-estimated. This strategy has the added benefit of only relying on pre-expiration information, ensuring that the expiration of the ban itself does not influence which municipalities remain in the sample. These estimates are shown in panel (c) of Table 6. A similar pattern emerges. There are consistently large positive estimates for the firearm homicide rate, and there remains no evidence of a secular increase in the general rate of crime.

After ruling out an underlying increase in the general rate of crime as a possible cause of the increase in the firearm homicide rate, an alternative explanation could be attributed to a change in the illicit narcotics

[25] President Calderon's administration begins in 2007 for the purposes of this paper, the exact inauguration date was December 1, 2006.

market facing the drug trafficking organizations in Mexico that coincided with the expiration of the AWB. Two pieces of information are used to examine whether or not such a change occurred. First, price data from the United Nations Office on Drugs and Crime's World Drug Report are shown in Figure 9. The figure plots the retail price in the United States, the central market for illicit narcotics trafficked from Mexico, for both heroin and cocaine. The third major drug trafficked into the United States is marijuana; however, it does not have a single market price because its production is extremely fragmented. Both prices steadily decline through the expiration of the AWB. It may be of interest to note that the increase in the cocaine price follows the Calderon offensive.[26] In addition to the stability in the price trend at the time of the AWB expiration, the National Survey on Drug Use and Health provides data on self-reported drug use which are displayed in Table 7. There is no evidence of a significant change in reported use for any of the three drugs that coincided with the expiration of the AWB. This information demonstrates that there was no change in either the price or use of narcotics at the time the AWB expired; this rules out changes in the illicit narcotics market as a possible cause for the increase in the firearm homicide rate.

In addition to ruling out alternative explanations of what caused the increase in the firearm homicide rate, it is also important to show that the estimated effect is unique to the timing of the AWB's expiration. This demonstrates the implicit assumption used in the identification strategy that the dissolution of the status quo should be consistent across periods where relative strength remains consistent. In practice, this means that comparisons of two RD estimates within a consistent state of the AWB should yield no difference in the status quo effect. The three-year cycle of mayoral elections can be exploited to create a set of smaller samples and pseudo cutoffs.

The first comparison uses the true cutoff, the AWB expiration, to define pre and post treatment years. All municipalities included in these estimates had an incumbent margin victory (or defeat) of less than seven percentage points. The year immediately following the expiration of the AWB, 2005, has a large number of eligible municipalities with newly elected mayors. Municipalities within the same set of states that had newly elected officials in 2005, also had newly elected officials three years prior, in 2002.[27] The balance in the two-year sample used here is essential to isolate the timing effect, and rule out changes in the sample composition as a cause of any difference between the discontinuities. Estimating the DiRD model with only these two years yields the point estimates seen in the column (3) of panel (b) in Table 8. The estimated increase in the firearm homicide rate is similar to the estimates seen using the full sample, and is again statistically significant. The sample can also be expanded to include one additional year on either side of

[26] Castillo et al. (2014) also find evidence that policy changes in Colombia may help explain the 2007 spike in cocaine prices, and the post-2007 increase in the homicide in Mexico.

[27] The state of Veracruz, which had newly elected officials in 2001, held their following round of elections prior to 2005. Therefore, the 2001 Veracruz observations are combined with the 2002 observations when paired with the data from 2005 to balance the set of states in the sample.

the AWB's expiration. Data from 2003 are added to the pre-expiration sample, and the set of observations three years later, from 2006, are added to the post-expiration sample. Although these years do not contain enough observations to be estimated on their own, they can complement the two-year sample from column (3). Estimates from DiRD model after expanding the sample to include an additional year on either side of the cutoff are shown in column (4) of panel (b), and yield a similar estimate.

To construct samples around pseudo cutoffs, each election cycle can be moved three years in either direction. This again allows for the inclusion of a consistent set of municipal elections around two pseudo cutoffs, one in 2002, and the other in 2008. For the early, pre-expiration, pseudo cutoff, data from the previous three-year election cycle are used. The "pre-treatment" sample is defined as data from 1999 (and 2000) municipalities with representatives elected in the previous year, and the "post-treatment" sample includes data from 2002 (and 2003). The estimate for the two-year sample is shown in column (1) of panel (a), and the estimate with two years on either side of the cutoff is shown in column (2) of panel (a). The estimates show no signs of the increase seen at the expiration of the AWB, both are smaller in magnitude and statistically insignificant.

A second pseudo cutoff can be constructed in the post-expiration period using the same technique. Moving the cutoff one election cycle after the expiration of the AWB yields a "pre-treatment" sample of 2005 (and 2006), and a "post-treatment" sample of 2008 (and 2009). As with earlier estimates using data from the Calderon period, the estimated effect is more difficult to interpret due to the importance of party affiliation. The post-expiration pseudo estimates for the full sample are shown in panel (c), and estimates for the restricted sample without municipalities in the top decile of pre-expiration narcotics crime are in panel (d). All four estimates are small and statistically insignificant.

Within both the pre and post-expiration periods, comparisons of RD estimates yield no evidence of a statistically significant difference in the status quo effect. This supports the key assumption of a consistent status quo effect over time. The evidence from Table 8 demonstrates that only when the sample crosses the expiration of the AWB, when there is an increase in the availability of high-powered firearms from the United States, is there evidence of an increase in the firearm homicide rate.

5.4 Distance to U.S. Border and the Firearm Homicide Rate

In addition to the timing of the effect, it would be expected that the magnitude of the increase in the firearm homicide rate would be greater in municipalities closer to U.S.-Mexico border crossings. Although this is not a necessary condition for the effect of the expiration of the AWB to exist, the further a municipality is from the border, the greater the cost of acquiring an illegal firearm from the United States. To investigate

whether this occurs, an additional distance term is added to the DiRD equation. The distance is again measured as the road distance from the municipality to the nearest border crossing into the United States. For ease of interpretation, an inverse distance term is calculated and normalized to be between zero and one using the following equation:

$$InverseDistance\,(I_m) = 1 - (Distance/2306). \tag{3}$$

A municipality that shares a border crossing with the United States has a value equal to one, and the value of I_m for the furthest municipality is zero. To estimate whether the effect of the expiration of the AWB is different for municipalities closer to the border, I_m is added to the DiRD equation. The estimating equation with the inclusion of the inverse distance variable can be expressed in the following form:

$$\begin{aligned} ln\,(y_{mst}) = \alpha + I_m P_t D_{mst}\beta + P_t D_{mst}\theta_1 + P_t I_m \theta_2 + I_m D_{mst}\theta_3 + I_m\theta_4 + D_{mst}\theta_5 + \sum_{p=1}^{P} v_{mst}^{p} D_{mst}\gamma_p \\ + \sum_{p=1}^{P} v_{mst}^{p}\gamma_p + P_t\left[\sum_{p=1}^{P} v_{mst}^{p} D_{mst}\mu_p + \sum_{p=1}^{P} v_{mst}^{p}\mu_p\right] + X_{mst}\pi + ln\left(y_{ms(t-1)}\right)\rho + \tau_t + \varepsilon_{mst}. \end{aligned} \tag{4}$$

The outcome of interest is again the logged firearm homicide rate in municipality m, in state s, and year t. The coefficient of interest is the coefficient on the triple interaction, β. This coefficient measures the change in the DiRD effect when moving from the furthest municipality ($I_m = 0$) to the border ($I_m = 1$), if the increase in the firearm homicide rate is greater closer to the border, the coefficient will be positive ($\beta > 0$). The inverse distance is also interacted with the post-expiration indicator variable (P_t) to capture any nationwide changes in the distance effect, and the regression discontinuity indicator (D_{mst}) to capture whether the distance matters in a different way if the incumbent is defeated. I_m is also included alone to capture any time and election invariant effect of the distance to the U.S.-Mexico border. The same set of trend and control variables are used, except for the previously included distance variables, and the model is estimated using year-specific fixed effects (τ_t). With the large number of items estimated in this model, the key focus of the output is not the exact point estimate of β, but whether the estimated value is positive, as expected.

Estimates from equation (4) can be seen in Table 9. Coefficient estimates using data from the 2000 to 2006 period are shown in the first two columns, and for the expanded 1999 to 2010 sample in the last four columns, all using a seven percent bandwidth. The sample used in the first two columns only include two years of post-AWB elections, and therefore, do not include municipalities from all parts of the country. In this context the extended sample has the benefit of balanced geographic distribution. Additionally, the relevant distance for illicit trafficking from the Yucatan Peninsula may not be road distance, for this part of the country movement over the Gulf of Mexico could be preferred. To take this into account, the states

that make up the peninsula are removed from columns (1), (3), and (5).[28] The sample used to estimate the results shown in panel (a) of Table 9 include municipalities on the U.S.-Mexico border; all six estimates yield evidence that the effect of the AWB is larger the closer a municipality is to the border.[29] Although estimates in the first two columns, for the pre-Calderon period, are smaller and statistically insignificant, they remain positive, yielding suggestive evidence of a stronger effect closer to the border. This is reinforced by the four large and statistically significant estimates using the extended sample.

Removing municipalities on the Yucatan Peninsula from the sample also removes Mexico's southern border. For balance, estimates in panel (b) only include municipalities that are at least 100 kilometers from the United States border. This is done to ensure that the distance effect is not solely driven by a large effect directly at the border. The estimates in panel (b) remain consistently positive, and almost identical when municipalities in the top decile of pre-expiration narcotics crime are removed. The effect of the AWB expiration is not limited to the U.S.-Mexico border region, the area analyzed in Dube et al. (2013), but impacted a much larger part of the country. In fact, estimating the basic DiRD estimate without municipalities within 100 kilometers of a U.S.-Mexico border crossing yields a point-estimate of 0.363 that is statistically significant at the 99-percent confidence level, similar to the main estimate from Table 5. These estimates reinforce that the expiration of the AWB led to a large increase in the firearm homicide rate, even in areas away from the U.S.-Mexico border.

6 Conclusion

In municipalities with close incumbent defeats the expiration of the AWB coincided with a 35 percent increase in the firearm homicide rate in Mexico, and this increase was isolated to the timing of the AWB's expiration. There was no concurrent increase in either non-firearm homicides or the overall rate of crime, and the illicit narcotics market was stable through the expiration of the AWB. Furthermore, the magnitude of the increase in firearm homicides is positively associated with proximity to the U.S.-Mexico border. The expiration of the AWB is the lone explanation for the increase in the firearm homicide rate that fits each criteria, and possesses the magnitude to have such a widespread effect. Finally, there is no evidence of a substitution away from non-firearm homicides, thus the expiration of the AWB also led to an increase in the overall homicide rate.

This paper provides a number of important contributions to the literature. It introduces a dataset that can be used to identify any number of municipal level outcomes and evaluate policy in the world's

[28] These states are Campeche, Chiapas, Quintana Roo, Tabasco, and Yucatan.

[29] All two dozen estimates for alternative bandwidths also show evidence of a larger impact closer to the border. Estimates are shown in Appendix Table A.8.

eleventh most populous country. The estimates from this paper show that the effect of the AWB expiration spread far beyond the border region, yielding evidence of an impact that is more widespread than previously documented. The paper also addresses the widely debated relationship between firearms and homicide prevalence, finding that access to high powered firearms, which were banned for a decade in the United States and are not legally available in Mexico, does lead to higher levels of homicides. Finally, much like DellaVigna and La Ferrara (2010), it highlights the fact that these type of rule of law issues are no longer domestic ones, but regulations made in one nation can significantly impact individuals who have no direct say in the decision making process.

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Figures

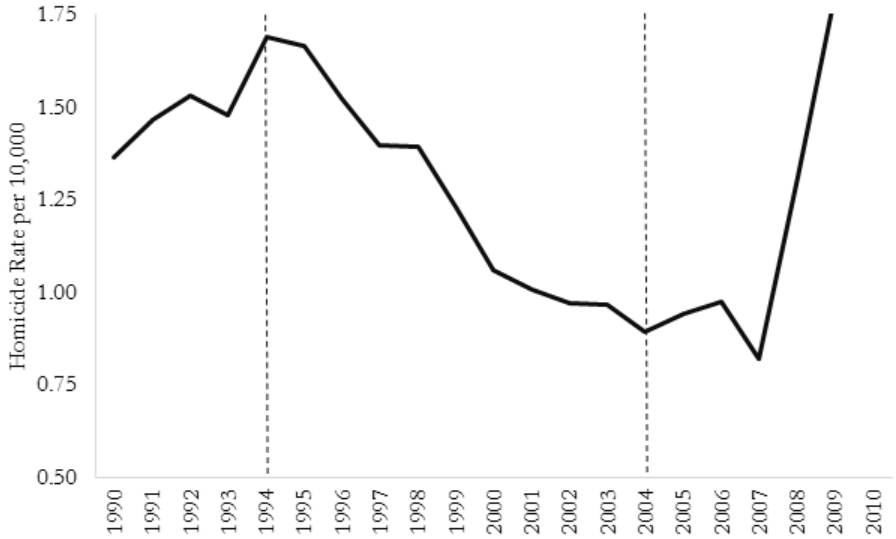


Figure 1: Homicide Rate in Mexico

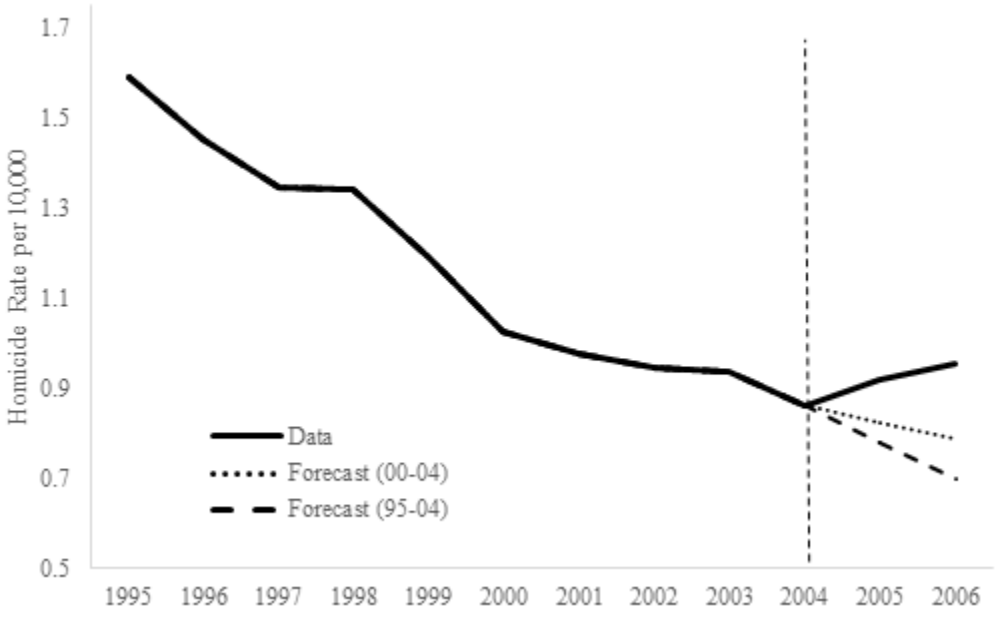


Figure 2: Overall Homicide Rate and Counterfactual Trends

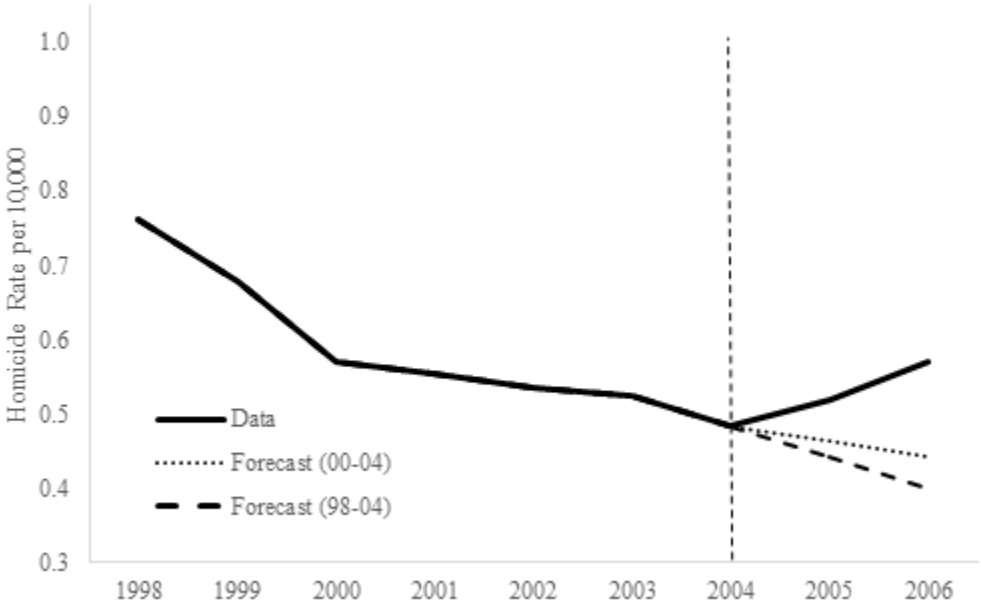


Figure 3: Firearm Homicide Rate and Counterfactual Trends

Note: Firearm homicide data is available from SINAIS beginning in 1998.

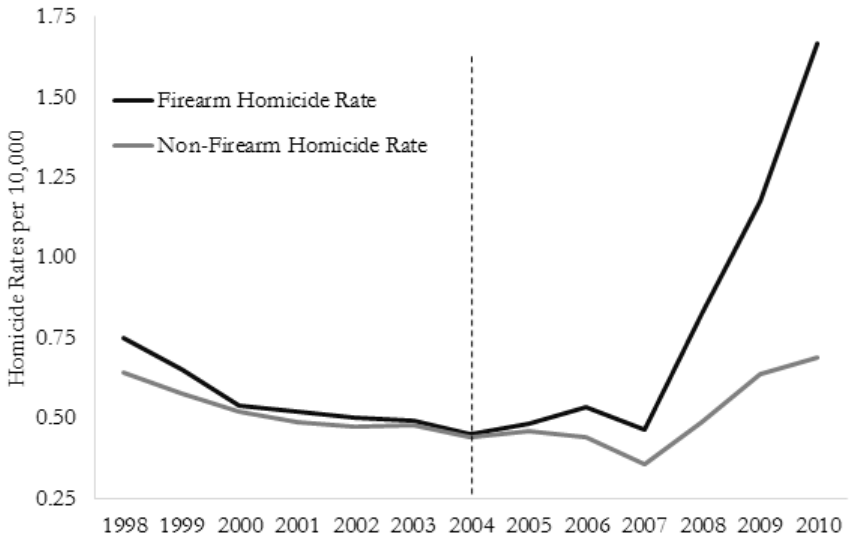
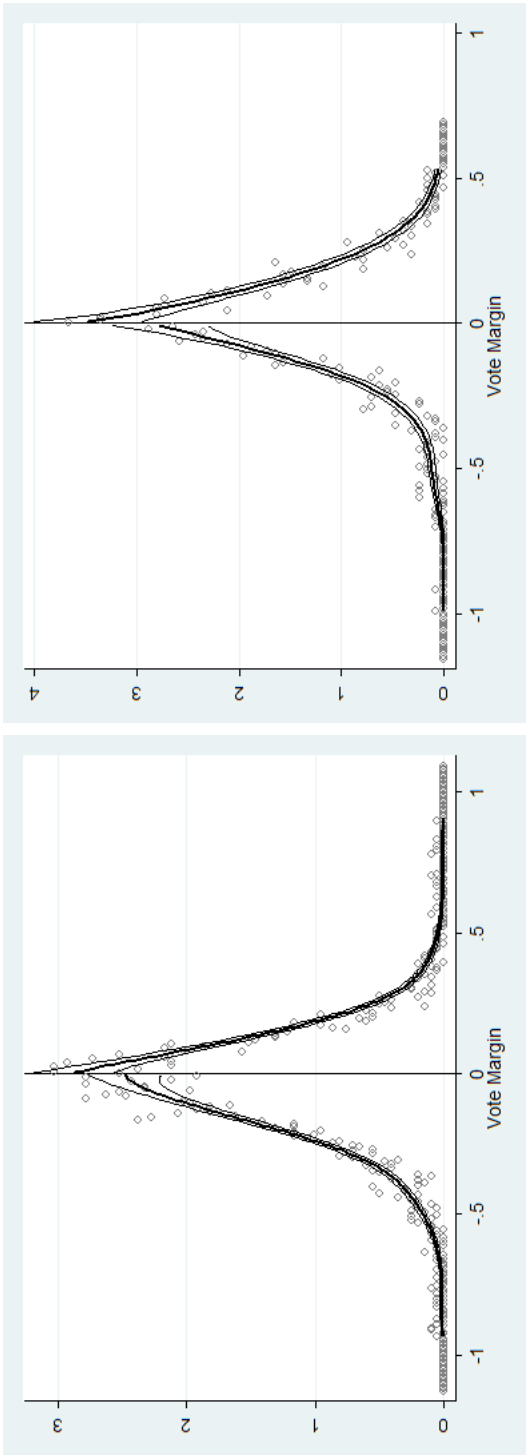


Figure 4: Firearm vs. Non-Firearm Homicide Rate in Mexico

Note: Firearm homicide data is available from SINAIS beginning in 1998.

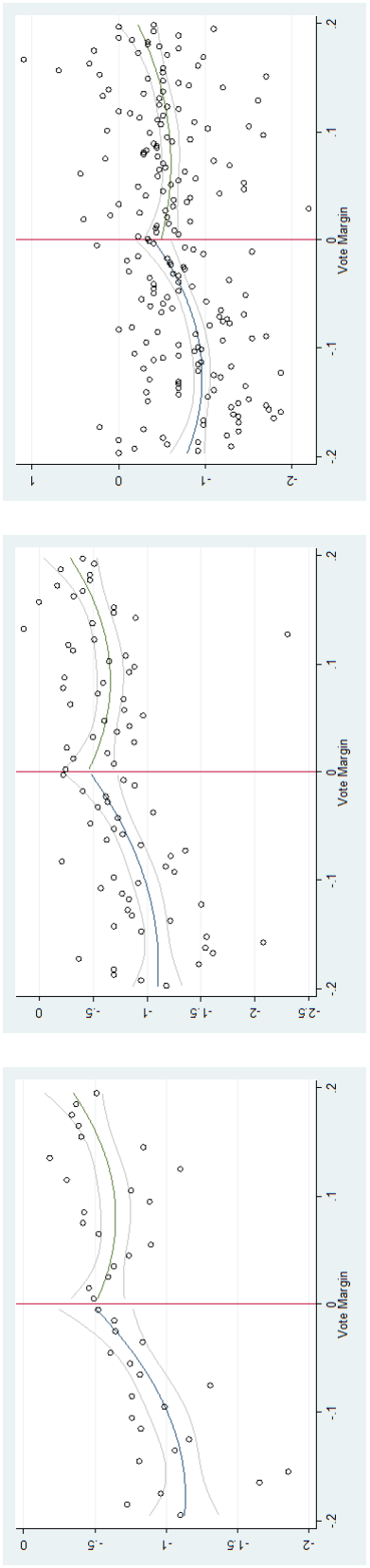
Figure 5: Close Municipal Elections: 1999 - 2010



(a) Pre-AWB Expiration Elections (2000 – 2004)

(b) Post-AWB Expiration Elections (2005 – 2006)

Figure 6: Frequency of Municipalities by Vote Margin (>0 if incumbent defeated)

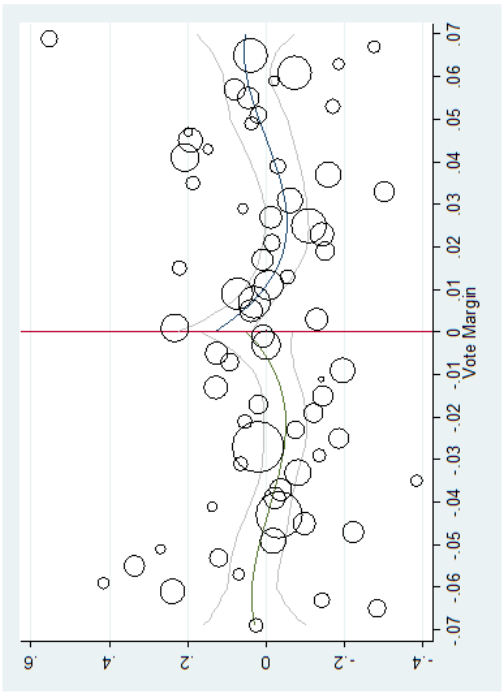


(a) Bin Size = 0.01

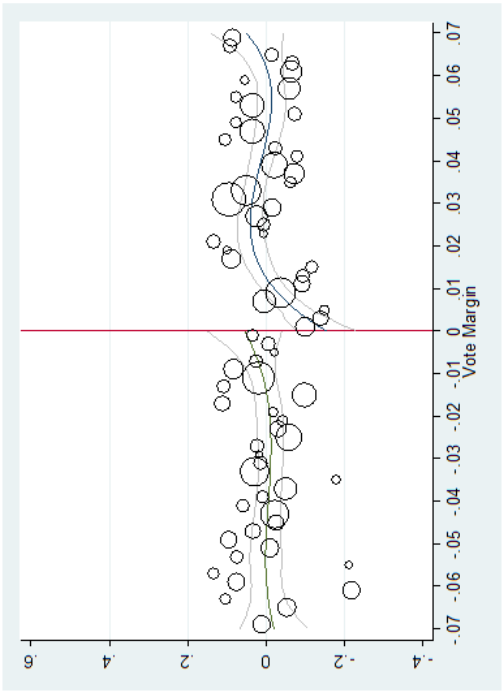
(b) Bin Size = 0.005

(c) Bin Size = 0.0025

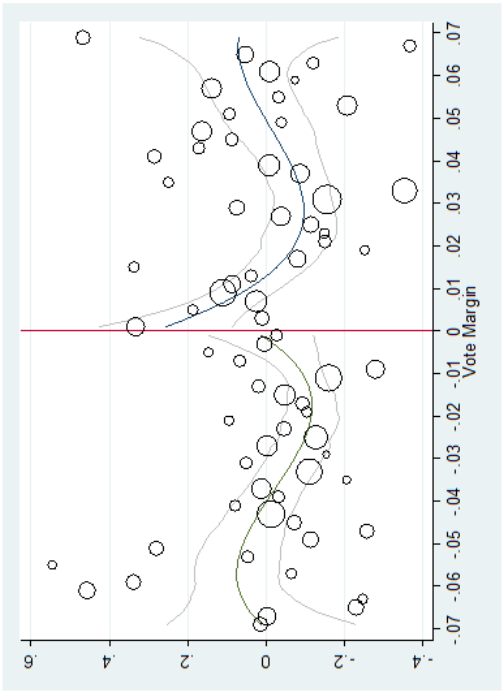
Figure 7: Difference in Logged Bin Count (Post – Pre; Bandwidth = 0.20)



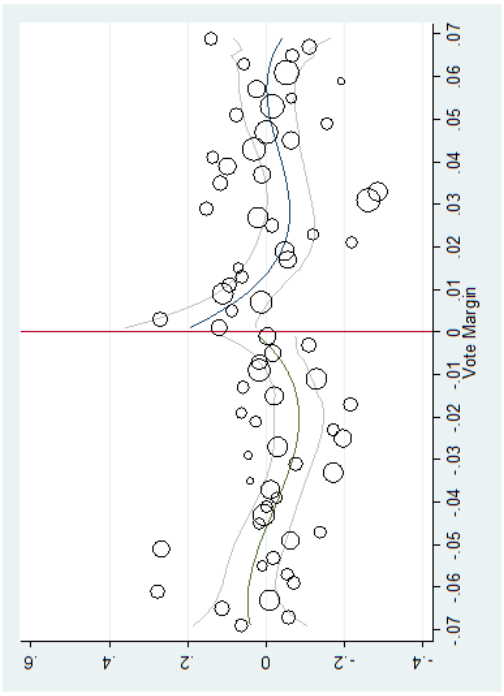
(a) Pre-AWB (2000 - 2004)



(b) Post-AWB (2005 - 2006)



(c) DiRD: Post - Pre (2000 - 2006)



(d) DiRD: Post - Pre (1999 - 2010)

Figure 8: Residuals of Ln(Firearm Homicide Rate): Using Equation (3) Controls $\left(X_{mst}, Ln\left(y_{ms(t-1)}\right), \tau_{(s)t}\right)$

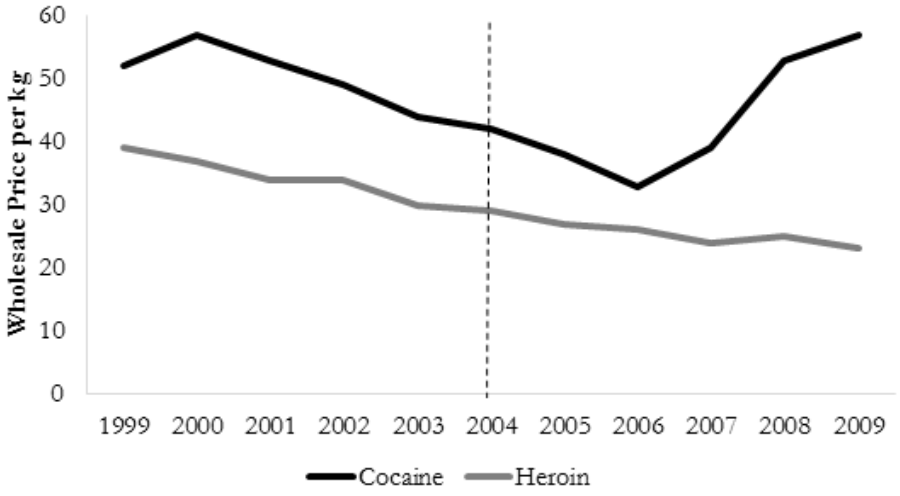


Figure 9: Wholesale Price per kg (adjusted for purity and inflation, 2009 USD)

Tables

Table 1: Mayoral Elections in the Previous Year

(a) Mayoral Elections in the Previous Year

	Number of Elections		Close Elections (< 7%)		Percent of Elections
	Count	% of Total	Count	% of Total	That Are Close: By Year
1999	796	9.82	197	7.07	24.75
2000	232	2.86	72	2.59	31.03
2001	771	9.51	245	8.80	31.78
2002	846	10.43	303	10.88	35.82
2003	233	2.87	77	2.76	33.05
2004	575	7.09	224	8.04	38.96
2005	1,018	12.55	396	14.22	38.90
2006	230	2.84	81	2.91	35.22
2007	577	7.12	207	7.43	35.88
2008	1,033	12.74	335	12.03	32.43
2009	190	2.34	65	2.33	34.21
2010	599	7.39	190	6.82	31.72
Total	7,100		2,392		

(b) Number of Incumbent Losses

Losses	Municipalities	
(of 4)	Count	Percent
0	252	13.52
1	395	21.19
2	640	34.33
3	447	23.98
4	130	6.97

Note: Shown in panel (a) are the total number of mayoral elections (left), number of close mayoral elections (center), and percent of elections each year that are close (right). Displayed in panel (b) are the number of incumbent losses (out of 4) in each municipality.

Table 2: Means for Pre and Post-AWB Expiration Time-Periods, by Election Outcome of the Incumbent Party

Bandwith = 0.07	**Pre-Expiration Means (2000-2004)**				**Post-Expiration Means (2005-2006)**				**Post Exp. Minus Pre Exp.**	
	Incumbent Victory	Challenger Victory	Difference	T-stat	Incumbent Victory	Challenger Victory	Difference	T-stat	Difference-in -Difference	T-stat
$Ln(FirearmHom.Rate)_{ms(t-1)}$	0.332	0.32	-0.012	[0.38]	0.292	0.212	-0.08	[1.90]*	-0.068	[-1.25]
$Ln(HomicideRate)_{ms(t-1)}$	0.533	0.508	-0.026	[0.70]	0.441	0.373	-0.067	[1.36]	-0.042	[-0.66]
$Ln(Non-Fire.Hom.Rate)_{ms(t-1)}$	0.27	0.251	-0.019	[0.80]	0.197	0.2	0.002	[-0.08]	0.022	[0.53]
$Ln(TheftRate)_{ms(t-1)}$	0.9	0.922	0.022	[-0.40]	0.728	0.604	-0.124	[1.73]*	-0.146	[-1.60]
$Ln(NarcoticsCrimeRate)_{ms(t-1)}$	0.402	0.362	-0.041	[1.02]	0.273	0.267	-0.005	[0.11]	0.035	[0.55]
$Ln(IllegalArmsRate)_{ms(t-1)}$	0.62	0.653	0.032	[-0.74]	0.622	0.524	-0.098	[1.67]*	-0.13	[-1.76]*
$Ln(AssaultBatt.Rate)_{ms(t-1)}$	1.037	1.007	-0.03	[0.59]	0.905	0.877	-0.028	[0.40]	0.002	[0.02]
$Ln(Prop.DamageRate)_{ms(t-1)}$	0.498	0.53	0.032	[-0.86]	0.48	0.433	-0.047	[0.85]	-0.079	[-1.21]
$Ln(RapeRate)_{ms(t-1)}$	0.252	0.269	0.017	[-0.70]	0.227	0.205	-0.022	[0.71]	-0.039	[-0.97]
$Distance_{ms}$	981	969	-11.71	[0.41]	1104	1102	-2.52	[0.06]	9.192	[0.18]
$Area_{ms}$	922	1069	146	[-0.82]	1115	778	-338	[1.27]	-484	[-1.54]
$Ln(GDPPerCapita)_{ms(t-1)}$	8.952	8.988	0.036	[-1.06]	8.764	8.79	0.026	[-0.57]	-0.01	[-0.17]
$Ln(InfantMort.Rate)_{ms(t-1)}$	1.341	1.316	-0.025	[0.53]	1.229	1.232	0.004	[-0.06]	0.029	[0.36]
$Ln(TaxRev.PerCapita)_{ms(t-1)}$	5.765	5.768	0.003	[-0.06]	6.003	5.961	-0.042	[1.06]	-0.045	[-0.68]
$Ln(VehicularMort.Rate)_{ms(t-1)}$	0.505	0.556	0.05	[-1.32]	0.495	0.484	-0.01	[0.19]	-0.061	[-0.92]
$Ln(Pop.Density)_{ms(t-1)}$	4.003	4.088	0.085	[-0.80]	3.943	4.023	0.08	[-0.63]	-0.004	[-0.03]
$PercentofPop.Male:20-49_{ms(t-1)}$	0.187	0.187	0	[-0.10]	0.185	0.185	0	[-0.16]	0	[0.08]
$YearsInPower_{ms(t-1)}$	4.288	4.446	0.158	[-1.55]	5.945	5.668	-0.277	[1.15]	-0.435	[-1.95]*
$PANWin_{ms(t-1)}$	0.172	0.362	0.19	[-6.63]***	0.174	0.405	0.231	[-5.66]***	0.041	[0.83]
$WinningVoteShare_{ms(t-1)}$	0.421	0.415	-0.006	[1.27]	0.418	0.406	-0.012	[1.60]	-0.006	[-0.63]
$PANVoteShare_{ms(t-1)}$	0.281	0.302	0.021	[-1.92]*	0.278	0.293	0.015	[-0.95]	-0.006	[-0.34]
$PRIVoteShare_{ms(t-1)}$	0.406	0.389	-0.017	[3.17]***	0.399	0.376	-0.023	[2.69]***	-0.005	[-0.54]
$PRDVoteShare_{ms(t-1)}$	0.244	0.221	-0.022	[1.87]*	0.252	0.26	0.008	[-0.51]	0.03	[1.52]
N	921				477				1398	

Note: *** p<0.01, ** p<0.05, * p<0.1. Data include all municipality observations, outside of Oaxaca, with an election in which the incumbent party margin is within seven percentage points. All rates are per 10,000 people, and logged variables are calculated as the natural log of the rate plus one to include observations with zero occurrences. Per capita GDP and municipal income are recorded in 1,000s of constant 1993 Mexican pesos. Population, homicide, and mortality data are from SINAIS; all other data are from INEGI.

Table 3: DiRD - Estimates of Time-Invariant, Election Outcomes, and Lagged Municipal Characteristics and Crime Rates (2000 - 2006)

(a) Time-Invariant		(c) Election Outcomes		(d) Lagged Alternative Crimes		(e) Lagged Municipal Characteristics	
	(1)		(2)		(3)		(4)
Distance_{ms}	-12.35	$\text{PAN Win}_{ms(t-1)}$	0.016	$\text{Ln(Theft Rate)}_{ms(t-1)}$	0.219	Ln(GDP	0.053
	(53.95)		(0.189)		(0.208)	$\text{Per Capita)}_{ms(t-1)}$	(0.094)
Area_{ms}	-1,644**	PAN Vote	-0.025	Ln(Narcotics	-0.127	Ln(Infant	-0.020
	(678)	$\text{Share}_{m(t-1)}$	(0.053)	$\text{Crime Rate)}_{ms(t-1)}$	(0.167)	$\text{Mort. Rate)}_{ms(t-1)}$	(0.194)
Area_{ms}	-744	PRI Vote	0.004	Ln(Illegal	-0.078	Ln(Tax Rev.	0.334*
(drop one municipality)	(563)	$\text{Share}_{ms(t-1)}$	(0.032)	$\text{Arms Rate)}_{ms(t-1)}$	(0.196)	$\text{Per Capita)}_{ms(t-1)}$	(0.188)
(b) Lagged Homicides		PRD Vote	0.053	Ln(Assault and	0.646***	Ln(Vehicular	0.240
Ln(Firearm	-0.127	$\text{Share}_{ms(t-1)}$	(0.056)	$\text{Battery Rate)}_{ms(t-1)}$	(0.213)	$\text{Mortality Rate)}_{ms(t-1)}$	(0.189)
$\text{Homicide Rate)}_{ms(t-1)}$	(0.144)						
		Winning Vote	0.024	Ln(Property	-0.151	Percent of Pop.	-0.006
Ln(Non-Firearm	0.147	$\text{Share}_{ms(t-1)}$	(0.028)	$\text{Damage Rate)}_{ms(t-1)}$	(0.164)	$\text{Male: 20-49}_{ms(t-1)}$	(0.005)
$\text{Homicide Rate)}_{ms(t-1)}$	(0.124)						
		Years In	-1.337	$\text{Ln(Rape Rate)}_{ms(t-1)}$	-0.188	$\text{Ln(Pop. Density)}_{ms(t-1)}$	0.888*
Ln(Homicide	0.032	$\text{Power}_{ms(t-1)}$	(0.818)		(0.123)		(0.464)
$\text{Rate)}_{ms(t-1)}$	(0.166)						
N	1,360	N	1,360	N	1,360	N	1,360

Note: *** p<0.01, ** p<0.05, * p<0.1. The dependent variable is the value of the specified municipal characteristic. The estimates shown are for the coefficient (β) on the interaction between the indicator variable for an incumbent defeat (D_{mst}) and the post-AWB indicator (P_t), from equation (2). Each regression includes state-year fixed effects, a cubic RD polynomial allowed to vary on either side of the election threshold and AWB expiration. The top five estimates in panel (c) include an indicator for whether the election is held in the last half of the year, and the predetermined outcomes of years in power (set of indicators), incumbent party (set of indicators), area, and distance (cubic). The estimate for years in power, determined in the previous election, only include the area and distance (cubic) controls. Excluding the dependent variable, all estimates in panels (b), (d), and (e) also include lagged values of the portion of the population that is male age 20 to 49, the logged population density, GDP per capita, municipal government income per capita, infant mortality rate, vehicular mortality rate, narcotics crime rate, theft rate, illegal weapons possession rate, rape rate, assault and battery rate, and the property damage rate. The eligible sample includes all municipalities outside of Oaxaca. Each regression is weighted using a triangular kernel, calculated using the vote margin, which is interacted with population for panels (b), (d), and (e), and total votes cast for panel (c).

Table 4: DiRD - Density Test - Estimates of the Logged Bin Count

	Ln(Bin Count)			
	BW=0.10	BW=0.15	BW=0.20	BW=0.25
	(1)	(2)	(3)	(4)
$P_t * D_{mst}$	0.040	-0.022	-0.028	-0.111
(Bin Size = 0.01)	(0.255)	(0.236)	(0.241)	(0.260)
$P_t * D_{mst}$	0.080	-0.007	0.017	-0.035
(Bin Size = 0.005)	(0.259)	(0.273)	(0.248)	(0.241)
$P_t * D_{mst}$	0.076	0.010	-0.086	-0.062
(Bin Size = 0.002)	(0.281)	(0.227)	(0.208)	(0.187)

Note: *** p<0.01, ** p<0.05, * p<0.1. The dependent variable is the logged count of observations within the specified bin size. The estimates shown are for the coefficient on the interaction between the indicator variable for an incumbent defeat and the post-AWB indicator, from a density test adjusted to the DiRD framework.

Table 5: DiRD - Effect of AWB Expiration on Ln(Firearm Homicide Rate) (2000 - 2006)

	Pre-Expiration (2000 - 2004)	Post-Expiration (2005 - 2006)	DiRD (2000 - 2006)
	(1)	(2)	(3)
	(a) CV Optimal Bandwidth		
Ln(Firearm	-0.262***	0.079	0.354***
Homicide Rate)$_{mst}$	(0.080)	(0.106)	(0.131)
BW = 0.07	N=897	N=463	N=1,360
	(b) Alternative Bandwidths		
Ln(Firearm	-0.180*	0.085	0.276*
Homicide Rate)$_{mst}$	(0.105)	(0.114)	(0.157)
BW = 0.05	N=638	N=344	N=982
Ln(Firearm	-0.197**	0.201*	0.434***
Homicide Rate)$_{mst}$	(0.090)	(0.108)	(0.142)
BW = 0.06	N=760	N=399	N=1,159
Ln(Firearm	-0.211***	0.003	0.273**
Homicide Rate)$_{mst}$	(0.072)	(0.096)	(0.119)
BW = 0.08	N=1,016	N=518	N=1,534
Ln(Firearm	-0.229***	0.115	0.333***
Homicide Rate)$_{mst}$	(0.065)	(0.092)	(0.110)
BW = 0.09	N=1,130	N=583	N=1,713

Note: *** p<0.01, ** p<0.05, * p<0.1. The dependent variable in all estimates is the natural log of the firearm homicide rate. The estimates shown in column (3) are for the coefficient (β) on the interaction between the indicator variable for an incumbent defeat (D_{mst}) and the post-AWB indicator (P_t), from equation (2); the estimates shown in column (1) and column (2) are for the coefficient (θ)on the indicator for an incumbent defeat, from equation (1). Regressions include state-year fixed effects, a lagged value of the dependent variable, a cubic RD polynomial allowed to vary on either side of the election threshold and AWB expiration, and the set of municipal controls described in footnote 11. The eligible sample includes all municipalities outside of Oaxaca. Each regression is weighted using an interaction between a triangular kernel, calculated using the vote margin, and the municipal population.

Table 6: DiRD - Effect of AWB Expiration on Alternative Crime Rates (2000 - 2006)

	Ln(Firearm Homicide Rate)	Ln(Non-Firearm Homicide Rate)	Ln(Homicide Rate)	Ln(Theft Rate)	Ln(Narcotics Crime Rate)	Ln(Illegal Arms Rate)	Ln(Assault and Battery Rate)	Ln(Property Damage Rate)	Ln(Rape Rate)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
(a) Alternative Crime Rates (2000 - 2006)									
$P_t * D_{mst}$	0.354***	0.121	0.320**	-0.060	-0.708***	0.032	0.141	0.256	-0.110
	(0.131)	(0.119)	(0.154)	(0.201)	(0.150)	(0.177)	(0.195)	(0.166)	(0.123)
N	1,360	1,360	1,360	1,360	1,360	1,360	1,360	1,360	1,360
(b) Extended Sample (1999 - 2010)									
$P_t * D_{mst}$	0.255**	0.015	0.190*	-0.040	-0.240**	0.097	0.146	0.233**	-0.023
	(0.101)	(0.086)	(0.113)	(0.140)	(0.113)	(0.130)	(0.136)	(0.115)	(0.084)
N	2,307	2,307	2,307	2,307	2,307	2,307	2,307	2,307	2,307
(c) Extended Sample: Remove Top Decile of Pre-Expiration Narcotics Crime (1999 - 2010)									
$P_t * D_{mst}$	0.240**	0.035	0.188	0.014	-0.220*	-0.116	0.057	0.190	-0.077
	(0.104)	(0.092)	(0.120)	(0.154)	(0.114)	(0.137)	(0.150)	(0.124)	(0.091)
N	2,073	2,073	2,073	2,073	2,073	2,073	2,073	2,073	2,073

Note: *** p<0.01, ** p<0.05, * p<0.1. The dependent variable is the stated crime. The estimates shown are for the coefficient (β) on the interaction between the indicator variable for an incumbent defeat (D_{mst}) and the post-AWB indicator (P_t), from equation (2). Regressions include state-year fixed effects, a lagged value of the dependent variable, a cubic RD polynomial allowed to vary on either side of the election threshold and AWB expiration, and the set of municipal controls described in footnote 11, excluding the dependent variable. The eligible sample includes all municipalities outside of Oaxaca. Each regression is weighted using an interaction between a triangular kernel, calculated using the vote margin, and the municipal population.

Table 7: Percent of Self-Reported Drug Use in the Past Year: United Sates (All Individuals 12 and older)

	2002	2003	2004	2005	2006	2007	2008	2009
Cocaine	2.5	2.5	2.4	2.3	2.5	2.3	2.1	1.9
Heroin	0.2	0.1	0.2	0.2	0.2	0.1	0.2	0.2
Marijuana	11	10.6	10.6	10.4	10.3	10.1	10.3	11.3

Note: From the National Survey on Drug Use and Health, as reported in various rounds of the National Drug Intelligence Center's National Drug Threat Assessment.

Table 8: Pseudo Estimates of Effect Away from Expiration of AWB

	(a) Pseudo Cutoff: Pre-Expiration		**(b) AWB Expiration**	
Pre-Cutoff Years	1999	1999/2000	2002	2002/2003
Post-Cutoff Years	2002	2002/2003	2005	2005/2006
	(1)	(2)	(3)	(4)
Ln(Firearm Homicide Rate)	-0.089	0.067	0.327*	0.317**
	(0.264)	(0.224)	(0.187)	(0.151)
N	479	621	681	915
	(c) Pseudo Cutoff: Post-Expiration		**(d) Pseudo Cutoff: Post-Expiration** (Remove Top 10% of Pre-2005 Narcotics Crime Rates)	
Pre-Cutoff Years	2005	2005/2006	2005	2005/2006
Post-Cutoff Years	2008	2008/2009	2008	2008/2009
Ln(Firearm Homicide Rate)	-0.036	-0.145	-0.004	-0.096
	(0.155)	(0.156)	(0.161)	(0.166)
N	713	854	667	789

Note: *** p<0.01, ** p<0.05, * p<0.1. The dependent variable in all estimates is the natural log of the firearm homicide rate. The estimates shown are for the coefficient (β) on the interaction between the indicator variable for an incumbent defeat (D_{mst}) and the post-cutoff indicator (P_t), from equation (2). The placement of the cutoff is defined by the pre and post years specified in each panel. Regressions include state-year fixed effects, a lagged value of the dependent variable, a cubic RD polynomial allowed to vary on either side of the election threshold and AWB expiration, and the set of municipal controls described in footnote 11. The eligible sample includes all municipalities outside of Oaxaca. Each regression is weighted using an interaction between a triangular kernel calculated using the vote margin and the municipal population.

Table 9: Inverse Distance to U.S.-Mexico Border and the Effect of AWB Expiration on Ln(Firearm Homicide Rate)

	(a) Full Sample					
	2000 - 2006		**1999 - 2010**			
					(Remove Top 10% of Pre-2005 Narcotics Crime Rates)	
	Remove Yucatan Peninsula	Full Sample	Remove Yucatan Peninsula	Full Sample	Remove Yucatan Peninsula	Full Sample
	(1)	(2)	(3)	(4)	(5)	(6)
Ln(Firearm Homicide Rate)$_{mst}$	0.226	0.093	0.429**	0.389***	0.610**	0.464***
	(0.263)	(0.155)	(0.189)	(0.124)	(0.239)	(0.137)
N	1,200	1,398	2,053	2,392	1,826	2,156
	(b) Distance from U.S. Border Greater than 100 KM					
	2000 - 2006		**1999 - 2010**			
					(Remove Top 10% of Pre-2005 Narcotics Crime Rates)	
	Remove Yucatan Peninsula	Full Sample	Remove Yucatan Peninsula	Full Sample	Remove Yucatan Peninsula	Full Sample
	(1)	(2)	(3)	(4)	(5)	(6)
Ln(Firearm Homicide Rate)$_{mst}$	0.129	0.160	0.219	0.437***	0.616**	0.481***
	(0.340)	(0.178)	(0.219)	(0.135)	(0.246)	(0.139)
N	1,164	1,362	1,989	2,328	1,802	2,132

Note: *** p<0.01, ** p<0.05, * p<0.1. The dependent variable in all estimates is the natural log of the firearm homicide rate. The estimates shown are for the coefficient (β) on the triple interaction between the indicator variable for an incumbent defeat (D_{mst}), the post-AWB indicator (P_t), and the inverse distance measure (I_m) from equation (4). All regressions include year fixed effects, the lagged value of the dependent variable, a cubic RD polynomial allowed to vary on either side of the election threshold and AWB expiration, and the set of municipal controls described in footnote 11, except for the previously described distance control. The eligible sample includes all municipalities outside of Oaxaca. Each regression is weighted using an interaction between a triangular kernel calculated using the vote margin and the municipal population.

A Online Appendix

A.1 Text of the U.S. Federal Assault Weapons Ban

The AWB was Subtitle A of Title XI in H.R. 3355, the Violent Crime Control and Law Enforcement Act of 1994. The key text banning assault weapons is as follows:

(1) It shall be unlawful for a person to manufacture, transfer, or possess a semiautomatic assault weapon.

The following paragraph allows for the continued possession of firearms legally owned before the law was enacted.

(2) Paragraph (1) shall not apply to the possession or transfer of any semiautomatic assault weapon otherwise lawfully possessed under Federal law on the date of the enactment of this subsection.

The assault weapons were defined as follows:

(b) DEFINITION OF SEMIAUTOMATIC ASSAULT WEAPON - Section 921(a) of title 18, United States Code, is amended by adding at the end the following new paragraph: (30) The term 'semiautomatic assault weapon' means–

(A) any of the firearms, or copies or duplicates of the firearms in any caliber, known as–

(i) Norinco, Mitchell, and Poly Technologies Avtomat Kalashnikovs (all models);

(ii) Action Arms Israeli Military Industries UZI and Galil;

(iii) Beretta Ar70 (SC-70);

(iv) Colt AR-15;

(v) Fabrique National FN/FAL, FN/LAR, and FNC;

(vi) SWD M-10, M-11, M-11/9, and M-12;

(vii) Steyr AUG;

(viii) INTRATEC TEC-9, TEC-DC9 and TEC-22; and

(ix) revolving cylinder shotguns, such as (or similar to) the Street Sweeper and Striker 12;

(B) a semiautomatic rifle that has an ability to accept a detachable magazine and has at least 2 of–

(i) a folding or telescoping stock;

(ii) a pistol grip that protrudes conspicuously beneath the action of the weapon;

(iii) a bayonet mount;

(iv) a flash suppressor or threaded barrel designed to accommodate a flash suppressor; and

(v) a grenade launcher;

(C) a semiautomatic pistol that has an ability to accept a detachable magazine and has at least 2 of–

(i) an ammunition magazine that attaches to the pistol outside of the pistol grip;

(ii) a threaded barrel capable of accepting a barrel extender, flash suppressor, forward handgrip, or silencer;

(iii) a shroud that is attached to, or partially or completely encircles, the barrel and that permits the shooter to hold the firearm with the nontrigger hand without being burned;

(iv) a manufactured weight of 50 ounces or more when the pistol is unloaded; and

(v) a semiautomatic version of an automatic firearm; and

(D) a semiautomatic shotgun that has at least 2 of–

(i) a folding or telescoping stock;

(ii) a pistol grip that protrudes conspicuously beneath the action of the weapon;

(iii) a fixed magazine capacity in excess of 5 rounds; and

(iv) an ability to accept a detachable magazine.

Finally, large capacity magazines were also banned.

(b) DEFINITION OF LARGE CAPACITY AMMUNITION FEEDING DEVICE - Section 921(a) of title 18, United States Code, as amended by section 110102(b), is amended by adding at the end the following new paragraph: (31) The term 'large capacity ammunition feeding device'–

(A) means a magazine, belt, drum, feed strip, or similar device manufactured after the date of enactment of the Violent Crime Control and Law Enforcement Act of 1994 that has a capacity of, or that can be readily restored or converted to accept, more than 10 rounds of ammunition;

A.2 Expiration of AWB and Relative Homicide Rate

A descriptive empirical analysis can be used to complement the observational evidence seen in Figure 1 and Figure 4. If the expiration of the AWB is contributing to the increase in the firearm homicide rate for any given level of violence, firearm homicides should become relatively more frequent, and for a given level of firearm prevalence, the type of guns made available should lead to a higher rate of firearm homicide. The following equation can be used to test these two concepts,

$$ln\left(y_{mst}\right) = \alpha + P_t ln\left(x_{mst}\right)\beta + ln\left(x_{mst}\right)\phi + X_{mst}\pi + \delta_m + \tau_{st} + \varepsilon_{mst}. \tag{A.1}$$

The dependent variable is the logged homicide rate per 10,000 people in municipality m, in state s, in year t. P_t is equal to one in the year 2005 and later ($P_t = 1[t \geq 2005]$). The logged x variable, $ln(x_{mst})$, measures the log of either the non-firearm homicide rate or the rate of illegal weapons possession. X_{mst} is a vector of municipal and year specific crime rates and other characteristics. Time-invariant municipal fixed effects (δ_m) are included along with state specific year fixed effects (τ_{st}). Estimates are weighted by municipal population and standard errors are clustered at the municipal level.[30] The coefficient ϕ captures the elasticity between the homicide rate on the left-hand side and the crime rate denoted by x, and the coefficient on the interaction between the post-AWB indicator and the logged crime rate, β, estimates the change in this relationship following the AWB's expiration.

Three estimates from equation (A.1) are shown in panel (a) of Table A.12 for the years 2000 to 2006. This time period spans a single administration and does not include any of the years following the military deployment by President Calderon. In the first two columns the dependent variable is the logged firearm homicide rate, and the dependent variable in column (3) is the logged non-firearm homicide rate. In column (1) the crime rate denoted by x_{mst} is the non-firearm homicide rate, and in the last two columns it is the logged illegal weapons possession rate. Using only municipality variation, the statistically significant estimates in the first column show that a ten percent increase in the non-firearm homicide rate following the AWB's expiration is associated with an additional increase in the firearm homicide rate of one-half of a percent, nearly double the pre-expiration elasticity. The second column yields a qualitatively similar estimate of the relationship between firearm homicides and illegal weapons possession. These two estimates demonstrate that the expiration of the AWB coincides with an increase in the level of firearm violence relative to the general level of violence as measured by the non-firearm homicide rate, and relative to the number of weapons illegally possessed. As a counterfactual, non-firearm homicides are compared to illegal weapons possession in column (3), and as would be expected, there is no statistically significant change in this relationship at the time of the AWB's expiration. The estimates in panel (b) extend the sample to include the years from 1999 to 2010, yielding a similar pattern. The only notable difference is that the inclusion of the drug war induces an increase in the level of non-firearm homicides, relative to illegal weapons possession, following the AWB expiration. However, this increase is less than half the estimated increase in the firearm homicide rate, and is not found in the better identified model in the following sections. All of these effects are consistent with the theory that the there need not be an increase in the number of available firearms, but a change in the type of firearms available, an increase in the availability of high powered firearms following the expiration of the AWB, contributed to the increase in the homicide rate in Mexico.

[30] The sample contains the same 1,864 municipalities that will be used in the paper's main analysis.

A.3 Tenure in Office and Violent Crime

A possible explanation for increased conflict between criminal groups and the government is that severing of some type of understanding that is built between the two entities over time. This could either be an explicit network of communication, or an implicit understanding of established boundaries. In either scenario, if both sides see benefit in lower levels of violent crime, there may exist a negative relationship between years of a single party in power as the mayor of a municipality and violent crime. This would be a plausible explanation of why the dissolution of the status quo following an incumbent defeat could lead to increased conflict between law enforcement and criminal groups. Although, the outcome of deterioration of the status quo is not clear, is it likely determined by the relative strength of the two competing entities, it would be helpful to understand why increased conflict could occur.

To examine whether a negative relationship does in fact exist between years a party remains in power and violent crime rates, a model similar to the one expressed in equation (A.1) is used.

$$ln\left(y_{mst}\right) = \alpha + YrslnPower_{mst}\beta + X_{mst}\pi + \delta_m + \tau_{st} + \varepsilon_{mst}. \tag{A.2}$$

The dependent variable is the log of the crime rate in municipality m, in state s, and year t. The set of controls are identical to equation (A.1). $YrslnPower_{mst}$ is the number of years the party of the mayor has been in power since 1995, and β is the coefficient of interest. Only measuring years in power from 1995 is unlikely to affect the measure; a single party, PRI (Partido Revolucionario Institucional), controlled most levels of government in Mexico for a number of decades prior to the years included in this study. The estimates for β are shown in Table A.13. Across all three types of homicides (firearm, non-firearm, total homicides) an additional year of a party in power is associated with a decline in the homicide rate, from a quarter of one percent per year for the non-firearm homicide rate to over four-tenths of a percent per year for the general homicide rate. This is evidence of a negative association between a party's tenure as mayor and violence, as suggested by the status quo assumption. This analysis can be expanded to show that the estimate is not driven by a single outlying year by substituting the years in power variable for a vector of indicator variables for each year in power, relative to the first year. These coefficient estimates are shown in Figure A.2, displaying the increasingly negative trend the longer a party remains in power. While the effect does exist, it is not statistically significant until a party is into its third three-year term.

A.4 Appendix - Figures and Tables

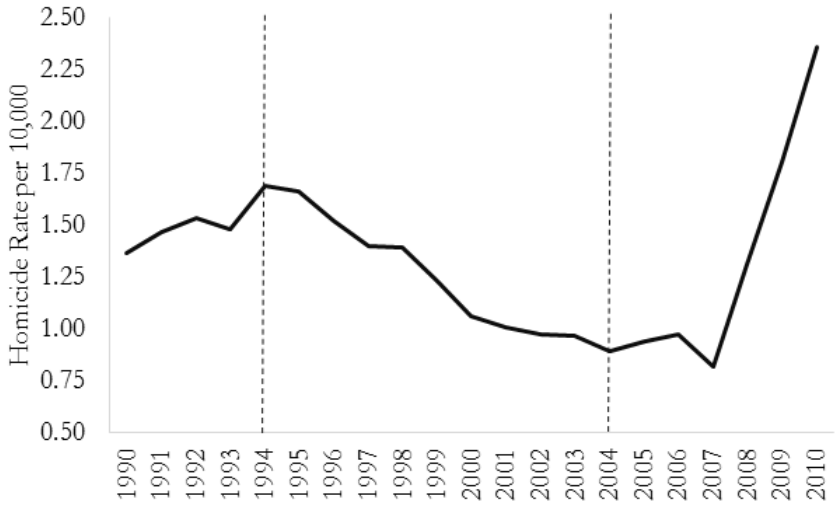


Figure A.1: Homicide Rate in Mexico (Full 2010 Scale)

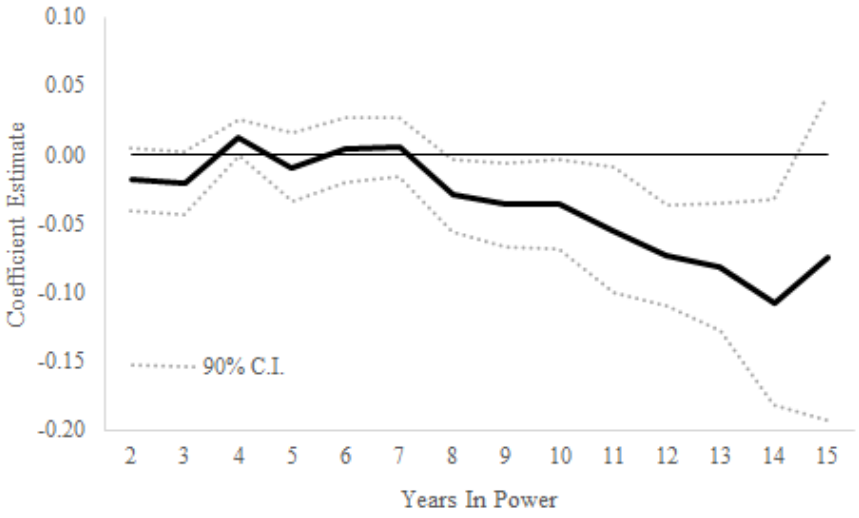
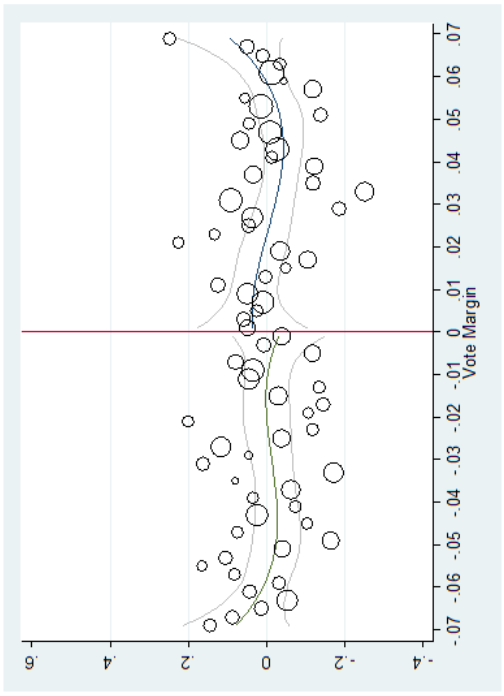
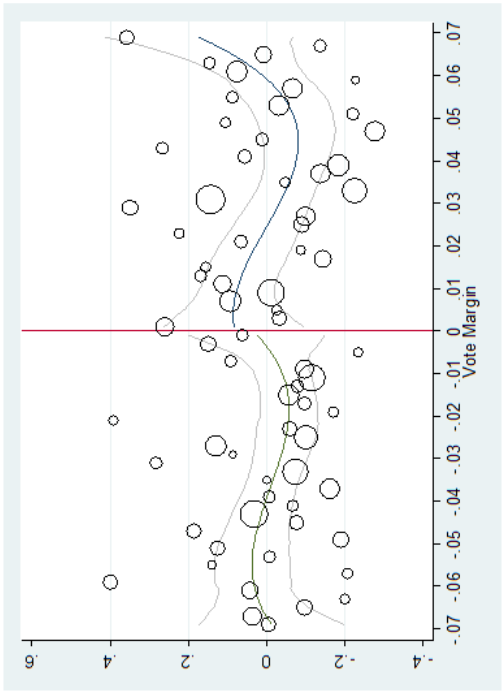


Figure A.2: Ln(Firearm Homicide Rate) and Years in Power: Relative to Year One

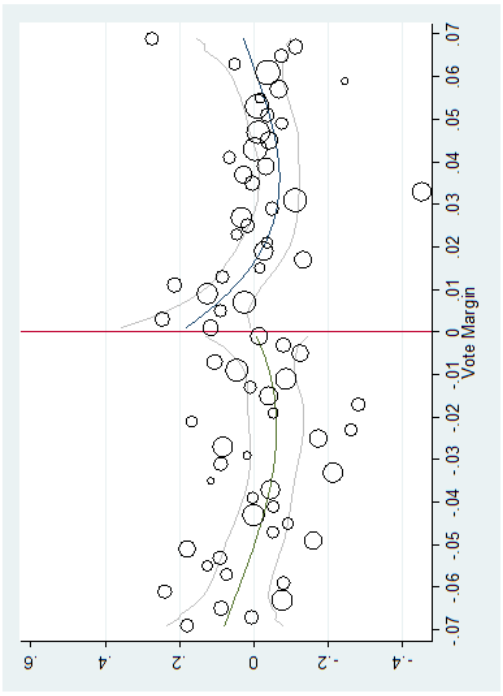
Figure A.3: Close Municipal Elections: 2000 - 2006



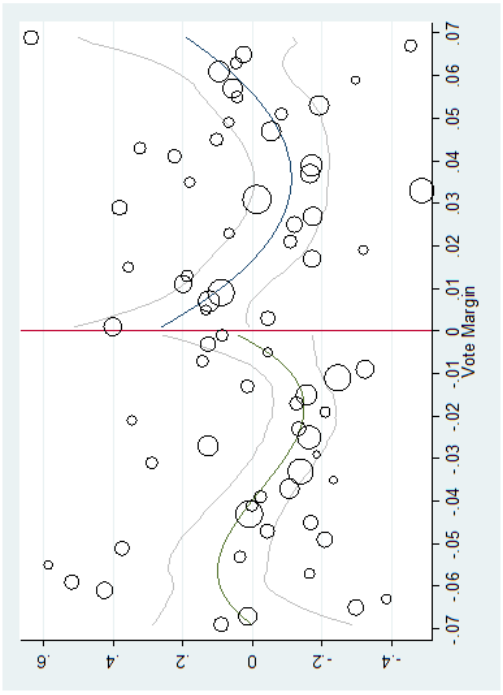
(a) Ln(Non-Firearm Homicide Rate): 2000-2006



(b) Ln(Non-Firearm Homicide Rate): 1999-2010



(c) Ln(Homicide Rate): 2000-2006



(d) Ln(Homicide Rate): 1999-2010

Figure A.4: DiRD Residuals of Ln(Non-Firearm Homicide Rate) and Ln(Homicide Rate)

Table A.1: DiRD - Estimates of Time-Invariant, Election Outcomes, and Lagged Municipal Characteristics and Crime Rates (Alternative Bandwidths)

(a) Time-Invariant

BW=	0.05	0.06	0.08	0.09
Distance_{ms}	-3.991	-19.96	-18.51	3.040
	(63.77)	(58.61)	(49.47)	(45.953)
Area_{ms}	-1,611*	-1,481*	-1,657***	-1,359**
	(839)	(763)	(609)	(567)
Area_{ms}	-610	-593	-801	-551
(drop one municipality)	(670)	(625)	(510)	(484)
N=	980	1,157	1,532	1,711

(b) Lagged Homicides

Ln(Firearm Homicide Rate)$_{ms(t-1)}$	-0.223	-0.236	-0.124	-0.078
	(0.174)	(0.158)	(0.132)	(0.120)
Ln(Non-Firearm Homicide Rate)$_{ms(t-1)}$	0.147	0.202	0.082	0.010
	(0.149)	(0.136)	(0.116)	(0.105)
Ln(Homicide Rate)$_{ms(t-1)}$	-0.069	-0.037	-0.023	-0.047
	(0.199)	(0.182)	(0.153)	(0.138)
N	982	1,159	1,534	1,713

(c) Election Outcomes

BW=	0.05	0.06	0.08	0.09
PAN $\text{Win}_{ms(t-1)}$	-0.158	-0.081	0.055	0.044
	(0.228)	(0.205)	(0.175)	(0.163)
Winning Vote $\text{Share}_{ms(t-1)}$	0.043	0.032	0.025	0.004
	(0.034)	(0.031)	(0.026)	(0.024)
PAN Vote $\text{Share}_{m(t-1)}$	-0.051	-0.065	-0.030	0.036
	(0.062)	(0.057)	(0.050)	(0.046)
PRI Vote $\text{Share}_{ms(t-1)}$	0.043	0.023	0.018	-0.019
	(0.038)	(0.034)	(0.030)	(0.027)
PRD Vote $\text{Share}_{ms(t-1)}$	0.102	0.054	0.049	0.045
	(0.067)	(0.061)	(0.052)	(0.048)
Years In $\text{Power}_{ms(t-1)}$	-1.129	-1.419	-1.577**	-1.000
	(0.973)	(0.892)	(0.757)	(0.707)
N	982	1,159	1,534	1,713

(d) Lagged Alternative Crimes

BW=	0.05	0.06	0.08	0.09
Ln(Theft $\text{Rate})_{ms(t-1)}$	-0.005	0.098	0.224	0.307*
	(0.252)	(0.230)	(0.191)	(0.176)
Ln(Narcotics Crime $\text{Rate})_{ms(t-1)}$	-0.193	-0.124	0.006	0.118
	(0.203)	(0.185)	(0.155)	(0.142)
Ln(Illegal Arms $\text{Rate})_{ms(t-1)}$	-0.126	-0.158	-0.202	-0.085
	(0.243)	(0.217)	(0.183)	(0.165)
Ln(Assault and Battery $\text{Rate})_{ms(t-1)}$	0.804***	0.613***	0.563***	0.405***
	(0.257)	(0.234)	(0.197)	(0.183)
Ln(Property Damage $\text{Rate})_{ms(t-1)}$	-0.219	-0.083	-0.048	-0.147
	(0.195)	(0.178)	(0.155)	(0.142)
Ln(Rape $\text{Rate})_{ms(t-1)}$	-0.184	-0.278**	-0.169	-0.079
	(0.145)	(0.132)	(0.115)	(0.105)
N	982	1,159	1,534	1,713

(e) Lagged Municipal Characteristics

BW=	0.05	0.06	0.08	0.09
Ln(GDP Per $\text{Capita})_{ms(t-1)}$	0.030	0.062	0.026	0.074
	(0.116)	(0.104)	(0.088)	(0.080)
Ln(Infant Mort. $\text{Rate})_{ms(t-1)}$	0.351	0.175	-0.024	0.132
	(0.242)	(0.216)	(0.178)	(0.163)
Ln(Tax Rev. Per $\text{Capita})_{ms(t-1)}$	0.565***	0.736***	0.392**	0.348**
	(0.218)	(0.205)	(0.174)	(0.154)
Ln(Vehicular Mortality $\text{Rate})_{ms(t-1)}$	-0.187	0.195	0.199	0.121
	(0.226)	(0.211)	(0.174)	(0.159)
Percent of Pop. Male: 20-49$_{ms(t-1)}$	-0.002	-0.003	-0.008*	-0.011**
	(0.006)	(0.006)	(0.005)	(0.004)
Ln(Pop. $\text{Density})_{ms(t-1)}$	0.277	0.287	1.237***	0.986**
	(0.560)	(0.516)	(0.421)	(0.387)
N	982	1,159	1,534	1,713

Note: *** p<0.01, ** p<0.05, * p<0.1. The dependent variable is the value of the specified municipal characteristic. The estimates shown are for the coefficient (β) on the interaction between the indicator variable for an incumbent defeat (D_{mst}) and the post-AWB indicator (P_t), from equation (2). Each regression includes state-year fixed effects, a cubic RD polynomial allowed to vary on either side of the election threshold and AWB expiration. The top five estimates in panel (c) include an indicator for whether the election is held in the last half of the year, and the predetermined outcomes of years in power (set of indicators), incumbent party (set of indicators), area, and distance (cubic). The estimate for years in power, determined in the previous election, only include the area and distance (cubic) controls. Excluding the dependent variable, all estimates in panels (b), (d), and (e) also include lagged values of the portion of the population that is male age 20 to 49, the logged population density, GDP per capita, municipal government income per capita, infant mortality rate, vehicular mortality rate, narcotics crime rate, theft rate, illegal weapons possession rate, rape rate, assault and battery rate, and the property damage rate. The eligible sample includes all municipalities outside of Oaxaca. Each regression is weighted using a triangular kernel, calculated using the vote margin, which is interacted with population for panels (b), (d), and (e), and total votes cast for panel (c).

Table A.2: DiRD - AWB Expiration and Ln(Firearm Homicide Rate) (Only July to November Elections)

	Pre-Expiration (2000 - 2004)	Post-Expiration (2005 - 2006)	DiRD (2000 - 2006)
	(1)	(2)	(3)
	(a) CV Optimal Bandwidth		
Ln(Firearm Homicide Rate)$_{mst}$	-0.225***	0.046	0.279**
	(0.083)	(0.112)	(0.137)
BW = 0.07	N=790	N=414	N=1,204
	(b) Alternative Bandwidths		
Ln(Firearm Homicide Rate)$_{mst}$	-0.167	0.066	0.230
	(0.108)	(0.122)	(0.164)
BW = 0.05	N=563	N=305	N=868
Ln(Firearm Homicide Rate)$_{mst}$	-0.158*	0.197*	0.367**
	(0.093)	(0.115)	(0.148)
BW = 0.06	N=669	N=356	N=1,025
Ln(Firearm Homicide Rate)$_{mst}$	-0.195***	-0.009	0.234*
	(0.075)	(0.102)	(0.124)
BW = 0.08	N=898	N=465	N=1,363
Ln(Firearm Homicide Rate)$_{mst}$	-0.219***	0.109	0.307***
	(0.068)	(0.099)	(0.115)
BW = 0.09	N=999	N=525	N=1,524

Note: *** p<0.01, ** p<0.05, * p<0.1. The dependent variable in all estimates is the natural log of the firearm homicide rate. The estimates shown in column (3) are for the coefficient (β) on the interaction between the indicator variable for an incumbent defeat (D_{mst}) and the post-AWB indicator (P_t), from equation (2); the estimates shown in column (1) and column (2) are for the coefficient (θ)on the indicator for an incumbent defeat, from equation (1). Regressions include state-year fixed effects, a lagged value of the dependent variable, a cubic RD polynomial allowed to vary on either side of the election threshold and AWB expiration, and the set of municipal controls described in footnote 11. The eligible sample includes all municipalities outside of Oaxaca with elections in July or later. Each regression is weighted using an interaction between a triangular kernel, calculated using the vote margin, and the municipal population.

Table A.3: DiRD - AWB Expiration and Ln(Firearm Homicide Rate) (With Violation of the Federal Firearms Law as Control Variable)

	Pre-Expiration (2000 - 2004)	Post-Expiration (2005 - 2006)	DiRD (2000 - 2006)
	(1)	(2)	(3)
	(a) CV Optimal Bandwidth		
Ln(Firearm Homicide Rate)$_{mst}$	-0.264***	0.080	0.360***
	(0.080)	(0.106)	(0.131)
BW = 0.07	N=897	N=463	N=1,360
	(b) Alternative Bandwidths		
Ln(Firearm Homicide Rate)$_{mst}$	-0.169	0.094	0.279*
	(0.105)	(0.114)	(0.157)
BW = 0.05	N=638	N=344	N=982
Ln(Firearm Homicide Rate)$_{mst}$	-0.195**	0.209*	0.440***
	(0.090)	(0.108)	(0.142)
BW = 0.06	N=760	N=399	N=1,159
Ln(Firearm Homicide Rate)$_{mst}$	-0.216***	-0.003	0.275**
	(0.072)	(0.097)	(0.119)
BW = 0.08	N=1,016	N=518	N=1,534
Ln(Firearm Homicide Rate)$_{mst}$	-0.228***	0.110	0.331***
	(0.065)	(0.093)	(0.110)
BW = 0.09	N=1,130	N=583	N=1,713

Note: *** p<0.01, ** p<0.05, * p<0.1. The dependent variable in all estimates is the natural log of the firearm homicide rate. The estimates shown in column (3) are for the coefficient (β) on the interaction between the indicator variable for an incumbent defeat (D_{mst}) and the post-AWB indicator (P_t), from equation (2); the estimates shown in column (1) and column (2) are for the coefficient (θ)on the indicator for an incumbent defeat, from equation (1). Regressions include state-year fixed effects, a lagged value of the dependent variable, a cubic RD polynomial allowed to vary on either side of the election threshold and AWB expiration, and the set of municipal controls described in footnote 11, except natural log of violation in federal firearm law per 10,000 people replaces the same calculation using illegal arms possession. The eligible sample includes all municipalities outside of Oaxaca. Each regression is weighted using an interaction between a triangular kernel, calculated using the vote margin, and the municipal population.

Table A.4: DiRD - Alternative Specifications of Ln(Firearm Homicide Rate) Estimates (Alternative Bandwidths)

	(a) Baseline (Table 5)				(b) Epanechnikov Kernel				(c) Uniform Kernel				(d) Any Party As Incumbent			
$P_t * D_{mst}$	0.263**	0.290**	0.354***	0.328**	0.261**	0.288**	0.356***	0.330**	0.261**	0.288**	0.356***	0.331**	0.250*	0.284**	0.353***	0.328**
	(0.130)	(0.131)	(0.131)	(0.138)	(0.132)	(0.132)	(0.132)	(0.139)	(0.132)	(0.132)	(0.132)	(0.139)	(0.128)	(0.129)	(0.129)	(0.140)
		N=1,398		N=1,360		N=1,398		N=1,360		N=1,398		N=1,360		N=1,398		N=1,360
	(e) Municipal Population > 10,000				(f) Municipal Population > 20,000				(g) Remove Top 1% of Change in Ln(Narcotics Crime Rate)				(h) Remove Top 5% of Change in Ln(Narcotics Crime Rate)			
$P_t * D_{mst}$	0.291*	0.323**	0.431***	0.421***	0.371**	0.394**	0.470**	0.489**	0.277**	0.305**	0.367***	0.372***	0.271**	0.298**	0.352***	0.373***
	(0.153)	(0.154)	(0.156)	(0.162)	(0.181)	(0.182)	(0.205)	(0.217)	(0.130)	(0.131)	(0.131)	(0.135)	(0.130)	(0.131)	(0.131)	(0.136)
		N=937		N=897		N=581		N=515		N=1,379		N=1,341		N=1,318		N=1,280
	(i) Remove Top 1% of Change in Ln(Illegal Arms Rate)				(j) Remove Top 5% of Change in Ln(Illegal Arms Rate)				(k) Remove Top 1% of Ln(Firearm Homicide Rate)				(l) Remove Top 5% of Ln(Firearm Homicide Rate)			
$P_t * D_{mst}$	0.262**	0.290**	0.353***	0.348**	0.274**	0.303**	0.384***	0.386***	0.243*	0.270**	0.310**	0.302**	0.206*	0.235*	0.293**	0.261**
	(0.131)	(0.131)	(0.131)	(0.135)	(0.134)	(0.134)	(0.134)	(0.139)	(0.129)	(0.129)	(0.128)	(0.132)	(0.122)	(0.123)	(0.119)	(0.123)
		N=1,388		N=1,350		N=1,310		N=1,274		N=1,387		N=1,349		N=1,327		N=1,290
	(m) Homicide Rate per 100,000				(n) Homicide Rate per 50,000				(o) Homicide Rate per 5,000				(p) Drop Ln(Tax) Outlier (Table 3)			
$P_t * D_{mst}$	0.614	0.697*	0.918**	0.905**	0.515*	0.577*	0.747**	0.711**	0.174**	0.191**	0.226***	0.212**	0.220*	0.248*	0.298**	0.295**
	(0.385)	(0.386)	(0.385)	(0.403)	(0.294)	(0.294)	(0.294)	(0.308)	(0.084)	(0.084)	(0.085)	(0.089)	(0.131)	(0.132)	(0.132)	(0.136)
		N=1,398		N=1,360		N=1,398		N=1,360		N=1,398		N=1,360		N=1,396		N=1,358
	(q) Quadratic				(r) Quartic				(s) Drop Area Outlier (Table 3)				(t) Drop Ln(Density) Outlier (Table 3)			
$P_t * D_{mst}$	0.219**	0.250***	0.234**	0.177*	0.479***	0.491***	0.499***	0.484***	0.263**	0.290**	0.354***	0.351***	0.264***	0.294**	0.339**	0.343**
	(0.095)	(0.096)	(0.098)	(0.103)	(0.159)	(0.159)	(0.164)	(0.166)	(0.130)	(0.131)	(0.131)	(0.135)	(0.131)	(0.131)	(0.132)	(0.135)
		N=1,398		N=1,360		N=1,398		N=1,360		N=1,396		N=1,358		N=1,397		N=1,359
P_t	X				X				X				X			
Year FE		X				X				X				X		
State-Year FE			X	X			X	X			X	X			X	X
$P_t * X_{mst}$				X				X				X				X

Note: *** p<0.01, ** p<0.05, * p<0.1. The dependent variable in all estimates is the natural log of the firearm homicide rate (per 10,000 unless otherwise noted in the table). The estimates shown are for the coefficient (β) on the interaction between the indicator variable for an incumbent defeat (D_{mst}) and the post-AWB indicator (P_t), from equation (2). Unless otherwise noted, regressions include time-specific fixed effects, as specified in the table, a lagged value of the dependent variable, a cubic RD polynomial allowed to vary on either side of the election threshold and AWB expiration, and the set of municipal controls described in footnote 11, and estimates in the fourth column of each panel interact each control variable (X_{mst})with the post-AWB indicator (P_t). Unless noted in the table, the eligible sample includes all municipalities outside of Oaxaca; each regression is weighted using an interaction between a triangular kernel, calculated using the vote margin, and the municipal population.

Table A.5: DiRD - (2000 - 2006) - Effect of AWB Expiration on Alternative Crime Rates (Alternative Bandwidths)

Bandwidth	Ln(Non-Firearm Homicide Rate)	Ln(Homicide Rate)	Ln(Theft Rate)	Ln(Narcotics Crime Rate)	Ln(Illegal Arms Rate)	Ln(Assault and Battery Rate)	Ln(Property Damage Rate)	Ln(Rape Rate)	Ln(Vehicular Mortality Rate)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
5%	0.084	0.182	-0.238	-0.847***	0.063	0.248	0.246	-0.144	-0.096
N=982	(0.144)	(0.185)	(0.244)	(0.183)	(0.215)	(0.235)	(0.206)	(0.146)	(0.212)
6%	0.131	0.361**	-0.006	-0.811***	0.094	0.137	0.290	-0.115	-0.105
N=1,159	(0.129)	(0.167)	(0.221)	(0.163)	(0.195)	(0.213)	(0.185)	(0.131)	(0.194)
8%	0.118	0.260*	-0.018	-0.656***	0.038	0.159	0.275*	-0.058	-0.029
N=1,534	(0.109)	(0.140)	(0.183)	(0.139)	(0.164)	(0.179)	(0.153)	(0.116)	(0.162)
9%	0.151	0.335***	-0.040	-0.678***	0.021	0.232	0.170	0.019	0.020
N=1,713	(0.099)	(0.128)	(0.168)	(0.128)	(0.151)	(0.162)	(0.140)	(0.106)	(0.148)

Note: *** p<0.01, ** p<0.05, * p<0.1. The dependent variable is the stated crime. The estimates shown are for the coefficient (β) on the interaction between the indicator variable for an incumbent defeat (D_{mst}) and the post-AWB indicator (P_t), from equation (2). Regressions include state-year fixed effects, a lagged value of the dependent variable, a cubic RD polynomial allowed to vary on either side of the election threshold and AWB expiration, and the set of municipal controls described in footnote 11, excluding the dependent variable. The eligible sample includes all municipalities outside of Oaxaca. Each regression is weighted using an interaction between a triangular kernel, calculated using the vote margin, and the municipal population.

Table A.6: DiRD - (1999 - 2010) - Expiration of the AWB and All Crime Rates (Alternative Bandwidths)

Bandwidth	Ln(Firearm Homicide Rate)	Ln(Non-Firearm Homicide Rate)	Ln(Homicide Rate)	Ln(Theft Rate)	Ln(Narcotics Crime Rate)	Ln(Illegal Arms Rate)	Ln(Assault and Battery Rate)	Ln(Property Damage Rate)	Ln(Rape Rate)	Ln(Vehicular Mortality Rate)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
5%	0.288**	0.004	0.188	-0.075	-0.301**	0.105	0.160	0.234	0.005	0.076
N=1,642	(0.127)	(0.109)	(0.142)	(0.174)	(0.141)	(0.162)	(0.172)	(0.143)	(0.103)	(0.154)
6%	0.268**	0.074	0.239*	0.025	-0.296**	0.035	0.172	0.249**	-0.024	0.050
N=1,954	(0.112)	(0.096)	(0.125)	(0.156)	(0.124)	(0.143)	(0.152)	(0.127)	(0.092)	(0.137)
8%	0.225**	0.042	0.197*	0.085	-0.305***	0.051	0.116	0.229**	-0.039	-0.070
N=2,618	(0.094)	(0.079)	(0.104)	(0.129)	(0.105)	(0.121)	(0.125)	(0.105)	(0.078)	(0.114)
9%	0.200**	0.042	0.192**	-0.005	-0.173*	0.004	0.130	0.191**	0.016	-0.097
N=2,942	(0.084)	(0.072)	(0.094)	(0.117)	(0.095)	(0.110)	(0.111)	(0.095)	(0.070)	(0.103)

Note: *** p<0.01, ** p<0.05, * p<0.1. The dependent variable is the stated crime. The estimates shown are for the coefficient (β) on the interaction between the indicator variable for an incumbent defeat (D_{mst}) and the post-AWB indicator (P_t), from equation (2). Regressions include state-year fixed effects, a lagged value of the dependent variable, a cubic RD polynomial allowed to vary on either side of the election threshold and AWB expiration, and the set of municipal controls described in footnote 11, excluding the dependent variable. The eligible sample includes all municipalities outside of Oaxaca. Each regression is weighted using an interaction between a triangular kernel, calculated using the vote margin, and the municipal population.

Table A.7: DiRD - (1999 - 2010) - Expiration of the AWB and All Crime Rates (Alternative Bandwidths - Cut Pre-Expiration Narcotics Crime)

Bandwidth	Ln(Firearm Homicide Rate)	Ln(Non-Firearm Homicide Rate)	Ln(Homicide Rate)	Ln(Theft Rate)	Ln(Narcotics Crime Rate)	Ln(Illegal Arms Rate)	Ln(Assault and Battery Rate)	Ln(Property Damage Rate)	Ln(Rape Rate)	Ln(Vehicular Mortality Rate)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
				(a) Remove Top Decile of Pre-Expiration Narcotics Crime						
5%	0.251*	-0.048	0.127	-0.007	-0.429***	-0.077	0.030	0.141	-0.010	-0.009
N=1,469	(0.129)	(0.113)	(0.147)	(0.189)	(0.139)	(0.168)	(0.189)	(0.152)	(0.110)	(0.163)
6%	0.226*	0.067	0.195	0.047	-0.325***	-0.126	0.034	0.181	-0.044	-0.098
N=1,748	(0.114)	(0.101)	(0.130)	(0.169)	(0.123)	(0.148)	(0.166)	(0.136)	(0.098)	(0.147)
8%	0.221**	0.083	0.216*	0.146	-0.266**	-0.073	-0.018	0.161	-0.077	-0.233*
N=2,369	(0.098)	(0.086)	(0.112)	(0.143)	(0.107)	(0.129)	(0.139)	(0.115)	(0.086)	(0.125)
9%	0.192**	0.074	0.194*	0.053	-0.171*	-0.087	0.003	0.129	-0.036	-0.233**
N=2,642	(0.089)	(0.079)	(0.102)	(0.131)	(0.099)	(0.119)	(0.126)	(0.106)	(0.078)	(0.115)

Note: *** p<0.01, ** p<0.05, * p<0.1. The dependent variable is the stated crime. The estimates shown are for the coefficient (β) on the interaction between the indicator variable for an incumbent defeat (D_{mst}) and the post-AWB indicator (P_t), from equation (2). Regressions include state-year fixed effects, a lagged value of the dependent variable, a cubic RD polynomial allowed to vary on either side of the election threshold and AWB expiration, and the set of municipal controls described in footnote 11, excluding the dependent variable. The eligible sample includes all municipalities outside of Oaxaca that were in the bottom nine deciles of pre-expiration narcotics crime. Each regression is weighted using an interaction between a triangular kernel, calculated using the vote margin, and the municipal population.

Table A.8: Inverse Distance to U.S.-Mexico Border and the Effect of AWB Expiration on Ln(Firearm Homicide Rate) - Alternative Bandwidths

	(a) 2000 - 2006		**(b) 1999 - 2010**			
					(Remove Top 10% of Pre-2005 Narcotics Crime Rates)	
	Remove Yucatan Peninsula	Full Sample	Remove Yucatan Peninsula	Full Sample	Remove Yucatan Peninsula	Full Sample
Bandwidth	(1)	(2)	(3)	(4)	(5)	(6)
5%	0.700**	0.199	0.699***	0.457***	0.638**	0.424***
	(0.316)	(0.179)	(0.229)	(0.146)	(0.279)	(0.157)
	N=870	N=1,022	N=1,476	N=1,728	N=1,317	N=1,561
6%	0.336	0.173	0.538***	0.460***	0.615**	0.439***
	(0.283)	(0.166)	(0.205)	(0.132)	(0.253)	(0.144)
	N=1,025	N=1,196	N=1,750	N=2,045	N=1,558	N=1,844
8%	0.184	0.011	0.324*	0.292**	0.514**	0.395***
	(0.241)	(0.144)	(0.181)	(0.119)	(0.227)	(0.130)
	N=1,354	N=1,574	N=2,321	N=2,700	N=2,065	N=2,435
9%	0.396*	0.072	0.165	0.228**	0.458**	0.375***
	(0.228)	(0.138)	(0.171)	(0.113)	(0.214)	(0.124)
	N=1,510	N=1,750	N=2,606	N=3,019	N=2,319	N=2,720

Note: *** p<0.01, ** p<0.05, * p<0.1. The dependent variable in all estimates is the natural log of the firearm homicide rate. The estimates shown are for the coefficient (β) on the triple interaction between the indicator variable for an incumbent defeat (D_{mst}), the post-AWB indicator (P_t), and the inverse distance measure (I_m) from equation (4). All regressions include year fixed effects, the lagged value of the dependent variable, a cubic RD polynomial allowed to vary on either side of the election threshold and AWB expiration, and the set of municipal controls described in footnote 11, except for the previously described distance control. The eligible sample includes all municipalities outside of Oaxaca. Each regression is weighted using an interaction between a triangular kernel calculated using the vote margin and the municipal population.

Table A.9: Municipal Months With At Least _____ Firearm Homicides

	Four		Five		Ten	
	Count	% Change	Count	% Change	Count	% Change
1998	389	—	277	—	86	—
1999	357	-0.08	240	-0.13	64	-0.26
2000	280	-0.22	190	-0.21	49	-0.23
2001	296	0.06	203	0.09	57	0.16
2002	281	-0.05	184	-0.09	54	-0.05
2003	265	-0.06	190	0.03	48	-0.11
2004	242	-0.09	166	-0.13	40	-0.17
2005	277	0.15	193	0.16	50	0.25
2006	309	0.12	215	0.11	66	0.32
2007	272	-0.12	186	-0.14	41	-0.38
2008	443	0.63	310	0.67	99	1.42
2009	657	0.48	476	0.54	170	0.72
2010	914	0.39	703	0.48	286	0.68

Counts are of monthly municipal observations with at least the stated number of homicides occuring within a given month; therefore, each municipality could have 12 observations above the inclusion threshold in a given year. This proxy matches the large post-2007 increase found in Ferguson et al. (2015).

Table A.10: Testing Impact of PAN Election Victory: Pre and Post-Calderon

Dependent Variable: Ln(Firearm Homicide Rate)$_{mst}$	Post-Calderon		Pre-Calderon	
	2007 - 2008	2007 - 2010	2000 - 2006	2000 - 2006
	(1)	(2)	(3)	(4)
$PAN\ Win_{ms(t-1)}$	0.503***	0.443***	-0.047	
	(0.163)	(0.148)	(0.069)	
P_t* $PAN\ Win_{ms(t-1)}$				0.019
				(0.170)
N	289	427	662	649

Note: *** p<0.01, ** p<0.05, * p<0.1. The dependent variable in all estimates is the natural log of the firearm homicide rate. The estimates shown in columns (1) to (3) are for the coefficient on an RD indicator variable equal to one for a PAN victory. The estimate in column (4) is for the coefficient on the DiRD interaction between the PAN victory indicator and an indicator equal to one for post-AWB years. In all estimates the running variable is the PAN vote margin; regressions include a lagged value of the dependent variable, a cubic RD polynomial allowed to vary on either side of the election threshold, and in column (4), on either side of the expiration of the AWB. The eligible sample includes all municipalities outside of Oaxaca. Each regression is weighted using an interaction between a triangular kernel, calculated using the vote margin, and the municipal population.

Table A.11: DiRD – Effect of Expiration of AWB on Ln(Firearm Homicide Rate) (2000 - 2006)

Controls	Bandwidth:	0.01	0.015	0.02	0.03	0.04	0.05	0.06	0.07	0.08	0.09
						A. Quadratic					
Only Ln(Firearm HomicideRate)$_{ms(t-1)}$	$P_t * D_{mst}$	0.745***	0.493**	0.361**	0.268*	0.207*	0.264**	0.178*	0.192**	0.260***	0.173**
		(0.238)	(0.193)	(0.170)	(0.142)	(0.123)	(0.117)	(0.105)	(0.095)	(0.086)	(0.079)
+ Full Set of Controls	$P_t * D_{mst}$	0.809***	0.481**	0.228	0.219	0.289**	0.352***	0.222**	0.219**	0.247***	0.155*
		(0.259)	(0.206)	(0.172)	(0.143)	(0.124)	(0.118)	(0.105)	(0.095)	(0.086)	(0.079)
	N	214	322	423	630	834	1,022	1,196	1,398	1,574	1,750
+ State-Year FE	$P_t * D_{mst}$	0.730**	0.539**	0.012	0.175	0.298**	0.304***	0.211**	0.234**	0.199**	0.136
		(0.335)	(0.243)	(0.184)	(0.158)	(0.132)	(0.118)	(0.106)	(0.098)	(0.088)	(0.084)
	N	150	269	372	573	788	982	1,159	1,360	1,534	1,713
						B. Cubic					
Only Ln(Firearm HomicideRate)$_{ms(t-1)}$	$P_t * D_{mst}$	0.908**	0.656**	0.616***	0.323*	0.327**	0.159	0.245*	0.174	0.129	0.243**
		(0.351)	(0.266)	(0.226)	(0.188)	(0.163)	(0.156)	(0.141)	(0.130)	(0.119)	(0.110)
+ Full Set of Controls	$P_t * D_{mst}$	0.753**	0.538*	0.656***	0.308*	0.382**	0.339**	0.388***	0.263**	0.197*	0.290***
		(0.369)	(0.287)	(0.226)	(0.182)	(0.160)	(0.155)	(0.140)	(0.130)	(0.118)	(0.109)
	N	214	322	423	630	834	1,022	1,196	1,398	1,574	1,750
+ State-Year FE	$P_t * D_{mst}$	0.619	0.745**	0.534**	0.216	0.239	0.276*	0.434***	0.354***	0.273**	0.333***
		(0.516)	(0.342)	(0.250)	(0.201)	(0.176)	(0.157)	(0.142)	(0.131)	(0.119)	(0.110)
	N	150	269	372	573	788	982	1,159	1,360	1,534	1,713
						C. Quartic					
Only Ln(Firearm HomicideRate)$_{ms(t-1)}$	$P_t * D_{mst}$	0.799**	1.266***	0.790***	0.673***	0.413**	0.488**	0.277	0.333**	0.264*	0.114
		(0.363)	(0.354)	(0.294)	(0.230)	(0.204)	(0.192)	(0.176)	(0.161)	(0.148)	(0.139)
+ Full Set of Controls	$P_t * D_{mst}$	0.777**	0.923**	0.773**	0.594**	0.463**	0.534***	0.392**	0.479***	0.374**	0.222
		(0.377)	(0.376)	(0.299)	(0.232)	(0.198)	(0.188)	(0.171)	(0.159)	(0.147)	(0.138)
	N	214	322	423	630	834	1,022	1,196	1,398	1,574	1,750
+ State-Year FE	$P_t * D_{mst}$	0.628	1.089**	0.570*	0.640**	0.364*	0.163	0.249	0.499***	0.428***	0.261*
		(0.551)	(0.429)	(0.320)	(0.257)	(0.216)	(0.192)	(0.174)	(0.162)	(0.149)	(0.141)
	N	150	269	372	573	788	982	1,159	1,360	1,534	1,713

Note: *** p<0.01, ** p<0.05, * p<0.1. The dependent variable in all estimates is the natural log of the firearm homicide rate. The estimates shown are for the coefficient (β) on the interaction between the indicator variable for an incumbent defeat (D_{mst}) and the post-AWB indicator (P_t), from equation (2). All regressions include the specified RD polynomial allowed to vary on either side of the election threshold and AWB expiration and a lagged value of the dependent variable, the second row in each panel adds the set of municipal controls described in footnote 11, and the third row adds a set of state-year fixed effects. The eligible sample includes all municipalities outside of Oaxaca. Each regression is weighted using an interaction between a triangular kernel, calculated using the vote margin, and the municipal population.

Table A.12: Change in Relative Homicide Rate At Time of AWB Expiration

	Ln(Firearm Homicide Rate)		**Ln(Firearm Homicide Rate)**	**Ln(Non-Firearm Homicide Rate)**
	(1)		(2)	(3)
		(a) 2000 - 2006		
P_t * $Ln(Non\text{-}Firearm\ HomicideRate)_{mst}$	0.051*	P_t * $Ln(Illegal\ WeaponsRate)_{mst}$	0.041**	-0.002
	(0.030)		(0.018)	(0.012)
$Ln(Non\text{-}Firearm\ HomicideRate)_{mst}$	0.060***	$Ln(Illegal\ WeaponsRate)_{mst}$	0.003	0.021***
	(0.013)		(0.008)	(0.008)
N	13,048		13,048	13,048
		(b) 1999 - 2010		
P_t * $Ln(Non\text{-}Firearm\ HomicideRate)_{mst}$	0.124***	P_t * $Ln(Illegal\ WeaponsRate)_{mst}$	0.058***	0.024**
	(0.029)		(0.015)	(0.011)
$Ln(Non\text{-}Firearm\ HomicideRate)_{mst}$	0.097***	$Ln(Illegal\ WeaponsRate)_{mst}$	0.025***	0.023***
	(0.014)		(0.009)	(0.008)
N	22,368		22,368	22,368

Note: *** p<0.01, ** p<0.05, * p<0.1. The dependent variable is the specified logged homicide rate. Each regression includes a set of state-year and municipal fixed effects. Control variables included are the fraction of the population that is male age 20 to 49, logged values of the population density, GDP per capita, municipal government income per capita, infant mortality rate, vehicular mortality rate, narcotics crime rate, theft rate, illegal weapons possession rate, rape rate, assault and battery rate, property damage rate. The sample includes all municipalities outside of Oaxaca. Each regression is weighted by the municipal population, and standard errors are clustered at the municipal level.

Table A.13: Incumbency and Homicide Rates (1999 - 2010)

	Ln(Firearm Homicide Rate)	**Ln(Non-Firearm Homicide Rate)**	**Ln(Homicide Rate)**
	(1)	(2)	(3)
$YrsInPower_{mst}$	-0.0037***	-0.0025**	-0.0044***
	(0.0013)	(0.0011)	(0.0013)
N	22,336	22,336	22,336

Note: *** p<0.01, ** p<0.05, * p<0.1. The dependent variable is the specified logged homicide rate. Each regression includes a set of state-year fixed and municipal effects. Control variables included are the fraction of the population that is male age 20 to 49, logged values of the population density, GDP per capita, municipal government income per capita, infant mortality rate, vehicular mortality rate, narcotics crime rate, theft rate, illegal weapons possession rate, rape rate, assault and battery rate, property damage rate. The eligible sample includes all municipalities outside of Oaxaca. Each regression is weighted by the municipal population, and standard errors are clustered at the municipal level.

EXHIBIT "15"

ORIGINAL INVESTIGATION

HEALTH CARE REFORM

Firearm Legislation and Firearm-Related Fatalities in the United States

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Importance: Over 30 000 people die annually in the United States from injuries caused by firearms. Although most firearm laws are enacted by states, whether the laws are associated with rates of firearm deaths is uncertain.

Objective: To evaluate whether more firearm laws in a state are associated with fewer firearm fatalities.

Design: Using an ecological and cross-sectional method, we retrospectively analyzed all firearm-related deaths reported to the Centers for Disease Control and Prevention Web-based Injury Statistics Query and Reporting System from 2007 through 2010. We used state-level firearm legislation across 5 categories of laws to create a "legislative strength score," and measured the association of the score with state mortality rates using a clustered Poisson regression. States were divided into quartiles based on their score.

Setting: Fifty US states.

Participants: Populations of all US states.

Main Outcome Measures: The outcome measures were state-level firearm-related fatalities per 100 000 individuals per year overall, for suicide, and for homicide. In various models, we controlled for age, sex, race/ethnicity, poverty, unemployment, college education, population density, nonfirearm violence–related deaths, and household firearm ownership.

Results: Over the 4-year study period, there were 121 084 firearm fatalities. The average state-based firearm fatality rates varied from a high of 17.9 (Louisiana) to a low of 2.9 (Hawaii) per 100 000 individuals per year. Annual firearm legislative strength scores ranged from 0 (Utah) to 24 (Massachusetts) of 28 possible points. States in the highest quartile of legislative strength (scores of ≥9) had a lower overall firearm fatality rate than those in the lowest quartile (scores of ≤2) (absolute rate difference, 6.64 deaths/100 000/y; age-adjusted incident rate ratio [IRR], 0.58; 95% CI, 0.37-0.92). Compared with the quartile of states with the fewest laws, the quartile with the most laws had a lower firearm suicide rate (absolute rate difference, 6.25 deaths/100 000/y; IRR, 0.63; 95% CI, 0.48-0.83) and a lower firearm homicide rate (absolute rate difference, 0.40 deaths/100 000/y; IRR, 0.60; 95% CI, 0.38-0.95).

Conclusions and Relevance: A higher number of firearm laws in a state are associated with a lower rate of firearm fatalities in the state, overall and for suicides and homicides individually. As our study could not determine cause-and-effect relationships, further studies are necessary to define the nature of this association.

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THE TOTAL NUMBER OF annual firearm fatalities in the United States has been stable over the last decade.[1,2] From 2007 to 2010, the range was 31 224 to 31 672 fatalities per year.[1] There is substantial variation in firearm fatality rates among states, however, with the average annual state-based firearm fatality rates ranging from a high of 17.9 (Louisiana) to a low of 2.9 (Hawaii) per 100 000 individuals during these years. In 2010, firearms killed 68% of the 16 259 victims of homicide. In the same year, there were 38 364 suicides, of which 51% were by firearms.[1] Beyond the loss of life and nonfatal traumatic injuries, the financial cost of firearm injuries is enormous. In 2005, the medical costs associated with fatal and nonfatal firearm injuries were estimated at $112 million and $599 million, respectively, and work loss costs were estimated at $40.5 billion.[1]

See Invited Commentary at end of article

CME available online at jamanetworkcme.com and questions on page 723

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Mass killings such as those in Columbine and Aurora in Colorado, the Wisconsin Sikh temple shooting, and most recently the Newtown, Connecticut, school massacre have renewed debate about the need for more stringent firearm legislation. Some have called for more restrictions on gun purchases.[3] Others have called for arming teachers.[4] It is challenging to calculate the exact number of firearm laws: a single law may have multiple parts; laws are potentially passed at the national, state, county, and city level; and there is no repository available for tallying these laws.[5] The factoid that there are "20 000 laws governing firearms"[5] has been erroneously quoted since 1965, but the most recent and reliable estimate, performed in 1999, counted about 300 state firearm laws.[6]

The real question is not about the number of firearm laws but whether the laws ultimately safeguard the citizens they are intended to protect. Although multiple studies have examined the relationship between federal and state firearm laws and homicide and suicide rates, the overall association between firearm legislation and firearm mortality is uncertain and remains controversial.[7,8]

We evaluated whether variations in the strength of state firearm legislation are associated with variations in the rates of firearm fatalities. We examined overall firearm death rates as well as firearm suicide and firearm homicide rates by state, controlling for other factors previously associated with firearm fatalities.

METHODS

The Boston Children's Hospital institutional review board approved the study.

DATABASE

We used data from the Web-Based Injury Statistics Query and Reporting System (WISQARS),[1] which provides mortality tables with the numbers of injury-related deaths and mortality rates according to cause (mechanism) and intent of injury (unintentional, violence-related [including homicide and suicide], or undetermined) by year, sex, age, race/ethnicity, and state. These mortality data are compiled by the National Center for Health Statistics, Centers for Disease Control and Prevention (CDC) from multiple cause of death data. The federal government mandates that each state provide information about deaths that occur within its border.[9] Mortality data on nonfirearm intentional deaths (suicides and homicides) were also obtained from WISQARS.

STUDY POPULATION

We identified all violence-related firearm fatalities between January 2007 and December 2010, and used data on age-adjusted firearm mortality, including suicides (60.9% of firearm-related fatalities) and homicides (39.1% of firearm-related fatalities). Homicides due to legal intervention, unintentional firearm fatalities, and fatalities of undetermined intent (1.1%, 1.9%, and 0.8% of total firearm-related fatalities, respectively) were excluded from the analyses.

STATE-LEVEL FACTORS

We studied all 50 states. To quantify state-level variation in gun regulations, we used data from the Brady Campaign to Prevent Gun Violence[10] and the Brady Center to Prevent Gun Violence (referred to collectively herein as *the Brady Center*). Working with the Law Center to Prevent Gun Violence (formerly Legal Community Against Violence), the Brady Center has tracked firearm legislation annually since 2007 and prepared legislative scorecards for every state each year. It divides firearm legislation into 5 categories according to the intended effect: (1) curb firearm trafficking; (2) strengthen background checks on purchasers of firearms beyond those required by the Brady Handgun Violence Prevention Act; (3) ensure child safety; (4) ban military style assault weapons; and (5) restrict guns in public places (**Table 1**). The Brady Act, which went into effect in 1994, requires background checks of potential buyers before a firearm may be purchased from a federally licensed dealer, manufacturer, or importer. Firearm sales are prohibited to convicted felons and fugitives. They are also prohibited to persons with a history of addiction to controlled substances, persons restrained by court order against harassment, those convicted of domestic violence, and those adjudicated as "mentally defective," among other groups. The Brady Center's fifth category, restricting guns in public places, refers to the absence of laws that would allow guns in public places.

For our primary analysis, we used a simplified approach to create a "legislative strength score" for each state. The legislative strength score was developed before the analyses were conducted. Each state could have enacted up to 28 laws; each enacted law received 1 point. This "1 law = 1 point" score gives each law equal weight. However, the Brady Center also prepares an empirical weight schema for each set of laws, scaling the scores out of 100 points and giving additional weight to laws believed to be more important. In their weighted scoring system, the "strengthen Brady background checks" category (which includes requiring universal background checks on all firearm purchases no matter who sells the firearm and requiring permits to purchase firearms) receives the greatest number of points. We separately analyzed the data using this weighted scoring system. A detailed description of each of the laws and the weighted scoring system is available from the Brady Center.[10]

We used US Census data to capture state-level statistics on factors and characteristics previously shown to be associated with firearm fatalities: race/ethnicity (white, black, Hispanic), sex, living below the federal poverty level, unemployment, college education, and state population density.[8] In addition, we calculated household firearm ownership rates per state using the firearm suicide/total suicide ratio, which is the proportion of all suicides in a state caused by firearms.[11] This ratio has been highly correlated with firearm ownership rates in the United States and other developed nations.[12-17] There are no direct data from 2007 through 2010 on firearm ownerships rates in the United States; the last large state-based survey of firearm ownership was performed in 2004 by the CDC's Behavioral Risk Factor Surveillance System.

OUTCOME MEASURES

Our primary outcome measures were overall firearm-related fatality rates per 100 000 individuals per year. The rates for firearm suicides and firearm homicides were considered separately.

DATA ANALYSIS

First, we obtained the number of firearm-related suicides and firearm-related homicides for each state. We calculated death rates by dividing the total number of deaths by the state populations each year and adjusting for age. We then divided states into quartiles based on their legislative strength score, with quar-

Table 1. Scoring System for Firearm Legislative Strength Score[a]

Legislation Intent	Description of Measures
Curb firearm trafficking (9 points)	
Gun dealer regulations (6 points)	State license required for firearm dealers
	Record keeping and retention by firearm dealers
	Report records to the state, and state retains records
	Mandatory theft reporting for all firearms by firearm dealers
	At least 1 store security precaution required
	Inspections by police allowed/required to inspect dealer inventories
Limit bulk purchases (1 point)	One handgun per month (exceptions possible)
Crime gun identification (1 point)	Ballistic fingerprinting or require microstamping on semi-automatic handguns
Report lost/stolen guns (1 point)	Mandatory reporting by firearm owners
Strengthen Brady background checks (8 points)	
Universal background check[b] (1 point)	All firearms
	Handguns only
Closed gun show loophole[c] (1 point)	Background check on firearm purchasers at gun shows
Permit to purchase (5 points)	Permits required to purchase firearms
	Fingerprinting of applicants required for identification
	Safety training and/or testing required
	Extend three-day limit for background checks
	Permit process involves law enforcement
Ammunition regulations (2 points)	Ammunition purchaser records kept/vendor license required
	Ammunition Brady check/permit required to purchase
Improve child safety (5 points)	
Childproof handguns (1 point)	Only authorized users are able to operate new handguns
Child safety locks[d] (2 points)	Integrated locks sold on all handguns
	External locks sold with all handguns
	Standards on all external locks – child safety locks certified
Child access prevention[e] (1 point)	Adults must store loaded guns in inaccessible place or lock the gun
Juvenile handgun purchases (1 point)	Must be 21 to purchase a handgun
Ban military-style assault weapons (2 points)	
Assault weapons ban (2 points)	Regulation of firearms with military-style features
	Maximum number of rounds per magazine 15 or less
Restrict guns in public places[f] (4 points)	
No guns in workplace (1 point)	Employers not required to allow firearms in parking lots
No guns on college campuses (1 point)	Colleges are not required to allow firearms on campus
Not carrying a concealed weapon shall issue state (1 point)	Law enforcement is not required to issue a permit to carry a concealed weapon to all individuals who can legally own a firearm
No state preemption of local laws (1 point)	Local governments can enact firearm laws and regulations that are stricter than state laws
Overall possible points, 28	

[a] Table data source, Brady Center State Scorecards.[10]
[b] States receive a point for background checks on either all firearms or handguns only.
[c] States with universal background checks on all firearms not eligible for gun show loophole points.
[d] One point for either integrated or external locks.
[e] If a child in the specified age ranges obtains a stored, loaded gun, the adult owner may be held criminally liable. Any age category receives credit: 16 to 17 years or younger, 14 to 15 years or younger, or 13 years or younger.
[f] Points assigned for restriction of guns in public places to trained law enforcement and security and preserve local control over municipal gun laws.

tile 1 including the states with the lowest scores and quartile 4, the states with the highest scores.

Our study design used an ecological and cross-sectional method. To evaluate the association of firearm-related fatalities (overall, suicide, and homicide) with the legislative strength score as the main predictor,[12] we constructed 3 models for each outcome. In model 1, we computed a Poisson regression, adjusting for age, to evaluate the association between the annual score and firearm fatality rates without further adjustments. In model 2, to account for other socioeconomic factors associated with firearm fatalities, we used a multivariable Poisson regression to adjust for age, race/ethnicity, sex, poverty, unemployment, college education, population density, and rates of nonfirearm suicides and/or nonfirearm homicides. In model 3 we added household firearm ownership rates to the variables included in model 2. Across all 3 models, we analyzed the firearm suicide data by year. Overall firearm-related fatalities and homicide fatalities were aggregated at the state level over the entire 4-year study period: the small numbers of firearm homicides in 12 states precluded the availability of annual data. These aggregate data were divided to derive a mean annual fatality rate. To evaluate whether weighting the relative significance of specific laws would alter the association of the legislative strength score with firearm fatalities, we ran the multivariable model 2 with the quartiles derived from the weighted

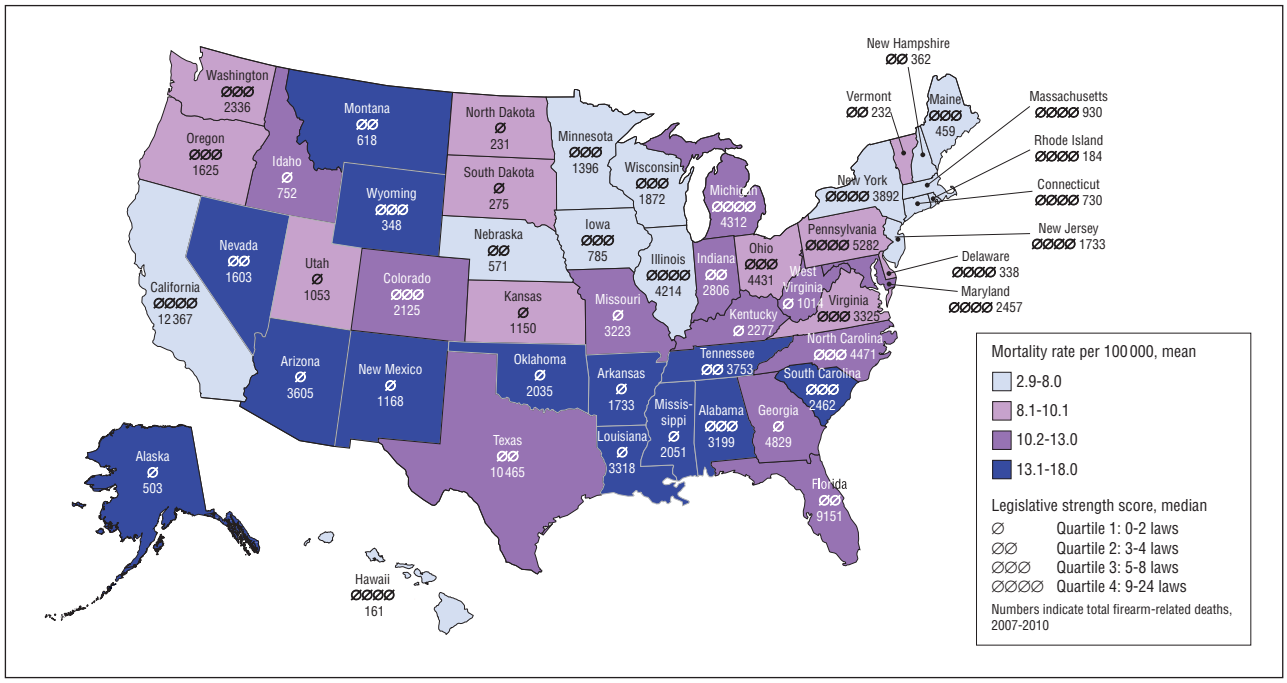

Figure 1. Firearm-related mortality rates, legislative strength scores, and total firearm deaths in the United States, 2007 through 2010.

Brady score as a separate analysis.[10] We present age-adjusted absolute rate differences, referenced to quartile 1.

To further explore whether some legislative categories may have a greater association with firearm fatalities than other legislative categories, we created a multivariable Poisson regression to evaluate the association of each of the 5 categories of legislation with firearm fatality rates (overall, suicide, and homicide). Similar to model 2, we adjusted for socioeconomic factors and nonfirearm suicides and/or homicides. For all modeling, we used clustered robust sandwich standard error estimates, which allow for intrastate correlation, relaxing the assumption that observations from the same state are independent.

Firearm ownership rates have been associated with firearm suicide and firearm homicide rates in other studies.[8,18] We hypothesized that an important way in which legislation might affect the firearm fatality rate in a state is through changes in firearm prevalence. For example, laws requiring background checks for all gun purchases or raising the purchase age to 21 can be expected to reduce firearm ownership rates. To explore this hypothesis, we conducted a stepwise analysis of firearm ownership. First, we examined the association of the legislative strength score with firearm ownership rates using a simple linear regression with firearm ownership rates as the outcome and the score as the predictor. Then, using simple linear regression, we evaluated whether household firearm ownership rates were associated with overall firearm fatality rates. Then we reanalyzed our multivariable model 3 with linear regression and evaluated the effect of firearm ownership rates on the legislative strength score and overall firearm fatalities using the Sobel-Goodman test.[19,20]

Finally, we examined whether differences between states in their rates of firearm-related fatalities were owing to a replacement effect, ie, the possibility that lower rates of firearm-related fatalities were being replaced with higher rates of nonfirearm-related violent fatalities. We controlled for nonfirearm suicide rates in the suicide regression and for nonfirearm homicide rates in the homicide regression. We performed a Poisson regression with nonfirearm violent fatalities as the outcome and firearm fatalities as the predictor. In addition, we used Poisson regression to evaluate the relationship between legislative strength scores and nonfirearm-related violent fatalities. If these fatalities were associated with firearm legislation, it would suggest that other unmeasured factors affected the rates of both firearm- and nonfirearm-related fatalities.

All of the data analyses were performed using STATA SE, version 11 (StataCorp).

RESULTS

Between 2007 and 2010, there were 121 084 firearm fatalities in the United States, including 73 702 firearm suicides and 47 382 firearm homicides. The overall firearm fatality rate was 9.9/100 000 individuals per year. The variation between the highest and lowest state-level mortality rates was up to a 6-fold difference (**Figure 1** and **Table 2**). Firearm legislative strength scores per year by state ranged from 0 (Utah) to 24 (Massachusetts) of 28 possible points, with some variation by year (Table 2). The median and range for each legislative strength score quartile were as follows: first quartile, 2 (0-2); second quartile, 3 (3-4); third quartile, 6 (5-8); and fourth quartile, 16 (9-24).

The simple regression model demonstrated that higher legislative strength scores were associated with lower rates of firearm fatalities overall ($P < .001$) (**Figure 2**A). In the multivariable overall fatality Poisson model, which controlled for state-specific socioeconomic and demographic factors, we found that compared with the referent group of the quartile with the fewest laws, the quartile of states with the most laws had an absolute rate difference of 6.64 deaths/100 000 per year, with an adjusted incident rate ratio (IRR) of 0.58 (95% CI, 0.37-0.92). In the multivariable suicide model, compared with the referent, the quartile with the most laws had an absolute rate difference of 6.25 deaths/100 000 per year, with an adjusted IRR of 0.63 (95% CI, 0.48-0.83). In the multivari-

Table 2. State Legislative Strength Scores and Firearm Fatality Rates per 100 000 Individuals per Year, 2007-2010[a]

Rank	State	Legislative Strength Score, Median (Range)[b]	Firearm Fatalities, Mean (SD)		
			Overall	Suicide	Homicide
1	Massachusetts	22.5 (22-24)	3.4 (0.42)	1.7 (0.31)	1.7 (0.18)
2	California	22 (22-23)	8.0 (0.45)	4.0 (0.06)	4.0 (0.45)
	New Jersey	22 (22-24)	4.9 (0.19)	1.9 (0.04)	3.0 (0.27)
4	Connecticut	20 (19-20)	5.1 (0.76)	2.6 (0.40)	2.5 (0.39)
5	New York	19 (19-19)	4.8 (0.18)	2.1 (0.10)	2.7 (0.06)
6	Hawaii	16 (15-16)	2.9 (0.44)	2.3 (0.39)	0.7 (0.08)
	Maryland	16 (15-17)	10.5 (1.20)	4.1 (0.35)	6.3 (1.00)
8	Rhode Island	14 (13-14)	4.1 (0.61)	2.6 (0.70)	1.5 (0.25)
9	Illinois	11.5 (11-12)	7.9 (0.18)	3.3 (0.15)	4.7 (0.22)
10	Michigan	11 (10-11)	10.6 (0.05)	5.6 (0.22)	5.1 (0.22)
11	Delaware	9 (8-9)	9.5 (1.10)	4.6 (0.34)	4.8 (1.20)
12	Pennsylvania	8.5 (8-9)	10.1 (0.24)	5.7 (0.25)	4.3 (0.27)
13	Alabama	8 (8-8)	16.3 (0.73)	9.0 (0.64)	7.2 (0.99)
	North Carolina	8 (7-8)	11.7 (0.44)	7.0 (0.27)	4.6 (0.56)
	Virginia	8 (8-8)	10.1 (0.28)	6.5 (0.33)	3.4 (0.30)
	Washington	8 (8-9)	8.4 (0.12)	6.6 (0.29)	1.8 (0.10)
17	Iowa	7 (3-7)	6.2 (0.87)	5.2 (0.72)	0.9 (0.30)
18	Minnesota	6 (5.6)	6.4 (0.33)	5.2 (0.18)	1.2 (0.22)
	Oregon	6 (6-6)	9.9 (0.64)	8.5 (0.51)	1.3 (0.19)
20	Colorado	5 (5-5)	10.3 (0.54)	8.3 (0.47)	2.1 (0.16)
	Maine	5 (5-5)	8.0 (0.44)	6.8 (0.58)	1.1 (0.09)
	Ohio	5 (4.5)	9.1 (0.70)	5.5 (0.51)	3.6 (0.19)
	South Carolina	5 (5-6)	13.0 (0.24)	7.5 (0.64)	5.4 (0.29)
	Wisconsin	5 (4-5)	8.0 (0.45)	6.0 (0.24)	1.9 (0.34)
	Wyoming	5 (4-5)	15.5 (1.80)	14.6 (1.50)	1.3 (0.004)
26	Georgia	4 (4-5)	12.2 (0.37)	7.2 (0.56)	5.1 (0.58)
	Nebraska	4 (3-4)	7.6 (0.56)	5.2 (0.28)	2.3 (0.40)
	New Hampshire[c]	4 (3-4)	6.4 (0.51)	6.0 (0.86)	NA
	Tennessee	4 (4-4)	14.3 (0.54)	8.9 (0.34)	5.3 (0.44)
	Vermont[c]	4 (4-4)	8.7 (0.75)	7.8 (1.50)	NA
31	Florida	3 (3-4)	11.8 (0.45)	6.9 (0.33)	4.8 (0.48)
	Indiana	3 (2-3)	10.5 (0.36)	6.7 (0.40)	3.8 (0.21)
	Mississippi	3 (3-3)	16.8 (1.10)	9.3 (0.55)	7.4 (0.68)
	Nevada	3 (3-3)	14.9 (0.73)	10.9 (0.35)	3.9 (0.78)
	Texas	3 (3-3)	10.5 (0.21)	6.6 (0.33)	3.9 (0.28)
36	Montana	2.5 (2-3)	14.8 (0.48)	12.8 (0.72)	1.8 (0.41)
37	Arkansas	2 (2-2)	14.5 (0.78)	9.1 (0.52)	5.3 (0.44)
	Kansas	2 (2-4)	9.9 (0.58)	7.0 (0.47)	2.8 (0.41)
	Missouri	2 (2-2)	13.0 (0.56)	7.4 (0.43)	5.5 (0.67)
	North Dakota[c]	2 (2-2)	8.4 (0.16)	7.9 (0.48)	NA
	New Mexico	2 (2-2)	13.8 (0.22)	9.6 (0.26)	4.2 (0.33)
	South Dakota	2 (2-2)	8.2 (1.50)	7.3 (1.50)	0.9 (0.02)
	West Virginia	2 (2-2)	12.7 (1.30)	9.9 (0.88)	2.7 (0.45)
44	Arizona	1.5 (1-2)	13.6 (0.68)	8.9 (0.57)	4.8 (0.89)
	Idaho	1.5 (1-2)	11.8 (0.85)	10.8 (1.00)	1.1 (0.62)
46	Alaska	1 (1-1)	17.5 (2.80)	14.4 (2.70)	3.2 (0.87)
	Kentucky	1 (1-1)	12.6 (0.71)	9.2 (0.36)	3.3 (0.41)
	Louisiana	1 (1-2)	18.0 (0.85)	7.8 (0.54)	10.1 (0.73)
	Oklahoma	1 (1-1)	13.4 (0.41)	9.4 (0.58)	4.0 (0.33)
50	Utah	0.5 (0-1)	9.8 (1.30)	8.8 (1.30)	1.1 (0.19)

Abbreviations: CDC, Centers for Disease Control and Prevention; NA, not available.

[a] Data are from the WISQARS (Web-based Injury Statistics Query and Reporting System)[1] and the legislative strength score.

[b] Legislative strength score is the median of the annual scores for 2007 through 2010. The highest legislative strength score received the lowest rank. States with the same legislative strength score are listed in alphabetical order within that score.

[c] State with a low number of annual deaths (<20) from homicide. Mean rate was not available from CDC.

able homicide model, compared with the referent, the quartile with the most laws had an absolute rate difference of 0.40 deaths/100 000 per year, with an adjusted IRR of 0.60 (95% CI, 0.38-0.95) (**Table 3**). In the models including firearm availability, an increased legislative strength score trended in the direction of lower firearm homicides but was significant only in quartile 3. Controlling for firearm availability attenuated the association between legislative strength score and firearm suicide. When the Brady Center weighted scores were used as the predictor in the models, the IRRs did not substantially change (data not shown).

For the specific legislative categories, only background checks had a significant relationship across all

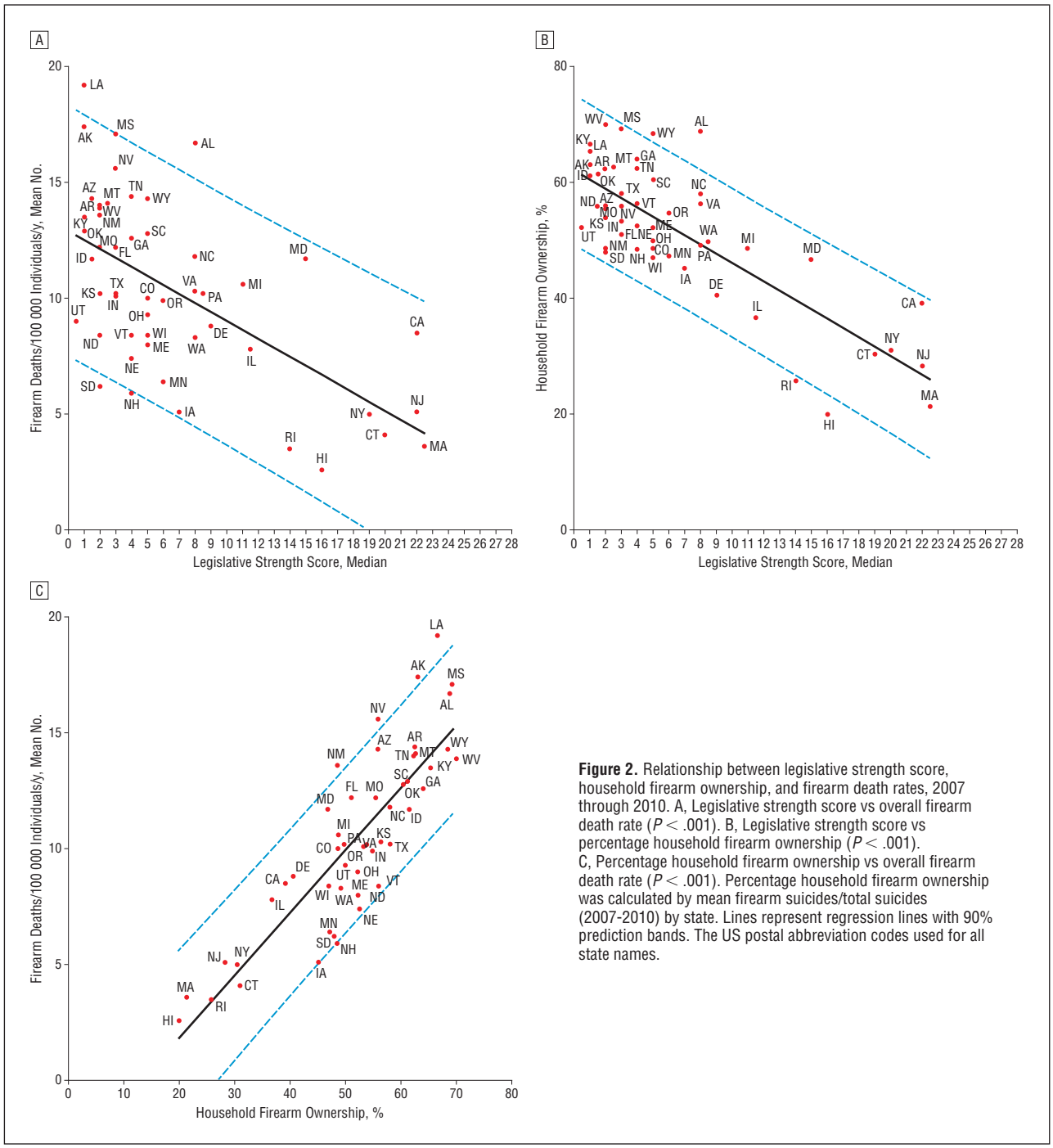

Figure 2. Relationship between legislative strength score, household firearm ownership, and firearm death rates, 2007 through 2010. A, Legislative strength score vs overall firearm death rate ($P < .001$). B, Legislative strength score vs percentage household firearm ownership ($P < .001$). C, Percentage household firearm ownership vs overall firearm death rate ($P < .001$). Percentage household firearm ownership was calculated by mean firearm suicides/total suicides (2007-2010) by state. Lines represent regression lines with 90% prediction bands. The US postal abbreviation codes used for all state names.

outcomes, with stronger background checks associated with lower overall firearm fatality rates: a 1-point increase in the background check category had an adjusted IRR of 0.84 (95% CI, 0.78-0.92), lower firearm suicide fatality rates (adjusted IRR, 0.90; 95% CI, 0.87-0.94), and lower firearm homicide fatality rates (adjusted IRR, 0.91; 95% CI, 0.84-0.99) (**Table 4**).

Higher legislative strength scores were associated with lower household firearm ownership ($P < .001$) (Figure 2B). Higher percentage of household firearm ownership was associated with higher rates of overall firearm fatalities ($P < .001$) (Figure 2C). The Sobel-Goodman test of mediation demonstrated a significant effect of firearm ownership on the relationship between the legislative strength score and overall firearm fatalities ($P < .001$).

The simple Poisson regression demonstrated no association between firearm-related deaths and nonfirearm violent deaths ($P = .50$). There was also no association between legislative strength scores and nonfirearm violence–related deaths ($P = .20$).

COMMENT

In an analysis of all states using data from 2007 through 2010, we found that a higher number of firearm laws in

Table 3. Change in Firearm Fatality Rates by Legislative Strength Quartile

Legislative Strength Quartile	Absolute Rate Difference[b,c]	Incident Rate Ratio (95% CI)[a]		
		Model 1[c]	Model 2[d]	Model 3[e]
		Overall Firearm Fatalities[f]		
1 (0-2 laws)	0 [Reference]	1 [Reference]	1 [Reference]	1 [Reference]
2 (3-4 laws)	1.48	0.88 (0.74-1.06)	0.92 (0.74-1.10)	0.95 (0.88-1.02)
3 (5-8 laws)	2.96	**0.77 (0.63-0.93)**	0.88 (0.65-1.19)	0.89 (0.79-1.00)
4 (9-24 laws)	6.64	**0.48 (0.36-0.65)**	**0.58 (0.37-0.92)**	1.00 (0.83-1.21)
		Firearm Suicide		
1 (0-2 laws)	0 [Reference]	1 [Reference]	1 [Reference]	1 [Reference]
2 (3-4 laws)	1.17	**0.85 (0.73-0.99)**	0.94 (0.82-1.08)	0.97 (0.94-1.00)
3 (5-8 laws)	2.52	**0.78 (0.65-0.93)**	0.94 (0.78-1.14)	0.99 (0.95-1.01)
4 (9-24 laws)	6.25	**0.34 (0.26-0.43)**	**0.63 (0.48-0.83)**	0.97 (0.92-1.02)
		Firearm Homicide[f]		
1 (0-2 laws)	0 [Reference]	1 [Reference]	1 [Reference]	1 [Reference]
2 (3-4 laws)	0.31	0.91 (0.57-1.46)	0.89 (0.71-1.12)	0.83 (0.68-1.08)
3 (5-8 laws)	0.44	0.88 (0.52-1.48)	0.69 (0.46-1.04)	**0.65 (0.46-0.93**
4 (9-24 laws)	0.40	0.89 (0.54-1.47)	**0.60 (0.38-0.95)**	0.79 (0.49-1.26)

[a] Change in firearm fatality rate represented by the incident rate ratio with reference to quartile 1; boldface type indicates a confidence interval that does not overlap 1.
[b] Absolute rate differences are per 100 000 individuals per year with reference to quartile 1.
[c] Absolute rate differences and model 1 are both age adjusted.
[d] Model 2 is adjusted for age and for control variables (state population density; nonfirearm violence–related fatalities; and percentage of the study population that was male, white, black, Hispanic, in poverty, unemployed, and college educated).
[e] Model 3 is adjusted for age and all control variables, including household firearm ownership.
[f] Data aggregated over 4 years for analysis.

Table 4. Change in Overall Firearm Fatality Rates Associated With 1-Point Increase in Each Legislative Category[a]

Legislative Category	Overall Firearm Fatalities[b]		Firearm Suicide		Firearm Homicide[b]	
	Absolute Rate Difference[c]	IRR (95% CI)[d]	Absolute Rate Difference[c]	IRR (95% CI)[d]	Absolute Rate Difference[c]	IRR (95% CI)[d]
Firearm trafficking	6.67	1.01 (0.96-1.07)	6.22	1.01 (0.97-1.05)	0.46	0.99 (0.92-1.06)
Strengthen Brady checks[e]	9.80	**0.84 (0.78-0.92)**	9.42	**0.90 (0.87-0.94)**	0.41	**0.91 (0.84-0.99)**
Child safety	5.52	0.87 (0.75-1.00)	5.84	**0.86 (0.78-0.95)**	−0.32	1.01 (0.89-1.13)
Ban assault weapons	6.35	**0.73 (0.59-0.90)**	5.37	**0.77 (0.67-0.89)**	0.97	0.84 (0.66-1.07)
Guns in public places[f]	6.35	**0.88 (0.77-0.99)**	6.61	**0.91 (0.82-0.99)**	−0.26	0.94 (0.82-1.09)

Abbreviations: IRR, incident rate ratio; US postal code abbreviations used to indicate individual US states.
[a] The models are adjusted for age and for control variables (state population density; nonfirearm violence–related fatalities; and percentage of the study population that was male, white, black, Hispanic, in poverty, unemployed, and college educated); bold type indicates a confidence interval that does not overlap 1.
[b] Data aggregated over 4 years for analysis.
[c] Absolute rate difference between states with lowest score and those with highest score in given legislative category. Rates are age adjusted and reflect the number per 100 000 individuals per year. Low and high scores in the given categories are as follows: Firearm trafficking low, 0 (20 states); high, 7-8 (CA, MA, and NJ). Strengthen Brady checks low, 0 (33 states); high, 6-7 (CT, HI, MA, and NJ). Child safety low, 0 (21 states); high, 4-5 (CA, MD, MA, and NJ). Ban assault weapons low, 0 (43 states); high, 2 (CA, HI, MA, NJ, and NY). Guns in public places low, 0-1 (10 states); high, 4 (CA, CT, HI, IL, MA, NJ, and NY).
[d] Change in firearm fatality rates, represented by the IRR, between scores 1 point apart in a specific legislative category.
[e] This includes universal background checks and permits to purchase. See Table 1 for further details.
[f] States that do not have laws that allow guns in public places. See Table 1.

a state was associated with a lower rate of firearm fatalities in the state. This association was present both before and after controlling for other state-specific and socioeconomic factors. Although the results across quartiles 2 through 4 of the legislative strength score demonstrated lower firearm fatalities, these results were only significant when the states with the highest scores were compared with those with the lowest scores. It is important to note that our study was ecological and cross-sectional and could not determine cause-and-effect relationship.

Previous studies evaluating the association of firearm legislation and reducing firearm injuries and fatalities in the United States have had mixed results. Most of the studies focused on specific laws, not the aggregate effect of all laws.[21] For example, a study evaluating the Brady Act, which mandates background checks for firearm purchases, found that suicide rates among persons 55 years or older were reduced, but there were no other differential effects of the law.[22] Despite the law's intent, background checks are relatively easily thwarted at gun shows, flea markets, and elsewhere, where a person who would otherwise be prohibited from purchasing firearms can purchase a gun from a private seller without a background check.[23,24]

Studies that have examined the cumulative impact of firearm legislation, rather than single laws, have often focused on the association of legislation and suicide.[25,26] Conner and Zhong,[27] using data across all 50 states from 1999 to 2000, demonstrated that more restrictive firearm laws were associated with lower rates of suicide. Price et al,[12] using data from 1999 across all 50 states, also found a strong association between restrictiveness of gun laws and firearm suicide but little association with firearm homicide. The association with firearm suicide was not significant after adjusting for household gun ownership levels.[12]

Another important factor affecting suicide is whether guns are stored safely in the home. Guns are the most common method of suicide overall[1] and teen suicide in particular,[28,29] and increased accessibility to loaded, unlocked guns is associated with an increased risk of suicide.[30-33] A case-control study found that safe gun storage practices, which can be required by state law, were associated with a decreased risk of teen suicide and unintentional firearm injuries.[34]

One way that firearm legislation may act to reduce firearm fatalities is through reducing firearm prevalence.[35] Studies have shown a strong connection between gun ownership and firearm suicide[8,36] and firearm homicide.[37] A cross-sectional study of all 50 states from 2001 to 2003 found that higher rates of household firearm ownership were associated with significantly higher rates of homicide.[38] Similarly, rates of suicide are higher in states with greater rates of household firearm ownership.[39]

Although our study found an association between legislation strength, firearm availability, and overall firearm fatalities, the nature of this association should be further characterized. Within a state, culture and attitudes toward firearms may confound the association between firearm ownership and firearm legislation. High levels of gun ownership might be related to both high rates of firearm deaths and a cultural environment in which it is more difficult for a state to enact strict firearm laws. Firearm ownership may also be a mediator of the relationship between the legislative strength score and overall fatalities. The change in the coefficients in the model after the inclusion of household gun ownership rates is consistent with both mediation and confounding.

As is not surprising in a cross-sectional ecological study, we found some heterogeneity in the firearm fatality rates among the states within each level of the legislative strength scores (eg, South Dakota has weak gun control laws and low rates of firearm fatality). Such heterogeneity is to be expected and is the reason to conduct a study that involves all 50 states.

Our study has limitations. First, the legislative strength score, which tallies a single point per law, has not been validated. Neither has the weighted Brady scoring system, and we are unaware of any such scoring systems that have been validated. Our results, which divided states into quartiles of legislative strength, were essentially the same with either of these scoring systems. Second, we examined only deaths by firearms, not nonfatal firearm injuries; fatality was our primary outcome. Approximately 2.6 nonfatal firearm injuries are treated for every fatal firearm injury.[1,40] Third, we were unable to control for the enforcement of firearm laws or the exploitation of loopholes, which may vary between states. Fourth, although we adjusted for many state-based factors associated with firearm fatalities, there may be additional factors not considered in our model that are relevant (eg, city laws and police enforcement). However, we included nonfirearm suicides and nonfirearm homicides in some of our analyses to control for the potential role of additional factors. We found little evidence of substitution—rates of firearm-related deaths were not correlated with rates of nonfirearm violent death in the multivariable model. Fifth, although we found that states with more legislation have lower fatality rates, ie, are "safer" states, in a cross-sectional ecological study we could not determine if the greater number of laws were the reason for the reduced fatality rates. The association could have been confounded by firearm ownership rates or other unaccounted factors.

In conclusion, we found an association between the legislative strength of a state's firearm laws—as measured by a higher number of laws—and a lower rate of firearm fatalities. The association was significant for firearm fatalities overall and for firearm suicide and firearm homicide deaths, individually. As our study could not determine a cause-and-effect relationship, further studies are necessary to define the nature of this association.

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INVITED COMMENTARY

Responding to the Crisis of Firearm Violence in the United States

The United States has belatedly awakened to the knowledge that it is, in effect, under armed attack. More than 30 000 people are purposely shot to death each year—more than 300 000 since the World Trade Center was destroyed in 2001. Rates of firearm-related violent crime have increased 26% since 2008.[1] Physicians have joined others in demanding a strong response to this crisis. We look to scientific research to provide the evidence on which that response should be based. Such evidence should include a thorough exploration of risk and protective factors and, most importantly, controlled studies showing which interventions work to reduce firearm violence and why.

At a time when guidance is urgently needed, Fleegler and colleagues[2] have examined the relationship between firearm laws and firearm-related deaths in the United States. Their state-level ecological study (a design in which the unit of analysis is a population in aggregate, not the individuals in it) correlated the presence or absence of 28 laws arguably related to firearm violence with firearm-related mortality rates. Their main finding is that having more laws on the books is associated with having lower rates of firearm-related homicide and suicide. This would be an important finding—if it were robust and if its meaning were clear.

Ecological studies of association are inherently weak, however; correlation does not imply causation. This fundamental limitation is beyond the power of the authors to redress. And there are additional concerns. The study's

EXHIBIT "16"

The Impact of State Firearm Laws on Homicide and Suicide Deaths in the USA, 1991–2016: a Panel Study

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BACKGROUND: Firearm injuries are a major cause of mortality in the USA. Few recent studies have simultaneously examined the impact of multiple state gun laws to determine their independent association with homicide and suicide rates.

OBJECTIVE: To examine the relationship between state firearm laws and overall homicide and suicide rates at the state level across all 50 states over a 26-year period.

DESIGN: Using a panel design, we analyzed the relationship between 10 state firearm laws and total, age-adjusted homicide and suicide rates from 1991 to 2016 in a difference-in-differences, fixed effects, multivariable regression model. There were 1222 observations for homicide analyses and 1300 observations for suicide analyses.

PARTICIPANTS: Populations of all US states.

MAIN MEASURES: The outcome measures were the annual age-adjusted rates of homicide and suicide in each state during the period 1991–2016. We controlled for a wide range of state-level factors.

KEY RESULTS: Universal background checks were associated with a 14.9% (95% CI, 5.2–23.6%) reduction in overall homicide rates, violent misdemeanor laws were associated with a 18.1% (95% CI, 8.1–27.1%) reduction in homicide, and "shall issue" laws were associated with a 9.0% (95% CI, 1.1–17.4%) increase in homicide. These laws were significantly associated only with firearm-related homicide rates, not non-firearm-related homicide rates. None of the other laws examined were consistently related to overall homicide or suicide rates.

CONCLUSIONS: We found a relationship between the enactment of two types of state firearm laws and reductions in homicide over time. However, further research is necessary to determine whether these associations are causal ones.

KEY WORDS: community health; firearms; health policy; injury; prevention; public health.

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INTRODUCTION

From 1991 to 2016, the average annual firearm death rate in the USA was 11.4 per 100,000 individuals.[1] This amounts to 859,871 lives lost due to a single cause of preventable death over a 26-year period.[1] Although numerous studies have evaluated the impact of state firearm laws on homicide or suicide rates (Online Supplemental Tables S1, S2), a major limitation is that most examined the impact of only one type of policy. Because states that enact one type of law are also more likely to enact others,[2] it is difficult to isolate the effect of one law without considering the simultaneous impact of other policies.

To improve our ability to draw causal inferences, a stronger study design would examine the relationship between the enactment of *multiple* types of state firearm laws over time and differences in fatality rates between states. However, we are aware of only one multi-year panel study of homicide rates that examined multiple laws and included data from the past decade; this study was conducted at the level of urban counties, and only 34 states were included.[3] We are not aware of any panel study at the state level that used data within the past decade to assess simultaneously the effect of multiple state firearm laws on homicide or suicide death rates.

One reason why many previous studies have focused on a single type of law is the absence of a comprehensive national database of state firearm laws. For most previous studies, researchers had to track down the status of state firearm laws by conducting their own legal research, a painstaking process that precluded a single study of a large range of gun-related policies. We recently created a novel database in which we recorded, quantified, and classified the largest-to-date compilation of firearm provisions by state over a 26-year period.[2] In this study, we examine the simultaneous impact of 10 different types of state firearm laws on overall homicide and suicide rates over a 26-year period using the same model specification.

METHODS

Data Sources

We ascertained the annual presence or absence of 10 state firearm laws in all 50 states from 1991 to 2016 using the State Firearm Law Database, which provides a panel of firearm-related laws in each state, for each year.[2] The database was

compiled using the Thompson Reuters Westlaw database of state statutes and session laws and a database assembled by Everytown for Gun Safety.[4]

We obtained homicide and suicide mortality data from the Centers for Disease Control and Prevention Web-Based Injury Statistics Query and Reporting System (WISQARS), which are derived from the vital statistics death registry of the National Center for Health Statistics.[1] WISQARS reports annual state-specific, age-adjusted fatality rates for homicide and suicide.

Study Population

We assembled annual, state-specific age-adjusted total homicide and suicide rates in each state from 1991 to 2016. We excluded homicides due to legal intervention (1% of firearm deaths), unintentional firearm fatalities (2.5% of firearm deaths), and fatalities of undetermined intent (1% of firearm deaths) from our analysis.

Outcome Measures

The main outcome measures were the annual, age-adjusted homicide rate and age-adjusted suicide rate in each state over the study period. Because there were 50 states and 26 years, the total number of possible observations was 1300. However, the CDC does not report death rates when the absolute number of deaths in a state during a given year is less than 10. For this reason, we did not have a complete panel of homicide data for three states: North Dakota, Vermont, and Wyoming. We therefore excluded these states from the homicide analyses, yielding a total of 1222 observations. There were no missing data for suicide death rates, so there were 1300 observations for analyses involving this outcome.

Main Predictor Variables

From the state law database, we selected 10 laws to analyze based on several considerations: (1) laws that are currently being considered by state legislatures; (2) laws that have been examined in prior research; and (3) laws that were enacted by at least two states during the study period. We analyzed the following 10 laws (defined in detail in Table 1): (1) universal background checks, either through point-of-purchase checks or a permit to purchase requirement; (2) ban on handgun possession for people convicted of a violent misdemeanor; (3) age 21 limit for handgun possession; (4) "shall issue" laws; (5) permitless carry laws; (6) prohibition against gun trafficking; (7) ban on "junk guns"; (8) "stand your ground" laws; (9) assault weapons ban; and (10) ban on large-capacity ammunition magazines. Laws were lagged by 1 year in the analysis; that is, we considered the potential effect of a law only in the full first year after its enactment.

Data Analysis

Unlike many earlier analyses in the public health literature, we employed a difference-in-differences approach to the analysis of policy outcomes,[5, 6] an approach that is widely used in the econometric and criminology literature on the effect of state firearm laws and was first introduced by Lott and Mustard in their classic 1997 paper.[7] Using multivariable linear regression, we evaluated the association between the firearm law provisions in each state (which were time-varying) and the homicide and suicide rates over the study period, while controlling for several other time-varying state-level factors. We included year and state fixed effects and estimated cluster-robust standard errors, which account for the clustering of observations, serial autocorrelation, and heteroskedasticity.[8] By including state fixed effects, our analysis focuses on the time series of observations within each state, comparing changes in homicide or suicide rates within a state from before to after the implementation of a particular firearm law, using states without that law as controls. Because the outcome variables are not normally distributed but skewed, we log-transformed the homicide and suicide rates.

Our final model was as follows:

$$ln\,(\mu_{st}) = \alpha + (B_{*}LAW_{st}) + (C_{*}CONTROL_{st}) + S + T + e,$$

where μ_{st} is the homicide or suicide rate in state s in year t, LAW_{st} is a dummy variable for the presence or absence of a particular state firearm law in state s in year t, $CONTROL_{st}$ is a vector of control variables, S represents state fixed effects, and T represents year fixed effects.

We controlled for the following time-varying state-level factors, chosen because of their association with homicide or suicide rates in the published literature and their association with both death rates and the adoption of firearm laws in our data set: (1) the percent of the population that is black; (2) the percent of population ages 15–29 that is male; (3) per capita law enforcement officers; (4) the violent crime rate (excluding homicide); (5) the divorce rate; (6) the unemployment rate; (7) the poverty rate; (8) per capita alcohol consumption; (9) the incarceration rate; (10) population density; (11) log of population; and (12) household gun ownership percentage.

Because annual survey data of household gun ownership at the state level are not available, most previous studies have used the ratio of firearm suicides to all suicides (FS/S) as a proxy for household firearm ownership.[9] This proxy is highly correlated ($r = 0.80$) with state-specific measures of firearm ownership on a cross-sectional basis.[10] Recently, we developed a new proxy measure that improves the correlation with survey-measured gun ownership from 0.80 to 0.95.[10] This new proxy measure incorporates a state's hunting license rate in addition to FS/S.[10] In this study, we used this new proxy.

Per capita law enforcement officers and violent crime rates were obtained from the FBI Uniform Crime Reports;[11] incarceration rates were obtained from the Bureau of Justice Statistics;[12] and per capita alcohol consumption was obtained from the National Institute on Alcohol Abuse and Alcoholism (NIAAA) for 1991–2015[13] and from Statistica[14] for 2016. Hunting licensing data were obtained from the U.S. Fish and Wildlife Service.[15] The remaining variables were obtained

Table 1 Description of State Firearm Laws Examined

Law	Brief description	Detailed description	States with law in 1991	Additional states with law in 2016	Law changes from 1991 to 2016
Universal background checks	Background checks conducted through permit requirement for all firearm sales or through required background checks for all sales)	Individuals must undergo a background check to purchase any type of firearm, either at the point of purchase or through a license/permit application. This may or may not include exemptions for buyers who have already undergone a background check for a concealed carry permit or other licensing requirements.	CA, IL, MA, NJ, RI	CO, CT, DE, HI, NY, OR, WA	7
Violent misdemeanor is prohibiting for handgun possession	Handgun possession is prohibited for people who have committed a violent misdemeanor punishable by less than 1 year of imprisonment	Must cover possession of handguns, not just purchase. Must cover assault, not just aggravated assault. Must extend beyond domestic violence-related misdemeanors, restraining orders, and stalking. Must not require that misdemeanor be punishable by imprisonment of more than 1 year. Must not require that misdemeanor involve use of a firearm or result in injury.	CA, HI, NY	CT, MD	2
Age 21 limit for handgun possession	No possession of handguns until age 21	You must be 21 to possess a handgun. No exemption for parental consent. Exclusions for adult-supervised hunting, sporting, or training activities are OK. Exception for possession on private premises NOT OK unless minor required to be under adult supervision.	IA, RI, SC	CT, HI, MD, MA, NJ, NY (SC repealed)	7
Shall issue law	Law provides no discretion to law enforcement authorities in deciding whether to grant a concealed carry permit.	A permit must be issued unless the applicant meets pre-established disqualifying criteria.	FL, GA, ID, IN, IA, ME, MS, MT, NH, ND, OR, PA, SD, WA, WV	AL, AR, CO, IL, KY, LA, MI, MN, MO, NE, NV, NM, NC, OH, OK, SC, TN, TX, UT, VA, WA, WI (WV moved to permitless carry)	23
Permitless carry	No permit is required to carry a concealed handgun.	Age restrictions may apply, and a voluntary permitting system may still be in place.	VT	AK, AZ, ID, KS, ME, MS, WV, WY	8
Trafficking prohibited	No person may purchase a firearm with the intent to re-sell to a person who is prohibited from buying or possessing a firearm	The law prohibits the purchase of a firearm with the intent to re-sell to a prohibited person. We make no distinction between whether the trafficker (original purchaser) must actually know or have reason to believe that the buyer is prohibited. An exemption for sale to relatives is acceptable.	FL, MA, ND, OH, VA	CA, CO, CT, DE, IL, MN, NY, UT, VA	9
Junk gun ban	Ban on junk guns (sometimes called "Saturday night specials")	The law prohibits the sale of handguns that fail to meet one or more of the following requirements: (1) Passes drop testing and firing testing; (2) Passes a melting point test; (3) Possesses specific handgun safety features; (4) Appears on a list of approved handguns. This may or may not apply to private sellers.	HI, IL, MD, MN, SC	CA, MA (SC repealed)	3
Stand your ground law	A "stand your ground" law is in place	Use of deadly force is allowed to be a first resort if you are threatened in a public place in which you have the right to be present. There is no duty to retreat. Does not count as stand your ground law if it only	None	AL, AK, AZ, FL, GA, IN, KS, KY, LA, MI, MS, MO, MT, NV, NH, NC, OK, PA, SC, SD, TN, TX, UT, WV	24

(continued on next page)

Table 1. (continued)

Law	Brief description	Detailed description	States with law in 1991	Additional states with law in 2016	Law changes from 1991 to 2016
		applies when person is in a vehicle.			
Assault weapons ban	Ban on sale of assault weapons beyond just assault pistols	Law bans the sale of both assault pistols and other assault weapons.	CA, NJ	CT, MD, MA, NY	4
Large capacity ammunition magazine ban	Ban on sale large capacity magazines beyond just ammunition for pistols	Law bans the sale of both assault pistol ammunition and other large-capacity magazines.	NJ	CA, CO, CT, MD, MA, NY	6

from the U.S. Census. We conducted the analysis using Stata version 15 (StataCorp LP, College Station, TX).

Because the outcome variables are log-transformed, the regression coefficients can be interpreted as the percentage change in the firearm homicide or suicide rate associated with the presence of a particular law by exponentiating the coefficient, subtracting 1, and then multiplying by 100 (i.e., a coefficient of 0.10 for a given law would indicate a 10.5% increase in the mortality rate associated with that law).

To test the plausibility of any observed associations between firearm laws and overall homicide or suicide rates, we conducted a falsification test: we analyzed the relationship between these laws and firearm compared to non-firearm mortality rates. These laws would be expected to primarily affect only the firearm-related rates.

In a final sensitivity analysis, we modeled the secular time trend in firearm homicide or suicide rates by including year as a continuous variable in the model rather than as a fixed effect.

RESULTS

Over the 26-year study period, there was a substantial variation in the violent death rates across states. In 2016, overall homicide rates ranged from a low of 1.3 per 100,000 in Maine and New Hampshire to a high of 14.2 per 100,000 in Louisiana (Table 2). In 2016, overall suicide rates ranged from a low of 7.2 per 100,000 in New Jersey to a high of 26.0 per 100,000 in Montana. Across the study period, there were a total of 93 law changes among the 10 laws studied (Table 1).

When examined individually, universal background checks and violent misdemeanor laws were significantly associated with lower overall homicide rates and "shall issue" laws were significantly associated with higher homicide rates (Table 3). After simultaneously controlling for all 10 firearm laws, universal background checks were associated with 14.9% lower overall homicide rates (95% confidence interval [CI], 5.2%–23.6%); violent misdemeanor laws were associated with 18.1% lower homicide rates (95% CI, 8.1–27.1%); and "shall issue" laws were associated with 9.0% higher homicide rates (95% CI, 1.1%–17.4%). None of the other seven laws were significantly associated with overall homicide rates. In a falsification test, each of these three laws was found to be significantly associated only with the firearm-related homicide rate, not the non-firearm-related homicide rate (Online Supplemental Table S3).

In the fully adjusted model, household gun ownership was not associated with overall rates of homicide (Table 3). Factors that were significant positive predictors of overall homicide rates were the percentage of males, the violent crime rate, and population density. Overall population was negatively associated with homicide rates.

When examined individually, four of the 10 firearm laws were significantly associated with overall suicide rates (Table 4). However, after simultaneously controlling for all 10 firearm laws, only two laws were significantly related to suicide rates: bans on junk guns were associated with 6.4% lower suicide rates (95% CI, 3.5–9.2%) and permitless carry laws were associated with 5.1% higher suicide rates (95% CI, 0.2–10.4%). Both laws failed the falsification test, as both were significantly related to non-firearm as well as firearm homicide rates (Online Supplemental Table S4). None of the other laws were significantly associated with overall suicide rates.

In the fully adjusted model, household gun ownership was not associated with overall rates of suicide (Table 4). Factors that were significant positive predictors of suicide rates were the violent crime rate, unemployment rate, poverty rate, and per capita alcohol consumption. Overall population was negatively related to suicide rates.

Entering year as a continuous variable instead of as a fixed effect had no appreciable impact on the results (Online Supplemental Table S5).

DISCUSSION

To the best of our knowledge, this is the first study using data from within the past decade to simultaneously model the effect of multiple state firearm laws on homicide and suicide rates at the state level using a multi-year panel design. Using a difference-in-differences analysis, we found that laws requiring universal background checks and those prohibiting firearm possession by people with a conviction for a violent

Table 2 Status of State Firearm Laws and Violent Death Rates, 2016

State	UBC	VM	21	SI	PC	TP	JG	SYG	AW	LCM	Age-adjusted overall homicide rate (per 100,000)	Age-adjusted overall suicide rate (per 100,000)
Louisiana				√				√			14.2	14.1
Mississippi					√			√			12.0	12.7
Alabama				√				√			11.8	15.6
Maryland		√	√				√		√	√	10.0	9.3
Missouri				√							9.9	18.3
New Mexico				√							9.5	22.5
Illinois	√			√		√	√				9.2	10.7
South Carolina				√				√			9.0	15.7
Tennessee				√				√			8.7	16.3
Arkansas				√							8.7	18.2
Oklahoma				√				√			8.6	20.9
Georgia				√				√			7.9	13.3
Alaska					√			√			7.5	25.4
Indiana				√				√			7.5	15.4
North Carolina				√				√			7.4	13.0
Nevada				√				√			7.4	21.4
Kentucky				√				√			7.1	16.8
Delaware	√					√					7.0	11.5
Florida				√		√		√			6.8	13.9
Michigan				√				√			6.6	13.3
Ohio				√		√					6.5	14.1
West Virginia					√			√			6.3	19.5
Arizona					√			√			6.3	17.6
Pennsylvania				√				√			6.0	14.7
Texas				√				√			6.0	12.6
Virginia				√		√					5.5	13.2
Kansas					√			√			5.3	17.9
California	√	√				√	√		√	√	5.2	10.5
Wisconsin				√							4.8	14.6
South Dakota				√				√			4.7	20.5
New Jersey	√		√						√	√	4.6	7.2
Montana				√				√			4.3	26.0
Colorado	√			√		√				√	4.2	20.5
New York	√	√	√			√			√	√	3.5	8.1
Nebraska				√							3.3	13.0
Oregon	√			√							3.2	17.8
Wyoming					√						3.0	25.2
Washington	√			√							2.9	14.8
Iowa			√	√							2.8	14.5
Hawaii	√	√	√				√				2.8	12.0
Connecticut	√	√	√			√			√	√	2.6	10.0
Utah				√		√		√			2.5	21.8
Minnesota				√		√	√				2.4	13.2
Rhode Island	√		√								2.3	11.1
North Dakota				√		√					2.2	19.0
Massachusetts	√		√			√	√		√	√	2.0	8.7
Idaho					√						2.0	21.3
Vermont					√						1.9	17.3
New Hampshire				√				√			1.3	17.3
Maine					√						1.3	15.7

Includes the following 10 laws: UBC, universal background checks; VM, violent misdemeanor prohibitor; 21, age 21 limit for handgun purchase; SI, shall issue; PC, permitless carry; TP, trafficking prohibited; JG, junk gun ban; SYG, stand your ground law; AW, assault weapons ban; LCM, large capacity magazine ban

misdemeanor were associated with significant reductions in the overall homicide rate, while "shall issue" laws were associated with a significant increase in the homicide rate. There was no significant association between homicide and the other laws studied, and we did not find consistent relationships between any of the laws and overall suicide rates.

This study has several strengths. First, it is one of the first studies to clearly define each law with attention to the detailed provisions of the law, including its scope, exceptions, and exemptions. One reason for some of the conflicting results of previous studies (Online Supplemental Tables S1, S2) may be the inconsistent definition of state statutes.

Second, using a difference-in-differences approach helps to address the major threat to validity in this type of research: states with lower homicide rates to begin with may be more likely to enact stronger gun laws. By including state and year fixed effects, we are using a "within-estimator" that assesses differences within states over time.[5, 6] Studies that do not include state fixed effects are also assessing differences across states at a given time ("between effects"), which may reflect different propensities of states with lower or higher homicide rates to enact laws, rather than law effects. Thus, the difference-in-differences approach is less subject to the possibility of "reverse causation" (i.e., it is the level of the homicide

Table 3 Linear Regression Model Results: Factors Affecting Homicide Rates, 1991–2016

	Regression coefficient for state firearm laws entered one at a time (95% CI)	Regression coefficient, fully adjusted model [all laws entered together] (95% CI)
Percent black		0.043 (−0.004, 0.089)
Percent male among population ages 15–29		*0.100* (0.021, 0.179)*
Per capita law enforcement officers		−0.023 (−0.079, 0.033)
Violent crime rate		*0.054* (0.026, 0.081)*
Divorce rate		−0.030 (−0.066, 0.005)
Unemployment rate		0.002 (−0.015, 0.019)
Poverty rate		0.002 (−0.005, 0.010)
Per capita alcohol consumption		0.138 (−0.021, 0.298)
Incarceration rate (per 1000 population)		−0.025 (−0.058, 0.008)
Population density (per 0.1 mile^2)		*0.032* (0.010, 0.054)*
Log of population		*−0.629* (−1.081, −0.177)*
Proxy for household gun ownership percentage		0.001 (−0.004, 0.007)
Firearm laws		
Universal background checks	*−0.173* (−0.299, −0.048)*	*−0.161* (−0.269, −0.053)*
Violent misdemeanor is prohibiting for handgun possession	*−0.155* (−0.276, −0.033)*	*−0.200* (−0.316, −0.084)*
Age 21 limit for handgun possession	−0.117 (−0.245, 0.010)	−0.068 (−0.200, 0.064)
Shall issue law	*0.082* (0.018, 0.146)*	*0.086* (0.011, 0.160)*
Permitless carry law	−0.063 (−0.152, 0.027)	0.015 (−0.101, 0.131)
Trafficking prohibited	−0.045 (−0.133, 0.044)	0.005 (−0.050, 0.061)
Junk gun ban	−0.028 (−0.177, 0.121)	−0.010 (−0.136, 0.116)
Stand Your Ground law	0.020 (−0.042, 0.083)	0.009 (−0.050, 0.067)
Ban on assault weapons	−0.143 (−0.300, 0.013)	−0.092 (−0.222, 0.039)
Ban on large capacity ammunition magazines	−0.089 (−0.205, 0.027)	0.038 (−0.036, 0.112)
R^2		0.94

Outcome variable is the log of the age-adjusted total homicide rate. All models include year and state fixed effects. Standard errors are robust and adjusted for state-level clustering
CI, confidence interval
**Coefficient is statistically significant from zero ($p < 0.05$). Also shown in italic*

rates that are affecting the law enactment, not the other way around). The inclusion of state fixed effects has the added advantage of controlling for any differences between states in time-invariant factors.

Third, including a large panel of time-varying state factors as independent variables helps address the problem of omitted variable bias. Nevertheless, it is still possible that states which were experiencing large declines in homicide were more likely to enact a particular law; even the within-estimator may not be sufficient to rule out the possibility of reverse causation.

Our finding of a negative association between universal background checks (including permit requirements) and homicide rates is consistent with several other studies.[3, 16–20] Our finding of a negative association between violent misdemeanor laws and homicide rates is consistent with one other recent study, which reported a 24% reduction in intimate partner homicide in states with these laws.[21] However, caution should be exercised when interpreting this finding because only two states implemented violent misdemeanor laws during the study period. While historically the literature on the impact of concealed carry–permitting laws has been inconsistent and several studies have found an association between "shall issue" laws and reduced murder rates,[7, 22–29] the three most recent studies to examine these laws found a positive association with homicide rates.[3, 30, 31]

Our finding that there was no association between stand your ground laws and homicide rates conflicts with the findings of two previous studies on these laws.[32, 33] However, both of these studies examined only the decade of 2000–2010. When we restrict our analysis to that decade, we obtain similar results.

A second important finding of this study is that changes in household gun ownership were not found to be significantly associated with homicide or suicide rates, a result that differs from several previous studies.[34, 35] The discrepancy in these results could possibly be due to our inclusion of state fixed effects. It is possible that although there is a strong cross-sectional relationship between the prevalence of firearm ownership and homicide and suicide rates, small changes in firearm ownership that are observed over time are not sufficient enough to result in measurable differences in overall population homicide or suicide rates. Even if we had survey-based measures of household gun ownership, the margin of error is probably greater than the actual change in gun ownership levels from year to year. There is too much noise in our measure of gun ownership and too little variability in true levels of household gun ownership to determine if changes in gun ownership are related to differences in homicide or suicide rates. Few of the previous studies included state fixed effects. Because of the conflict with the existing literature, further study is required before any definitive conclusion is drawn.

It is important to note that the absence of an observed association of a law and overall homicide or suicide rates does not necessarily mean that these laws are ineffective. It may also be that the laws are not broad enough to affect overall population death rates or that the laws are not being adequately enforced.

Table 4 Linear Regression Model Results: Factors Affecting Suicide Rates, 1991–2016

	Regression Coefficient for State Firearm Laws Entered One at a Time (95% CI)	Regression Coefficient, Fully Adjusted Model [All Laws Entered Together] (95% CI)
Percent black		−0.015 (−0.033, 0.003)
Percent male among population ages 15–29		0.018 (−0.014, 0.049)
Per capita law enforcement officers		0.006 (−0.015, 0.027)
Violent crime rate		*0.018* (0.007, 0.029)*
Divorce rate		−0.008 (−0.028, 0.012)
Unemployment rate		*0.008* (0.001, 0.016)*
Poverty rate		*0.004* (0.000, 0.007)*
Per capita alcohol consumption		*0.075* (0.012, 0.138)*
Incarceration rate (per 1000 population)		0.007 (−0.011, 0.025)
Population density (per 0.1 mile^2)		−0.001 (−0.010, 0.007)
Log of population		*−0.349* (−0.601, −0.097)*
Proxy for household gun ownership percentage		0.001 (−0.001, 0.003)
Firearm laws		
Universal background checks	0.008 (−0.034, 0.050)	−0.010 (−0.033, 0.053)
Violent misdemeanor is prohibiting for handgun possession	−0.024 (−0.064, 0.016)	−0.043 (−0.090, 0.004)
Age 21 limit for handgun possession	*−0.040* (−0.078, −0.001)*	−0.030 (−0.070, 0.010)
Shall issue law	0.000 (−0.025, 0.024)	0.004 (−0.022, 0.029)
Permitless carry law	*0.063* (0.006, 0.120)*	*0.050* (0.002, 0.099)*
Trafficking prohibited	−0.013 (−0.047, 0.021)	−0.002 (−0.043, 0.038)
Junk gun ban	*−0.074* (−0.101, −0.047)*	*−0.066* (−0.097, −0.036)*
Stand Your Ground law	−0.014 (−0.033, 0.006)	−0.018 (−0.037, 0.001)
Ban on assault weapons	−0.037 (−0.081, 0.006)	0.001 (−0.063, 0.066)
Ban on large-capacity ammunition magazines	*−0.052* (−0.099, −0.005)*	−0.004 (−0.053, 0.046)
R^2		0.94

Outcome variable is the log of the age-adjusted total suicide rate. All models include year and state fixed effects. Standard errors are robust and adjusted for state-level clustering
CI confidence interval
**Coefficient is statistically significant from zero ($p < 0.05$). Also shown in italic*

Several other limitations deserve mention. First, the firearm ownership proxy has been validated with cross-sectional data, but not with longitudinal data.[36] It is not clear whether this proxy is able to accurately measure changes in household gun ownership over time.

Second, while we controlled for a range of state-level factors associated with homicide death rates, there may be unidentified omitted variables. For example, in the early 1990s, firearm homicide rates were very high in many cities, seemingly related to the crack cocaine epidemic.[37, 38] Nevertheless, when we restrict the analysis to the period 2000–2016, our results remain essentially unchanged, although the precision of the estimates decreases.

Third, we accounted only for the presence or absence of firearm law provisions, not for the implementation and enforcement of these laws. Fourth, trying to incorporate the most important explanatory variables in a large regression almost invariably leads to some multicollinearity. For example, when we use all the other independent variables to explain variations in the gun ownership proxy, the adjusted R^2 is 0.69.

Finally, we do not disaggregate homicide rates by the age or other characteristics of either the offender or victim, which could mask the effect of laws intended to affect a particular subpopulation. For example, age restrictions on gun possession would only be expected to affect youth suicide rates, not adult rates.

In conclusion, this study provides evidence that universal background checks and laws prohibiting gun ownership by people with a history of a violent misdemeanor are associated with lower overall homicide rates, while laws that provide no discretion to law enforcement officials in approving concealed carry permits are associated with higher homicide rates. Further research on the impact of state firearm laws is necessary to assess causality and should rely upon detailed definitions of each law.

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EXHIBIT "17"

Firearms laws and the reduction of violence: A systematic review

Article *in* American Journal of Preventive Medicine · March 2005

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Firearms Laws and the Reduction of Violence

A Systematic Review

Robert A. Hahn, PhD, MPH, Oleg Bilukha, MD, PhD, Alex Crosby, MD, MPH, Mindy T. Fullilove, MD, Akiva Liberman, PhD, Eve Moscicki, ScD, MPH, Susan Snyder, PhD, Farris Tuma, ScD, Peter A. Briss, MD, MPH, Task Force on Community Preventive Services

Overview

The Task Force on Community Preventive Services (the Task Force) is conducting systematic reviews of scientific evidence about diverse interventions for the prevention of violence, and resulting injury and death, including, among others, early childhood home visitation,[1,2] therapeutic foster care,[3] the transfer of juveniles to the adult justice system, school programs for the teaching of prosocial behavior, and community policing. This report presents findings about the effectiveness of firearms laws in preventing violence. Studies of the following firearms laws were included in the review: bans on specified firearms or ammunition; restrictions on firearms acquisition; waiting periods for firearms acquisition; firearms registration; licensing of firearms owners; "shall issue" carry laws that allow people who pass background checks to carry concealed weapons; child access prevention laws; zero tolerance laws for firearms in schools; and combinations of firearms laws.

The Task Force found the evidence available from identified studies was insufficient to determine the effectiveness of any of the firearms laws reviewed singly or in combination. A finding that evidence is insufficient to determine effectiveness means that we do not yet know what effect, if any, the law has on an outcome—not that the law has no effect on the outcome. This report describes how the reviews were conducted, gives detailed information about the Task Force's findings, and provides information about research gaps and priority areas for future research.

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Introduction

Although rates of firearms-related[a] injuries in the United States have declined since 1993, they remained the second leading cause of injury mortality in 2001, the most recent year for which complete data are available.[4] Of 29,573 firearms-related deaths in 2001—an average of 81 per day—16,869 (57.0%) were suicide; 11,671 (39.5%) were homicide or legal intervention (e.g., homicide by police); 802 (2.7%) were unintentional; and 231 (0.8%) were of undetermined circumstances. In 1998, for each firearm-related death, 2.1 nonfatal firearm-related injuries were treated in emergency departments.[5] It is estimated that 24.3% of all violent crimes—murder, aggravated assault, rape, and robbery—committed in 1999 (a total of 1,430,693) were committed with a firearm.[6] Rates of firearm-related homicide, suicide, and unintentional death in the United States exceed those of 25 other high-income nations (i.e., 1996 GNP ≥US$9636 per capita) for which data are available (Figure 1).[7] The cost of firearm-related violence in the United States is estimated to be approximately $100 billion per year.[8]

Approximately 4.5 million new (i.e., not previously owned) firearms are sold each year in the United States, including 2 million handguns. In addition, estimates of annual secondhand firearms transactions range from 2 to 4.5 million.[9,10] Further, it is estimated that approximately 0.5 million firearms are stolen annually.[10] Thus, the estimated total number of firearms transactions ranges from 7 to 9.5 million per year, of which between 47% and 64% are new firearms.

New firearms can be sold legally only by federal firearms licensees (FFLs); FFL transactions comprise the primary market.[10] FFLs are required to comply with the Permanent "Brady Law" (P.L. 103-159, Title XVIII, Section 922(t)) and initiate background checks to investigate whether would-be purchasers violate federal or state purchasing requirements (e.g., people convicted of a felony must be excluded). In the "secondary market" of firearms not sold by FFLs, private citizens

[a]A firearm is a weapon (e.g., handgun, rifle, or shotgun) in which a shot is propelled by gunpowder.

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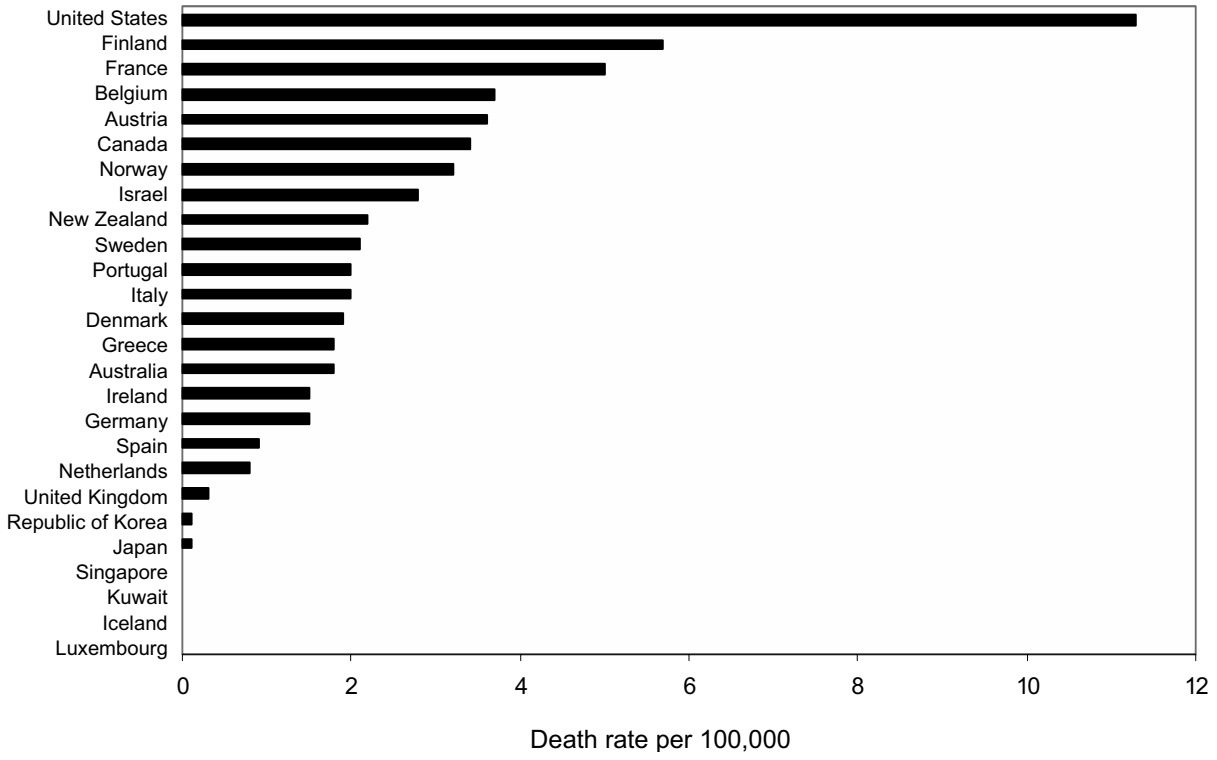

Figure 1. Firearm-related mortality for high-income World Health Organization Member States (most recent year available between 1990 and 2000). (Note: A firearm is defined as a weapon [e.g., handgun, rifle, or shotgun] in which a shot is propelled by gunpowder.)

may sell their firearms without a license; firearms shows constitute an important segment of the secondary market.[10] Private citizens are not supposed to knowingly sell firearms to people in excluded categories, but, although several states require background checks for private sales,[9] private sales are not federally regulated.[11]

The 1994 National Survey of the Private Ownership of Firearms (NSPOF) indicated that adults in the United States owned approximately 192 million working firearms—an average of one per adult.[12] NSPOF also indicated that firearms ownership was unevenly distributed in the population: 24.6% of U.S. adults owned a firearm—41.8% of men and 9.0% of women. Another survey[6] found that 41% of (adult) respondents reported having a firearm in their home in 1994, as did 32% in 2000. A third survey[13] reported that 35% of homes with children aged <18 years had at least one firearm. Of the 192 million firearms owned in the United States in 1994, 65 million were handguns, 70 million rifles, 49 million shotguns, and the remainder, other firearms.[12] Approximately 40% of handguns and long firearms were semiautomatic. Among handgun owners, 34.0% kept their firearms loaded and unlocked. An estimated 10 million handguns—one sixth of the handguns owned—are regularly carried by their owners, about half in the owners' cars and the other half on the owners' persons.[10]

The NSPOF also found that, among adult firearm owners, 9.7 million owned more than an average of ten firearms each, whereas 34.4 million owned a mean of approximately 2.5 firearms each. Among owners who only owned handguns, 74.4% reported owning for self-defense, 0.5% for hunting, 10.8% for target or sport shooting, and the remaining 13.5% for other purposes. Among owners of long firearms only (i.e., rifles and shotguns), 14.9% reported owning for self-defense, 69.9% for hunting, 6.1% for target or sport shooting, and the remaining 9.1% for other purposes.

This review examines firearms laws as one of many potential approaches to the reduction of firearm-related violence.[14,15] The manufacture, distribution, sale, acquisition, storage, transportation, carrying, and use of firearms in the United States are regulated by a complex array of federal, state, and local laws and regulations. The focus of this review is on assessing the effects of selected federal and state laws on violence-related public health outcomes, including death and injury resulting from violent crimes, suicide, and unintentional incidents; we also note effects on other outcomes, such as property crime, the apprehension of criminals, and school expulsion.

Reviews of firearms laws and studies of their effects have been conducted by many others.[16–20] The present review of selected laws differs from those reviews in that

Table 1. Selected *Healthy People 2010*[21] objectives related to firearms legislation, and proposed health-related outcomes

Injury prevention
Reduce firearm-related deaths from 11:3 to 4.1 per 100,000 population[a] (Objective 15-3).
Reduce the proportion of persons living in homes with firearms that are loaded and unlocked from 19% to 16%[a] (Objective 15-4).
Reduce nonfatal firearm-related injuries from 24.0 (in 1997) to 8.6 per 100,000 population (Objective 15-5).
Unintentional injury prevention
Reduce deaths caused by unintentional injuries from 35.0 to 17.5 per 100,000 population[a] (Objective 15-13).
(Developmental) Reduce nonfatal unintentional injuries (Objective 15-14).
Violence and abuse prevention
Reduce homicides from 6.5 to 3.0 per 100,000 population[a] (Objective 15-32).
Reduce the rate of physical assault by current or former intimate partners from 4.4 (in 1998) to 3.3 per 1000 persons aged ≥12 years (Objective 15-34).
Reduce the annual rate of rape or attempted rape from 0.8 (in 1998) to 0.7 per 1000 persons aged ≥12 years (Objective 15-35).
Reduce sexual assault other than rape from 0.6 (in 1998) to 0.4 per 1000 persons aged ≥12 years (Objective 15-36).
Reduce physical assaults from 31.1 (in 1998) to 13.6 per 1000 persons aged ≥12 years (Objective 15-37).
Reduce weapon carrying by adolescents on school property from 6.9% (in 1999) to 4.9% (students in grades 9 through 12, carrying during the past 30 days) (Objective 15-39).
Mental health and mental disorders
Reduce the suicide rate from 11.3 to 5.0 per 100,000 population[a] (Objective 18-1).
Reduce the 12-month average rate of suicide attempts from 2.6% to 1% among adolescents in grades 9 though 12 (Objective 18-2).

[a]Baseline: 1998 data, age adjusted to the year 2000 standard population.

it is based on systematic epidemiologic evaluations and syntheses of all available literature meeting specified criteria.

The *Guide to Community Preventive Services*

The systematic reviews in this report represent the work of the independent, nonfederal Task Force on Community Preventive Services (the Task Force). The Task Force is developing the *Guide to Community Preventive Services* (the *Community Guide*) with the support of the U.S. Department of Health and Human Services (DHHS) in collaboration with public and private partners. The Centers for Disease Control and Prevention (CDC) provides staff support to the Task Force for development of the *Community Guide*. A special supplement to the *American Journal of Preventive Medicine*, "Introducing the Guide to Community Preventive Services: Methods, First Recommendations and Expert Commentary," published in January 2000 (volume 18, supplement 1), presents the background and the methods used in developing the *Community Guide*. The *Community Guide* conducts reviews on a wide array of public health topics. The present review is part of a broader *Community Guide* review of violence prevention. The broader review focuses on youth as victims and perpetrators of violence, but this review addresses firearms laws affecting both adults and youth, since there are few laws directed specifically toward youth.

Healthy People 2010 Goals and Objectives

This review provides information on the state of knowledge about firearms laws interventions related to the violence prevention objectives in *Healthy People 2010*,[21] the disease prevention and health promotion agenda for the United States. These objectives identify some of the significant preventable threats to health and help focus the efforts of public health systems, policymakers, and law enforcement officials in their efforts to address those threats. Many of the proposed *Healthy People* objectives in Chapter 15, "Injury and Violence Prevention," include outcomes that might be affected by firearms laws (Table 1).

Conceptual Approach and Analytic Framework

The general methods for conducting systematic reviews for the *Community Guide* have been described in detail elsewhere.[22–25] This section describes the conceptual approach, the selection of laws for review, review methods, and the determination of which outcomes to consider in assessing the effects of firearms laws on violence.

The logic model used by the review team to evaluate the effectiveness of firearms laws in reducing violence (Figure 2) depicts the flow of influences of firearms laws on firearms from their manufacture, through their distribution, acquisition, storage, carrying, and use, to violent acts (including self-defense) and physical or psychosocial injury to direct and indirect victims. Enforcement plays a role at several stages in this process. The enforcement of firearms laws may prevent violence by averting illegal firearms use and may also deter potential violence. Inadequate enforcement may diminish the effect of a law and make it difficult to assess the potential effect of a law.

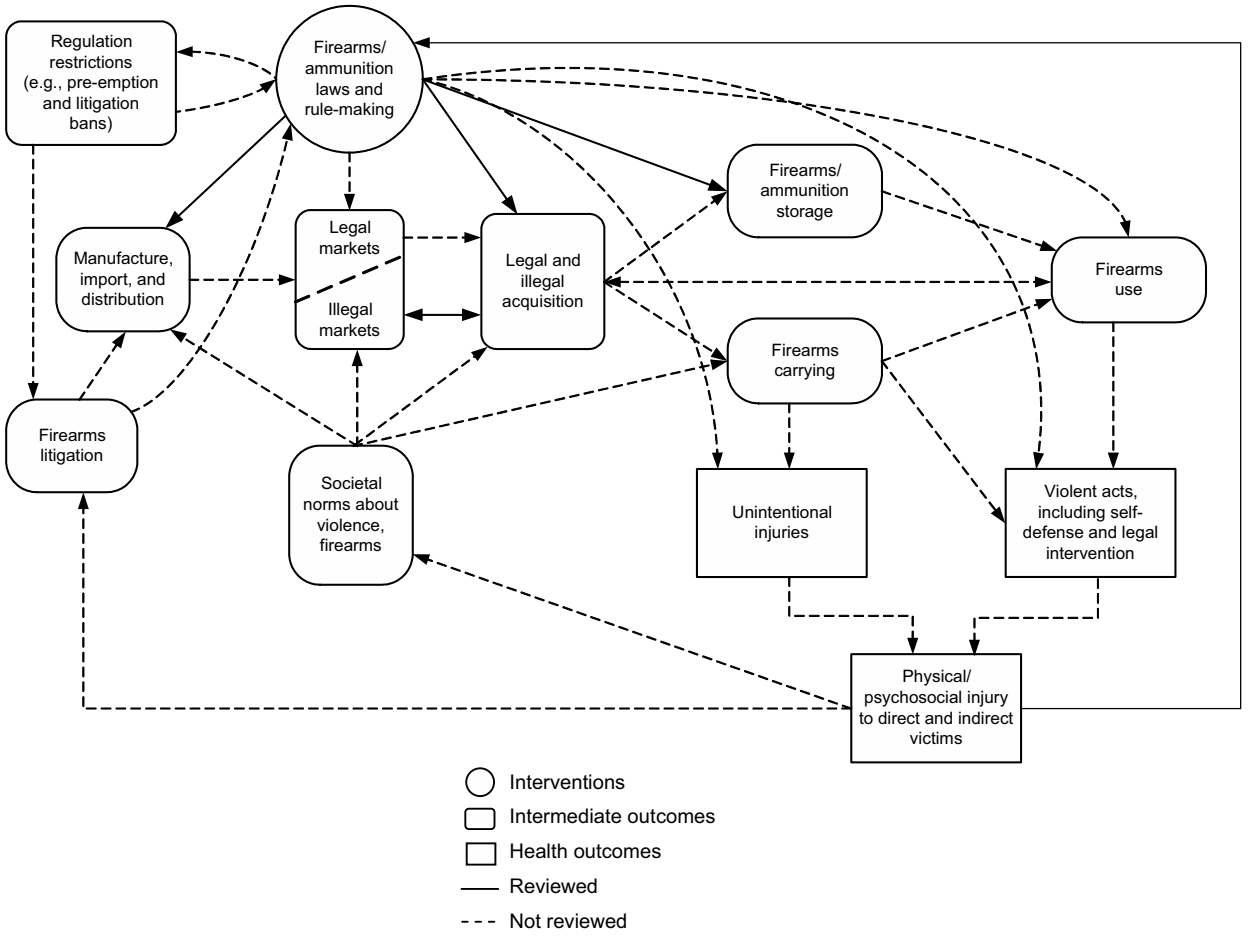

Figure 2. Effects of firearms laws on violence.

We note (Figure 2) two ways in which the legal process may itself be limited by regulation restrictions: (1) through bans on firearms litigation, and (2) through preemption laws that prohibit lower-level legislative bodies (e.g., counties) from enacting stronger firearms laws than those enacted at a higher level (e.g., states). The model also indicates how violent outcomes may, in turn, affect the legislative process by means of several feedback loops (e.g., the effects of mass shootings on efforts to pass laws). The laws reviewed here were chosen to cover different facets of this model. Many other legal measures also merit study (e.g., laws requiring firearm safety training, allowing purchase of only one firearm per month, increasing taxes, and requiring background checks in private sales).[26]

The present review focuses on firearms laws as one means of preventing violence. Our approach is consistent with the preventive orientation of public health and with the general approach of the *Community Guide*. Prevention is regarded as a complement to, not a replacement for, law enforcement. Subsequent reviews will examine several aspects of the justice system in reducing violence.

The scientific evidence of effectiveness was reviewed for seven firearms laws and for combinations of firearms laws (including combinations of the other laws reviewed):

Bans on specified firearms or ammunition
Restrictions on firearms acquisition
Waiting periods between application to purchase and acquisition of firearm
Licensing of firearms users and registration of firearms
Shall issue concealed-weapons carry laws (which obligate issuing agencies to grant permits for carrying concealed weapons to applicants unless excluded by specific criteria)
Child access prevention laws requiring safe storage of firearms by owners
Zero tolerance of firearms in school
Combinations or systems of firearms laws

Methods

In the *Community Guide*, evidence is summarized about (1) the effectiveness of interventions; (2) the applicability of findings (i.e., the extent to which available effectiveness data might

apply to diverse populations and settings); (3) other positive or negative effects of the intervention, including positive or negative health and nonhealth outcomes; (4) economic impact; and (5) barriers to implementation of interventions. In the present review, in which sufficient evidence to determine the effects of firearms laws on violence was not found, we rarely included comments on applicability or barriers to implementation, and no economic evaluations were conducted.

As with other *Community Guide* reviews, the process that was used to review evidence systematically and then translate that evidence into the conclusions presented in this article involved:

Forming a systematic review development team
Developing a conceptual approach to organizing, grouping, and selecting interventions
Selecting interventions to evaluate
Searching for and retrieving evidence
Assessing the quality of and abstracting information from each study
Assessing the quality of and summarizing/synthesizing the body of evidence of effectiveness
Translating the evidence about effectiveness into conclusions

Systematic Review Development Team

Three groups of individuals served on the systematic review development team:

A coordination team drafted the conceptual framework for the reviews, coordinated the data collection and review process, and drafted evidence tables, summaries of the evidence, and the reports. This team consisted of a Task Force member, experts in the methods of systematic reviews and economics from the *Community Guide* and Prevention Effectiveness Branches, Division of Prevention Research and Analytic Methods, Epidemiology Program Office, CDC; and experts on violence prevention from the National Center for Injury Prevention and Control, CDC, the National Institutes of Health, and the National Institute of Justice, U.S. Department of Justice.

A consultation team set initial priorities for the reviews and reviewed and commented on materials developed by the coordination team. The consultants are experts on violence-related topics in state and local public health settings, academic organizations, federal agencies, and voluntary organizations. These experts have backgrounds in sociology, medicine, public health, economics, health promotion, intervention design and implementation, health education, health policy, and epidemiology.

An abstraction team collected and recorded data from studies for possible inclusion in the systematic reviews. (See Evaluating and Summarizing the Studies section.)

Search for Evidence

Electronic searches for literature were conducted in MEDLINE, EMBASE, ERIC, NTIS (National Technical Information Service), PSYCHLIT, PAIS (Public Affairs Information Service), Sociological Abstracts, NCJRS (National Criminal Justice Reference Service), CJPI (Criminal Justice Periodicals Index), Gale Group Legal Research Index, and ECONLIT. We also reviewed the references listed in all retrieved articles, and consulted with experts on the systematic review development team and elsewhere to find additional published reports of studies. We included journal articles, governmental reports, books, and book chapters. We also reviewed several papers that were in press at the time, identified in web searches and by consultants.

Articles were considered for inclusion in the systematic review if they had the following characteristics:

Evaluated the specified law
Assessed at least one of the violent outcomes specified
Were conducted in an established market economy[b]
Reported on a primary study rather than, for example, a guideline or review
Compared a group of people who had been exposed to the intervention with a group of people who had not been exposed or who had been less exposed (the comparisons could be concurrent or in the same group over a period of time)
Published between 1979 and March 2001.

We define a "study" as a research project conducted by a researcher or research group on a particular (study) population during a given time period, assessing specified research questions using specified methods. Some studies report analyses of a population at more than one time; multiple findings may thus be included within the study. A study may result in several "reports" on different aspects of the study (e.g., study theory or methods, study population, specific findings). We consider all reports together to constitute the study and use aspects of the reports that correspond to the topics of our review and our review criteria. In some cases, the distinction between studies and reports may be arguable—there is not always a clear line. When a research team completes a study, another team responds to it with a different analysis of the original population (a second study) and the original team then conducts yet a different version of their original study (e.g., using a new control population); we count the original team's new study as a different, third study, and note the connection to the original study and study team.

Outcomes Reviewed

The outcome measures evaluated to determine the effect of the laws reviewed were specific violent crimes (i.e., murder, aggravated assault, robbery, and rape), suicide, and unintentional firearm injury. Aggravated assault is considered a health-related outcome insofar as it is "an unlawful attack by one person upon another for the purpose of inflicting severe or aggravated bodily injury."[6] Similarly, robbery is considered a health-related outcome insofar as it is "the taking or attempting to take anything of value from the care, custody, or control of a person or persons by force or threat of force or violence and/or by putting the victim in fear."[6]

[b]Established market economies as defined by the World Bank are Andorra, Australia, Austria, Belgium, Bermuda, Canada, Channel Islands, Denmark, Faeroe Islands, Finland, France, Germany, Gibraltar, Greece, Greenland, Holy See, Iceland, Ireland, Isle of Man, Italy, Japan, Liechtenstein, Luxembourg, Monaco, the Netherlands, New Zealand, Norway, Portugal, San Marino, Spain, St. Pierre and Miquelon, Sweden, Switzerland, the United Kingdom, and the United States.

Although some studies reported firearm-specific outcomes (e.g., firearm-related suicide), we preferred to use outcomes that did not specify a relationship to firearms, because of a concern that the reduction in the firearm-specific outcome might be accompanied by an increase in non–firearm-specific outcomes (e.g., suicide by hanging), thus possibly reducing or even outweighing the firearm-related benefit. Because violence is not reduced if those who intend to commit suicide with firearms find other means when firearms are no longer available, we measure the overall change in outcomes (e.g., the rate of suicide).

Some studies[27,28] assessed the numbers of firearms retrieved in the course of investigating crimes, including violent crimes, as outcome measures. For example, studies of firearms bans may have considered counts of firearms found at crime scenes before and after bans as an indication of the effects of the bans. The use of such evidence to assess the effects of interventions on violent outcomes rests on many assumptions, for example, that rates of firearms retrieval are similar over time for different kinds of firearms, in different settings. We use such studies only as secondary evidence unless the researchers provide evidence that a high proportion of crime firearms are recovered or other evidence that the recovery process does not bias the assessment of the violent outcomes of interest.

Abstraction and Evaluation of Individual Studies

Two reviewers read each study that met the inclusion criteria, using standardized *Community Guide* criteria to assess the study evidence.[25] Disagreements between the reviewers were reconciled by consensus of the coordination team members. In addition, to ensure consistent assessment of study design suitability and limitations in execution quality within the body of evidence for each intervention, evaluated studies were discussed by the coordination team.

Assessing Suitability of Study Design

Design suitability was assessed for every included study.[22] Our study design classifications, chosen to ensure consistency in the review process, sometimes differ from the classification or nomenclature used by study authors. Studies of "greatest design suitability" were those with a concurrent comparison group, in which data were collected prospectively; studies of "moderate design suitability" were retrospective studies or those with multiple pre- or post-intervention measurements, but no concurrent comparison group; studies of "least suitable design" were cross-sectional studies or those with no concurrent comparison group and only single pre- and post-intervention measurements. Noncomparative studies (i.e., those without before-and-after intervention comparison or distinct concurrent comparison populations) were not considered in our reviews.

Assessing Study Quality and Summarizing the Body of Evidence of Effectiveness

Quality of study execution was systematically assessed using the published *Community Guide* methods.[22,25] Studies can have as many as nine limitations, including failure to describe the study population and intervention, measure exposures or outcomes effectively, demonstrate effective follow-up, use appropriate analytic methods, and control for confounding or other bias. Studies with zero or one limitation are reported to have "good execution"; studies with two to four limitations are reported to have "fair execution"; and studies with five or more limitations are reported to have "limited execution" and are not included in the body of evidence.

Unless otherwise noted, we represented results of each study as point estimates for the relative change in the rate of violent crime, suicide, or unintentional injury or death attributable to the interventions. We calculated percent changes and baselines using the following formulas for relative change:

For studies with before-and-after measurements and concurrent comparison groups:

$$(\text{Ipost}/\text{Ipre})/(\text{Cpost}/\text{Cpre}) - 1$$

where

Ipost = last reported outcome rate in the intervention group after the intervention
Ipre = reported outcome rate in the intervention group immediately before the intervention
Cpost = last reported outcome rate in the comparison group after the intervention
Cpre = reported outcome rate in the comparison group immediately before the intervention

For studies with post-intervention measurements only and concurrent comparison groups:

$$(\text{Ipost} - \text{Cpost})/\text{Cpost}$$

For studies with before-and-after measurements but no concurrent comparison:

$$(\text{Ipost} - \text{Ipre})/\text{Ipre}$$

We report the effect as "desired" when, compared with the absence of such a law, the law is associated with a decrease in a violent outcome examined, and as "undesired" when the law is associated with an increase in the violent outcome. When effect measures reported by the authors could not be converted into percentage changes (e.g., when results were presented as absolute change in rates, without information on baseline rates), the reported findings are described in the text. In the reporting of study findings, we used the standard two-tailed p-value cut-off at the 0.05 level as a measure of statistical significance.

We often had to select among several possible effect measures for inclusion in our summary measures of effectiveness. When available, we used measures adjusted for potential confounders in multivariate analysis in preference to crude effect measures. Although no studies were excluded from evaluation strictly on the basis of an insufficient follow-up period, follow-up periods of <1 year were considered an execution flaw, and studies with longer follow-up were preferred.

The studies we examined did not always share our research goals; they examined or provided data to assess outcomes of interest to us, but may have focused on outcomes that differed from those we sought to examine. For example, one study[29] examined the effect of misdemeanor restrictions on firearms purchase on subsequent first arrests for firearms or violent crime. Because we were specifically interested in

violent, but not nonviolent, firearm-related crime, and in all subsequent arrests rather than only the first, we used only study findings on the outcomes of interest to our review, rather than those focused on by the authors.

As noted above, we often transformed the researcher's findings mathematically to make measures comparable across studies. For example, in one study[30] of the effects of shall issue concealed-weapons carry laws, the author focused on the difference in changes of rates for juveniles and adults, on the premise that the law should reduce homicide among adults at a greater rate than among juveniles (because the law does not directly apply to juveniles), and assuming that this comparison was an effective way to control confounding. We could not use the results of this analysis to compare with other studies that assessed changes in rates, so we used baseline information provided in this study, and calculated changes in adult and juvenile homicide rates associated with implementation of the law. Our modifications of study approaches are noted in the summary evidence tables available at the *Community Guide* website (*www.thecommunityguide.org/violence*).

In several firearms law reviews, two or more studies—most often conducted by different research teams—examined the same intervention (e.g., a specific law, in the same population, over the same time period) and reported on the same outcome(s), but differed in study design and execution quality. We characterize such studies as nonindependent because they represent a single experience of the assessed intervention. To avoid double counting of a single experience, we chose the study with the best combination of design suitability and quality of execution to represent the overlapping group of studies. We refer to separate analyses from one study, including distinct publications, as "reports." Some studies were only partially overlapping (e.g., providing overlapping national estimates but one or more unique state estimates). In those cases, we excluded the overlapping estimates but used the nonoverlapping ones. Some studies provided findings on several firearms laws and may thus be analyzed in two or more of our reviews.

We summarized the strength of the body of evidence based on numbers of available studies, strength of their design and execution, and size and consistency of reported effects using the *Community Guide* approach described in detail elsewhere.[22] When the number of studies and their design and execution quality were sufficient by *Community Guide* standards to draw a conclusion on effectiveness, results are summarized graphically and statistically. To summarize the findings about the effectiveness of an intervention across the studies in a body of evidence, we display results of individual studies in tables and figures and report median and interquartile range of effect measures. We note whether or not zero is included within the upper and the lower interquartile ranges. When the range includes zero, we infer that the results are inconsistent in direction; when the interquartile range does not include zero, we infer that the results are consistent in direction.

It is critical to note that when we conclude that evidence for the effectiveness of a given firearms law on an outcome is insufficient, we mean simply that we do not yet know what effect, if any, the law has on that outcome. We do not mean that the law has no effect on the outcome.

Other Effects

We routinely sought information on other (i.e., not violence-related) effects of these population-based interventions, such as property crime and school expulsions. We sought evidence of potential harms or benefits if they were mentioned in the effectiveness literature or considered important by the coordination team. With the exception of property crime, additional outcomes were not specifically assessed in the papers that we reviewed.

Economic Evaluations, Applicability of Interventions, and Barriers to Implementation

In *Community Guide* reviews, economic evaluations are summarized for each intervention found to have at least sufficient evidence of effectiveness.[22] Because we did not find sufficient evidence of effectiveness of any of the laws reviewed, no economic evaluations were performed.[24] The applicability of the intervention to populations and settings not specifically studied and the barriers to implementation of the intervention may be assessed whether or not the intervention is found to be effective.

Summarizing Research Gaps

Many systematic reviews in the *Community Guide* identify existing information on which to base public health practice. Whether or not a sufficient evidence base supports practice recommendations, an important benefit of these reviews is identification of areas where information is lacking or of poor quality. For the topics reviewed here, evidence was insufficient to develop recommendations. We summarized remaining questions about effectiveness, and identified key issues that had emerged from the review, based on the informed judgment of the systematic review development team.

Sources of Information for Firearms Law Effectiveness Studies

Studies of firearms law effectiveness have employed several sources of information, and the limitations of these sources should be understood. Information on laws—the "exposures" in these studies—are derived from federal government reports (e.g., Bureau of Alcohol, Tobacco and Firearms, 2000[11]) and published analyses (e.g., Cramer and Kopel, 1995[31]). There have been substantial discrepancies among sources in the specification of which jurisdictions have enacted which laws; this has led to differences in the classification of "exposure" to laws in evaluation studies and systematic reviews.

Evaluations of the effectiveness of firearms laws most often rely on two sources of information on violent outcomes: the Uniform Crime Report (UCR) from the Federal Bureau of Investigation (FBI), and Vital Statistics of the United States from the National Center for Health Statistics of the CDC. These record systems were initially developed for administrative uses and simple statistical monitoring, but have been widely used for research.

Most studies of the effects of firearms laws use the UCR to assess outcomes; the UCR documents reports of and arrests for violent crimes (i.e., murder, robbery, aggravated assault,

and rape) and property crimes (i.e., burglary, larceny, automobile theft, and arson) sent to the FBI by the 18,413 law enforcement reporting agencies in the United States.[32] There are several limitations of UCR data. First, crime reporting to the police is not complete.[6] A population-based survey, the National Crime Victimization Survey, indicates that, in 2001, U.S. adults reported to law enforcement agencies only 61.4% of their 1.8 million experiences of violent crime victimization (excluding murder).

In addition to incomplete reporting to police by victims, law enforcement agencies substantially under-report crime to the FBI. For example, during the 36-month period from 1992 to 1994, only 64% of these agencies reported crimes for each month, and 5% provided no data at all.[32] Moreover, quality of reporting varies substantially by time and by state: from the mid-1980s to the late 1990s, 12 states reported problems with their data (e.g., using definitions for specific crimes that differed from UCR definitions) for >1 year, and these data could not be used in the UCR.

When data are missing in the UCR, they are imputed, generally on the assumption that information not reported for given reporting areas at given times is similar to that reported in other places or time periods. Maltz and Targonski[33] recently argued that UCR crime data at the county level are currently too unreliable for use in research; however, because crime generally occurs at higher rates in cities, city-level crime data are regarded as sufficiently reliable for research use. The problems of police reporting described by Maltz[32] compound the under-reporting of crime by victims. Since nationally representative surveys of victims indicate that victims report only 43.9% of violent victimizations and since the UCR represents 87% of the U.S. population, UCR crime data are likely to represent approximately 38.2% (i.e., 0.87×0.439) of violent victimizations in the United States.[34] Under-reporting by itself might not result in bias, but if under-reporting differs systematically across times or places—a plausible scenario—it could result in biases in either direction. The UCR data source supplies a special population data set that is reduced in numbers in proportion to the under-reporting in each reporting area: use of standard, unreduced population estimates from the Bureau of the Census will underestimate rates in these circumstances.

In addition to under-reporting, UCR data present another challenge for research: They are aggregated, so that numbers of events are reported, but not information on the circumstances of each event. Aggregate reporting limits the analysis of social "mechanisms" by which firearms laws might work. Several studies of the effects of firearms laws on homicide have used the FBI's *Supplemental Homicide Reports*[35] in which individual record information is available, allowing fuller analysis of the circumstances of homicides. The implementation of the FBI's National Incident-Based Reporting System[36] and the development of the National Violent Death Reporting System[37] may substantially address this limitation of the UCR data system.

The other principal source of data for firearms law evaluation outcomes, Vital Statistics of the United States—a report of U.S. deaths prepared by CDC's National Center for Health Statistics—includes information on homicides, suicides, and unintentional deaths, including firearm-related deaths. Although virtually all U.S. deaths, including deaths in all counties, are counted in this system, some misclassification occurs by cause of death (particularly for causes such as suicide and unintentional injury)[38] as well as by demographic characteristics.[39,40] Unintentional firearm-related deaths appear to be substantially undercounted (i.e., misclassified as due to another cause).[41] Furthermore, there is a lack of circumstantial detail in vital statistics data, particularly about the perpetrators of homicide and the agents of unintentional injuries.

Finally, sources of information on potential confounders in firearms law effectiveness studies have presented a challenge. Major confounders include phenomena such as poverty, unemployment, gangs, drug cycles, intensity of law enforcement, and other existing laws. There have been disagreements about how best to conceptualize and measure these, and data for some have been difficult, if not impossible, to find. Information on arrests for crime has been used as an independent variable in firearms law studies to control for degree of enforcement activity. Yet FBI arrest data may be even more problematic than UCR crime data in terms of under-reporting and differential reporting by crime and other characteristics.[32] Arrest rates (i.e., number of arrests per number of crimes) have been used to control for potential confounding by degree of law enforcement; however, the use of arrest rates creates statistical problems, because crime is then both the dependent and an independent variable in these analyses. Taken together, all of these features of available data sources severely limit the ability to understand the effectiveness of firearms law in preventing violence.

Results: Part I—Intervention Effectiveness

Bans on Specified Firearms or Ammunition

Bans on specified firearms and ammunition prohibit the acquisition and possession of certain categories of firearms (e.g., machine guns or assault weapons) or ammunition (e.g., large-capacity magazines or hollow-point bullets). They can also include prohibitions on the importation or manufacture of the specified firearms. Bans may be adopted at the federal, state, or local level, and may be combined with additional firearms regulations, such as requirements for safe storage, age restrictions on acquisition, or restrictive licensing requirements for firearms dealers. Bans are intended to decrease the availability of certain types of firearms to potential offenders, and thus reduce the capacity of such offenders to perpetrate crime.[27]

Bans are usually imposed on the types of firearms or ammunition that are either thought to be particularly dangerous and not well suited for hunting or self-defense (e.g., semiautomatic and fully automatic assault weapons) or disproportionately involved in crime (such as cheap, low-quality, small-caliber handguns usually referred to as "Saturday night specials"). Sometimes, especially in high-crime urban settings, bans may include a broad spectrum of firearms (e.g., the ban enacted in Washington DC in 1976,[42] on purchase, sale, transfer, and possession of all handguns by civilians unless the handguns were previously owned and registered).

Table 2. Bans of gun acquisition or possession: descriptive information about included studies

	Studies (*n*)
Studies meeting inclusion criteria	9[17,27,28,42–47]
Studies excluded, limited design or execution quality	0
Qualifying studies	9[17,27,28,42–47]
Independent studies included in body of evidence[a]	3[17,42,45–47b]
Studies assessing nonrecommendation outcomes	2[27,28]
Nonindependent studies, not in body of evidence[a]	2[43,44]
Designs of included studies	
Time series with concurrent comparison group	4[42,45–47]
Time series, no concurrent comparison group	1[28]
Retrospective with concurrent comparison group	1[27]
Cross-sectional	1[17]
Outcomes reported in included studies	
Homicide	3[17,42,45–47b]
Aggravated assault	1[17]
Robbery	1[17]
Rape	1[17]
Suicide	3[17,42,47]
Unintentional firearm-related injury death	1[17]
Gun counts or proportions	2[27,28]

[a]Studies are described as "independent" if they do not assess the same intervention in the same population for a similar follow-up period. Among nonindependent studies, the one with the longest follow-up or the best design or execution is chosen to represent this intervention experience.
[b]Three studies[42,46,47] are nonindependent, with no clear superiority of one study over the others in design or execution. All assessed the Washington DC handgun ban; each used a different control population.

Bans commonly exempt firearms in the banned category owned prior to implementation of the ban (i.e., they "grandfather" those weapons), although such bans may require the registration of grandfathered firearms. Grandfathering is a critical element in bans insofar as it could allow large stocks of the banned items to remain available after the ban goes into effect.

Review of evidence: effectiveness. Our search identified nine studies on the effects of bans on violent outcomes or on the use of the banned firearms.[17,27,28,42–47] Descriptive information about execution quality, design suitability, and outcomes evaluated in these studies is provided in Table 2. More detailed information on the studies used in this review are provided at the website (*www.thecommunityguide.org/violence*); Appendix A, which shows evidence used in the review of the effects of bans, is an example of the detailed tables for all firearms law evidence reviews available on the website.

Among the seven studies that evaluated violent outcomes, one[17] was of least suitable design; all seven studies had fair execution quality. Five studies[42–44,46,47] evaluated the 1976 Washington DC handgun ban. Two of these were not considered because they assessed follow-up periods that were relatively short (2 years) compared with the remaining studies of the DC ban.

Because the three remaining studies[42,46,47] (two[42,47] conducted by the same team of researchers) assessed the effects of the DC handgun ban on homicide during a similar time period, they were counted as nonindependent and as one study experience. They reached inconsistent conclusions about the effects of the law on homicide, principally because of methodologic differences and differences in comparison populations. Two found a decrease in homicide in Washington DC compared with surrounding regions,[42] and with Memphis and Philadelphia,[47] cities of comparable size. The third[46] found increases in homicide rates in Washington DC compared with Baltimore, a city with comparable crime rates. Because of the limitations of all the studies and inconsistent results and conclusions, and because there was no best study, we concluded that the evidence was insufficient to determine the effectiveness of the Washington DC handgun ban on reducing homicide.

Two studies of the Washington DC handgun ban[42,47] found a decrease in suicide, compared with control regions without a similar ban. These results, however, were inconsistent with the other study of the effect of bans on suicide,[17] which found increases as well as decreases in suicides associated with several types of bans.

One study examined the effects on homicide rates of the 1994 Federal Violent Crime Control Act that banned assault weapons and large-capacity ammunition magazines. Comparing states with bans similar to but enacted before the federal ban with states with no such ban, the study found a relative decline in homicide rates in states without a prior ban, suggesting a benefit associated with the new ban.[45] A study of least suitable (i.e., cross-sectional) design[17] assessed the effects of handgun possession, handgun sales, and bans of sales of Saturday night specials on homicide, aggravated assault, robbery, rape, fatal unintentional firearm-related injury, and suicide in the 170 U.S. cities with populations >100,000 in 1980, and found no consistent results.

Two studies evaluated Maryland laws—a 1988 law banning manufacture and sale of Saturday night specials,[27] and a 1994 law banning sales of assault pistols.[28] These studies evaluated outcomes not directly related to health, such as proportions of banned firearms among all recovered crime firearms, or counts of recovered banned firearms used in crime. They indicated reductions in banned firearms, either in comparison with firearms used prior to the ban[28] or with other cities without such a ban.[27] Because the decrease in the number of banned firearms exceeded the increase in

the number of additional nonbanned firearms, there was a net reduction in firearm retrievals overall.[27]

Overall, the number of independent studies was small (three) and available evidence on violent outcomes was inconsistent. One study of greatest design suitability found a decrease in homicide,[45] while other nonindependent studies[42,46,47]—also of greatest design suitability—showed inconsistent findings. A study with a least suitable design[17] also found mixed effects for multiple outcomes. Additional evidence suggested that banned firearms are about half as likely to be used in crimes after the ban, compared with before the ban period or with areas where the same firearms are not banned.[27,28]

Other effects. In the period immediately preceding initiation of a ban, the production and sales of firearms about to be banned can increase dramatically.[45] Banning cheap firearms has been asserted[48] to decrease the capacity for self-protection among people in economically disadvantaged populations, who are also more likely to reside in high-crime neighborhoods. There is, however, no evidence for or against this hypothesis.

Conclusion. According to *Community Guide* criteria,[22] available evidence is insufficient to determine the effectiveness or ineffectiveness on violent outcomes of banning the acquisition and possession of firearms. The number of available studies was small, some available studies were limited in their design and execution, and results were inconsistent. Further research is needed to evaluate the effects of bans of specified weapons or ammunition on violence and related health and social outcomes.

Acquisition Restrictions

State governments and the federal government have made concerted efforts to deny the purchase of firearms to people with specified characteristics thought to indicate high risk for illegal or other harmful use of firearms. Restriction characteristics include criminal histories (e.g., felony conviction or indictment, domestic violence restraining order, fugitive of justice, or conviction on drug charges); personal histories (e.g., people adjudicated as "mental defective," illegal immigrants, those with a dishonorable military discharge); and other characteristics (e.g., juveniles). (The term "mental defective" is a determination by a lawful authority that a person, as a result of marked subnormal intelligence or mental illness, is a danger to self or others, or lacks the mental capacity to manage his or her own affairs. The term also includes a court finding of insanity in a criminal case, incompetence to stand trial, or not guilty by reason of lack of mental responsibility.[49])

The federal Interim Brady Handgun Violence Prevention Act (P.L. 103-159), hereafter Interim Brady Law, was implemented in March 1994 to strengthen the Gun Control Act of 1968 (P.L. 90-618) and to require the active investigation of the backgrounds of people applying to purchase handguns. Applications can be rejected if the applicant's background is found to include a felony indictment or conviction, domestic violence restraining order, unlawful use of or addiction to drugs, or dishonorable discharge, or if the applicant is a fugitive from justice or an illegal alien or has been adjudicated a "mental defective." The Interim Brady Law required a 5-day waiting period to allow the background investigation. (Evidence about the Interim Brady Law is included in the review of the effects of waiting periods.) The interim law was to be replaced by a permanent law following implementation of the National Instant Background Check System in 1998. The Lautenberg amendment (P.L. 104-208) of 1996 added a restriction that prohibits the sale of firearms to those convicted of a domestic violence misdemeanor. In 1997, in *Printz v. United States* (521 U.S. 98, 117 S.Ct. 2365 (1997)), the U.S. Supreme Court ruled that states could not be required to conduct background checks for the Interim Brady Law; for states that chose not to conduct background checks, the FBI had to conduct the checks.

The Permanent Brady Act (November 1998, P.L. 103-159), subsequently referred to as the Brady Law, required instant background checks for all firearms purchases, not only handguns. It eliminated the 5-day waiting period, but required firearms dealers to wait a maximum of 3 days to allow the location of required records, after which, if no prohibitory information had been identified, the purchase could proceed. Some states have restrictions in addition to the federal ones, and some states had such laws preceding the Interim Brady Law.[50,51]

Studies by the federal government[52,53] indicate difficulties in the instant background check system, primarily because of a lack of records on many restriction categories (e.g., on individuals adjudicated "mental defective," with a history of drug addiction, or with illegal immigrant status) or because criminal records are difficult and sometimes impossible to retrieve. The Bureau of Justice Statistics reports[54] that in 1999, of an estimated 59 million criminal history records available to states, 89.4% were automated. However, only a median of 69% of state records systems had the records of conviction status required to assess firearms restrictions. The investigation of individual applicant criminal histories may thus require the search of paper files—a time-consuming, costly, and not always successful activity, especially within the 3 days allowed.[55] Notable improvements in the background check system have been made,[56] but the system is still incomplete and lacks the records needed to be fully effective.

The Brady Law has prevented some prohibited people from purchasing firearms at the point of applica-

Table 3. Legal restrictions on gun acquisition: descriptive information about included studies

	Studies (*n*)
Studies meeting inclusion criteria	4[17,29,50,59]
Studies excluded, limited design or execution quality	0
Qualifying studies	4[17,29,50,59]
Independent studies included in body of evidence[a]	4[17,29,50,59]
Nonindependent studies, not in body of evidence[a]	0
Designs of included studies	
Prospective with concurrent comparison group	2[29,50,59]
Cross-sectional	1[17]
Outcomes reported in included studies	
Homicide	2[17,50]
Aggravated assault	1[17]
Robbery	1[17]
Rape	1[17]
Violent crime	2[29,59]
Suicide	2[17,50]
Unintentional firearm-related injury death	1[17]

[a]Studies are described as "independent" if they differ by intervention, population, or follow-up period.

tion for purchase. A review conducted in 1999[57] indicated that of 12.7 million handgun purchase applications (approximately 2.8 million per year) made during the period of the Interim Brady Law, 312,000 (2.4%) had been rejected—63.3% of those because of a felony conviction, 13.3% because of a domestic violence misdemeanor conviction or restraining order, 6.6% because of state-specific prohibitions, 6.1% because the applicant was a fugitive from justice, and 8.3% for other reasons. During the first year of the Permanent Brady Law, there were 8.8 million background checks, 2% resulting in denial; 17% of denials were appealed, of which 22% were reversed.[58] During the same period, 2230 fugitives of the law were identified, and 3353 prohibited people were found to have been erroneously permitted to acquire firearms.

Review of evidence: effectiveness. Our search identified four studies on the effects of acquisition restrictions on violent outcomes.[17,29,50,59] One additional study[60] examined only the waiting period component of the Brady Law (see review of waiting periods, below). Descriptive information about execution quality, design suitability, and outcomes evaluated in these studies is provided in Table 3. Details of the four independent qualifying studies are available at the website (*www.thecommunityguide.org/violence*).

Two studies[50,59] examined the effects of restrictions based on prior felony conviction; one[59] assessed overall violent crime as an outcome, and the second[50] assessed homicide and suicide. One of these studies[50] examined the effect of the Interim Brady Law as a whole. Because felony convictions constitute the exclusion factor for the largest proportion of those excluded by the law, we assessed this study as evaluating the felony conviction restriction, and note that evaluation studies often assess several intervention components at once. One study[29] examined the effect of restrictions based on misdemeanor convictions on violent crime overall. Another study[17] examined the effect of "mental defective" status, drug abuse, alcohol, and (unspecified) age restrictions against minors on specific violent crimes, suicide, and unintentional injury. The studies of felony conviction restrictions[50,59] were of greatest design suitability and fair execution; the study of misdemeanor restrictions[29] was of greatest design suitability and good execution; and the study of "mental defective" status, drug abuse, alcohol, and age restrictions[17] was of least suitable design and fair execution.

One study[59] evaluated the effect of felony conviction restrictions in California, and concluded that subsequent arrest for violent crime among restricted felons was 19.4% lower (95% confidence interval [CI]=9.9%, 28.1%) than would have been expected had these felons been allowed to purchase firearms. The second study of felony conviction restrictions[50] indicated statistically nonsignificant declines for firearm-related homicide and suicide and total homicide and suicide in the U.S. population aged ≥21 years, and a statistically significant decline in firearm-related suicide deaths among people aged ≥55 years. However, by comparing outcomes in states that had a waiting period prior to the Brady Law with states that did not previously have a waiting period, this study showed that this reduction was attributable not to the felony restriction per se, but to the waiting period component of the Interim Brady Law.

A single study[29] indicated that a misdemeanor conviction restriction reduces the rate of first arrest for violent crime by 19.4% and arrests over a 3-year period for firearm or violent crime by 10.7%; however, neither result is statistically significant, and the single study is thus not sufficient to draw a conclusion about effectiveness, because it is not clear that either finding differs from no change.

One study[17] examined four personal history restrictions (i.e., "mental defective," drug abuse, alcohol, and minor age) and their associations with homicide, aggravated assault, robbery, rape, suicide, and unintentional injury. This cross-sectional study had 10 effects in the desired direction and 14 in the undesired direction, 2 of them statistically significant. Overall, evidence of consistent effect by restriction or outcome is limited, because of small numbers of studies of each outcome and inconsistent directions of effect.

One study[50] allowed assessment of the substitution effect (i.e., because the restriction or a waiting period makes firearms unavailable, people substitute other means to harm others or commit suicide). The researchers found evidence of a substitution effect for

suicide, but not for homicide; however, the suicide substitution effect is relatively minor: an increase of 3.0% in non-gun suicide, compared with the firearm-specific suicide decline of 8.6%.

Other effects. Restrictions may facilitate the identification and capture of wanted persons.[56] Background checks may also act as a deterrent to application by people prohibited from purchasing weapons. However, we found no evidence of this or of whether denied applicants subsequently acquired firearms by other means (e.g., from the secondary market). One potential harm is false positives, that is, people falsely reported as having a restriction, who may subsequently be stigmatized and mistakenly denied a firearm.

Conclusion. According to the *Community Guide* criteria,[22] the available evidence is insufficient to determine the effect of firearms acquisition restrictions on public health and criminal violence, because of a small number of available studies, limitations in their design and execution, and variability in the direction and statistical significance of findings. The only restriction for which study design suitability and execution met our criteria was the misdemeanor conviction restriction; in this instance, the effect was in the expected direction, but was not statistically significant, and we were thus unable to draw a conclusion. Further research is needed to evaluate the effects of acquisition restriction laws on violence, other health-related outcomes, and related health and social effects.

Waiting Periods for Firearms Acquisition

Waiting periods for firearms acquisition require a specified delay between application for and acquisition of a firearm. This requirement is usually imposed to allow time to check the applicant's background or to provide a "cooling-off" period for people at risk of committing an impulsive crime or suicide. In addition to background checks, waiting periods can be combined with other provisions, such as a requirement for safety training.

The Interim Brady Handgun Violence Prevention Act, a federal law that went into effect in 1994, mandated a background check and a 5-day waiting period for handgun purchasers. In 1998, the 5-day waiting period required by the Interim Brady Law expired, and was replaced by a mandatory, computerized National Instant Criminal Background Check System (required not only for handguns, but for all firearms purchases), allowing dealers to sell the firearm if the FBI reported no adverse evidence to the dealer within 3 days of application. However, many states have their own provisions mandating longer waiting periods for handgun or long firearm purchases or both. Reports on the number of states with waiting periods for handgun purchases vary from 10 (National Rifle Association website: *www.nra.org*) to 15[61] to 19[62], with waiting periods ranging from 2 days (in Alabama, Nebraska, South Dakota, and Wisconsin) to 6 months (in New York).[61]

Table 4. Waiting periods for firearm acquisition: descriptive information about included studies

	Studies (*n*)
Studies meeting inclusion criteria	7[17,50,63–67]
Studies excluded, limited design or execution quality	0
Qualifying studies	7[17,50,63–67]
Designs of included studies	
Time series with concurrent comparison group	2[50,64]
Before and after, no concurrent comparison group	1[63]
Cross-sectional	4[17,65–67]
Outcomes reported in included studies	
Homicide	6[17,50,64–67]
Aggravated assault	5[17,64–67]
Robbery	5[17,64–67]
Rape	2[17,64]
Suicide	6[17,63–67]
Unintentional firearm-related injury death	3[17,64,66]

Review of evidence: effectiveness. Our search identified seven studies on the effects of waiting periods on violent outcomes.[17,50,63–67] Descriptive information about execution, design suitability, and outcomes evaluated in these studies is provided in Table 4. Details of the seven independent qualifying studies are available at the website (*www.thecommunityguide.org/violence*). One study[63] was conducted in Queensland, Australia; the remaining studies were conducted in the United States.

Among the seven qualifying studies, five[17,63,65–67] were of lowest design suitability, and two[50,64] of greatest design suitability; all seven studies had fair execution. One study[64] presented the effectiveness results as a mathematical function of the length of waiting period; for purposes of this review, we calculated an effect estimate for a 5-day waiting period (as required by the Interim Brady Law).

Of six studies that evaluated the effects of waiting periods on homicide, four[17,65–67] had least suitable designs. Results were mixed: three point estimates showed a reduction in homicide, two showed an increase (one study with results for 2 decades, the 1960s and 1970s), and none of these findings were statistically significant. Two studies[66,67] found that results were not statistically significant without providing either size or direction of the effect.

Six studies evaluated effects of waiting periods on suicide. One study[63] evaluated the effect of waiting periods for long firearm purchase, one[50] for handgun purchase (under the Interim Brady Law 5-day waiting period), and four[17,64,66,67] for both long firearm and handgun purchases. Two[17,63] studies presented data

that allowed the calculation of relative percentage change in suicide rates; one[17] found a small (0.5%) increase and one[63] a small (2.9%) decrease in total suicides. Two[50,64] studies reported only absolute changes in suicide rates without data on baseline rates, which did not allow calculation of relative percent change. One study reported decreases in firearm suicide rates among children (aged 0 to 14 years) and adolescents[64] (aged 15 to 19 years), and the second study reported a decrease in both firearm-related and total suicide rates among adults (aged ≥21 years).[50] However the second study's decrease was statistically significant only in a subsample of people aged ≥55 years, and only for firearm-related suicide.[50] Two studies[66,67] reported that results were not significant, without providing either size or direction of the effect.

Evidence of the law's effects on aggravated assault, robbery, rape, and unintentional firearm-related injury death were inconsistent in direction, with six of the effect estimates indicating an increase, five indicating a decrease, and none being statistically significant.

Comparison of the effect on suicide of a 28-day waiting period for long firearms (in Queensland, Australia)[63] with a 5-day waiting period for handguns (associated with the Interim Brady Law)[50] indicated a greater effect associated with the longer waiting period for firearm-related suicide, but not for total suicide.

Several studies,[17,50,63] for which both firearm and non-firearm effect estimates were available, suggested the presence of a partial substitution effect for suicide, in which decreases in firearm-related suicide are offset, but at substantially lower levels, by increases in non-gun suicide. No such substitution effects were found for homicide, aggravated assault, or robbery.

Other effects. It has also been asserted[60] that waiting periods may give criminals (who may be more likely to acquire firearms by illegal means and avoid the waiting period) an advantage in obtaining firearms over law-abiding citizens (who may lack means of self-defense during the waiting period). However, there is no evidence for or against this hypothesis. One study[64] reported inconsistent effects of waiting periods on property crime; it found an increase in burglary and a decrease in larceny and auto theft.

Conclusion. According to the *Community Guide* criteria,[22] the evidence is insufficient to determine the effectiveness of waiting periods for the prevention of suicide, homicide, aggravated assault, robbery, rape, and unintentional firearm-related injury death, because of the small number of available studies, limitations in the design and execution of available studies, and effects that are inconsistent in direction or fail to reach statistical significance. Further research is needed to evaluate the effects of waiting period laws on violence, other health-related outcomes, and associated health and social effects.

Firearms Registration and Licensing of Firearm Owners

Registration requires that a record of the owners of specified firearms be created and retained.[68] Licensing requires an individual to obtain a license or other form of authorization or certification that allows the purchase or possession of a firearm.[68] Licensing and registration requirements are often combined with other firearms regulations, such as safety training or safe storage requirements.

The registration practices of states and the federal government vary widely.[69] Recorded information may be retained by a specified recorder, such as by federal firearms licensees; such records may be accessible under specified circumstances, such as criminal investigations. In some states, recorded information is kept in centralized registries. The Firearm Ownership Protection Act of 1986 specifically precludes the federal government from establishing and maintaining a national registry of firearms and their owners. Likewise, there are no current federal firearms licensing requirements or provisions for individual purchasers. However, several states have laws that require the licensing of firearm owners or registration of firearms, and recorded information is kept in centralized registries. For example, licensing of handgun owners is required in 17 states and the District of Columbia.[6] Statewide handgun registration laws currently exist in four states. Licensing and registration may serve as instruments for the control of illegal firearms ownership, transfer, and use,[56,70] and might also deter illegal acquisition and use.

Review of evidence: effectiveness. Our search identified five studies[17,65–67,71] on the effects of licensing on violent outcomes, two[17,71] of which also report on the effects of registration. One study[17] was based on data collected in 1979 to 1981, one[65] on data collected in the 1960s and 1970s, one[66] on data collected in 1978, and one[67] on data collected in 1969–1970; one[71] assessed firearms retrieved from crimes during a 1-year period (1997–1998). All five studies were of least suitable (cross-sectional) design and had fair execution. Descriptive information about execution quality, design suitability, and outcomes evaluated in these studies is provided in Table 5, and at the *Community Guide* website (*www.thecommunityguide.org/violence*). Details of the four independent qualifying studies are also available at the website.

Evidence of the effects of licensing and registration on diverse study outcomes was inconsistent, with eight of the effect estimates showing increases in violence, and eight showing decreases. (One study had data on three outcomes each for 1960 and 1970.) Two studies[66,67] reported that results were statistically nonsignif-

Table 5. Firearm registration and owner licensing: descriptive information about included studies

	Studies (*n*)
Studies meeting inclusion criteria	5[17,65–67,71]
Studies excluded, limited design or execution quality	0
Qualifying studies	5[17,65–67,71]
Studies used as secondary evidence[a]	1[71]
Designs of included studies	
Cross-sectional	5[17,65–67,71]
Outcomes reported in included studies	
Homicide	4[17,65–67]
Aggravated assault	4[17,65–67]
Robbery	4[17,65–67]
Rape	1[66]
Suicide	4[17,65–67]
Unintentional firearm-related injury death	2[17,66]
Gun counts or proportions	1[71]

[a]Secondary evidence does not directly measure a violent outcome, but may be suggestive of an effect.

icant without providing either size or direction of the effect.

One study[71] assessed recovered firearms that had been used in crimes in states with and without licensing and registration laws. We counted this study as secondary evidence because it provided neither a direct measure of violent outcomes or evidence that the use of recovered firearms is a good proxy measure of crime. This study reported that crime firearms purchase in-state was 48.5% lower in cities that had both licensing and registration requirements, compared with cities that had neither.

Other effects. Potential benefits that have been associated with the licensing of firearm owners and the registration of firearms include increased ability to enforce firearms laws, tracing sources of illegally possessed or used firearms, and data for research on the etiology of harmful and illegal firearms uses.[70,72] Potential harms that have been associated with licensing and registration are the perceived threat to the privacy and rights of owners.[73]

Conclusion. According to the *Community Guide* criteria,[22] the evidence on licensing and registration is insufficient to determine their effectiveness in reducing violence. Only a few studies were available, there were limitations in the studies' design and execution, and results were inconsistent. Further research is needed to evaluate the effects of licensing and registration laws on violence, other health-related outcomes, and associated health and social effects.

Shall Issue Concealed Weapons Carry Laws

Shall issue concealed-weapons carry laws (shall issue laws) require authorities to issue permits to carry concealed weapons to all applicants who are not found to have specified characteristics that disqualify them. In contrast, some states have adopted "may issue" laws, in which the issuing authority has the discretion to issue or deny a firearms permit based on criteria such as the perceived need or moral character of the applicant, and other states prohibit all carrying of concealed weapons (as of 2001, six states had such a prohibition).[6] Disqualification criteria in shall issue laws vary by state, but generally include, among others, prior felony conviction, conviction on a drug charge in the past 3 years, commitment to a mental hospital in the past 5 years, fugitive from justice, or age below a specified minimum. States also differ substantially in requirements such as firearms safety training, permit fees, and specifying places where firearms may not be carried.[11]

Before 1977, only eight states had shall issue laws, compared with 31 states as of 2000.[62] Researchers disagree on which states adopted shall issue laws and when.[74–76] For example, several studies consider Virginia to have had a shall issue law in 1988.[74,75,77–80] However, although the Virginia law at that time included the phrase "shall issue," the law also required demonstration of the applicant's need and "good character"—both characteristics of the more discretionary "may issue" laws. Differential classification of the laws may affect analyses of their effects.

Two principal hypotheses, which are not mutually exclusive, have been proposed to predict the consequences of shall issue laws. Some analysts have reasoned that, because the law allows for self-defense, potential criminals may be deterred by fear that a possible victim could be armed.[60] If so, publicity about the law and the perception on the part of potential criminals that individuals could be carrying concealed firearms is likely to be more important in reducing violence than the actual numbers of firearms carried. Others have reasoned that the presence of more firearms increases rates of unintended and intended injury in interpersonal confrontations, and, in addition, leads potential criminals to carry and use more lethal firearms more often.[81] If this is so, the actual number of additional firearms carried is important. In the only available survey on the attitudes of (imprisoned) felons, Wright and Rossi[82,83] report that felons claim to be deterred from committing a crime if they think that potential victims might be armed, but also carry firearms themselves to deter violence by victims. This finding suggests that shall issue laws may have contrary effects on firearms behavior—both deterring and escalating firearms carrying in the criminal population—with unknown net effect.

Review of evidence: effectiveness. Our search identified 12 studies[17,30,60,75,77–80,84–87] on the effects of shall issue laws on violent outcomes. Descriptive information about the quality, study design, and outcome measures from these studies is provided in Table 6. Details of all

Table 6. "Shall issue" carry laws: descriptive information about included studies

	Studies (*n*)
Studies meeting inclusion criteria	12[17,30,60,75,77–80,84–87]
Studies excluded, limited design and execution quality	0
Studies excluded, limited data quality[a]	8[60,75,77–80,84,85]
Qualifying papers	4[17,30,86,87]
Studies included in body of evidence	4[17,30,86,87]
Designs of included studies	
Time series with concurrent comparison group	2[30,87]
Time series, no concurrent comparison group	1[86]
Cross-sectional	1[17]
Outcomes reported in included studies	
Homicide	3[17,30,86]
Homicide of the police (i.e., police as homicide victims)	1[87]
Aggravated assault	2[17,30]
Robbery	2[17,30]
Rape	2[17,30]

[a]Because county-level crime data have been shown to be highly unreliable,[33] and because they have not been consistently used correctly, we excluded studies based on these data regardless of other design or execution qualities.

qualifying studies are available at the website (*www.thecommunityguide.org/violence*).

In 1997, Lott and Mustard[74] published an analysis of the effects of shall issue laws based on a large data set and spanning a 17-year period. They tested multiple hypotheses about the effects of shall issue laws on diverse outcomes, including violent crimes, property crimes, unintentional injury deaths, and suicide.[60,64,88] Because crime rates vary considerably among counties, Lott and Mustard[74] focused their analysis on U.S. county-level rather than state-level data. Five additional studies[75,78–80,85] used Lott and Mustard's data[74] or independently derived county-level data[77] as the basis for their own analyses. However, county-level crime data are highly problematic.

At the county level, missing data and under-reporting are prevalent. Concerns have been raised about the procedures for extrapolating to estimate the extensive missing county-level data.[33] Lott and Mustard[74] and those who used these authors' data did not adjust for missing information by using population denominator data that corresponded to crime numerator data. Thus, Lott and Mustard's denominator numbers were often too high, leading to underestimated crime rates in regions with poor reporting.[33] For example, less populous regions may have lower rates of crime as well as less complete reporting; comparisons by region would then be biased. Finally, these county-level studies may have misclassified as many as three out of ten reviewed states as shall issue jurisdictions.[30] The relationships among available studies of shall issue laws by data source and unit of analysis (Figure 3)) indicate that most studies of these laws suffer from basic data problems associated with county-level information. Because of these critical concerns about the accuracy of county-level crime data for research purposes,[33] we did not use data from any of the county-level studies in our assessment of the effects of shall issue laws on violence.

The four qualifying studies of shall issue laws include one study[30] that examined national level effects on homicide using Vital Statistics reports (from the CDC's National Center for Health Statistics), one study[17] that used both Vital Statistics and UCR data to examine the effects of shall issue and other firearms laws on multiple violent outcomes, one study[86] that used Vital Statistics to assess the effects of shall issue laws in five selected counties, and one study[87] that used state-level UCR data to assess the effects of shall issue laws on homicides of police (homicides in which police are the victims). Thus, three qualifying studies assessed homicide as an outcome; of these one assessed homicide of police officers, and another multiple violent outcomes. Two of these studies are of greatest design suitability, one each of moderate and least suitable design, and all had fair execution. In contrast to county-level data from the UCR, county-level mortality Vital Statistics data are essentially complete.[89]

Two studies[17,30] suggested a reduction in homicide associated with shall issue laws at the national level, and the third[86] suggested mixed effects in five counties, with an overall increase in homicide associated with the laws. The study of police homicide[87] shows a small, statistically nonsignificant decline in the homicide of police associated with shall issue laws. Homicides of police occur at a rate of <100 per year, accounting for 0.6% of all U.S. homicides.

Conclusion. According to *Community Guide* criteria,[22] the small number of qualifying studies that evaluate the effects of shall issue laws on homicide, aggravated assault, robbery, rape, and homicide of police is not sufficient to determine the effectiveness of these laws in reducing the rate of these crimes. We have not included data from studies based on county-level evidence in our assessment, because county-level data have important systematic flaws that preclude reliable conclusions. Further research is needed to assess the effects of shall issue laws on violence.

Child Access Prevention Laws

Child access prevention (CAP) laws are designed to limit children's access to and use of firearms; states vary in the ages of children covered by the laws, from <14 to <18 years. The laws require firearm owners to store their firearms locked, unloaded, or both. In some states, firearm owners are liable when firearms are improperly stored or when a child uses the owner's

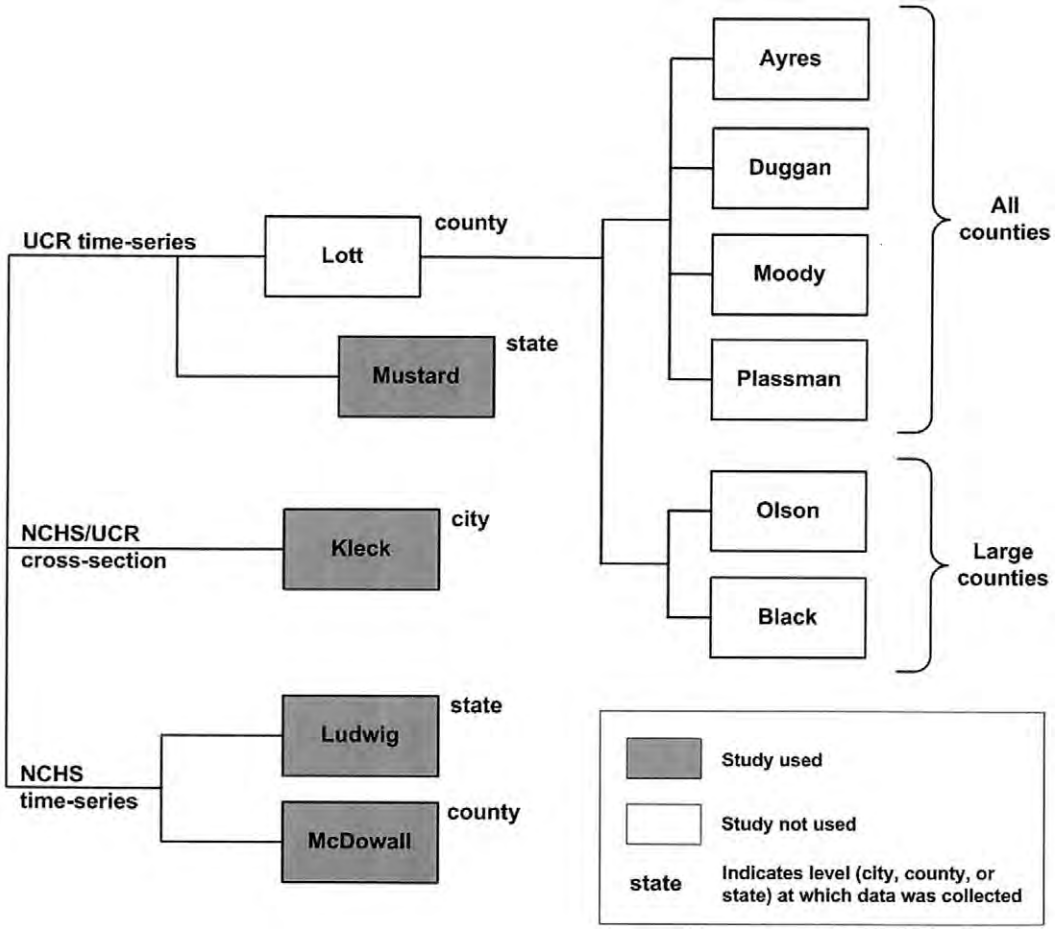

Figure 3. Sources of data and designs in studies of "shall issue" laws. NCHS, National Center for Health Statistics; UCR, Uniform Crime Report.

improperly stored firearm to threaten or harm him- or her-self or another person.

Laws aimed at preventing child access are a relatively recent development: Florida passed the first CAP law in 1989, and after the Columbine shootings in April 1999, two more states adopted CAP laws.[90] By 2000, a total of 16 states had adopted CAP laws.[62] In three states (FL, CT, CA), violating a CAP law is a felony; in the other states with CAP laws, it is a misdemeanor.

Review of evidence: effectiveness. Our search identified three studies[64,91,92] of the effect of CAP laws; all examined unintentional firearm-related injury deaths as outcomes, and one[92] examined firearm-related and non–firearm-related suicides and homicides. All three studies were of greatest design suitability and fair execution. On the untested assumption that locked and unloaded firearms may hinder rapid access to firearms for self-defense, one study[64] examined multiple outcomes, including violent crimes (i.e., homicide, aggravated assault, robbery, and rape) committed with and without firearms. All studies assessed outcomes among juveniles; one study[64] also examined effects for older age groups. Descriptive information about the quality, study design, and outcome measures from these studies is provided in Table 7. Details of the three qualifying studies are available at the website (*www.thecommunityguide.org/violence*).

All the studies present a common challenge for purposes of analysis: The law is intended to reduce injuries

Table 7. Child access prevention laws: descriptive information about included studies

	Studies (*n*)
Studies meeting inclusion criteria	3[64,91,92]
Studies excluded, limited design and execution quality	0
Qualifying studies	3[64,91,92]
Designs of included studies	
Time series with concurrent comparison group	3[64,91,92]
Outcomes reported in included studies	
Homicide	2[64,92]
Aggravated assault	1[64]
Robbery	1[64]
Rape	1[64]
Suicide	2[64,92]
Unintentional firearm-related injury death	3[64,91,92]

caused by juveniles. The studies, however, assess juvenile **victims**, whose injuries (other than suicide) could be caused either by adults or by juveniles. As a result, the assessment of the effects of CAP laws on outcomes other than suicide may be biased. None of the studies assessed levels of publicity, awareness, or enforcement of CAP laws as mediators of their potential effects.

Two studies[64,91] examined effects of the same laws on unintentional firearm-related injury in the same populations in similar time periods. Of these, we chose the study[91] with the greatest suitability of design and execution scores to assess effects of the laws on unintentional firearm-related injury. However, we used the study with a lower execution score[64] to assess additional outcomes (i.e., homicide, assault, robbery, and rape).

An earlier study[92] indicated a reduction in unintentional firearm-related injury death among juveniles aged <15 years that was statistically significant in states providing a felony prosecution for CAP law violation, and a nonsignificant increase in unintentional firearm-related injury death among juveniles in states providing a misdemeanor prosecution. However, a later study,[91] including data from three additional states that had passed CAP laws and 3 more years of follow-up, confirms the earlier finding on states with misdemeanor prosecution, but shows that, among states with a felony prosecution, the effect of the law on unintentional firearm-related injury death among juveniles aged <15 years is statistically significant only in Florida (a state with a felony sanction) but not in the other two felony states.

One study[92] indicated a reduction associated with CAP laws in firearm-related suicide among juveniles aged <15 years. Data from studies of homicide, assault, robbery, and rape[64,92] indicate mixed results, with two findings indicating reductions (in firearm-related homicide among juveniles aged <15 years, and in assault among all ages), and three indicating increases (in total homicide, robbery, and rape in all ages) associated with CAP laws. Only the findings on robbery and rape are statistically significant. However, too few studies examine each outcome to determine the effect of the law on specific types of violence.

Other effects. One study[64] suggests that CAP laws may be associated with an increase of 2% in property crimes; the increase was statistically significant for burglary but not for property crime overall.

Conclusion. According to *Community Guide* criteria,[22] the small number of studies of CAP laws, all of limited quality of execution and inconsistent findings, is insufficient to determine the effectiveness of the laws in reducing violence or unintentional firearm-related injury and other violent outcomes. Further research with longer follow-up periods is needed to assess effects of CAP laws on violence, unintentional injury, and other outcomes of interest.

Zero Tolerance of Firearms in Schools

The Gun-Free Schools Act,[93] which affected 94% of schools in 1996–1997, stipulates that each state receiving federal funds under the Act must have a law requiring local education agencies to expel a student from school for ≥1 year if the student is found in possession of a firearm at school, although this expulsion requirement can be modified on a case-by-case basis. Expulsion may lead to alternative school placement or to "street" placement (full expulsion, with no formal education, for a specified length of time), after which students are generally allowed to return to their regular schools.

In the 1998–1999 school year, 3523 students were expelled for having a firearm in school. Of the total expelled, 44% were referred to alternative schools. (A national survey[94] indicated that, as of 1993, 66% of school districts reported implementing some type of alternative program to address school violence.) In 1996–1997, 4% of public schools reported having random, handheld metal detector checks on students, and in 1% of schools, students were required to pass through metal detectors every day.[95]

A national survey[96] indicates that approximately 3% of the 12th graders in 1997 (an estimated 80,190 students nationwide) reported carrying firearms on school property in the previous 4 weeks. According to these separate estimates, even if only seniors carry firearms, <4.4% of firearms (i.e., 3523/80,190) are being detected in association with the Gun-Free Schools Act. If students in lower grades also carry firearms (statistics are not available to determine this), the proportion of firearms being detected would be even lower.

The Gun-Free Schools Act does not require reporting on possible effects of its requirements on school safety conditions other than numbers of firearm-carrying students detected and expelled; however, reports from other sources indicate changes in some aspects of violence in the school environment. The carrying of weapons appears to have declined steadily during the 1990s, as did involvement in physical fights on school property.[97,98] However, the proportion of high school students who reported being threatened or injured with a weapon on school property in the past 12 months remained steady over this period, at 7% to 9%. The rate of serious violent crimes at or on the way to or from school peaked in 1994, and has declined from then until at least 2000.[98]

Review of evidence: effectiveness. No studies were located that attempted to evaluate the effects on school violence of zero tolerance of firearms in schools; nor did any study measure the specific effect of the Gun-Free Schools Act on firearm carrying in schools.

There was one study[99] of the effectiveness of metal detector programs in reducing the carrying of firearms in schools. Although firearms detection is not explicitly required in the Gun-Free Schools Act, the effectiveness of the law may depend on the ability to detect firearms. The study was a cross-sectional survey of New York school administrators and students to assess the association of metal detector programs with student behavior and attitudes. The metal detection program studied consisted of approximately weekly scanning of "randomly selected students" with a handheld device; the likelihood of detection was unclear. The study was of least suitable design and fair execution. Details about the quality, design, and outcome measure from this study are available at the website (*www.thecommunityguide.org/violence*).

The study compared rates or counts of firearms detection at schools with and without metal detection programs. Compared with schools without metal detection programs, schools with such programs had rates of carrying firearms to, from, or in school that were half as great (1.9% to 2.1% vs 4.0% to 4.6%), but did not differ in weapons carrying overall. Moreover, the study reported that schools did not differ in rates of threats or fights outside or inside of school. We could not determine the effectiveness of these programs because only a single study of least suitable design was available, and because the intermediate outcome of firearms carrying is not necessarily a good proxy for violence or injury.

Other effects. The effects of firearms detection programs in schools on students, school staff, or community are unknown; it is possible that such programs either reduce fear of harm or increase awareness, concern, and fear about the possibility of firearm-related violence. These effects may vary to the extent that a program is more or less effective in reducing firearms in schools.

A major, albeit unintended, harm of the Gun-Free Schools Act of 1994, particularly if firearms detection becomes more effective, is the "street" expulsion of thousands of students with low school achievement and high risk of violence. One review for the U.S. Department of Education[100] indicates that alternative schools for violent students may be effective as well as cost-effective in reducing violent behavior and enhancing emotional development for youth suspended or expelled from their usual schools; however, the review also notes that attendance at alternative schools may stigmatize students and increase discrimination against them. Even though the specific effect of firearm-related expulsion is not known, expulsion can result in a life course with fewer opportunities for (legal) employment, fewer resources, and a greater likelihood of criminal behavior and imprisonment compared with retention in special school programs.[101] The resulting lower productivity and increased criminal activity are likely to have high societal costs.[101]

Conclusion. It was not possible to assess the effectiveness of zero tolerance of firearms in schools because no studies of zero tolerance were identified and only a single study of least suitable design was identified that measured the effect of a school metal detector program on firearm-carrying behavior but not on violence per se. The effectiveness of such widespread policies in reducing violence and related health and social outcomes needs additional evaluation.

Combinations of Firearms Laws

Government jurisdictions (e.g., states or nations) differ in the degree to which they regulate firearms possession and use as well as in rates at which specific forms of violence occur (as is the case with the United States and Canada).[102] In our review, we considered whether these characteristics—degree of firearms regulation, and firearm-related and other forms of violent behavior—are causally associated. Causality is difficult to assess because levels of firearm-related violence and the degree of firearms regulation may each affect the other: high levels of firearm-related violence may lead to the increased regulation of firearms, and regulation may also lead to the reduction of violence. Moreover, these possibilities are not mutually exclusive. The interpretation of association is thus difficult and depends on temporal sequence, which cannot be determined in simple cross-sectional studies. An additional challenge to establishing a causal link may be the lack of comparable information from nations about laws, violent outcomes, and possible confounders of the association between them.

Review of evidence: effectiveness. We reviewed three forms of evidence: studies of the effects of comprehensive national laws within nations; cross-national studies of firearms law systems; and studies in which law types within jurisdictions (i.e., regulation of specific, defined aspects of firearms acquisition and use) are categorized and counted, and the counts correlated with rates of specific forms of violence within the same jurisdictions. We refer to these last as "index studies" because they develop indices of regulation based on the kinds and numbers of firearms laws found in different jurisdictions. We considered the three kinds of evidence together in drawing conclusions. Descriptive information about execution quality, design suitability, and outcomes evaluated in these studies is provided in Tables 8, 9, and 10. Details of the studies that met inclusion criteria are available at the website (*www.thecommunityguide.org/violence*).

We considered available studies of two comprehensive national laws, the Gun Control Act of 1968 (P.L. 90-618) in the United States and the Criminal Law Amendment Act of 1977 in Canada. Our search identified two studies[65,113] of the U.S. law that assessed violent outcomes and ten studies[103–112] of the Canadian

Table 8. Combinations of laws: Gun Control Act of 1986 (United States) and Firearms Control Legislation of 1977 (Canada)

	Studies (*n*)
Studies meeting inclusion criteria	12[65,103–113]
Studies excluded, limited design and execution quality	0
Qualifying papers	12[103]
Studies included in body of evidence	2[65,109]
Nonindependent studies, not in body of evidence[a]	10[103–108,110–113]
Designs of included studies	
Before-after, no concurrent comparison group	1[65]
Time series, no concurrent comparison group	1[109]
Outcomes reported in included studies	
Homicide	2[65,109]
Suicide	1[109]
Unintentional firearm-related injury death	1[109]

[a]Nonindependent studies are not included in the body of evidence because they assess the same intervention in the same population for the same (or a shorter) follow-up period, are not as well designed or executed as an included study, or both.

law that assessed violent outcomes (Table 8). Because the studies of each law were not independent, we chose the study with the greatest design and execution scores to represent the effects of the U.S. law[65] and one Canadian study[109] to represent the effects of the Canadian law (on rates of homicide, suicide, and unintentional firearm-related deaths). The U.S. study was of least suitable design and fair execution; the Canadian study was of moderate design suitability and fair execution. The study of the Gun Control Act of 1968 yielded two nonsignificant results in opposing directions (i.e., an increase in homicide, adjusted for new firearms, and a decrease in homicide, adjusted for the total firearms stock). The study of the comprehensive Canadian firearms law indicated decreased rates of homicide, but increased rates of firearm-related suicide.

In the cross-national studies of comprehensive laws, the effects of more and less comprehensive firearms regulations on violence were assessed by comparing regions within two nations, the United States and Canada. Our search identified three such studies (Table 9).[114–116] All three studies were of least suitable design and fair execution. Because the two Canadian–U.S. comparisons of homicide assessed largely distinct populations in different time periods—1976 to 1980[114] and 1980 to 1986[115]—these two studies were regarded as independent.

Table 9. Combinations of laws: international comparative studies (United States and Canada)

	Studies (*n*)
Studies meeting inclusion criteria	3[114–116]
Studies excluded, limited design and execution quality	0
Qualifying papers	3[114–116]
Study designs	
Cross-sectional	3[114–116]
Outcomes reported	
Homicide	2[114,115]
Aggravated assault	2[114,115]
Robbery	1[115]
Suicide	1[116]

Table 10. Combinations of laws: firearm law index studies

	Studies (*n*)
Studies meeting inclusion criteria	8[17,65–67,117–120]
Studies excluded, limited design and execution quality	0
Qualifying papers	8[17,65–67,117–120]
Studies included in body of evidence	6[17,65,66,117,118,120]
Papers excluded, nonindependent[a]	2[67,119]
Study designs	
Cross-sectional	6[17,65–67,117,118,120]
Outcomes reported	
Homicide	4[17,65,66,118]
Suicide	5[17,66,117,118,120]
Unintentional firearm-related injury death	3[17,66,118]
Aggravated assault	4[17,65,66,118]
Robbery	4[17,65,66,118]
Rape	1[17]

[a]Nonindependent studies are not included in the body of evidence because they assess the same intervention in the same population for the same (or a shorter) follow-up period, are not as well designed or executed as an included study, or both.

One study[115] comparing Seattle with Vancouver found an inverse association between the degree of firearms regulation in these cities and their rates of firearm-related aggravated assault (relative risk 7.7, 95% CI=6.7, 8.7) and homicide (relative risk 5.1, 95% CI=3.5, 7.3), but not of other forms of interpersonal violence. A second study in the same setting[116] found a similar inverse association of the degree of firearms regulation and firearm-related suicide, counterbalanced by an opposing difference in other forms of suicide; that is, the degree of regulation was associated with lower rates of firearm-related suicide and higher rates of other forms of suicide. The third study[114] compared U.S. and Canadian border states and provinces, respectively, and indicated no association between national levels of firearms regulation and rates of homicide; no summary statistic was reported.

The index studies compared degrees of firearms regulation and violent outcomes among U.S. states and cities. We found eight index studies,[17,65–67,117–120] of which all but two[67,119] qualified for analysis (Table 10). Several qualifying studies include separate analyses of data from different years; thus, separate findings from a single study (e.g., from 1960 and 1979 in the study by Maggadino and Medoff[65]) are included in our analysis, insofar as the study is independent from other studies.

One qualifying study[66] noted only the lack of statistically significant differences between levels of violence associated with the degree of regulation, without indicating the quantity or even the direction of difference. All six qualifying studies were of least suitable design and fair execution.

Index studies yielded heterogeneous results. Of six findings on homicide from three studies,[17,65,118] one indicated a statistically significant increase and five indicated decreases, two of which are statistically significant. One study with rape as an outcome indicated a statistically nonsignificant decrease. Three studies with aggravated assault, robbery, and unintentional firearm-related injury death as outcomes had inconsistent findings, some indicating an increase in the outcome associated with greater regulation, and others a decrease. Only for suicide did all index studies show a reduction associated with a greater amount of regulation; two of five results were statistically significant. Overall, index studies were found to have inconsistent results on violent outcomes.

Other effects. High levels of regulation may be seen as an infringement on individual rights.

Conclusion. Based on findings from national law assessments, cross-national comparisons, and index studies, evidence is insufficient to determine whether the degree or intensity of firearms regulation is associated with decreased (or increased) violence. Current evidence is inconsistent and, in general, methodologically inadequate, based on Task Force standards, to draw conclusions about causal effects. Moreover, even if findings were clear, the design of index studies conducted to date would not allow us to specify which firearms laws did or did not contribute to the reduction of violence. Additional research is needed to determine the relationship(s) between specific types and degree of firearms regulation and the rates of specified types of violence in given jurisdictions.

Results: Part II—Research Issues for Firearms Laws

Review of eight firearms laws and law types found insufficient evidence to determine whether the laws reviewed reduce (or increase) violence. Additional high-quality research is required to determine whether a relationship exists between firearms laws and violent outcomes. Areas for further potential study are discussed below.

General Research Issues

1. Violent outcome data sources

It was noted at the outset of this article and in the assessments of specific laws that multiple problems exist with the available data on outcomes used in studies of firearms laws. Much remains to be done to improve the recording of events and accessibility of the relevant data. Improvements would allow better evaluation of the effects of firearms laws as well as improvements in understanding of other aspects of violence and injury. These include:

- Reporting systems for individual criminal and violent events and details of their circumstances
- More detailed data on the location and perpetrators of the crime
- More detailed data on agents in unintentional firearm-related injuries, linked to information on both the victim and the storage conditions of firearms involved
- More detailed information on firearms used in crimes (e.g., type of firearm used, whether the firearm was carried legally, was registered, how it was acquired, and whether the owner was licensed)
- More statistics relevant to changes in behaviors that can be attributed to laws (e.g., the numbers of concealed carry permits issued, or changes in safe storage practices).

2. Measurement of exposure: What laws are in place, and where?

Classification: There have been disputes about which states have which types of laws. Misclassification of state laws and their dates of implementation hinders firearms law research. Some differences among states in the effects of laws may be attributable to differences among states in provisions of the law, such as their requirements, penalties, or the presence of other laws. A recent analysis of firearms laws[62] may help to resolve some of these issues for researchers by providing a recent, systematic, and detailed analysis of major federal, state, and local firearms laws.

Implementation and enforcement. As with any intervention, the degree of implementation may affect the intervention's effectiveness. Data on implementation have typically not been included in the evaluation of firearms laws. How do the intensity and visibility of law enforcement differ among jurisdictions, and how do they affect the law's effectiveness?

Publicity and awareness of laws. Knowledge about laws may be one means by which they become effective. If deterrence is a factor in the effectiveness of a law, then public (and criminal) awareness is of particular importance. Awareness can mitigate a law's potential effects, as when firearms are purchased at increased rates prior to the implementation of a ban.

Duration of exposure and follow-up. Follow-up periods of <2 years may be inadequate to assess the long-term societal effects of a law. It will be useful to determine whether specific laws have immediate or gradual impact, and how effects change over time.

3. Measurement of violent outcomes

Specific measures. Studies should measure outcomes directly associated with the law being evaluated (e.g., violence outside the home for laws about firearm carrying outside the home, and child violence perpetration for laws about child access to and use of firearms in the home). Failure to do so may result from a lack of information on direct measures of the outcome of interest.

Intermediate outcomes. Even when outcomes of interest are directly assessed, it may be useful to have information on intermediate outcomes in order to understand the way in which the outcome of interest is achieved (e.g., decreasing violence by changing firearm storage or carrying behavior).

Population-specific effects. The measurement of the effects of laws (e.g., acquisition restrictions) on violence perpetrated by criminals is important. It is also important to measure or estimate overall population effects of the same laws—for example, whether felony conviction restrictions for firearms purchase affect not only rates of violence among people with felony convictions, but also rates of violence in the general population.

Substitution of weapons. If the goal of a firearms law is the reduction of harm, it is essential to determine whether, given that one weapon may become less available because of the law, that weapon is not readily replaced by another that causes the same (or more or less) harm.

Substitution of place. Similarly, given that many firearms laws are local, it is important to determine whether enacting a law in one location displaces harm from that setting to another (e.g., affecting crime in neighboring jurisdictions that do not have such a law).

4. Measurement of potential confounders and effect modifiers

Measuring and adjusting for confounders. In the analysis of firearms laws, important confounders (e.g., gang activity, drug-related issues, crime cycles, law enforcement practices) are often difficult to measure. Better measures should be developed and used.

Effect modification. It is critical to assess the conditions under which laws may work, may work best, and may not work (e.g., alone or in combination with other laws, or in some settings but not in others). Many laws have multiple provisions, and it is important to determine which combinations of laws or provisions are the most effective.

5. Methods

Appropriate design and analytic techniques. Where possible, the data should be collected as prospective time-series measurements; analyses of trends are preferable to analyses of before-and-after changes. Analytic techniques should include appropriate adjustment for autocorrelation of data in time series and in adjacent geographic locations.

Assumptions and validation. Analytic techniques commonly rest on assumptions about the study design or the characteristics of the study data. Assumptions should be validated, and, to the extent that they are violated, the consequences of violation considered and addressed.

Research Issues Specific to Reviewed Firearms-Related Topics

Several data and research gaps were uncovered in this evaluation that could be potential topics for study.

Bans

Examine effect of grandfathering and registration of grandfathered banned firearms on ban effectiveness.
Examine effects on purchases of firearms to be banned prior to implementation of the ban.
Examine substitution effects.

Restrictions

Examine effects of restriction requirements in the secondary market (gun shows, private sales).
Assess the proportion of firearm-related crimes committed by people in each of the prohibited categories.
Examine the effect of specific restrictions on violence by populations to whom the restrictions apply (e.g., felons, drug abusers, or those adjudicated "mental defective").

Waiting periods

Examine the effect of length of waiting period on violent outcomes.
Examine substitution effects (especially for suicide).
Compare effects of Interim and Permanent Brady laws on firearm-related violence.

Licensing and registration

Assess substitution effects.
Look for specifics in state laws (e.g., fingerprinting or other requirements) as effect modifiers.
Examine effects of licensing and registration in a recent time period, with before-and-after study design and comparison populations.

Shall issue carry laws

Focus specifically on crimes outside the home as outcomes.
Examine permit status for firearms used in crimes.
Examine the effects of differences in state laws on the number of permits issued.
Examine the deterrent effects of publicity about the law.

Child access prevention laws

Assess effects of laws on juvenile firearms users rather than victims.

Examine the effect of laws on storage practices, stratified by the presence of children in the home.

Assess the storage of firearms involved in unintentional injuries, suicide, and crime.

Assess effects of enforcement, punishment, and conviction on storage violation.

Compare effects of the CAP law in Florida (a state with felony sanction for CAP law violation) with effects in other states where violation is a misdemeanor.

Zero firearms tolerance in schools

Assess effects of zero firearms tolerance policies on school violence, firearm-related violence, and the school environment.

Assess school policies and practices for firearms detection, and their relative effectiveness.

Assess cost and benefit of "street" expulsion.

Multiple laws and systems of laws

Assess the effects of combinations of specific laws on specific forms of violence. Studies should allow the determination of which laws are critical to effective combinations and which are not.

Other Effects

The reviews also identified potential research questions related to outcomes in addition to violence. These include:

Property crime. Assess the effects of firearms laws on property crime.

Self-defense. Assess the effects of firearms laws on people's capacity to defend themselves legally. Determine whether all demographic population segments are similarly affected.

Legal rights. Assess the effects of firearms laws on legal rights. For example, expulsion under the Gun-Free Schools Act to keep schools safe may conflict with the rights of students to an education.

Justice. Assess the effects of firearms laws (such as licensing, registration, background checks of applicants) on the apprehension of "wanted persons," such as fugitives from justice.

Cost. Assess the costs and benefits associated with implementing and enforcing firearms laws.

Discussion: Reviewing Firearms Law Effects in the United States

International comparisons indicate that firearm-related violence is considerably higher in the United States than in other developed, industrialized nations.[7] As with other public health problems, efforts have been made to reduce firearm-related violence by means of legal interventions. However, at least based on identified studies of the range of firearms laws reviewed here, the evidence is insufficient to determine whether U.S. firearms laws affect violence. When we conclude that evidence for the effectiveness of a given firearms law on an outcome is insufficient, we do not imply that the law has no effect; rather, we mean that we do not yet know what effect, if any, the law has on that outcome. Other researchers have also noted "the absence of a critical mass of high-quality published studies evaluating the effectiveness of specific gun laws, relative to the magnitude of the problem in the United States."[62]

There are numerous challenges to evaluating the effects of firearms laws on violence in the United States. Information about firearms is collected to regulate, monitor, and investigate firearms transactions, but the collection and use of this information is also limited to protect the privacy of firearms owners. For example, firearms application information used in Brady Law background checks must be destroyed within a given time period. And the Firearms Owners Protection Act of 1986 (P.L. 99-308, 99 Cong., 2d Sess., 100 Stat. 449–461) forbids the federal government from establishing a federal registry of firearms owners. In addition, some of the data sources for violent outcomes (e.g., UCR) that have been most available and most widely used have also been of questionable value because of substantial under-reporting and questionable validity.

However, there are also emerging opportunities to determine whether existing laws are an effective means of reducing violence. The FBI's National Incident-Based Reporting System is designed to replace the UCR and will focus on the detailed circumstances of criminal events. The National Violent Death Reporting System will link multiple sources of information on violent deaths—including death certificates, and medical examiner, police, and crime lab reports—to provide comprehensive information on the circumstances of child abuse deaths, suicides, domestic violence homicides, and other forms of violent death. These reporting systems will greatly enhance the ability to evaluate the effects of firearms laws and other interventions to reduce these forms of violence.

Laws can and have played a prominent role in public health in the United States,[121] and may be one reasonable approach to the problem of firearm-related violence.[14] Further research is needed to understand how laws might affect firearm-related injury and death in the United States.

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Points of view are those of respective affiliated authors and do not necessarily reflect those of the Centers for Disease Control and Prevention; National Institute of Justice, U.S. Department of Justice; or National Institutes of Health.

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Appendix A

Table A1. Studies measuring effect of gun acquisition or possession bans on violence

Author & year Design suitability: design Type of analysis Quality of execution (# of limitations) Specific limitations	Intervention; additional intervention components when used (date) Comparison	Study period Location Unit of analysis Sample size Sample characteristics Follow-up percent and length	Results			
			Reported effect measure	Reported baseline	Reported effect (p value)	Value used in review (p value)
Britt[a] (1996)[1] Greatest: time=series with comparison ARIMA, examines effect of law and timing of effect Fair (3) Description: minimal population description Outcome: ecological measurement[b] Confounding: no control for some important confounders	Intervention: DC law, Firearm Control Regulations Act—ban on handgun purchases, registration of preowned handguns, and safe gun storage regulations (signed 7/23/76; fully in effect since 2/21/77) Control: Baltimore MD (no comparable law), and before-and-after comparison	1968–1987/1989 Washington, DC and Baltimore, MD DC and Baltimore as units of analysis Sample size: two cities Sample characteristics: comparable sociodemographics and crime rates Follow-up percent: NA; regionwide study Follow-up length: 21 yr	Monthly firearm-related and nonfirearm-related homicide counts	None reported	Change in monthly firearm-related homicide counts (1968–1987, no effect, confirmed by additional years of data, 1987–1989) FBI data: Washington 1.5 (NS) Baltimore −2.6 ($p<0.05$) NCHS data (change in natural logarithm rate): Washington −0.002 (NS) Baltimore −3.8 ($p<0.01$)	Relative percent change in homicide rates: not calculable (no baseline provided)
Kleck (1993)[2] Least; cross-sectional Regression Fair (2) Outcome: ecological measurement[b] Confounding: no control for some important confounders	Intervention: ban on handgun possession, ban on handgun sales, ban on Saturday night specials (multiple dates, not specified) Control: cities with no such laws	1980 (1979–1981) USA, cities with population >100,000 Cities with >100,000 residents in 1980 as unit of analysis n=170 Multiple sample characteristics summarized Follow-up percent and length: NA	Natural logarithm of difference in total and firearm-related-specific crime, suicide, and unintentional injury rate between cities that had specified bans and those that did not	None reported	Effects of ban on handgun possession: Homicide total: 0.087 (NS) Assault total: 0.022 (NS) Robbery total: 0.104 (NS) Rape total: −0.092 (NS) Suicide total: −0.062 (NS) Firearm-related unintentional death: 0.009 (NS) Effects of ban on handgun sales:	Relative percent change Ban on handgun possession: Homicide total: 9.1 (NS) Assault total: 2.2 (NS) Robbery total: 11.0 (NS) Rape total: −8.8 (NS) Suicide total: −6.0 (NS) Firearm-related unintentional death: 0.9 (NS)

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Table A1. *(continued)*

Author & year Design suitability: design Type of analysis Quality of execution (# of limitations) Specific limitations	Intervention; additional intervention components when used (date) Comparison	Study period Location Unit of analysis Sample size Sample characteristics Follow-up percent and length	Results			
			Reported effect measure	Reported baseline	Reported effect (p value)	Value used in review (p value)
					Homicide total: 0.001 (NS) Assault total: −0.106 (NS) Robbery total: −0.105 (NS) Rape total: −0.112 (NS) Suicide total: −0.066 (NS) Firearm-related unintentional death: −0.099 (NS) Effects of Saturday night specials ban: Homicide total: 0.083 (NS) Assault total: 0.069 (NS) Robbery total: 0.060 (NS) Rape total: 0.084 (NS) Suicide total: 0.094 (NS) Firearm-related unintentional death: 0.063 (NS)	Ban on handgun sales: Homicide total: 0.1 (NS) Assault total: −10.1 (NS) Robbery total: −9.9 (NS) Rape total: −10.6 (NS) Suicide total: −6.4 (NS) Firearm-related unintentional death: −9.4 (NS) Saturday night specials ban: Homicide total: 8.7 (NS) Assault total: 7.1 (NS) Robbery total: 6.2 (NS) Rape total: 8.8 (NS) Suicide total: 9.9 (NS) Firearm-related unintentional death: 6.5 (NS)

(continued on next page)

Table A1. *(continued)*

Author & year Design suitability: design Type of analysis Quality of execution (# of limitations) Specific limitations	Intervention; additional intervention components when used (date) Comparison	Study period Location Unit of analysis Sample size Sample characteristics Follow-up percent and length	Results			
			Reported effect measure	Reported baseline	Reported effect (p value)	Value used in review (p value)
Loftin (1991)[3] Greatest: time-series with comparison Before-and-after t-test and ARIMA Fair (4) Description: no population description Outcome: ecological measurement[b] Confounding: no control for some important confounders Other biases: change in rates before law adoption, population changes not accounted for	Intervention: DC law, Firearm Control Regulations Act—ban on handgun purchases, registration of preowned handguns, and safe gun storage regulations (signed 7/23/76; fully in effect since 2/21/77) Control: neighboring counties with no such law, and before-and-after comparison	1968–1987 Washington, DC and adjacent comparison counties of MD and VA (combined; DC-MD-VA SMSA) DC and adjacent comparison counties (combined) as unit of analysis Sample size: three regions Sample characteristics not described Follow-up percent: NA; regionwide study Follow-up length: 19 yr	Monthly nomicide and suicide counts: pre-law average levels and change after the law	Firearm-related homicides (deaths/month) DC: 13.0 MD/VA: 5.8 Non-firearm-related homicides DC: 7.3 MD/VA: 3.0 Firearm-related suicides DC: 2.6 MD/VA: 9.2 Non-firearm-related suicides DC: 4.4 MD/VA: 9.9	Change in firearm-related homicides (deaths/month): DC: −3.3 ($p<0.001$) MD/VA: −0.4 (NS) Change in non-firearm-related homicides: DC: −0.3 (NS) MD/VA: 0.7 ($p<0.05$) Change in firearm-related suicides: DC: −0.6 ($p<0.05$) MD/VA: 1.1 ($p<0.05$) Change in non-firearm-related suicides: DC: −0.4 (NS) MD/VA: −0.2 (NS)	Relative percent change (total estimates calculated from firearm-related and non-firearm-related estimates) Firearm-related homicide: −19.9 ($p<0.001$) Total homicide: −20.4 (NS) Firearm-related suicide: −12.6 ($p<0.005$) Total suicide: −18.1 (NS)
McDowall (1996)[4] Greatest: time-series with comparison Before-and-after change t-test Fair (4) Description: minimal population description Outcome: ecological measurement[b] Confounding: no control for some important confounders	Intervention: DC law, Firearm Control Regulations Act—ban on handgun purchases, registration of preowned handguns, and safe gun storage regulations (signed 7/23/76; fully in effect since 2/21/77) Control: Boston and Memphis—similar size cities with no such law, and before-and-after change comparison	1968–1987/1990 Washington, DC and Baltimore, Boston, and Memphis DC and Baltimore, Boston, and Memphis as units of analysis Sample size: four regions Sample characteristics not described Follow-up percent: NA; regionwide study Follow-up length: 19 to 22 yr	Monthly homicide and suicide counts: change in average levels before and after law	None reported	Change in firearm-related homicides (deaths/month): DC: 2.08 (1968–1990) Memphis: 0.74 (1968–1987) Boston: −0.80 (1968–1987) Baltimore: −3.01 (1968–1987) Change in non–firearm-related homicides: DC: 0.61 (1968–1990) Memphis: 0.37 (1968–1987)	Relative percent change not calculable. Baseline rates not provided for comparison cities; data collection periods in this report differ for intervention and comparison cities, but available in earlier study[3]

(continued on next page)

Table A1. *(continued)*

Author & year Design suitability: design Type of analysis Quality of execution (# of limitations) Specific limitations	Intervention; additional intervention components when used (date) Comparison	Study period Location Unit of analysis Sample size Sample characteristics Follow-up percent and length	Results			
			Reported effect measure	Reported baseline	Reported effect (p value)	Value used in review (p value)
Other biases: change in rates before law adoption, population changes not accounted for					Boston: −0.31 (1968–1987) Baltimore: −1.41 (1968–1987) Change in firearm-related suicides: DC: −0.47 (1968–1990) Memphis: 0.65 (1968–1987) Boston: 0.10 (1968–1987) Baltimore: 0.17 (1968–1987) Change in non–firearm-related suicides: DC: −0.33 (1968–1990) Memphis: 0.30 (1968–1987) Boston: −0.26 (1968–1987) Baltimore: −0.62 (1968–1987)	
Roth (1999)[5] Greatest: time series with comparison Regression Fair (4) Description: minimal population description Outcome: ecological measurement[b] Follow-up: short follow-up period Confounding: no control for some important confounders	Intervention: Federal Violent Crime Control and Law Enforcement Act banning manufacture, transfer, and possession of certain semiautomatic firearms and large-capacity ammunition magazines, plus restrictions on firearms dealer licensing and age of gun acquisition (1994)	1980–1995 USA, 42 states State as unit of analysis n=42 Sample characteristics: U.S. states, populations not described Follow-up percent: NA, statewide study Follow-up length: 1 yr	Percentage difference between predicted and observed firearm homicide rates	None reported	States (n=15) that had no similar assault weapons ban before and had prior ban on juvenile handgun possession; New York State excluded, because of enactment of other firearms laws in same period: −6.7 (NS)	Relative percent change in firearm homicide rates, comparing states with and without similar weapons bans prior to federal ban; intervention and comparison states had prior bans on juvenile handgun possession; NY and CA excluded

(continued on next page)

Table A1. *(continued)*

Author & year Design suitability: design Type of analysis Quality of execution (# of limitations) Specific limitations	Intervention; additional intervention components when used (date) Comparison	Study period Location Unit of analysis Sample size Sample characteristics Follow-up percent and length	Results			
			Reported effect measure	Reported baseline	Reported effect (p value)	Value used in review (p value)
	Control: states that had similar laws before 1994					from comparison because of enactment of other firearms laws in same period: −6.7 (NS)
Vernick (1999)[6] Moderate: retrospective design with comparison Pre–post proportions of requests for traces of crime firearms; proportions of banned guns traced to purchase year pre- and post-ban in ban and non-ban cities Fair (4) Description: minimal population description Sampling: convenience sample of 16 cities in YCGII, excluding Washington, DC Outcome: ecological measurement[b] Confounding: no control for some important confounders	Intervention: MD law banning manufacture and sale of Saturday night specials (passed, 1988, effective 1990) Control: 15 YCGII cities without such a law	1985–1996/1997 Location: Baltimore and 15 comparison cities City as unit of analysis, n=16 Population characteristics not provided Follow-up percent: NA Follow-up length: 12 yr retrospective	Relative percent of banned crime gun trace requests (process by which law enforcement identifies source of weapon) among all gun trace requests in other cities compared with Baltimore, after the law, controlling for confounders	Baltimore, before the law: 13.6% Other cities before the law: 17.6%	Ratio of percent of banned crime gun trace requests among all gun trace requests in other cities compared with Baltimore, after the law, controlling for some confounders: 2.3 (p value <0.05)	Relative percent change in proportion of crime guns used between July 1996 and April 1997 that were traced to purchase dates before and after the ban, in Baltimore and comparison cities: −107.6 (p value NA)

(continued on next page)

Table A1. *(continued)*

Author & year Design suitability: design Type of analysis Quality of execution (# of limitations) Specific limitations	Intervention; additional intervention components when used (date) Comparison	Study period Location Unit of analysis Sample size Sample characteristics Follow-up percent and length	Results			
			Reported effect measure	Reported baseline	Reported effect (p value)	Value used in review (p value)
Weil (1997)[7] Moderate: time-series with no comparison Regression Fair (4) Description: population Outcome: ecological measurement[b] Follow-up: short follow-up period Confounding: no control for some important confounders	Intervention: MD law banning sales of assault pistols and high-capacity ammunition magazines (1994) Comparison: no separate control population, before-and-after comparison only	1989–1995 Location: Baltimore, MD Baltimore (data from first 6 months of each year) as unit of analysis Population characteristics not provided Follow-up percent: NA; regionwide study Follow-up length: 6 months	Difference between expected and actual number of assault guns recovered in first 6 months of 1995	None reported	Expected number of assault guns recovered: 52.5 Actual number of assault guns recovered: 24 55% reduction (p=0.018)	Relative percent change: −55.0 (p=0.018)

[a]Publications excluded because they report on the same intervention in the same population were: Jones ED. The District of Columbia's "Firearms Control Regulation Act of 1975": the toughest handgun control law in the United States—or is it? *Ann Am Acad Political Social Sci* 1981;455:138–49; and Nicholson R, Garner A. The Analysis of the Firearms Control Act of 1975: Handgun Control in the District of Columbia. Washington DC: U.S. Conference of Mayors, 1980.

[b]In ecological measurement, exposures and outcomes are measured in the same population, but it cannot be determined whether those in the population who are exposed are also those with the outcome (or whether those in the population who are not exposed are also those without the outcome), and thus, whether exposure and outcome are associated.

ARIMA, autoregressive integrated moving average; DC, Washington DC; FBI, Federal Bureau of Investigation; MD, Maryland; NCHS, National Center for Health Statistics; NA, not applicable or not available; NS, not statistically significant; SMSA, standard metropolitan statistical area; VA, Virginia; yr, year(s); YCGII, Youth Crime Gun Interdiction Initiative.

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EXHIBIT "18"

RAND CORPORATION

OBJECTIVE ANALYSIS.
EFFECTIVE SOLUTIONS.

Effects of Bans on the Sale of Assault Weapons and High-Capacity Magazines on Mass Shootings

March 2, 2018

Summary: Evidence for the effect of assault weapon bans on mass shootings is inconclusive.

We identified two studies that examined the effects of bans on the sale of assault weapons and high-capacity magazines and met our inclusion criteria. Both used a two-way fixed-effects model, controlling for both state-specific and year-specific effects, to estimate the effects of state or federal assault weapon bans on mass shooting incidents or casualties.[1]

Key Findings

Assault weapon bans have **uncertain** effects on mass shootings.

Evidence for this relationship is **inconclusive**.

Studies with comparable methodological strengths identified inconsistent evidence for the policy's effect on an outcome, or a single study found only uncertain or suggestive effects. Read more about how we determined the strength of gun policy analysis research.

Using a Poisson model and data from 1982 through 2011, Gius (2015c) tested whether state assault weapon bans influence public mass shooting fatalities or public mass shooting injuries, controlling for the federal assault weapon ban and state-level variation in demographic, socioeconomic, and criminal justice characteristics. Although the author found a large and statistically significant association between implementation of the *federal* assault weapon ban and reductions in mass shooting deaths and injuries, the analysis of the federal ban does not meet our criteria for inclusion because the model included an indicator for years prior to and after the federal ban as a control, but there was no comparison group. However, findings showed that *state* assault weapon bans had a statistically significant but smaller effect of reducing mass shooting death rates to 55 percent of what would have been expected without the bans, but uncertain effects on mass shooting injuries (see the figure below). This report provided little detail describing variation in the timing of the state bans in relation to the federal ban, and it is unclear whether the estimated effects were confounded by correlation between the state and federal bans. The model did not account for serial correlation in panel data, which can result in large biases in standard errors (Aneja, Donohue, Zhang, 2014).

Using a linear probability model and data from a later period (1989–2014), Luca, Deepak, and Poliquin (2016) estimated the effects of state assault weapon bans on a binary indicator for whether a mass shooting occurred in a given state-year. In contrast to Gius (2015c), Luca, Deepak, and Poliquin (2016) did not control for the federal assault weapon ban from 1994 through 2004, but they controlled for a host of other state-level gun policies and for state-level demographic, socioeconomic, and political characteristics. Their findings showed uncertain effects of state assault weapon bans on the probability of a mass shooting incident occurring. However, the effects of gun policies on mass

The Experts Weigh In

Compare expert opinions on how laws that ban assault weapons and high-capacity magazines may affect mass shootings in your state and the U.S. as a whole. »

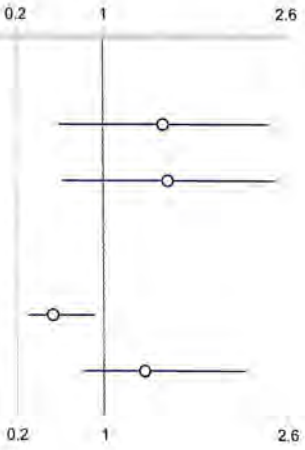
shootings were not the primary focus of Luca, Deepak, and Poliquin (2016), and the authors intended the estimates to serve solely as a robustness check for their main specification (the effects of mass shootings on gun policy). Although the paper provided limited information to use in evaluating the reported statistical models (e.g., on how these policies were coded), it is clear that the analysis used a linear model to predict a dichotomous outcome. Therefore, model assumptions were violated, making confidence intervals (CIs) unreliable.

The figure below displays the incidence rate ratios (IRRs) and CIs associated with the assault weapon ban policies examined in these studies. We exclude estimates of the federal assault weapon ban from Gius (2015c) because they do not meet our criteria for inclusion.

Incidence Rate Ratios Associated with the Effect of Assault Weapon Bans on Mass Shootings

HOW TO READ THIS CHART

Study, by Policy	Outcome Measure	Effect Size (IRR) [95% CI]
Assault weapon ban	**Mass shooting**	
Luca, Deepak, & Poliquin (2016)	State-year indicator (no political controls)	1.52 [0.60, 2.43]
Luca, Deepak, & Poliquin (2016)	State-year indicator (political controls)	1.56 [0.63, 2.49]
State assault weapon ban	**Mass shooting**	
Gius (2015c)	Deaths	0.55 [0.33, 0.92]
Gius (2015c)	Injuries	1.35 [0.81, 2.23]



NOTE: IRR values marked with empty circles indicate that we identified concerns with the study's methodology, and these concerns are described in the text above. Filled circles indicate that we identified no significant methodological concerns.

Conclusions

We identified two qualifying studies that estimated the effects of state assault weapon bans on different aspects of mass shootings. Gius (2015c) found that these bans significantly reduce mass shooting deaths but have uncertain effects on injuries resulting from mass shootings. Using a similar data set, Luca, Deepak, and Poliquin (2016) found uncertain effects of state assault weapon bans on the annual incidence of mass shootings. Based on an assessment of these findings and the relative strengths of these studies, we find *inconclusive evidence for the effect of assault weapon bans on mass shootings.*

BANS ON THE SALE OF ASSAULT WEAPONS AND HIGH-CAPACITY MAGAZINES MASS SHOOTINGS

Notes

1 The two studies adopted slightly different definitions for *mass shooting* (see the essay on mass shooting definitions and trends for further detail). Gius (2015c) focused on *public mass shootings*, which the author defined as incidents resulting in four or more firearm-related fatalities (excluding the offender), where the shooting occurred in a relatively public place, victims were selected indiscriminately, and the shooting was not related to criminal activity. Luca, Deepak, and Poliquin (2016) set the same casualty threshold and also excluded any incident that occurred in connection with criminal activity, but they did not restrict to public settings and excluded all events in which fewer than three of the fatally injured victims were not related to the shooter (e.g., family, romantic partner). ⤴

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