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9	
10	UNITED STATES DISTRICT OF CALIFORNIA
11	SOUTHERN DISTRICT OF CALIFORNIA
12	MATTHEW JONES, et al., ) Case No.: 3:19-cv-01226-L-AHG
13	Plaintiffs,
14 15	v. ) CENTER TO PREVENT GUN VIOLENCE FOR LEAVE TO FILE
15 16 17	XAVIER BECERRA, in his official capacity as Attorney General of the State of California et al
17 18	Defendants Defendants Judges: Honorable M. James Lorenz
19	) and Magistrate Judge Allison H. ) Goddard
20	) No hearing set for this motion pursuant
21	) to Dkt. 23.
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Giffords Law Center to Prevent Gun Violence ("Giffords Law
Center") respectfully moves for leave to file the attached *amicus curiae* brief in
support of Defendants' Opposition to Plaintiffs' Motion for Preliminary Injunction
(ECF No. 25). Defendants consent to this Motion; Plaintiffs have informed
counsel for Giffords Law Center that they object.

7 Giffords Law Center is a non-profit policy organization dedicated to 8 researching, writing, enacting, and defending laws and programs proven to 9 effectively reduce gun violence. The organization was founded more than a 10 quarter-century ago following a gun massacre at a San Francisco law firm and was 11 renamed Giffords Law Center in October 2017 after joining forces with the 12 gun-safety organization founded by former Congresswoman Gabrielle Giffords. 13 Today, Giffords Law Center provides free assistance and expertise to lawmakers, 14 advocates, legal professionals, law enforcement officials, and citizens who seek to 15 improve the safety of their communities. Giffords Law Center has provided 16 informed analysis as an *amicus* in many firearm-related cases, including in 17 Hirschfeld v. Bureau of Alcohol, Tobacco, Firearms & Explosives, 2019 WL 18 4923955 (W.D. Va. Oct. 4, 2019), District of Columbia v. Heller, 554 U.S. 570 19 (2008), McDonald v. City of Chicago, 561 U.S. 742 (2010), Fyock v. City of 20 Sunnyvale, 779 F.3d 991 (9th Cir. 2015), and Teixeira v. Ctv. of Alameda, 873 F.3d 670 (9th Cir. 2017) (en banc).<sup>1</sup> 21

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<sup>&</sup>lt;sup>1</sup> Several courts have cited research and information from Giffords Law Center's *amicus* briefs in Second Amendment rulings. *E.g., Hirschfeld*, 2019 WL 4923955, at \*5, \*9; *Ass 'n of N.J. Rifle & Pistol Clubs* v. *AG N.J.*, 910 F.3d 106, 121-22 (3d Cir. 2018); *Md. Shall Issue* v. *Hogan*, 353 F. Supp. 3d 400, 403-05 (D. Md. 2018); *Stimmel* v. *Sessions*, 879 F.3d 198, 204, 208, 210 (6th Cir. 2018); *Peruta* v. *Cty. of San Diego*, 824 F.3d 919, 943 (9th Cir. 2016) (*en banc*) (Graber, J., concurring).
Giffords Law Center filed the latter two briefs under its former name, the Law Center to Prevent Gun Violence.

1 Giffords Law Center has significant expertise with laws restricting 2 firearm purchase by minors under the age of 21, and represents the interests of 3 citizens who benefit from those laws.<sup>2</sup> Giffords Law Center's proposed *amicus* brief, attached to this Motion, identifies compelling data and social science 4 5 demonstrating that 18-to-20-year-olds commit a disproportionate share of violent crimes and are at a higher risk of suicide, including because their brains are still 6 7 developing and they are more impulsive and emotionally volatile than older adults. 8 This research confirms that there is a reasonable fit between California's 9 restrictions on 18-to-20-year-olds' purchase and possession of firearms and the 10 state's important interest in public safety.

- 11 A "district court has broad discretion to appoint *amici curiae*."
- 12 Hoptowit v. Ray, 682 F.2d 1237, 1260 (9th Cir. 1982), abrogated on other
- 13 grounds, Sandin v. Conner, 515 U.S. 472 (1995). The "classic role" of amici
- 14 *curiae* is "assisting in a case of general public interest, supplementing the efforts of
- 15 counsel, and drawing the court's attention to law that escaped consideration."
- 16 Miller-Wohl Co., Inc. v. Comm'r of Labor and Indus., 694 F.2d 203, 204 (9th
- 17 Cir. 1982). "District courts frequently welcome amicus briefs from nonparties
- 18 concerning legal issues that have potential ramifications beyond the parties directly
- 19 involved or if the amicus has 'unique information or perspective that can help the
- 20 court beyond the help that the lawyers for the parties are able to provide." *Safari*
- 21 Club Int'l v. Harris, 2015 WL 1255491, at \*1 (E.D. Cal. Jan. 14, 2015) (citing
- 22 *NGV Gaming, Ltd.* v. Upstream Point Molate, LLC, 355 F.Supp.2d 1061, 1067
- 23 (N.D. Cal. 2005)). "Even when a party is very well represented, an amicus may
- 24 provide important assistance to the court." Jamul Action Comm. v. Stevens, 2014
- 25 WL 3853148, at \*6 (E.D. Cal. Aug. 5, 2014) (quotation omitted). More
- 26

 <sup>&</sup>lt;sup>27</sup> See, e.g., Minimum Age to Purchase & Possess, GIFFORDS LAW CENTER, https://lawcenter.giffords.org/gun-laws/policy-areas/who-can-have-agun/minimum-age/ (last visited Jan. 3, 2020).

1 specifically, district and appellate courts in the Ninth Circuit have found *amicus* 2 briefs appropriate in cases raising Second Amendment questions, including *amicus* 3 briefs submitted by Giffords Law Center and other gun-violence-prevention groups. See, e.g., Peruta v. Cty. of San Diego, 824 F.3d 919, 943 (9th Cir. 2016) 4 (Graber, J., concurring) (citing Giffords Law Center brief (under former name Law 5 Center to Prevent Gun Violence) in upholding California's restrictions on 6 7 concealed firearms); Fyock v. City of Sunnyvale, Case No. 14-15408, Dkt. 83 (9th 8 Cir. Oct. 21, 2014) (granting motion of Brady Center to Prevent Gun Violence and the Major Chiefs Association to file amici curiae brief); Rupp v. Becerra, 401 F. 9 Supp. 3d 978, 991 (C.D. Cal. 2019) (relying on statistics from "Everytown for Gun 10 11 Safety's amicus brief''). 12 Giffords Law Center's participation as an *amicus curiae* is desirable 13 because the organization has substantial expertise with the minimum-age 14 restriction at issue in this case and the relevant data and social science demonstrating its effectiveness. The important constitutional principles at stake 15 16 here—including California's ability to protect public safety through calibrated 17 firearm regulations-are fundamental to Giffords Law Center's mission and 18 critically important to its supporters. Its proposed *amicus* brief can help the Court 19 evaluate the parties' claims by presenting data and social science that support California's restrictions on 18-to-20-year-olds' ability to purchase and possess 20 21 firearms. 22 23 24 25 26 27 28 -3-

OPPOSED MOTION BY GIFFORDS LAW CENTER TO PREVENT GUN VIOLENCE FOR LEAVE TO FILE AMICUS CURIAE BRIEF (19-cv-01226-L-AHG)

1	* * *
2	For the foregoing reasons, Giffords Law Center respectfully requests
3	that the Court grant its motion for leave to file an <i>amicus</i> brief.
4	Dated: January 3, 2020
5	<u>/s/ Robert A. Sacks</u> Robert A. Sacks (Bar No. 150146)
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### **CERTIFICATE OF SERVICE**

1	CERTIFICATE OF SERVICE
2	I certify that on January 3, 2020, I filed the foregoing document with
3	the Clerk of the Court for the United States District Court, Southern District of
4	California, by using the Court's CM/ECF system, which will send notification of
5	electronic filing (NEF) to all counsel of record.
6	
7	/s/ Robert A. Sacks Robert A. Sacks
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6	Giffords Law Center to Prevent Gun V	iolence
7	[ADDITIONAL COUNSEL ON SIGN	IATURE PAGE]
8		
9	UNITED STATE	ES DISTRICT COURT
10	SOUTHERN DIST	RICT OF CALIFORNIA
11		
12	MATTHEW JONES, et al.,	) Case No.: $3:19$ -cv- $01226$ -L-AHG
13	v	) GIFFORDS LAW CENTER TO ) PREVENT GUN VIOLENCE IN
14	XAVIER BECERRA, in his	) SUPPORT OF DEFENDANTS' OPPOSITION TO PLAINTIFFS'
15 16	official capacity as Attorney General of the State of	) MOTION FOR PRELIMINARY ) INJUNCTION
17	California, et al.,	) Judges: Honorable M. James Lorenz
18	Defendants.	) and Magistrate Judge Allison H. ) Goddard
19		No hearing set for this motion pursuant to Dkt. 23.
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### **CORPORATE DISCLOSURE STATEMENT**

Giffords Law Center to Prevent Gun Violence has no parent corporations. It has no stock and hence no publicly held company owns 10% or more of its stock. -i-BRIEF OF AMICUS CURIAE GIFFORDS LAW CENTER TO PREVENT GUN VIOLENCE IN SUPPORT OF

1	TABLE OF CONTENTS			
2	INTRODUCTION			
2	ARGUMENT4		4	
5	I. At Most, Section 27510 Is Subject to Intermediate Scrutiny5		5	
4	II. Section 27510 Easily Satisfies Intermediate Scrutiny7		7	
2		A.	Legislative History Demonstrates that Section 27510 Is a	
6 7			Commonsense and Targeted Response to a Grave Public Safety Risk	8
, 8		B.	Scientific Research Confirms that the Legislature's	
8 9			Concerns Were Well Founded and Its Solution Is Data- Driven.	0
10			1. Eighteen-to-Twenty-Year-Olds Are	
11			Disproportionately Likely to Commit Violent Crimes, Including Homicide, by Firearm1	1
12			2. Eighteen-to-Twenty-Year-Olds Attempt Suicide at	
13			Disproportionately High Rates and Access to Firearms Increases the Likelihood and Lethality of	
14			Those Suicide Attempts	4
15 16			3. State-Level Gun Control Measures, Including Age Restrictions, Are Effective	6
17	III.	Plain Decla	ntiffs Rely on Dubious Evidence from a Widely Discredited	8
18		A.	Mr. Lott's Research Methodologies Have Been Widely	0
19			Criticized	9
20		B.	Mr. Lott Has a History of Failing to Provide Data and Evidence to Back up His Claims	1
21		C.	Mr. Lott's Work Is Rarely Published in Peer-Reviewed	
22			Journals	2
23		D.	Mr. Lott Has Committed Several Ethical Violations to	-
24			Defend His Work from Criticism	2
25				
26				
27				
28				
			-ii-	

### **TABLE OF AUTHORITIES**

1	TABLE OF AUTHORITIES
2	Page(s)
3	Cases
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### Case 3:19-cv-01226-L-AHG Document 26-1 Filed 01/03/20 PageID.5030 Page 8 of 34

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22	
24	
25	
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### **INTEREST OF AMICUS CURIAE**

3 Amicus curiae Giffords Law Center to Prevent Gun Violence ("Giffords Law Center") is a non-profit policy organization dedicated to researching, 4 5 writing, enacting, and defending laws and programs proven to effectively reduce gun violence. The organization was founded more than a quarter-century ago following 6 7 a gun massacre at a San Francisco law firm and was renamed Giffords Law Center in October 2017 after joining forces with the gun-safety organization founded by 8 9 former Congresswoman Gabrielle Giffords. Today, Giffords Law Center provides 10 free assistance and expertise to lawmakers, advocates, legal professionals, law enforcement officials, and citizens who seek to improve the safety of their 11 12 communities. Giffords Law Center has provided informed analysis as an *amicus* in 13 many firearm-related cases, including in *Hirschfeld* v. *Bureau of Alcohol, Tobacco,* 14 Firearms & Explosives, 2019 WL 4923955 (W.D. Va. Oct. 4, 2019), District of Columbia v. Heller, 554 U.S. 570 (2008), McDonald v. City of Chicago, 561 U.S. 15 742 (2010), Fyock v. City of Sunnyvale, 779 F.3d 991 (9th Cir. 2015), and Teixeira 16 v. Ctv. of Alameda, 873 F.3d 670 (9th Cir. 2017) (en banc).<sup>1</sup> 17 18 19 20 21 22 23 24 Several courts have cited research and information from Giffords Law Center's amicus briefs in Second Amendment rulings. E.g., Hirschfeld, 2019 WL 25 4923955, at \*5, \*9; Ass 'n of N.J. Rifle & Pistol Clubs v. AG N.J., 910 F.3d 106, 121-22 (3d Cir. 2018); Md. Shall Issue v. Hogan, 353 F. Supp. 3d 400, 403-05 (D. Md. 26 2018); Stimmel v. Sessions, 879 F.3d 198, 204, 208, 210 (6th Cir. 2018); Peruta v. Cty. of San Diego, 824 F.3d 919, 943 (9th Cir. 2016) (en banc) (Graber, J., 27 concurring). Giffords Law Center filed the latter two briefs under its former name,

<sup>28</sup> the Law Center to Prevent Gun Violence.

**INTRODUCTION<sup>2</sup>** 

The data is clear that young people aged 18-to-20 disproportionately 2 3 use firearms to commit crime and attempt suicide. Cognitive science explains these troubling statistics: minors are more impulsive and volatile than adults because their 4 brains are still developing. This impulsivity makes them more likely to use guns 5 irresponsibly. Accordingly, restricting 18-to-20-year-olds' access to firearms 6 represents a calibrated, data-driven solution to reduce gun violence. 7

California lawmakers recognized this when they enacted California 8 Penal Code § 27510 in 2011. Cal. Penal Code § 27510 (2011). The 2011 law 9 restricted 18-to-20-year-olds from purchasing handguns, but the law did not extend 10to long guns, including assault rifles. Then, "[o]n February 14, 2018 Nikolas Cruz 11 shot and killed seventeen people and wounded an additional seventeen people at 12 13 Marjory Stoneman Douglas High School in Parkland, Florida. The perpetrator was 19-years old at the time of the incident, and he used assault rifles." SB 1100 Senate 14 Floor Analysis (May 26, 2018), at 5. 15

The Parkland massacre prompted the California Legislature to 16 reexamine the distinction between long guns and hand guns. The Legislature 17 considered the fact that, "of the 26,682 guns used in crimes that were entered into 18 the California Department of Justice Automated Firearms Systems database, 11,500 19 were long guns." Assembly Standing Committee on Public Safety Hearing of 06-19-202018, 2017-2018 Sess. (Cal. 2018) (statement of Sen. Anthony Portantino). The 21 Legislature also considered the outsized role of long guns in youth suicides, 22 emphasizing that "[d]ata shows about 39% of all suicides by people under 21 are 23 committed with a gun, and more often than not a long gun is used rather than a 24 25

1

<sup>2</sup> No counsel for a party authored this brief in whole or in part. No person other 27 than *amicus* or its counsel contributed money to fund this brief's preparation or 28 submission.

1 handgun." Assembly Floor Hearing of 08-28-2018, 2017-2018 Sess. (Cal. 2018)

2 (statement of Assemb. Rob Bonta).

3 Faced with these stark statistics, and the grave responsibility to protect its citizens, the California Legislature determined that the distinction between long 4 5 guns and handguns was unsupportable. In 2019, the Legislature amended California Penal Code § 27510. SB 1100, enacted in 2018 and effective as of January 1, 2019, 6 7 prohibits federally licensed firearms dealers ("FFLs") in California from transferring 8 long guns to individuals under 21 years of age. This restriction does not apply to 9 those who "possess[] a valid, unexpired hunting license"; are employed as peace 10 officers, federal officers or law enforcement agents; or are members of the military. 11 Cal. Penal Code § 27510(b). SB 61, enacted in 2019 and effective as of January 1, 2020, narrowed these exceptions by prohibiting FFLs from transferring semi-12 13 automatic centerfire rifles (a subset of long guns that does not include, for example, 14 shotguns) to any person under the age of 21. Law enforcement agents and active and reserve members of the Armed Forces are exempted from SB 61, but the 15 16 exemption does not extend to those with a hunting license or retired members of the Armed Forces. SB 61, 2019-2020 Sess. (Cal. 2019). This means that under the new 17 18 measure, hunting license-holders may still purchase shotguns from FFLs, but may 19 not purchase semi-automatic centerfire rifles from them. Neither SB 1100 nor SB 20 61 prevents transfers of firearms to 18-to-20-year-olds by immediate family.

21 Plaintiffs now challenge amended California Penal Code § 27510 22 ("Section 27510"). They acknowledge (as they must) that California's interest in 23 reducing gun violence is "important," and that 18-to-20-year-olds commit gun violence "at a higher rate comparatively." (ECF No. 21-1 ("Pls.' Mem.") at 18, 23.) 24 25 Nevertheless, they contend that Section 27510 "will have no effect on homicides, suicides, or mass shootings." (Id. at 29.) This is simply wrong. 26 Studies the 27 California legislature could legitimately rely on repeatedly find a robust connection 28 between the enactment of age-based restrictions such as Section 27510 and a decline

in firearm-related adolescent deaths. This is unsurprising, given the outsized role of
 18-to-20-year-olds in violent crimes and suicides.

For these reasons, and those explained in Defendants' Opposition to
Plaintiffs' Motion for Preliminary Injunction (ECF No. 25 ("Defs.' Opp. to Pls.'
Mot.")), Section 27510 comports with the Second Amendment. Plaintiffs failed to
establish that they are likely to succeed on the merits of their claim and their motion
should be denied.

8

### **ARGUMENT**

Courts in the Ninth Circuit must apply a "two-step inquiry to analyze
claims that a law violates the Second Amendment." *United States* v. *Torres*, 911
F.3d 1253, 1258 (9th Cir. 2019). This test "(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." *Id.* Section 27510 easily passes this test.

"[T]he first step of [the] analysis requires [the court] to explore the
amendment's reach based on a historical understanding of the scope of the [Second
Amendment] right" because a court "cannot apply the Second Amendment to protect
a right that does not exist under the Amendment." *Id.* (internal quotations omitted).
If the challenged law does not burden conduct historically protected by the Second
Amendment, then the law is valid without further review. *Teixeira* v. *Cty. of Alameda*, 873 F.3d 670, 682 (9th Cir. 2017) (*en banc*).

"If a prohibition falls within the historical scope of the Second 21 Amendment, [the court] must then proceed to the second step of the Second 22 Amendment inquiry to determine the appropriate level of scrutiny." Jackson v. City 23 & Cty. of San Francisco, 746 F.3d 953, 960 (9th Cir. 2014). The level of scrutiny 24 "depend[s] on (1) how close the law comes to the core of the Second Amendment 25 right, and (2) the severity of the law's burden on the right." United States v. Chovan, 26 735 F.3d 1127, 1138 (9th Cir. 2013) (internal quotations omitted). The core of the 27 Second Amendment is "the right of law-abiding, responsible citizens to use arms in 28

defense of hearth and home." District of Columbia v. Heller, 554 U.S. 570, 635 1 2 (2008) (emphasis added). Only a law that "implicates the core of the Second 3 Amendment right and severely burdens that right warrants strict scrutiny. Otherwise, intermediate scrutiny is appropriate." Silvester v. Harris, 843 F.3d 816, 4 821 (9th Cir. 2016) (citation omitted). "To uphold a regulation under intermediate 5 6 scrutiny, [the Ninth Circuit has] identified two requirements: (1) the government's 7 stated objective must be significant, substantial, or important; and (2) there must be 8 a 'reasonable fit' between the challenged regulation and the asserted objective." Id. 9 at 821-22.

10 A law may be upheld as constitutional at either step of the two-step 11 inquiry. Here, Section 27510 survives at both steps. *First*, as the Defendants 12 explained, history and tradition show that state and federal governments have 13 regulated 18-to-20-year-olds' access to firearms since the founding of this nation. 14 (See Defs.' Opp. to Pls.' Mot. at 7-9.) Section 27510 is therefore constitutional at the threshold inquiry. Second, as explained below, even if this Court proceeds to 15 16 step two, Section 27510 survives because (1) at most, intermediate scrutiny applies, 17 as the law does not substantially burden the core of the Second Amendment; and 18 (2) legislative history and scientific data demonstrate that Section 27510 easily 19 survives intermediate scrutiny: California's interests in public health and safety are significant, substantial and important, and there is a reasonable fit between Section 20 21 27510's restrictions and California's public safety objectives.

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# I. AT MOST, SECTION 27510 IS SUBJECT TO INTERMEDIATE SCRUTINY.

The second step of the Ninth Circuit's Second Amendment inquiry "directs courts to apply an appropriate level of scrutiny." *Chovan*, 735 F.3d at 1136. "[I]ntermediate scrutiny is appropriate 'if a challenged law does not implicate a core Second Amendment right, or does not place a substantial burden on the Second Amendment right." *Torres*, 911 F.3d at 1262 (quoting *Jackson*, 746 F.3d at 961).
 There is "near unanimity in the post-*Heller* case law that, when considering
 regulations that fall within the scope of the Second Amendment, intermediate
 scrutiny is appropriate." *Id.* (quoting *Silvester*, 843 F.3d at 823).

5 This case is no exception: Section 27510 is a common-sense measure that does not substantially burden Second Amendment rights. *First*, it applies only 6 7 to a class of people—minors—who fall outside the core of the Second Amendment's 8 protections. Heller defines the core Second Amendment right as "the right of law-9 abiding, *responsible* citizens to use arms in defense of hearth and home." *Heller*, 10 554 U.S. at 635 (emphasis added). Section 27510 does not apply to this core group, 11 but to minors, a group recognized throughout history to require close supervision to 12 access firearms responsibly. Furthermore, the measure is limited in time, as those 13 covered by the age restriction will age out in a limited number of years and be able 14 to fully exercise the right to bear arms once they have developed the maturity to responsibly do so. Cf. Torres, 911 F.3d at 1263 (applying intermediate scrutiny and 15 finding the challenged law's burden was "tempered, because there is nothing 16 17 indicating that the prohibition on firearm possession extends beyond the time that an 18 alien's presence in the United States is unlawful"); Chovan, 735 F.3d at 1138 (applying intermediate scrutiny even when law imposes a "lifetime ban" on all 19 20 firearm possession by individuals with domestic violence convictions because it 21 "exempts those with expunged, pardoned, or set-aside convictions, or those who 22 have had their civil rights restored").

Second, Section 27510 is a commercial regulation on sales and
transfers. It is not a ban on possession. Several avenues remain open for 18-to-20year-olds to possess and use handguns and long guns, including transfers from
immediate family members and loans for target shooting. (See Defs.' Opp. to Pls.'
Mot. at 3-4 (citing Cal. Penal Code §§ 16720, 16960(g), 26545, 27875, 27880,
27885, 27910).) Section 27510 also provides numerous exemptions, including

1 permitting individuals under the age of 21 to purchase long guns (other than semi-2 automatic centerfire rifles) if they have a valid hunting license. The Legislature 3 found this exception appropriate because California residents who wish to obtain a hunting license must complete safety and other instruction to help ensure they use 4 5 firearms responsibly. In particular, license applicants must complete the California 6 Hunter Education Certification requirements, including an online safety course, a 7 four-hour lesson with a certified hunter education instructor, a student demonstration of safe firearm handling, and a test.<sup>3</sup> And though Plaintiffs complain that a hunting 8 license is not "easy to obtain" (Pls.' Mem. at 8 n.5), courts have upheld much more 9 10 burdensome firearm eligibility requirements under intermediate scrutiny, such as the 11 requirement that one get a criminal record expunged. Chovan, 735 F.3d at 1138 12 ("[W]hile we recognize that [the challenged law] substantially burdens Second 13 Amendment rights, the burden is lightened by these exceptions.").<sup>4</sup>

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# II. SECTION 27510 EASILY SATISFIES INTERMEDIATE SCRUTINY.

Intermediate scrutiny requires "(1) the government's stated objective to
be significant, substantial, or important; and (2) a reasonable fit between the
challenged regulation and the asserted objective." *Chovan*, 735 F.3d at 1139. In
evaluating the constitutionality of a firearm restriction, courts should not "substitute
[their] own policy judgment for that of the legislature." *Pena* v. *Lindley*, 898 F.3d
969, 979 (9th Cir. 2018). Rather, they "must accord substantial deference to the
predictive judgments of" the legislature and allow it "a reasonable opportunity to

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<sup>3</sup> See SB 1100 Senate Floor Analysis (May 26, 2018), at 6-7.

<sup>Even if this Court applies strict scrutiny (it should not), Section 27510 should
be upheld because, for the reasons discussed further below, the Legislature narrowly
tailored the law to a compelling governmental interest in safety and crime reduction.</sup> *See Chovan*, 735 F.3d at 1150 (Bea, J., concurring) ("[T]he government's interest in
public safety and preventing gun violence is sufficiently compelling and narrowly
tailored to satisfy th[e] prongs of strict scrutiny analysis.").

1 experiment with solutions to [this] admittedly serious problem[]." Id. at 979-80 (quoting Turner Broad. Sys., Inc. v. FCC, 520 U.S. 180, 195 (1997) and City of 2 3 Renton v. Plavtime Theatres, Inc., 475 U.S. 41, 52 (1986)). Courts may look to legislative history as well as scientific or other studies to determine whether 4 5 intermediate scrutiny is satisfied. Id. at 979 (citing Fyock v. Sunnyvale, 779 F.3d 991, 1000 (9th Cir. 2015) (considering legislative history "as well as studies in the 6 record or cited in pertinent case law")). Here, both the legislative history and studies 7 8 on adolescent gun violence demonstrate that Section 27510 is a constitutional and 9 prudent measure to address gun violence.

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A. Legislative History Demonstrates that Section 27510 Is a Commonsense and Targeted Response to a Grave Public Safety Risk.

13The California Legislature amended Section 27510 to address a serious14public safety concern.<sup>5</sup> California's interest in protecting citizens is undoubtedly15"substantial"; indeed, the Ninth Circuit found it "self-evident" that the government's16"interests in promoting public safety and reducing violent crime are substantial and17important government interests." *Fyock*, 779 F.3d at 1000 (affirming denial of18preliminary injunction motion).

Here, the Legislature "recognize[d] the fact that young adults ages 18
to 20 are statistically far more likely to commit homicide than older adults."<sup>6</sup> The
Legislature also considered these troubling facts: "In 2015, 23.4 percent of those
arrested for murder and non-negligent manslaughter in the U.S. were under 21 and
26.5 percent of those arrested for 'weapons carrying, possession, etc.' were under

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- SB 1100 Assembly Floor Analysis (Aug. 23, 2018), at 3, available at https://leginfo.legislature.ca.gov/faces/billAnalysisClient.xhtml?bill\_id=20172018 0SB1100.
- <sup>6</sup> Assembly Floor Hearing of 08-28-2018, 2017-2018 Sess. (Cal. 2018) (statement of Assemb. Rob Bonta).

age 21."<sup>7</sup> Further, while "[i]ndividuals age 18 to 20 compromise [sic] only 4% of
the population[,]" they "commit 17% of gun homicides."<sup>8</sup> Ultimately, the
Legislature agreed with the point that "[m]aturity, impulsive or reckless behavior,
and responsibility vary greatly among 18-20 year olds," which is why this age group
is subject to numerous restrictions on otherwise lawful activity, including "buy[ing]
alcohol, rent[ing] a car, or purchas[ing] a handgun" and that "the same age restriction
should apply to long guns."<sup>9</sup>

8 Prior California law set different age requirements for handguns and
9 long guns (at 21 and 18, respectively), but statistical evidence undermined that
10 distinction:

11	• "Over the years, long guns have changed Today's semi-
12	automatic guns are more powerful and more lethal. While
13	handguns are used in the majority of gun deaths, long guns have
14	been used to perpetrate many of the largest mass shootings in
15	U.S. history, including the tragic event that took place in San
16	Bernardino, California." <sup>10</sup>

- "Of the 26,682 guns used in crimes that were entered into the
   California Department of Justice Automated Firearms Systems
   database, 11,500 were long guns."<sup>11</sup>
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Assembly Standing Committee on Public Safety Hearing of 06-19-2018, 2017 2018 Sess. (Cal. 2018) (statement of Sen. Anthony Portantino); see also SB 1100
 Senate Committee on Public Safety Bill Analysis (Apr. 16, 2018), at 6-7, available

 <sup>&</sup>lt;sup>7</sup> SB 1100 Senate Floor Analysis (Aug. 28, 2018), at 5, available at https://leginfo.legislature.ca.gov/faces/billAnalysisClient.xhtml?bill\_id=20172018
 <sup>22</sup> 0SB1100.

<sup>23 &</sup>lt;sup>8</sup> *Id.* 

 <sup>&</sup>lt;sup>9</sup> Id.; see also SB 1100 Assembly Committee on Public Safety Bill Analysis
 (June 18, 2018), at 2-3, available at https://leginfo.legislature.ca.gov/faces/
 billAnalysisClient.xhtml?bill\_id=201720180SB1100.

<sup>26</sup> Assembly Floor Hearing of 08-28-2018, 2017-2018 Sess. (Cal. 2018) (statement of Assemb. Rob Bonta).

1	• "[T]he two most deadly recent school tragedies have been
2	perpetrated by people under 21 with long guns." <sup>12</sup>
3	• "[A]bout 38% of all suicides by people under 21 are committed
4	with a gun, and more often than not a long gun is used rather than
5	a handgun." <sup>13</sup>
6	The California Legislature responded to these specific and significant concerns by
7	enacting SB 1100 and SB 61, which provide similar restrictions on 18-to-20-year-
8	olds' access to long guns as already existed for handguns. As SB 61's author stated,
9	"[m]ore and more shootings are occurring with long guns so it is important that we
10	treat the laws of both handguns and long guns the same." <sup>14</sup>
11	<b>B.</b> Scientific Research Confirms that the Legislature's
12	Concerns Were Well Founded and Its Solution Is Data-
13	Driven.
14	Empirical research overwhelmingly supports the Legislature's
15	judgment: 18-to-20-year-olds are disproportionately likely to commit violent crimes
16	and attempt suicide. Firearms make violent crimes and suicide attempts far deadlier.
17	Research also confirms that age-based firearm restrictions reduce firearm-related
18	injuries and deaths.
19	This is unsurprising. Scientific literature is clear that 18-to-20-year-
20	olds are prone to take risks and deprioritize long-term outcomes. See Nat'l Rifle
21	Assoc. of Am., Inc. v. Bureau of Alcohol, Tobacco, Firearms, and Explosives, 700
22	
23	_id=201720180SB1100.
24 25	<sup>12</sup> Senate Floor Hearing of 08-29-2018, 2017-2018 Sess. (Cal. 2018) (statement of Sen. Anthony Portantino).
26	<sup>13</sup> Assembly Floor Hearing of 08-28-2018, 2017-2018 Sess. (Cal. 2018) (statement of Assemb. Rob Bonta).
27	<sup>14</sup> SB 61 Assembly Committee on Public Safety Bill Analysis (June 24, 2019), at 3, <i>available at</i> https://leginfo.legislature.ca.gov/faces/billAnalysisClient.xhtml
28	?bill_id=201920200SB6110-

F.3d 185, 210 n.21 (5th Cir. 2012) ("[M]odern scientific research supports the 1 2 commonsense notion that 18-to-20-year-olds tend to be more impulsive than young 3 adults aged 21 and over."). Because "[t]he brain's frontal lobes are still structurally immature well into late adolescence, and the prefrontal cortex is one of the last brain 4 regions to mature," key brain functions such as "response inhibition, emotional 5 regulation, planning and organization . . . continue to develop between adolescence 6 and young adulthood." Id. (quoting submission from the American Medical 7 Association). 8

9 These qualities—impulsiveness and emotional volatility—make easy
10 gun access a disproportionate public health risk. "Adolescents commit more crimes
11 per capita than children or adults in the USA and in nearly all industrialized cultures.
12 Their proclivity toward . . . risk taking has been suggested to underlie the inflection
13 in criminal activity observed during this time."<sup>15</sup> The result is a pressing public
14 safety problem, for which Section 27510 was a targeted and reasonable solution.

 15
 1. Eighteen-to-Twenty-Year-Olds Are Disproportionately Likely to Commit Violent Crimes, Including Homicide, by Firearm.

18 Eighteen-to-twenty-year-olds account for a disproportionate share of19 violent crimes and homicides in California.

In California, 18-to-20-year-olds make up 4% of the state population. From 2014 to 2018, there were 6,657 homicide offenders for whom the age of the offender was known.<sup>16</sup> 860 of these homicide offenders were between 18 and 20, amounting to approximately 13%. This means that, where the offender's age 25

- <sup>15</sup> Sacks Decl., Ex. 1 at 1, 2, Michael Dreyfuss et al., *Teens Impulsively React Rather than Retreat from Threat*, 36 DEVELOPMENTAL NEUROSCIENCE 220, 220 (2014).
- 28 16 This count excludes negligent homicides.

1	is known, 4% of California's population is responsible for	
2	13% of homicides. <sup>17</sup>	
3	• Firearms are the most common method for committing homicide	
4	in the nation and in California. Eighteen-to-twenty-year-old	
5	homicide offenders in California are also more likely to use	
6	firearms than any other means. Roughly 70% of California	
7	homicide offenders in this age range used firearms to kill. <sup>18</sup>	
8	These statistics from California are consistent with nationwide research	
9	demonstrating the specific and significant danger posed by 18-to-20-year-olds with	
10	firearms:	
11	• Arrests for homicide, rape, and robbery peak from ages 18 to	
12	20.19	
13	• Though 18-to-20-year-olds make up under 5% of the population,	
14	they account for over 15% of homicide and manslaughter	
15	arrests. <sup>20</sup>	
16	• This general pattern has persisted over time. The following	
17	chart, from 2009 and showing homicide offending rate by age,	
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20		
21	<sup>17</sup> Federal Bureau of Investigation: U.S. Department of Justice Uniform Crime	
22	Reporting Program Data: Supplementary Homicide Reports, 2014–2018, Ann Arbor	
23	all offenders, including co-offenders.	
24	$^{18}$ Id.	
25	5 U.S. Department of Justice, <i>Crime in the United States</i> , Arrests, by Age, 2017, at Table 38, https://ucr.fbi.gov/crime-in-the-u.s/2017/crime-in-the-u.s2017/topic-	
26	pages/tables/table-38.	
27	<sup>20</sup> <i>Id.</i> ; U.S. Census Bureau, <i>Current Population Reports</i> , Population Projections of the United States by Age, Sex, Race, and Hispanic Origin: 1995 – 2050 at 76.	
28	available at https://www.census.gov/prod/1/pop/p25-1130/p251130.pdf.	
	DDIEE OF AMICUS CUDIAE CIEFODDS I AW CENTED TO DREVENT CUN VIOLENCE IN SUDDORT OF	

Case 3:19-cv-01226-L-AHG Document 26-1 Filed 01/03/20 PageID.5043 Page 21 of 34



2. Eighteen-to-Twenty-Year-Olds Attempt Suicide at 1 Disproportionately High Rates and Access to Firearms 2 Increases the Likelihood and Lethality of Those Suicide Attempts. 3 Eighteen-to-twenty-year-olds are also disproportionately at risk of 4 attempting suicide, and firearm access exacerbates this risk. Many major psychiatric 5 conditions first develop in adolescence,<sup>24</sup> and suicide risk "increase[s] steeply during 6 the first few years after" an individual's first contact with psychiatric services.<sup>25</sup> 7 Data from the Centers for Disease Control and Prevention show that suicide accounts 8 for a higher percentage of deaths for 15-to-24-year-olds than for any other age 9 group.<sup>26</sup> Indeed, suicide is the second-most common cause of death among 18-to-10 20-year-olds.<sup>27</sup> 11 "Access to firearms is a key risk factor for suicide."<sup>28</sup> Firearm suicide 12 is the suicide method with the highest fatality rate. Whereas 4% of suicide attempts 13 14 15 16 24 See Sacks Decl., Ex. 4 at 38, 44, Tomáš Paus et al., Why Do Many Psychiatric Disorders Emerge During Adolescence?, 9 NATURE REVIEWS NEUROSCIENCE 947, 17 952 (2008) ("Anxiety disorders, bipolar disorder, depression, eating disorder, psychosis (including schizophrenia) and substance abuse all most commonly emerge 18 during adolescence."); Sacks Decl., Ex. 5 at 50, 52, Mental Health Disorder Statistics, JOHNS HOPKINS MEDICINE, https://www.hopkinsmedicine.org/health/ 19 wellness-and-prevention/mental-health-disorder-statistics (explaining that 20 schizophrenia typically "first appears in men during their late teens or early twenties") (last visited Jan. 2, 2020). 21 25 Sacks Decl., Ex. 3 at 30, 34, Merete Nordentoft et al., Absolute Risk of Suicide 22 after First Hospital Contact in Mental Disorder, 68 ARCHIVES OF GENERAL PSYCHIATRY 1058, 1061 (2011). 23 26 Centers for Disease Control and Prevention, Web-based Injury Statistics 24 Query and Reporting System (WISQARS), Leading Cause of Death Reports, https://webappa.cdc.gov/sasweb/ncipc/leadcause.html. 25 27 Id. 26 28 Sacks Decl., Ex. 6 at 54, 56, American Public Health Association, *Reducing* Suicides by Firearms (2018), https://www.apha.org/policies-and-advocacy/public-27 health-policy-statements/policy-database/2019/01/28/reducing-suicides-by-28 firearms. -14are fatal when the attempts do not involve a firearm,<sup>29</sup> 85% of suicide attempts
 involving a firearm are fatal.<sup>30</sup> Suicide attempts are rarely repeated—more than 90%
 of people who survive a suicide attempt do not later die by suicide.<sup>31</sup> Therefore a
 minor's access to firearms during a suicide attempt often determines whether he dies
 or recovers.

6 As the Legislature recognized, see supra Section II.A, 18-to-20-year-7 olds are particularly at risk for suicides involving long guns. Of suicides where the 8 firearm type is known, most adults are twice as likely to die by handgun suicide as they are by long gun suicide.<sup>32</sup> But 18-to-20-year-olds are much more likely to die 9 by long gun suicides than other groups, likely at least in part because, prior to the 10 11 effective date of Section 27510, they have had far easier access to long guns 12 compared to handguns. A recent study found that, while handguns are used in most suicides, long gun use is relatively higher among adolescents compared with 13 adults.<sup>33</sup> In fact, 18- and 19-year-olds are the *only groups more likely to die by long* 14 gun suicide than handgun suicide.<sup>34</sup> Long guns pose a unique risk to the 18-to-20-15 16 year-old age group. Section 27510's long-gun age restriction addresses this risk. 17

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 $22 \begin{vmatrix} attempts \\ 30 \end{bmatrix} Id.$ 

26 <sup>33</sup> *Id.* 

<sup>&</sup>lt;sup>19</sup><sup>29</sup> Sacks Decl., Ex. 7 at 64, 69, Matthew Miller et al., Suicide Mortality in the United States: The Importance of Attending to Method in Understanding Population-Level Disparities in the Burden of Suicide, 33 ANN. REV. PUB. HEALTH
<sup>21</sup> 393, 397 (2012) (establishing that in 2001, there were 333,765 non-firearm suicide attempts and 13,753 fatalities).

<sup>&</sup>lt;sup>23</sup> *Id.* at Ex. 7 at 74-75, Miller et al. at 402-03.

 <sup>&</sup>lt;sup>32</sup> Sacks Decl., Ex. 8 at 85, 87, Thomas J. Hanlon et al., *Type of Firearm Used in Suicides: Findings from 13 States in the National Violent Death Reporting System*, 2005–2015, 65 J. ADOLESCENT HEALTH 366, 367 (2019).

 <sup>27 &</sup>lt;sup>34</sup> Centers for Disease Control and Prevention, Wide-ranging Online Data for
 28 Epidemiologic Research (WONDER), https://wonder.cdc.gov/controller/
 28 datarequest?stage=search&action=current.

## 3. State-Level Gun Control Measures, Including Age Restrictions, Are Effective.

Studies also show that there is a more than "reasonable fit" between the 3 government's safety objectives and Section 27510. Plaintiffs assert that "there is no 4 evidence that gun laws banning the purchase or possession of legal firearms based 5 on age restrictions have the intended effect of reducing gun homicides and suicides." 6 (Pls.' Mem. at 22 (quoting Marvell Decl.  $\P$  5).)<sup>35</sup> This is false. Studies have 7 repeatedly found a connection between age restrictions such as Section 27510 and a 8 decline in firearm-related adolescent deaths, especially suicides and unintentional 9 shootings. For instance, an August 2004 study found that state laws raising the 10 minimum legal age to purchase a handgun to 21 were associated with a nine percent 11 decline in firearm suicide rates among 18-to-20-year-olds.<sup>36</sup> A survey of convicted 12 gun offenders in 13 states also found that 17% of the offenders would have been 13 prohibited from obtaining firearms at the time of the crime if the minimum legal age 14 15

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<sup>19</sup> 35 In a declaration, Thomas Marvell states that "[t]he impact of increasing minimum age to purchase a firearm from 18 to 21 is difficult to determine because 20 very few states have made such a change." (ECF No. 21-15 (Marvell Decl.) ¶ 8.) Nevertheless, Mr. Marvell states that, "based on [his] and others' research ... [he 21 has] no reason to believe" that an increase in the minimum age could reduce the 30,000 annual gun deaths per year in the United States. (Id. ¶ 8, 19.) First, Mr. 22 Marvell's view of the literature is at odds with the twenty years of scholarship cited 23 Second, Mr. Marvell's uncertainty about the effectiveness of in this Section. minimum age laws has no bearing on the constitutionality of Section 27510 because 24 "[California is] entitled to rely on any evidence reasonably believed to be relevant to substantiate its important interests." Fyock, 779 F.3d at 1000 (internal quotation 25 marks omitted). Here, the record shows that California considered crime and suicide 26 data when enacting Section 27510. See supra Section II.A.

<sup>ECF No. 21-15 (Marvell Decl.), Ex. 7 at 76, 81, Daniel W. Webster et al.,
Association between Youth-Focused Firearm Laws and Youth Suicides, 292 JAMA 594, 598 (2004).</sup> 

1 in that state had been 21 years, a finding that, according to the authors, "underscore[d] the importance of minimum-age restrictions."<sup>37</sup> 2

3 State gun control measures more generally have also proven effective 4 in reducing gun violence among young people, including in the 18-to-20-year-old 5 range. An August 2019 study examined the 21,241 firearm-related deaths among 6 U.S. children from 2011 to 2015. Eighteen-to-twenty-one-year-olds made up more 7 than half of these deaths (68.7%). But state laws make a difference: the study found 8 that every 10-point increase in a score measuring the strictness of a state's gun 9 control laws "decreases the firearm-related mortality rate in children by 4%" in its fully adjusted model.<sup>38</sup> Another study published in August 2019 examined states 10 11 using the same gun-law scores and found that the quartile of states with the strictest 12 laws "have an annual pediatric firearm mortality rate of 2.563 per 100,000 [children] 13 aged 0-to-19-years-old] compared with states in the lowest quartile [with the least strict laws], where the mortality rate is almost twice as high at 5.005 per 100,000."<sup>39</sup> 14 Finally, research on the characteristics of mass shooters contradicts

15 Plaintiffs' casual assumption that state law cannot influence criminals' behavior. 16 17 Plaintiffs suggest that criminal shooters will simply continue to get guns in disregard 18 of the law (see Pls.' Mem. at 19 (quoting Lott Decl. ¶ 9)), such as by obtaining 19 weapons illegally or from out-of-state. In fact, most mass shooters obtain their 20 weapons lawfully. In a report examining active shootings from 2000 to 2013, the 21 FBI concluded that "only very small percentages [of shooters] obtain[ed] a firearm 22

<sup>37</sup> Sacks Decl., Ex. 9 at 91, 95-96, Katherine A. Vittes et al., Legal Status and 24 Source of Offenders' Firearms in States with the Least Stringent Criteria for Gun *Ownership*, 19 INJURY PREVENTION 26, 29-30 (2013). 25

<sup>38</sup> Sacks Decl., Ex. 10 at 98, 101, Monika K. Goyal et al., State Gun Laws and 26 *Pediatric Firearm-Related Mortality*, 144 PEDIATRICS No. 2, at 3 & tbl. 1 (2019).

Sacks Decl., Ex. 11 at 109, 112, Sriraman Madhavan et al., Firearm 27 Legislation Stringency and Firearm-Related Fatalities Among Children in the US, 28 229 J. AM. COLLEGE SURGEONS 150, 152 (2019).

illegally,"<sup>40</sup> indicating that these perpetrators are not necessarily sophisticated 1 participants in the firearms black market. Lawmakers therefore can, and should, 2 3 assume that restricting access to long guns will deter criminal use of long guns precisely the type of reasonable assumption that underlies virtually all laws aimed at 4 regulating dangerous products. Accord, e.g., Nat'l Paint & Coatings Ass'n v. City 5 of Chicago, 45 F.3d 1124, 1128-29 (7th Cir. 1995) ("Legislatures often enact laws 6 that reduce but cannot eliminate the effects of movements across municipal and state 7 8 borders.").

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#### III. PLAINTIFFS RELY ON DUBIOUS EVIDENCE FROM A WIDELY DISCREDITED DECLARANT.

Plaintiffs rely on a 22-paragraph declaration by economist John R. Lott 12 to argue that Section 27510 does not survive intermediate scrutiny. (Pls.' Mem. at 13 17-30; see ECF No. 21-17 (Lott Decl.).) Plaintiffs cite Mr. Lott extensively, 14 including for the (incorrect) propositions that there is "no credible evidence" that 15 "raising the age to purchase or acquire a firearm will make any difference in 16 curtailing mass school shootings," that data suggesting that those under the age of 17 21 are "disproportionately linked to crime" is "badly skewed," and that "Defendants 18 have no plausible argument that enjoining enforcement of California's age-based 19 gun ban will endanger public safety." (Pls.' Mem. at 19, 23, 28.) But as explained 20 above, supra Section II.B, social science research establishes that 18-to-20-year-olds 21 face a unique risk for firearm suicides and homicides. The research also confirms 22 that laws like Section 27510 are effective in protecting against this risk. 23

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It is unsurprising, however, that Mr. Lott takes a position opposite to the wide body of research. Mr. Lott makes his living offering intellectually dubious 25

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- 40 Sacks Decl., Ex. 22 at 393, 400, U.S. Dep't of Justice, Federal Bureau of Investigation, A Study of the Pre-Attack Behaviors of Active Shooters in the United 27
- States Between 2000 and 2013, at 7 (June 2018), https://www.fbi.gov/file-28
- repository/pre-attack-behaviors-of-active-shooters-in-us-2000-2013.pdf/view.

and scientifically flawed opinions on firearm policy. His declaration in this case
 should be given no weight.

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# A. Mr. Lott's Research Methodologies Have Been Widely Criticized.

Mr. Lott's research methodology on the impact of firearm restrictions 6 on crime rates has been criticized as fundamentally flawed. Academics including 7 Stanford University Professors Abhay Aneja and John J. Donohue III, in reviewing 8 Mr. Lott's research on "right-to-carry" laws and the National Research Council's 9 subsequent critical evaluation of that research, concluded that Mr. Lott's data set 10included "several coding errors" that skewed his conclusions, such as incorrect 11 coding for the year in which several states adopted right-to-carry laws, and an "area 12 variable," which was used to compute county density, that was "missing data."<sup>41</sup> 13 Moreover, as Professors Aneja and Donohue observed, Mr. Lott neglected "major 14 factors influencing the pattern of U.S. crime in recent decades," such as increases in 15 the prison and police populations.<sup>42</sup> Professor Donohue published a separate critique 16 with Yale Law School Professor Ian Ayres, concluding that Mr. Lott's research had 17 "not withstood the test of time" as his results "collapsed" when "more complete" 18 data sets were "tweaked in plausible ways."<sup>43</sup> Like Professors Aneja and Donohue, 19 Professors Ayres and Donohue also noted significant coding errors in Mr. Lott's past 20 work, including one that, when corrected, undermined Mr. Lott's claim that "right 21 to carry" laws resulted in a "statistically significant" reduction in robbery.44 22

- 27 <sup>43</sup> Sacks Decl., Ex. 13 at 187, 292, Ian Ayres et al., *Shooting Down the "More Guns, Less Crime" Hypothesis*, 55 STAN. L. REV. 1193, 1296 (2003).
- <sup>28</sup> <sup>44</sup> *Id.* at Ex. 13 at 257, Ayres et al. at 1261.

Sacks Decl., Ex. 12 at 118, 167-68, Abhay Aneja et al., *The Impact of Right-to-Carry Laws and the NRC Report: Lessons for the Empirical Evaluation of Law and Policy*, 13 AM. LAW & ECON. REV. 565, 585, 613-14 (2011).

<sup>26 &</sup>lt;sup>42</sup> *Id.* at Ex. 12 at 168-69, Aneja et al. at 614-15.

1 Critics have also observed that Mr. Lott has adopted carefully crafted 2 definitions for what constitute "mass shootings" and "gun-free zones," citing 3 significant discrepancies between his research—which he claimed showed that 94% 4 of mass shootings since 1950 occurred in gun free zones—and other experts', who have found that only 10% of mass shootings occur in gun-free zones.<sup>45</sup> For example, 5 6 Mr. Lott excludes from the definition of mass shootings those "that resulted from 7 gang or drug violence or during the commission of a crime," as well as shootings that occur in a private residence.<sup>46</sup> As for gun-free zones, Mr. Lott has adopted a 8 9 sweeping definition that includes even areas where law enforcement and military police are regularly present and permitted to carry guns.<sup>47</sup> This bizarre definition 10 11 leads Mr. Lott to an illogical result: he classifies shootings that occurred in areas 12 where guns were both *permissible* and *present*—such as those that occurred at Fort 13 Hood, the Washington Navy Yard, and Pensacola Naval Base—as having occurred 14 in gun-free zones.<sup>48</sup> In order to support his outlier claims about the efficacy of 15 firearm restrictions, Mr. Lott relies on assumptions that diverge from other experts 16 and defy common sense.

 <sup>&</sup>lt;sup>45</sup> Sacks Decl., Ex. 14 at 309, Meg Kelly, Do 98 Percent of Mass Public
 <sup>45</sup> Shootings Happen in Gun-Free Zones?, WASH. POST., May 10, 2018, https://www.washingtonpost.com/news/fact-checker/wp/2018/05/10/do-98-

<sup>20</sup> percent-of-mass-public-shootings-happen-in-gun-free-zones; Sacks Decl., Ex. 15 at 313, UPDATED: Mass Public Shootings Keep Occurring In Gun-Free Zones: 94%

<sup>21</sup> *of Attacks Since 1950*, Crime Prevention Research Center (updated July 6, 2019), https://crimeresearch.org/2018/06/more-misleading-information-from-bloombergs-22 everytown-for-gun-safety-on-guns-analysis-of-recent-mass-shootings/: Sacks

everytown-for-gun-safety-on-guns-analysis-of-recent-mass-shootings/; Sacks Decl., Ex. 16 at 330, Evan Defilippis et al., *The GOP's Favorite Gun 'Academic' is* 

 <sup>&</sup>lt;sup>23</sup> Decl., Ex. 16 at 350, Evan Demppis et al., *The GOP's Favorite Gun' Academic' is a Fraud*, ThinkProgress (Aug. 12, 2016), https://thinkprogress.org/ debunking-john <sup>24</sup> lott-5456e83cf326/.

 <sup>&</sup>lt;sup>46</sup> Sacks Decl., Ex. 14 at 311-12, Meg Kelly, Do 98 Percent of Mass Public Shootings Happen in Gun-Free Zones?, WASH. POST., May 10, 2018, https://www.washingtonpost.com/news/fact-checker/wp/2018/05/10/do-98-

percent-of-mass-public-shootings-happen-in-gun-free-zones/.

<sup>27</sup>  $\begin{bmatrix} 1 \\ 47 \end{bmatrix}$  Id.

<sup>28</sup> 48 *Id.* 

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# B. Mr. Lott Has a History of Failing to Provide Data and Evidence to Back up His Claims.

3 In order for a scientific finding to be credible, the underlying research 4 must be replicable. But Mr. Lott is known for publishing research without revealing 5 his underlying source data, preventing others from testing the validity of his 6 conclusions. For instance, critics have questioned Mr. Lott's inability to produce 7 any of the poll data underlying a 1997 survey, which purportedly showed that 8 "merely brandishing a weapon successfully deterred criminal attacks 98 percent of 9 the time."<sup>49</sup> According to Mr. Lott, the data was lost when his computer crashed. 10 But experts have found this explanation implausible. Northwestern Professor James 11 Lindgren, for example, commented that "all evidence of a study with 2,400 12 respondents does not just disappear when a computer crashes," noting the absence 13 of records relating to the funding of the survey, the cost of the survey, student 14 volunteers who assisted with the survey, the survey instrument, or any individual 15 responses to the survey.<sup>50</sup>

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- lambert/guns/lindgren.html. One individual, Minnesota lawyer David M. Gross,
   came forward several years after Mr. Lott had purportedly conducted his survey,
   claiming to have been a participant. But Mr. Gross's emergence does not help Lott's
   credibility. Mr. Gross was a former National Rifle Association board member and
   the founding director of the Minnesota Gun Owners Civil Rights Alliance. Even
   assuming Mr. Lott's 1997 survey actually happened, a leading pro-gun advocate's
   inclusion in a sample of 2,400 out of 3 million Americans suggests that Mr. Lott's
- inclusion in a sample of 2,400 out of 3 million Americans suggests that Mr. Lott's "sampling" was not random. Sacks Decl., Ex. 19 at 375, 379-381, Timothy Noah, *The Bellesiles of the Right? Another Firearms Scholar Whose Dog Ate His Data*, SLATE, Feb. 3, 2003, https://slate.com/news-and-politics/2003/02/bellesiles-of-the-
- 28 right.html.

 <sup>&</sup>lt;sup>49</sup> Sacks Decl., Ex. 17 at 344, 345-46, Claudia Deane et al., *A Fabricated Fan and Many Doubts*, WASH. POST, Feb. 11, 2003, https://www.washingtonpost.com/archive/politics/2003/02/11/a-fabricated-fan-and-many-doubts/b086b96f-0c86-417e-afe9-c4623d5e936f/.

Sacks Decl., Ex. 18 at 347, 355-56, James Lindgren, Comments on Questions
 About John R. Lott's Claims Regarding a 1997 Survey (Jan. 17, 2003), https://web.archive.org/web/20130304061928/http://www.cse.unsw.edu.au/~
 Immert/gung/lindgren.html. One individual Minnegota lawyer David M. Gross

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# C. Mr. Lott's Work Is Rarely Published in Peer-Reviewed Journals.

3 Mr. Lott is not affiliated with a university, and "[1]ittle of his gun research has been published in peer-reviewed journals."<sup>51</sup> The Supreme Court, in 4 the context of evaluating the reliability of expert testimony, has recognized that peer 5 review—*i.e.*, "submission to the scrutiny of the scientific community"—"increases 6 7 the likelihood that substantive flaws in methodology will be detected" and is thus a "relevant consideration" in evaluating the "validity of a particular technique or 8 methodology on which an opinion is premised." Daubert v. Merrell Dow Pharm., 9 Inc., 509 U.S. 579, 593-94 (1993). Mr. Lott's failure to publish peer-reviewed work 10 calls into question the reliability of his methods and validity of his conclusions. Mr. 11 Lott apparently shares this concern, having publicly claimed that he was published 12 13 in a peer-reviewed journal that, in reality, actually rejected his work.<sup>52</sup>

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# D. Mr. Lott Has Committed Several Ethical Violations to Defend His Work from Criticism.

In addition to ample criticism about his substantive work and methods,
 Mr. Lott has engaged in a pattern of unethical behavior that further undermines his
 credibility. For example, Mr. Lott has used an assumed, fictional identity to post
 praise for himself online and to defend himself from critics, even masquerading as a
 former student and posting: "he was the best professor I ever had."<sup>53</sup> On another

 <sup>&</sup>lt;sup>51</sup> Sacks Decl., Ex. 20 at 384, 385, Peter Moskowitz, *Inside the Mind of America's Favorite Gun Researcher*, PACIFIC STANDARD (Updated Sept. 23, 2018), https://psmag.com/magazine/inside-the-mind-of-americas-favorite-gun-researcher.

Sacks Decl., Ex. 16 at 330, 334, Evan Defilippis et al., *The GOP's Favorite Gun 'Academic' is a Fraud*, ThinkProgress (Aug. 12, 2016), https://thinkprogress.org/debunking-john-lott-5456e83cf326.

Sacks Decl., Ex. 21 at 390, 391, Richard Morin, *Scholar Invents Fan to Answer His Critics*, WASH. POST, Feb. 1, 2003, https://www.washingtonpost.com/
 archive/lifestyle/ 2003/02/01/ scholar-invents-fan-to-answer-his-critics/f3ae3f46 68d6-4eee-a65e-1775d45e2133/.
### Case 3:19-cv-01226-L-AHG Document 26-1 Filed 01/03/20 PageID.5053 Page 31 of 34

occasion, Mr. Lott wrote a first-person narrative in which he claimed to be
 Dartmouth student and stalking victim Taylor Woolrich, criticizing Dartmouth for
 not letting her carry a gun for self-defense.<sup>54</sup>

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\* \*

In short, Mr. Lott's work is highly suspect. Experts have widely
critiqued his methodology, pointing out coding errors, biases, and questionable
definitions and assumptions. He has published his gun research without peerreview, and, when asked to produce his source data, he has been unable to deliver.
Finally, he is notorious for deceptive tactics such as posting under false identities.
Accordingly, this Court should give no weight to Mr. Lott's declaration.

12 Even if the Court were inclined to credit some of Mr. Lott's assertions, 13 at most, his arguments suggest that research on the effectiveness of minimum age 14 laws is subject to academic debate. On the one side, the California legislature relied 15 on crime data from official sources and made judgments supported by peer-reviewed 16 social science studies; on the other side, the challengers produced expert witnesses 17 who reject that research. The disagreement of experts does not render California's 18 law unconstitutional because courts "do not demand of legislatures 'scientifically 19 certain criteria of legislation." Paris Adult Theatre I v. Slaton, 413 U.S. 49, 60 20 (1973) (internal citation omitted). Indeed, when a state regulates "in areas fraught 21 with medical and scientific uncertainties, legislative options must be especially 22 broad and courts should be cautious not to rewrite legislation." Marshall v. United States, 414 U.S. 417, 427 (1974); Kansas v. Hendricks, 521 U.S. 346, 360 n.3 (1997) 23 (where psychiatric professionals joined conflicting amicus briefs, their 24 25 disagreements "do not tie the State's hands" in its policy choices). The contrary 26

<sup>Sacks Decl., Ex. 16 at 330, 340-41, Evan Defilippis et al.,</sup> *The GOP's Favorite Gun 'Academic' is a Fraud*, ThinkProgress (Aug. 12, 2016, 4:45 PM), https://thinkprogress.org/debunking-john-lott-5456e83cf326.

<sup>-23-</sup>

conclusions of Plaintiffs' experts, including Mr. Lott, have no bearing on the constitutionality of Section 27510 in light of the substantial record showing that California considered robust and reliable data when enacting Section 27510 (see supra Section II), and drew "reasonable inferences" from that data. Mahoney v. Sessions, 871 F.3d 873, 883 (9th Cir. 2017). -24-

1	CONCLUSION								
2	For the foregoing reasons, and those set forth by Defendants, the Court								
3	should deny the Plaintiffs' Motion for Preliminary Injunction.								
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6									
7	Dated: January 3, 2020								
8	Robert A. Sacks Robert A. Sacks (Bar No. 150146) SULLIVAN & CROMWELL LLP								
9 10	1888 Century Park East Los Angeles, California 90067 Telephone: (310) 712-6600								
11	Facsimile: (310) 712-8800								
12	Counsel for Amicus Curiae Giffords Law Center to Prevent Gun								
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24	125 Broad Street New York, NY 10004								
25	Telephone: 212-558-4000 Facsimile: 212-558-3558								
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### **CERTIFICATE OF SERVICE**

1	CERTIFICATE OF SERVICE						
2	I certify that on January 3, 2020, I filed the foregoing document with						
3	the Clerk of the Court for the United States District Court, Southern District of						
4	California, by using the Court's CM/ECF system, which will send notification of						
5	electronic filing (NEF) to all counsel of record.						
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7	/s/ Robert A. Sacks						
8	Robert A. Sacks						
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1 2 3 4	Robert A. Sacks (Bar No. 150146) SULLIVAN & CROMWELL LLP 1888 Century Park East Los Angeles, California 90067 Telephone: (310) 712-6600 Facsimile: (310) 712-8800							
5 6	Attorney for <i>Amicus Curiae</i> Giffords Law Center to Prevent Gun Violence							
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8								
9	UNITED STATE	ES DISTRICT COURT						
10	SOUTHERN DIST	RICT OF CALIFORNIA						
11								
12	MATTHEW JONES, et al.,	Case No.: 3:19-cv-01226-L-AHG						
13	Plaintiffs,	DECLARATION OF ROBERT A. SACKS IN SUPPORT OF BRIEF OF						
14	V.	AMICUS CURIAE GIFFORDS LAW CENTER TO PREVENT GUN						
15 16	XAVIER BECERRA, in his official capacity as Attorney General of the State of California et al	<ul> <li>VIOLENCE IN SUPPORT OF</li> <li>DEFENDANTS' OPPOSITION TO</li> <li>PLAINTIFFS' MOTION FOR</li> <li>DEFLIMINARY INJUNCTION</li> </ul>						
17	Defendents	) PRELIMINART INJUNCTION						
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### **DECLARATION OF ROBERT A. SACKS**

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I, Robert A. Sacks, declare:

I am a member of the Bars of the State of California and this Court, and
 am a partner in the firm of Sullivan & Cromwell LLP, attorneys for *amicus curiae* Giffords Law Center to Prevent Gun Violence ("Giffords Law Center"). I submit
 this declaration in support of Giffords Law Center's *amicus* Brief in Support of
 Defendants' Opposition to Plaintiffs' Motion for Preliminary Injunction. I have
 personal knowledge of the facts set forth in this Declaration and, if called as a
 witness, I would testify competently thereto.

Attached as Exhibit 1 is a true and correct copy of Michael Dreyfuss et
 al., *Teens Impulsively React Rather than Retreat from Threat*, 36 DEVELOPMENTAL
 NEUROSCIENCE 220 (2014).

3. Attached as Exhibit 2 is a true and correct copy of Daniel W. Webster
et al., *The Case for Gun Policy Reforms in America*, JOHNS HOPKINS CTR. FOR GUN
POLICY & RESEARCH (2012), https://www.jhsph.edu/research/centers-andinstitutes/johns-hopkins-center-for-gun-policy-and-

17 research/publications/WhitePaper020514 CaseforGunPolicyReforms.pdf.

Attached as Exhibit 3 is a true and correct copy of Merete Nordentoft
 et al., *Absolute Risk of Suicide after First Hospital Contact in Mental Disorder*, 68
 ARCHIVES OF GENERAL PSYCHIATRY 1058 (2011).

5. Attached as Exhibit 4 is a true and correct copy of Tomáš Paus et al.,
 Why Do Many Psychiatric Disorders Emerge During Adolescence?, 9 NATURE
 REVIEWS NEUROSCIENCE 947 (2008).

246. Attached as Exhibit 5 is a true and correct copy of Mental Health25DisorderStatistics,JOHNSHOPKINSMEDICINE,26https://www.hopkinsmedicine.org/health/wellness-and-prevention/mental-health-disorder-statistics (last visited Jan. 2, 2020).Hopkins

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7. Attached as Exhibit 6 is a true and correct copy of American Public
 Health Association, *Reducing Suicides by Firearms* (2018),
 https://www.apha.org/policies-and-advocacy/public-health-policy-

4 statements/policy-database/2019/01/28/reducing-suicides-by-firearms.

8. Attached as Exhibit 7 is a true and correct copy of Matthew Miller et
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REV. PUB. HEALTH 393 (2012).

9 9. Attached as Exhibit 8 is a true and correct copy of Thomas J. Hanlon et
10 al., *Type of Firearm Used in Suicides: Findings from 13 States in the National*11 Violent Death Reporting System, 2005–2015, 65 J. ADOLESCENT HEALTH 366
12 (2019).

13 10. Attached as Exhibit 9 is a true and correct copy of Katherine A. Vittes
14 et al., *Legal Status and Source of Offenders' Firearms in States with the Least*15 *Stringent Criteria for Gun Ownership*, 19 INJURY PREVENTION 26 (2013).

16 11. Attached as Exhibit 10 is a true and correct copy of Monika K. Goyal
17 et al., *State Gun Laws and Pediatric Firearm-Related Mortality*, 144 PEDIATRICS
18 No. 2 (2019).

19 12. Attached as Exhibit 11 is a true and correct copy of Sriraman Madhavan
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14. Attached as Exhibit 13 is a true and correct copy of Ian Ayres et al.,
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(2003).

28

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15 https://www.washingtonpost.com/archive/politics/2003/02/11/a-fabricated-fan16 and-many-doubts/b086b96f-0c86-417e-afe9-c4623d5e936f/.

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18 *Comments on Questions About John R. Lott's Claims Regarding a 1997 Survey* (Jan.
19 17, 2003), https://web.archive.org/web/20130304061928/http://www.cse.
20 unsw.edu.au/~lambert/guns/lindgren.html.

21 20. Attached as Exhibit 19 is a true and correct copy of Timothy Noah, *The*22 *Bellesiles of the Right? Another Firearms Scholar Whose Dog Ate His Data*,
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25 21. Attached as Exhibit 20 is a true and correct copy of Peter Moskowitz,
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27 (Updated Sept. 23, 2018), https://psmag.com/magazine/inside-the-mind-of28 americas-favorite-gun-researcher.

1	22. Attached as Exhibit 21 is a true and correct copy of Richard Morin,
2	Scholar Invents Fan to Answer His Critics, WASH. POST, Feb. 1, 2003,
3	https://www.washingtonpost.com/archive/lifestyle/2003/02/01/scholar-invents-fan-
4	to-answer-his-critics/f3ae3f46-68d6-4eee-a65e-1775d45e2133/.

5	23. Attached as Exhibit 22 is a true and correct copy of U.S. Dep't of
6	Justice, Federal Bureau of Investigation, A Study of the Pre-Attack Behaviors of
7	Active Shooters in the United States Between 2000 and 2013 (June 2018),
8	https://www.fbi.gov/file-repository/pre-attack-behaviors-of-active-shooters-in-us-
9	2000-2013.pdf/view.

10 I declare under penalty of perjury under the laws of the United States11 of America that the foregoing is true and correct.

12 Executed on: January 3, 2020

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14	/s/ Robert A. Sacks
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	TABLE OF CONTENTS	
<b>Exhibit</b>	Description	Pages
1	Michael Dreyfuss et al., <i>Teens Impulsively React</i> <i>Rather than Retreat from Threat</i> , 36 DEVELOPMENTAL NEUROSCIENCE 220 (2014)	1-9
2	Daniel W. Webster et al., <i>The Case for Gun Policy</i> <i>Reforms in America</i> , JOHNS HOPKINS CTR. FOR GUN POLICY & RESEARCH (2012)	10-29
3	Merete Nordentoft et al., <i>Absolute Risk of Suicide after</i> <i>First Hospital Contact in Mental Disorder</i> , 68 ARCHIVES OF GENERAL PSYCHIATRY 1058 (2011)	30-37
4	Tomáš Paus et al., <i>Why Do Many Psychiatric</i> <i>Disorders Emerge During Adolescence?</i> , 9 NATURE REVIEWS NEUROSCIENCE 947 (2008)	38-49
5	Mental Health Disorder Statistics, JOHNS HOPKINS MEDICINE (last visited Jan. 2, 2020)	50-53
6	American Public Health Association, <i>Reducing</i> Suicides by Firearms (2018)	54-63
7	Matthew Miller et al., Suicide Mortality in the United States: The Importance of Attending to Method in Understanding Population-Level Disparities in the Burden of Suicide, 33 ANN. REV. PUB. HEALTH 393 (2012)	64-84
8	Thomas J. Hanlon et al., <i>Type of Firearm Used in</i> <i>Suicides: Findings from 13 States in the National</i> <i>Violent Death Reporting System, 2005–2015</i> , 65 J. ADOLESCENT HEALTH 366 (2019)	85-90
9	Katherine A. Vittes et al., Legal Status and Source of Offenders' Firearms in States with the Least Stringent Criteria for Gun Ownership, 19 INJURY PREVENTION 26 (2013)	91-97

10	Monika K. Goyal et al., <i>State Gun Laws and Pediatric Firearm-Related Mortality</i> , 144 PEDIATRICS No. 2 (2019)	98-108
11	Sriraman Madhavan et al., <i>Firearm Legislation</i> Stringency and Firearm-Related Fatalities Among Children in the US, 229 J. AM. COLLEGE SURGEONS 150 (2019)	109-117
12	Abhay Aneja et al., <i>The Impact of Right-to-Carry Laws</i> and the NRC Report: Lessons for the Empirical Evaluation of Law and Policy, 13 AM. LAW & ECON. REV. 565 (2011)	118-186
13	Ian Ayres et al., <i>Shooting Down the "More Guns, Less Crime" Hypothesis</i> , 55 STAN. L. REV. 1193 (2003)	187-308
14	Meg Kelly, Do 98 Percent of Mass Public Shootings Happen in Gun-Free Zones?, WASH. POST., May 10, 2018	309-312
15	UPDATED: Mass Public Shootings Keep Occurring In Gun-Free Zones: 94% of Attacks Since 1950, Crime Prevention Research Center (updated July 6, 2019)	313-329
16	Evan Defilippis et al., <i>The GOP's Favorite Gun</i> <i>'Academic' is a Fraud</i> , ThinkProgress (Aug. 12, 2016, 4:45 PM)	330-343
17	Claudia Deane et al., <i>A Fabricated Fan and Many Doubts</i> , WASH. POST, Feb. 11, 2003	344-346
18	James Lindgren, Comments on Questions About John R. Lott's Claims Regarding a 1997 Survey (Jan. 17, 2003)	347-374
19	Timothy Noah, <i>The Bellesiles of the Right? Another</i> <i>Firearms Scholar Whose Dog Ate His Data</i> , CHATTERBOX, Feb. 3, 2003	375-383

1 2	20	Peter Moskowitz, <i>Inside the Mind of America's</i> <i>Favorite Gun Researcher</i> , PACIFIC STANDARD (Updated Sept. 23, 2018)	384-389
3	21	Richard Morin, Scholar Invents Fan to Answer His Critics, WASH. POST, Feb. 1, 2003	390-392
5 6 7	22	U.S. Dep't of Justice, Federal Bureau of Investigation, A Study of the Pre-Attack Behaviors of Active Shooters in the United States Between 2000 and 2013 (June 2018)	393-423
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# **Exhibit 1**

Ex. 1 Page 1 Developmental Neuroscience

Dev Neurosci 2014;36:220–227 DOI: 10.1159/000357755 Received: October 23, 2013 Accepted after revision: December 3, 2013 Published online: May 8, 2014

### Teens Impulsively React rather than Retreat from Threat

Michael Dreyfuss<sup>a</sup> Kristina Caudle<sup>a</sup> Andrew T. Drysdale<sup>a</sup> Natalie E. Johnston<sup>a</sup> Alexandra O. Cohen<sup>a</sup> Leah H. Somerville<sup>b</sup> Adriana Galván<sup>c</sup> Nim Tottenham<sup>c</sup> Todd A. Hare<sup>d</sup> B.J. Casey<sup>a</sup>

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### **Key Words**

Adolescence · Fear · Impulsivity · Limbic circuitry · Orbitofrontal cortex · Medial prefrontal cortex

### Abstract

There is a significant inflection in risk taking and criminal behavior during adolescence, but the basis for this increase remains largely unknown. An increased sensitivity to rewards has been suggested to explain these behaviors, yet juvenile offences often occur in emotionally charged situations of negative valence. How behavior is altered by changes in negative emotional processes during adolescence has received less attention than changes in positive emotional processes. The current study uses a measure of impulsivity in combination with cues that signal threat or safety to assess developmental changes in emotional responses to threat cues. We show that adolescents, especially males, impulsively react to threat cues relative to neutral ones more than adults or children, even when instructed not to respond. This adolescent-specific behavioral pattern is paralleled by enhanced activity in limbic cortical regions implicated in the detection and assignment of emotional value to inputs and in the subsequent regulation of responses to them when successfully suppressing impulsive responses to threat cues. In contrast, prefrontal control regions implicated in detecting and resolving competing responses show an adolescent-emergent pattern (i.e. greater activity in adolescents and adults relative to children) during successful suppression of a response regardless of emotion. Our findings suggest that adolescence is a period of heightened sensitivity to social and emotional cues that results in diminished regulation of behavior in their presence.

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### Introduction

Adolescents commit more crimes per capita than children or adults in the USA [1] and in nearly all industrialized cultures [2]. Their proclivity toward incentives [3, 4] and risk taking [5–8] has been suggested to underlie the inflection in criminal activity observed during this time. Yet heightened sensitivity to incentives and risk taking are only part of the equation, as criminal behaviors often occur in emotionally charged situations of negative valence. Does negative emotional information impact self-

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E-Mail karger@karger.com www.karger.com/dne B.J. Casey, PhD Sackler Institute for Developmental Psychobiology Weill Cornell Medical College, 1300 York Avenue, Box 140 New York, NY 10065 (USA) E-Mail bjc2002@med.cornell.edu control differently across development? Previous work has shown that positive emotional cues lead to poorer self-control in adolescents relative to children and adults [3], but do negative emotional cues also lead to poor impulse control? The current study tests whether adolescents are more impulsive relative to adults or children when there is a signal of potential threat, using a measure of impulsivity in combination with cues that signal threat (e.g. a frightened face) relative to neutral ones (calm face) and examines potential mechanisms for developmental differences in behavior.

The fight-or-flight response is a physiological reaction to perceived threat [9]. Fearful faces are a reliable indicator of threat in the immediate environment [10], evoking a well-defined neural response [11, 12]. Negatively valenced stimuli such as fearful faces generally inhibit behavior, slowing response times and inhibiting motor responses in various tasks [13–15]. Adolescents, however, show difficulty suppressing attention and actions toward emotional stimuli even when irrelevant to the task at hand [16, 17]. This relative lack of cognitive control in the presence of emotional and motivational cues may underlie the behavioral risks that are characteristic of adolescence [18].

Prior work suggests that diminished self-control during adolescence may result from competition between limbic and control circuitry [17–20]. A combination of evidence from human imaging [3, 21-25], postmortem [26] and animal [27, 28] studies of regional brain changes over the course of development indicate that limbic and prefrontal circuitry interact differentially across development [29]. Specifically, limbic circuitry is thought to develop earlier than control circuitry as a result of evolutionary pressure and changes in gonad hormone levels that impact limbic structures. This developmental imbalance is suggested to result in a greater influence of limbic than prefrontal regions on behavior during adolescence. This pattern is in contrast to that observed in adulthood when these circuits have matured or in childhood when they are still developing.

The current study uses a A go/no-go paradigm to measure impulsivity in combination with cues that signal threat or safety (fearful or calm facial expressions) to assess developmental changes in emotional responses to such cues, their influence on behavior and their neurobiological correlates. In previous work using the same task and overlapping sample, we have shown a heightened sensitivity to emotional cues during adolescence. In the first study [30] we showed longer response latencies to negative (fear faces) relative to positive (happy faces) emotional cues across ages but adolescent-specific increases in amygdala activity when having to respond (go) to fear faces. In a second study [3], we focused on the ability to withhold a response to positive cues, focusing solely on happy no-go trials and showed that adolescents made more false alarms to happy cues than to neutral cues compared to children and adults. This pattern was paralleled by greater ventral striatal activity in adolescents relative to children and adults. Finally, recent reports by other laboratories have noted decrements in behavioral performance on cognitive control tasks in the presence of negatively valenced stimuli versus neutral stimuli in adolescents relative to children or adults [15, 16].

In the current study, expanding on these previous adolescent-specific findings toward emotionally valenced stimuli, we test for developmental differences in brain and behavior when required to suppress responses to cues of potential threat. Second, we explore individual differences in brain activity associated with overall behavioral performance. Finally, we explore possible sex differences in behavior and brain responses to cues of potential threat.

### Methods

### Subjects

A total of 80 participants between the ages of 6 and 27 years were scanned using functional magnetic resonance imaging (fMRI). Data from 23 participants were excluded due to poor overall accuracy (mean no-go accuracy <70%, n = 9), too much head motion (>2 mm translational or 2° rotational motion within a run, n = 12) or technical problems (n = 2), resulting in data from 57 usable subjects (27 females) in all reported analyses. Participants were grouped into child (aged 6–12 years, n = 18, 10 male), adolescent (aged 13–17 years, n = 19, 10 male) and adult (18 years or older, n = 20, 10 male) age groups. Data from this sample have been published previously on a different subset of the data [3, 30]. All participants provided informed written consent (parental consent and subject assent for children and adolescents) approved by the Institutional Review Board of Weill Cornell Medical College.

### Behavioral Paradigm

Participants completed six runs of a go/no-go task [3, 30] using fearful, happy and calm facial expressions as target (go) and nontarget (no-go) stimuli (fig. 1a). Within each run, two types of facial emotions were presented, one serving as the target (go) stimulus, to which they were instructed to press a button, and the other serving as a nontarget (no-go) stimulus, for which they were instructed to withhold a button press. Facial expressions were pseudorandomized across the run to control for presentation order, and all combinations of expression were used as both targets and nontargets, resulting in a 2 (response: go, no-go)  $\times$ 3 (emotion: fear, calm, happy) factorial design. Prior to each run, participants were instructed as to which expression served as the target (go) stimulus and that they should respond with a button press only to that ex-

Teens Impulsively React rather than Retreat from Threat

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**Fig. 1.** Development of impulse control to threat cues. **a** The emotional go/no-go task illustrating 5 trials with calm faces as the target stimuli, for which participants should go by pressing a button. Fearful faces are the nontarget (no-go) stimuli, to which participants should withhold a button press. Each face was displayed for

500 ms followed by a variable intertrial interval. **b** False alarms (dark gray line) to fear relative to calm no-go trials show an adolescent-specific pattern of more commission errors for adolescents than either children ( $t_{35} = 2.79$ , p < 0.009) or adults ( $t_{37} = 2.30$ , p < 0.03).

pression. Participants were also instructed to respond as fast as possible but to try to avoid making errors. The present report focuses specifically on the analysis of fear no-go trials relative to calm no-go trials. Previously published work on this task focused on no-go trials to happy facial expressions [3] and go trials to fearful facial expressions [30].

### Stimuli and Apparatus

The stimuli consisted of fearful, happy and calm faces from the NimStim set of facial expressions [31]. We used calm faces (mildly pleasant neutral faces) because we [32] and others [33, 34] have shown that developmental populations may perceive neutral faces as negative. The task was programmed using E-Prime software and presented to subjects on an overhead liquid crystal display panel integrated with the IFIS-SA system (fMRI Devices Corporation, Waukesha, Wisc., USA). Button responses and reaction times were logged using E-Prime software integrated with the IFIS system.

### Task Parameters

The data were acquired in six functional imaging runs that combined each emotion (happy, calm and fear) and response (go and no-go; fig. 1) using a rapid event-related design. On each trial, a face appeared for 500 ms followed by a jittered intertrial interval of between 2 and 14.5 s (mean 5.2 s) during which participants were presented with a fixation crosshair. A total of 48 trials were presented per run in pseudorandomized order (36 go and 12 nogo). A total of 24 no-go trials and 72 go trials were acquired for each expression type.

### Image Acquisition

Participants were scanned with a General Electric Signa 3.0-T fMRI scanner (General Electric Medical Systems, Milwaukee, Wisc., USA) and quadrature head coil. A high-resolution, T1weighted anatomical scan ( $256 \times 256$  in-plane resolution, 240-mm field of view, 124 1.5-mm slices) was acquired for each subject for transformation and localization of data to Talairach grid space. A spiral in and out sequence [35] was used to acquire functional imaging data (repetition time = 2,500 ms, echo time = 30 ms, field of view = 200 mm, flip angle = 90, skip 0,  $64 \times 64$  matrix). In all, 34 4-mm-thick coronal slices ( $3.125 \times 3.125$  mm resolution) covering the entire brain except for the posterior portion of the occipital lobe were acquired per repetition time.

#### Behavioral Analysis

Behavioral data from the emotional go/no-go task were analyzed for false alarms (incorrect presses to a 'no-go' stimulus) to fear and calm cues. Errors were calculated as a difference score between errors to fear nontargets relative to calm nontargets to isolate the effects of negative valence from the overall error rate. Error rates were compared between age groups (children, adolescents and adults). A positive value represents a greater proportion of errors to nontarget fear faces than calm faces, while a negative value represents the inverse. Mean reaction times and hit rates have been reported elsewhere [30]. A two-way ANOVA was performed with age group and sex as the between-subject variables and a difference score between errors to fear nontargets and errors to calm nontargets as the dependent variable of interest.

#### Imaging Analysis

Imaging data processing and analyses were performed using AFNI (analysis of functional neuroimages) software [36]. Functional imaging data were slice-time corrected, realigned within and across runs to correct for head movement, coregistered with each participant's high-resolution anatomical scan, scaled to percent signal change units, and smoothed with a 6-mm FWHM gaussian kernel. A general linear model (GLM) analysis was performed on each subject to characterize task effects with task regressors (calm/ go, calm/no-go, happy/go, happy/no-go, fear/go, fear/no-go, errors), convolved with a gamma-variate hemodynamic response function. Separate regressors were created for correct go and no-go trials, broken down by emotion (errors were grouped and modeled separately with insufficient numbers to analyze separately). Only correct fear and calm trials were considered of interest and included in the second-level analysis.

We modeled the effects of response (go vs. no-go), age group (child, adolescent or adult) and emotion (fear or calm) on brain activity using a linear mixed-effects model [37]. Parameter estimate  $(\beta)$  maps representing task effects were then transformed into the standard coordinate space of Talairach and Tournoux [38] (1988) by applying the warping parameters obtained from the transformation of each subject's high-resolution anatomical scan. Talairach-transformed parameter estimate maps were resampled to a resolution of  $3 \times 3 \times 3$  mm. A group linear mixed-effects model was performed to identify functional regions of interest (ROIs) implicated in the interaction of response, age group and emotion. Imaging findings considered statistically significant exceeded whole-brain correction for multiple comparisons to preserve an alpha <0.05 by using a p value/cluster size combination stipulated by Monte Carlo simulations run in the AlphaSim program within AFNI. Off-line analyses were conducted in SPSS Statistics 17.0 software (SPSS, Chicago, Ill., USA). Beta values were extracted from whole-brain-corrected ROIs (drawing a 5-mm sphere around the peak voxel in each region) and submitted to offline post hoc analyses with SPSS.

### Control Analyses

All imaging analyses were based on correct no-go trials. As task performance was significantly different between age groups, a second analysis was conducted to verify that the observed developmental effects were not due to less power in one age group relative to another. First-level GLMs were estimated in which number of correct trials were equated for all participants across conditions (fear/go, fear/no-go, calm/go, calm/no-go), using the lowest mean number of correct trials of all age groups (calm no-go trials in children; mean = 17 out of 24 possible, or 70% mean accuracy). New regressors were generated by randomly selecting 17 (of 24) trials per condition for inclusion. All other trials were modeled as separate regressors that were not further examined. Beta values were extracted from the 17-trial regressors using the previously defined ROIs, tested for replication, and reported in Results.

### Results

### Behavioral Results

The 2-way ANOVA showed a main effect of age group on false alarm rates to fear relative to calm nontargets ( $F_{2,59} = 8.58$ , p < 0.001), but no main effect of sex ( $F_{1,51} =$ 0.05, p > 0.85 ) or interaction with sex ( $F_{2,51} = 0.27$ , p > 0.77). Post hoc t tests showed that adolescents made more false alarms to fear nontargets in comparison to calm nontargets than either children ( $t_{35} = 2.79$ , p < 0.009) or adults ( $t_{37} = 2.30$ , p < 0.03; fig. 1b).

### Imaging Results

The whole-brain age group  $(3) \times$  response (go/nogo) × emotion (fear, calm) linear mixed-effects model revealed 7 ROIs (see table 1). Given the behavioral results we performed post hoc tests on beta values extracted from each whole-brain-corrected ROI to determine if teens differed from adults and from children in these regions. When we tested each region to determine whether significant variance could be attributed to adolescent-specific differences in response to fear relative to calm nontargets, two patterns emerged (see fig. 2): (1) adolescent-specific effects were of greater activity in adolescents compared to children or adults on correct threat no-go trials relative to calm no-go trials and (2) adolescentemergent effects of adolescents and adults activated this region more than children on correct threat no-go trials. The left orbitofrontal cortex (LOFC) and medial prefrontal cortex (mPFC) showed adolescent-specific effects. Although the striatum showed a similar developmental pattern post hoc tests did not reach significance between age groups (adolescents vs. children: p = 0.09 and adolescents vs. adults: p = 0.11). The right inferior frontal gyrus (RIFG), right anterior cingulate cortex (RACC) and left premotor cortex showed adolescent-emergent effects. Our control analysis, equating power across age groups and conditions, revealed similar patterns of activity, but to a lesser degree given less overall power of the analysis. However, the LOFC maintained a robust pattern of activity across analyses (adolescents vs. children:  $t_{35} = 2.74$ , p < 0.01 and adolescents vs. adults:  $t_{37} = 2.27$ , p < 0.03).

### Sex Differences

We performed exploratory analyses to test for sex differences within the three adolescent-specific findings (i.e. false alarm rates and OFC and mPFC activity to threat nontargets relative to calm nontargets). These exploratory analyses revealed that males rather than females appeared to be driving the inflection in false alarms to threat nontargets during adolescence (fig. 3a). Independent t tests revealed that in males, adolescents made more false alarms than children ( $t_{18} = 2.28$ , p < 0.04) or adults ( $t_{18} =$ 2.96, p < 0.009) and showed a similar pattern in the activation of the OFC, a region implicated in the regulation of approach-related behavior (adolescents vs. children:  $t_{18} = 2.31$ , p < 0.04; adolescents vs. adults:  $t_{18} = 2.39$ , p < 0.03; fig. 3b).

In contrast, the female age groups did not differ from one another in performance (children vs. adolescents: p = 0.44 and adolescents vs. adults: p = 0.07) or in OFC activity (children vs. adolescents: p = 0.19 and adolescents vs.



**Fig. 2.** Adolescent-specific and adolescent-emergent brain regions. Representative axial images and beta weights for those regions showing an age effect on correct fear no-go trials relative to calm ones from the whole-brain-corrected age  $(3) \times \text{response}(2) \times \text{emotion}(2)$  interaction. L = Left. Adolescent-specific effects on correct fear relative to calm no-go trials were found in contrasts between adolescents relative to children and adults together in the LOFC

( $t_{55}$  = 2.612, p < 0.012) and left mPFC ( $t_{55}$  = 2.832, p < 0.006) Adolescent-emergent effects were found in activation contrasts in children relative to adolescents and adults together on correct fear relative to calm no-go trials in the RIFG ( $t_{55}$  = 2.503, p < 0.02), RACC ( $t_{55}$  = 2.44, p < 0.02) and left premotor cortex ( $t_{55}$  = 3.658, p < 0.001).

Voxels, n	Region	Brodmann's area	Coordinates (peak)

**Table 1.** ROIs (Talairach) for the interaction of age group × emotion × response type

Voxels, n	Region	Brodmann s area	Coordinates (peak)	F value	
193	RIFG	45	32, 17, 18	8.41	
104	LOFC	11	-38, 41, -7	8.86	
78	L mPFC	9	-8, 53, 24	7.95	
72	L premotor	6	-41, 2, 7	8.68	
58	L striatum		-20, 8, -10	6.59	
56	L motor/premotor	4, 6	-14, -8, 63	7.74	
51	RACC	32	11, 2, 45	6.86	

Results are whole-brain corrected (alpha = 0.05, 47 voxels). L = Left.

Dreyfuss et al.



**Fig. 3.** Sex Differences in behavior and limbic activity by age group. **a** Difference score in number of false alarms to fear no-go trials relative to calm no-go trials by age group and sex. **b** Beta weights

for OFC to correct fear no-go trials relative to calm no-go trials by age group and sex. **c** Beta weights for mPFC to correct fear no-go trials relative to calm no-go trials by age group and sex. L = Left.

adults: p = 0.76). Rather, adolescent females showed greater activity in the mPFC, a region implicated in the regulation of avoidance-related behavior (fig. 3c; children vs. adolescents  $t_{15} = 2.53$ , p < 0.03 and adolescents vs. adults  $t_{17} = 2.65$ , p < 0.02). Males did not differ across age groups in this region (children vs. adolescents: p = 0.79 and adolescents vs. adults: p = 0.26).

### Discussion

Prior research has focused almost exclusively on how incentives and positive social cues lead to impulsive decisions during adolescence to help explain inflections in risk taking and criminal behavior during this period [3, 8, 25, 39]. The current study examined the effect of threat cues on impulse control and the underlying neural circuitry in adolescents. We found that just as positive cues can lead to more impulsive responses by adolescents relative to children and adults [3], so too can threat cues. This adolescent-specific inflection in false alarms to threat cues was paralleled by marked increases in limbic prefrontal (orbitofrontal and medial prefrontal) regions, implicated in regulating emotional and behavioral responses, particularly in the case of threat-related stimuli.

In contrast to the adolescent-specific effects in limbic prefrontal regions, prefrontal control circuitry implicated in detecting and resolving conflict between two competing responses showed an adolescent-emergent pattern [40– 42]. Specifically, activity in RIFG and RACC increased from childhood to adolescence and then plateaued. These findings are consistent with developmental studies showing that the ability to ignore irrelevant information on cognitive tests like the flanker and go/no-go tasks reaches maturity levels roughly by adolescence [16, 41, 43–45].

The difficulty of adolescents in suppressing attention and actions specifically toward negatively valenced information in the current study is a pattern that is emerging in the developmental literature [15, 16]. This diminished performance in adolescents is not observed in tasks demanding suppression of attention or actions toward neutral information [3, 16]. One explanation for the results reported here may be a failure of adolescents to withhold responses to any emotional stimuli [41]. However, recent work suggests that the actions of adolescents may be disrupted more easily by negative than positive emotional information [15] and differential patterns of activity have been shown for positive and negative emotional stimuli [3, 30]. Together these findings suggest that changes in behavior and limbic circuitry during adolescence coincide with a heightened sensitivity to emotional cues that may cause them to impulsively react rather than retreat from cues of potential threat.

Theoretical and empirical accounts for this diminished performance during adolescence fall along two lines of evidence. The first is evidence of regional brain development with lateral PFC continuing to reach structural and functional maturity throughout the adolescent years [3, 23] and the connections between subcortical and cortical structures continuing to strengthen [46, 47]. Given the role of the lateral PFC in the regulation of behavior, immature connections between it and limbic structures might reduce the capacity to exert cognitive control, particularly in emotionally salient contexts [15, 16]. The second line of evi-

Teens Impulsively React rather than Retreat from Threat

Ex. 1 Page 7 dence comes from neuroendocrinology studies, showing an influx of hormones during puberty thought to sensitize functional properties of certain brain circuits [19, 48, 49], potentially resulting in adolescent-specific enhanced signaling in limbic regions that are especially sensitive to hormonal changes. Thus the heightened recruitment of regulatory prefrontal circuitry when successfully suppressing attention to emotional cues may suggest an adolescentspecific hyper-responsiveness to emotional cues that requires greater recruitment of regulatory regions. Together, these observations suggest that diminished regulation of sensitized limbic circuits may heighten the detection of, and response to, salient social cues during adolescence, even when irrelevant for goal-directed behavior.

An elevated sensitivity or reaction to threat cues during adolescence may have important implications for understanding risky or criminal-related behaviors under a heightened sense of threat. These behaviors have been reported to be higher in males than females [50–52]. So how might the adolescent-specific behavioral and imaging findings relate to sex differences observed in real world behavior? Although there was no main effect of, or interaction with, sex in the 2-factor ANOVA, exploratory independent t tests revealed that males rather than females appeared to be driving the inflection in false alarms to threat cues during adolescence. Specifically, male adolescents made more false alarms than either male children or adults and showed a parallel increased activation pattern in the OFC when successfully inhibiting a response, a region implicated in the regulation of approach-related behavior. In contrast, female adolescents did not significantly differ from female children or adults in their performance or in activity in this region. Rather, they showed greater activity in the mPFC, a region implicated in regulation of avoidance-related behavior. Adolescent males did not significantly differ from children or adults in this region. These

exploratory results suggest a possible double dissociation between adolescent males and females in cortical limbic activity related to impulsively reacting and retreating from cues of potential threat, respectively, that warrants further investigation in a larger sample. In addition, a number of other factors, not specifically measured in this study, may have contributed to the observed age and sex differences such as discrepancies between the sexes in pubertal onset, pubertal stage and quality and/or lack of sleep.

The present study demonstrates that impulsive behavior during adolescence is as likely to occur in the presence of threat as reward cues. We show that rather than retreating or withholding a response to threat cues, adolescents are more likely than children or adults to impulsively react to them, even when instructed not to respond. This developmental pattern is mirrored by adolescentspecific changes in limbic cortical circuitry implicated in the detection and assignment of emotional value to inputs and in the subsequent regulation of responses to them [53–56]. Clearly more research will be required to specify the impact of threat cues on adolescent behavior. Nonetheless, these findings may have significant implications for conditions in which adolescents impulsively react and put themselves and others in harm's way.

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Teens Impulsively React rather than Retreat from Threat

Ex. 1 Page 9

## Exhibit 2

Ex.2 Page 10

### The Case for Gun Policy Reforms in America



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\*On February 5, 2014, this White Paper was updated to reflect a correction concerning the number of states that allow 18-20 year-olds to legally purchase handguns (page 5).



COVER PHOTO CREDIT: Photo used with permission of Garen J. Wintemute from Inside Gun Shows: What Takes Place When Everybody Thinks Nobody is Watching. Violence Prevention Research Group, Department of Emergency Medicine, University of California, Davis, 2009. © 2009 by Garen Wintemute, MD, MPH. All rights reserved. Published September 2009.

### The Burden of Gun Violence in the United States

More than 31,000 people a year in the United States die from gunshot wounds.<sup>1</sup> Because victims are disproportionately young, gun violence is one of the leading causes of premature mortality in the U.S. In addition to these deaths, in 2010, there were an estimated 337,960 non-fatal violent crimes committed with guns,<sup>2</sup> and 73,505 persons treated in hospital emergency departments for non-fatal gunshot wounds.<sup>3,4</sup>

Gun violence in the United States is unusually high for a nation of such wealth. Although there is little difference in the overall crime rates between the United States and other high-income countries, the homicide rate in the U.S. is seven times higher than the combined homicide rate of 22 other high-income countries.<sup>5</sup> This is because the *firearm* homicide rate in the U.S. is twenty times greater than in these other high-income countries. The higher prevalence of gun ownership and much less restrictive gun laws are important reasons why violent crime in the U.S. is so much more lethal than in countries of similar income levels.

There are enormous economic costs associated with gun violence in the U.S. Firearmrelated deaths and injuries resulted in medical and lost productivity expenses of about \$32 billion in 2005.<sup>6</sup> But the overall cost of gun violence goes well beyond these figures. When lost quality of life, psychological and emotional trauma, decline in property values, and other legal and societal consequences are included, the cost of gun violence in the U.S. was estimated to be about \$100 billion annually in 1998.<sup>7</sup> A new study has examined the direct and indirect costs of violent crime in eight geographically-diverse U.S. cities, and estimated the average annual cost of violent crime was more than \$1,300 for every adult and child. Because much of these costs are due to lowering residential property values, violent crime greatly reduces tax revenues that local governments need to address a broad array of citizens' needs. The direct annual cost of violent crime to all levels of government was estimated to be \$325 per resident.<sup>8</sup>

### Gun Control Policies in the United States

Debates about gun control often drift towards general arguments about whether guns make us safer or less safe, and gun control is equated with restricting gun ownership. However, with recent Supreme Court decisions overturning laws which ban firearm possession in the District of Columbia and Chicago, current gun control policies in the U.S. do not disarm lawabiding adults over the age of 21. Rather, gun control laws today focus on one or more of four general objectives. These laws aim to:

- 1. Define conditions that prohibit a person from possessing firearms;
- 2. Implement regulations to prevent prohibited persons from possessing firearms;
- 3. Restrict carrying of concealed firearms outside the home; and
- 4. Regulate the design of firearms to enhance public and personal safety.

Below, we draw upon research evidence to suggest how improvements in each of these types of gun policies could enhance public safety in the United States.

### Rationale for Current Conditions that Prohibit Firearm Possession

Federal law prohibits certain categories of individuals from possessing firearms, including: felons; fugitives; persons convicted of a misdemeanor crime for domestic violence; those who are subject to certain restraining orders for domestic violence; unlawful users of or those addicted to controlled substances; those who have been found by a judge to be mentally incompetent, a danger to themselves or others as a result of mental illness, or been involuntarily committed to a mental institution; those who have been dishonorably discharged from the military; illegal aliens; and persons who have renounced their U.S. citizenship. In addition, federal law sets 21 years as the minimum age at which a person can lawfully purchase a handgun from a federally licensed firearms dealer, but sets 18 as the minimum legal age for handgun possession and for transfers of handguns from anyone who is not a licensed gun dealer.<sup>9</sup>

Most of these categories of persons prohibited from possessing firearms can be justified based on data indicating increased risk for violence. Individuals with prior felony convictions are far more likely to commit future crimes of violence than non-felons. A history of perpetrating intimate partner violence (IPV) is associated with increased risk of subsequent murder of an intimate partner, and a perpetrator's ownership of a firearm increases the risk of domestic homicide five-fold for victims.<sup>10</sup> Several studies indicate that a significant proportion of domestic violence abusers also commit serious offenses against strangers and non-family members.<sup>11,12,13,14</sup>

Firearm prohibitions for drug abusers are also justified. Substance abuse is associated with increased risk of domestic violence,<sup>15,16,17,18</sup> and incarceration for violent crime,<sup>19</sup> as well as suicide.<sup>20,21</sup> Homicide offenders are nearly five times more likely to abuse drugs than non-offender controls.<sup>22</sup> Although the majority of persons with mental illnesses are not violent,<sup>23,24</sup> and only a small portion of violence is attributable to mental illness alone,<sup>25</sup> persons with serious mental illnesses, such as schizophrenia, bipolar disorder, and major depression, are more likely to commit violence against others and themselves than are individuals who do not have these disorders.<sup>26,27, 28</sup>

Minimum age restrictions for firearm possession are prudent because the risk of perpetrating or being victimized by serious violent crimes increases rapidly during adolescence and in the early 20s.<sup>29, 30</sup> (See Figure below.) Brain structures related to risk-taking and impulse-control are developing throughout adolescence, and this may contribute to heightened risk of violent behavior among this age group.<sup>31,32</sup>

### Why Firearms Prohibitions for High-Risk Persons Should be Broadened

### Criminal Prohibitions

In addition to the exclusion criteria for firearm possession under federal law, many states have additional disqualifications for legal firearm possession. The differences in exclusion criteria across states are significant. For example, New Jersey prohibits firearm possession by anyone who has been convicted of a crime for which the penalty can be 6 months or more of imprisonment, and sets the minimum legal age for handgun possession at 21 years. In contrast, 13 states have standards for legal firearm possession that either mirror or are weaker than federal standards.

Most people believe that criminals should not be able to possess firearms lawfully. Yet, our current laws permit many people who have been convicted of crimes—most misdemeanor crimes adjudicated in adult court and felony crimes handled in juvenile court—to possess firearms. Data from two studies of individuals who have committed the most serious crimes indicate that prior to committing these crimes, the perpetrators were not prohibited from possessing a firearm under federal law. A recent study, based on surveys of inmates in state prisons, examined the criminal history and ages of persons imprisoned for committing crimes with a gun in the 13 states with standards for legal gun ownership that do not go beyond those set in federal law. At the time when they committed the gun crime leading to their incarceration, only 27 percent of these gun offenders were prohibited from possessing firearms because they had previously been convicted of a felony. Of these offenders, 60 percent could legally possess guns prior to committing the gun crime that led to their incarceration, including four percent who had prior misdemeanor convictions involving violence and/or firearms, six percent convicted of other misdemeanors, five percent convicted of a felony in a juvenile court, and 13 percent with prior arrests but no convictions.<sup>33</sup>

Some may assume that persons convicted of misdemeanor crimes do not pose a significant threat for committing serious violent crimes. But many suspects charged with felony crimes are convicted of lesser charges as a result of a plea agreement. Research has shown that misdemeanants who were legally able to purchase handguns committed crimes involving violence following those purchases at a rate two to ten times higher than that of handgun purchasers with no prior convictions.<sup>34</sup> Handgun purchasers with a history of arrest but no convictions have an equally high or higher risk of committing violent crimes following handgun purchases as do misdemeanants who legally purchased a handgun.<sup>35</sup>

We believe the evidence above justifies an extension of firearm prohibitions for persons with a history of criminal behavior to include persons convicted of all misdemeanor crimes of violence, as well as individuals who have committed felony crimes as a juvenile. Such prohibitions do not necessarily need to be life-long. Many states have laws prohibiting firearm possession for individuals convicted of serious crimes as juveniles. These restrictions are timelimited, based on either the age of the individual or the number of years since the prohibiting conviction.

### Substance Abusers

Federal law prohibits firearm possession by anyone who is addicted to illegal drugs, but regulations written to implement the law provide a relatively narrow definition of who would be prohibited. A person would be determined to be prohibited if he has "a conviction for use or possession of a controlled substance within the past year; multiple arrests for such offenses within the past 5 years, if the most recent arrest occurred within the past year; or … [is] found through a drug test to use a controlled substance unlawfully, provided the test was administered within the past year."<sup>36</sup> The number of drug abusers prohibited from possessing firearms might

be increased significantly by revamping these regulations to, for example, expand the period following a drug conviction for which a person is prohibited from possessing firearms.

Expanding firearm prohibitions to include persons who are alcoholics or problem drinkers could potentially reduce alcohol-related violence. Alcohol abuse is strongly associated with the perpetration of violence. Yet federal firearm laws do not prohibit alcoholics from possessing firearms, and only 16 states have statutes prohibiting alcohol abusers from possessing firearms. Furthermore, some states with gun prohibitions for alcohol abusers lack regulations to allow authorities to enforce the prohibition.<sup>37</sup>

### Youth Under Age 21

Restrictions on youths' ability to purchase and possess firearms should be broadened. Although federal law and most state law allows youth 18 to 20 years of age to legally possess a handgun, youth of these ages have some of the highest rates of homicide offending. Age-specific homicide offending rates rise sharply in the late teens and peak at age 20.<sup>38</sup> (See figure below.) In an examination of the background and legal status of gun offenders incarcerated in the 13 states with the weakest standards for legal firearm ownership, the largest segment of offenders who would have been prohibited in other states with stricter standards were those who were between 18 and 20 years of age.<sup>33</sup> Heightened risk-taking, and concerns for protecting youth and the public from alcohol abuse resulted in laws in all 50 states, establishing 21 as the minimum legal age for alcoholic beverage consumption. These laws led to significant reductions in deaths from motor vehicle crashes involving drivers ages 18-20.<sup>39</sup> Yet, \*thirty-eight states allow 18- to 20-year-olds to legally possess as many handguns as they desire.



### Homicide offending per 100,000 population in 2009 for specific ages of offenders.\*

<sup>\*</sup> Data from the Supplemental Homicide Reports, Uniform Crime Reporting System, Federal Bureau of Investigation, U.S. Department of Justice.

\*On February 5, 2014, this White Paper was updated to reflect the following correction: Thirty-eight states allow18-20 year olds to legally possess as many handguns as they desire. An earlier version incorrectly reported forty-five states allowed 18-20 year olds to legally possess as many handguns as they desire.

### Regulating Gun Sales Can Prevent Diversion of Guns to Criminals and Underage Youth

### The Brady Law – Necessary But Insufficient

Central to effective gun policy is being able to identify higher-risk, prohibited persons attempting to buy guns, and to prevent those purchases. The Brady Law is the foundation for the federal government's attempt to achieve this objective. Before the Brady Law, "gun control" within many states worked on the honor system. Firearm purchasers simply completed a form indicating whether they met any of the exclusion criteria for legal firearm possession, without independent verification of the information provided. With the passage of the Brady bill, gun purchasers buying from a federally licensed firearm dealer are subject to a background check. Since the Brady Law was enacted in 1994, more than 2 million applications to purchase or transfer firearms were denied because the applicant was prohibited from purchasing firearms.<sup>40</sup> Some unknown, but likely larger number of prohibited individuals did not attempt to purchase a firearm because they were legally prohibited.

But the Brady Law only requires prospective purchasers to pass a background check if they are purchasing the firearm from a licensed firearm dealer. Data from a nationally representative sample of gun owners indicate that 40 percent of firearm acquisitions are from individuals who are not licensed gun dealers. Not surprisingly, criminals exploit the private sales loophole. Data from a national survey of inmates indicated that nearly 80 percent of those who had used a handgun in a crime had acquired it through a transaction with an individual who was not a licensed gun dealer. An advocate for closing the private sale loophole<sup>†</sup> once likened current federal gun policy to an airline security system which offers passengers a choice between submitting oneself to our current screening system, or side-stepping it, and boarding with whatever you would like to bring on board. Should we expect gun laws with the private-sale purchaser screening loophole to be any more effective than voluntary airline passenger screening?

A widely cited study evaluated the impact of the Brady Law and concluded that the law did not affect homicide and suicide rates.<sup>41</sup> The ability of the Brady Law to reduce homicides and suicides is certainly diminished by the giant loophole for private firearm sales that criminals and traffickers exploit. We believe that the Brady Law should be viewed as a necessary foundation for federal gun control laws, but that it is insufficient for achieving the goal of preventing prohibited persons from accessing firearms.

### Federal Regulations of Retail Gun Sellers are Weak, Seller Protections are Broad

Any effective system of gun control must have adequate regulation and oversight of those who could reap substantial profits by evading the laws. Unfortunately, Congress has repeatedly weakened regulation, oversight, and accountability of federally licensed gun dealers. The Firearm Owners Protection Act of 1986 reduced penalties for gun sales law violations, increased standards of evidence for successful prosecution of gun sellers, and limited the Bureau of Alcohol, Tobacco and Firearms (ATF) compliance inspections. Federal gun policies adopted first

<sup>&</sup>lt;sup>†</sup> Jeri Bonavia, Executive Director, Wisconsin Anti-Violence Effort.

in 2003, and known as the Tiahrt amendments, after the legislation's sponsor, Rep. Todd Tiahrt (R-KS), limit public access to crime gun trace data, prohibit the use of gun trace data in hearings pertaining to licensure of gun dealers and litigation against gun dealers, and restrict ATF's authority to require gun dealers to conduct a physical inventory of their firearms. Another act of Congress, The Protection of Lawful Commerce in Arms Act of 2005 provides broad protections from lawsuits against firearm manufacturers and retail sellers.<sup>42</sup>

Available research shows the harms of policies which inadequately hold gun sellers accountable for dangerous and illegal practices. In a national survey of armed criminals, illegal purchases from licensed gun dealers (e.g., no background check conducted) were as common as were legal purchases from licensed gun dealers.<sup>43</sup> Data from federal gun trafficking investigations indicate that scofflaw gun dealers are the most important channels for diverting guns to traffickers and criminals.<sup>44</sup> Phone surveys of gun dealers reveal that many are willing to bend or break the law to make a sale.<sup>45,46</sup> Findings from a study of Chicago's underground illegal gun markets found that certain retailers, set up just across the city's border, colluded with traffickers to funnel large numbers of guns to gang members.<sup>47</sup>

### Better Regulation and Oversight of Gun Sellers Reduces Diversions of Guns to Criminals

Diversion of guns to criminals shortly following retail sales is much less common in states that license retail gun sellers, require careful record keeping that can be reviewed by local or state law enforcement, and where law enforcement agencies conduct regular compliance inspections.<sup>48</sup> Undercover stings to catch retailers facilitating illegal sales, followed by lawsuits against scofflaw gun dealers, also deter the diversion of guns to criminals.<sup>49,50</sup>

The case of a large gun shop near Milwaukee demonstrates how accountability measures, or the lack thereof, can have a dramatic effect on the diversion of guns to criminals. In 1999, a report was released that indicated that Badger Guns & Ammo had sold more guns that were later traced to crime than any other retail gun seller in the nation. Within days of the report being publicized in the news, the gun dealer voluntarily changed his sales practices. Abruptly after this change, the diversion of guns to criminals in Milwaukee within a year of being sold by the dealer dropped by 73 percent.<sup>51</sup> The enactment of the Tiahrt amendments, which, among several protections for gun sellers, prevented the release of data connecting gun shops to crime guns, was associated with a 204 percent increase in the diversion of guns to criminals soon after sales by Badger Guns & Ammo.<sup>52</sup>

Weaknesses in U.S. gun laws may cause skepticism about whether gun control can work. Yet, a growing body of research shows that common-sense policies adopted at the state and local level succeed in reducing the diversion of guns to criminals. A study using crime gun trace data from 54 U.S. cities examined the association between gun sales regulations and the diversion of guns to criminals. Strong regulation and oversight of licensed gun dealers, regulation of gun sales by private sellers, and permit-to-purchase licensing systems (which require potential gun purchasers to apply for a license directly with a law enforcement agency, where they are typically photographed and fingerprinted) were each associated with significantly fewer guns that were diverted to criminals.<sup>48</sup> A systematic observational study of gun sales at gun shows found anonymous undocumented firearms sales to be ubiquitous, and illegal straw sales more than six times as common in states that do not regulate private sales, compared with California,

which does regulate such sales.<sup>53</sup> Separate research shows that states which do not regulate private gun sales, adopt permit-to-purchase licensing systems, or have gun owner accountability measures, like mandatory reporting of gun thefts, export significantly more guns used by criminals to other states that have constrained the supply of guns for criminals by adopting strict gun sales regulations.<sup>54,55</sup> Broad adoption of these policies could greatly enhance our ability to keep guns from those most likely to use them in crime.

A common response to calls for stricter gun control laws from opponents of reform is that there is no need to change our gun laws; we just have to "enforce the laws on the books." But we do not have to choose between needed reforms and better enforcement. Effective enforcement of gun control laws can deter illegal gun trafficking,<sup>56</sup> but loopholes, high standards of evidence, and weak penalties make it difficult to enforce laws designed to keep guns from prohibited persons. Stronger gun laws will lead to better enforcement of those laws.

### Firearm Prohibitions for High-Risk Groups Reduce Violence and Save Lives

There has been very little research of high scientific quality that directly examines whether laws prohibiting individuals in high-risk groups from purchasing or possessing firearms reduce criminal offending by prohibited individuals.<sup>57,58</sup> One study examined the impact of a California law that expanded firearm prohibitions to include persons convicted of misdemeanor crimes of violence. A study of legal handgun purchasers in California before and after the law found that denial of firearm purchase applications by violent misdemeanants was associated with lower rates of violence by this high-risk group.<sup>59</sup>

Federal law and laws in many states prohibit firearm possession by individuals who were either previously convicted of a misdemeanor for domestic violence, or currently subject to a restraining order sought by a current or former intimate partner. Enforcement of these laws appears to be spotty,<sup>60, 61</sup> and could be improved through proactive efforts to disarm prohibited IPV offenders.<sup>62</sup> Nevertheless, evaluations have found that laws prohibiting firearm possession by persons restrained by court-issued protective orders for victims of domestic violence reduce domestic homicides; however, there was no measurable effect of laws prohibiting persons convicted of misdemeanors for domestic violence.<sup>63,64</sup>

Youth under age 18 are forbidden by federal law from purchasing or possessing handguns.<sup>65</sup> Most states have enacted their own laws setting a minimum legal age for handgun purchase or possession, usually at age 18. Only five have set the minimum age for handgun possession at 21. Evaluations have failed to find any beneficial effects of these laws on either juvenile homicide victimization or youth suicide.<sup>66, 67</sup> But prior studies have not examined the most direct outcome, violent crime perpetration by the restricted ages. Another type of age-based firearm restriction are so-called "child access prevention" (CAP) laws requiring gun owners to store their firearms so that children and teens cannot easily access firearms unsupervised. Studies have found CAP laws to be effective in reducing accidental shootings of children by as much as 23 percent,<sup>68,69</sup> and suicides of adolescents by 8 percent.<sup>70</sup>

### Right-to-Carry Laws Do Not Make Us Safer and Likely Increase Aggravated Assaults

So-called right to carry (RTC) laws allow individuals who are not legally proscribed from possessing firearms to carry concealed weapons in public, either by making it easy to get a permit to do so, or by not requiring such permits at all. Arguments for RTC laws are premised on the idea that everyone who is eligible to legally own a firearm is law-abiding, and is at low risk for committing a violent crime. Research cited above concerning weak standards for legal firearm ownership calls this into question. A recent review of concealed carry permit holders in North Carolina examined criminal offending in the group over a five-year period. During that period, more than 2,400 permit holders were convicted of crimes (excluding traffic violations), including more than 200 felonies and 10 murders or manslaughters. An additional 900 had been convicted of a drunk driving offense, an offense commonly associated with substance abuse.<sup>71</sup>

A large body of research has been conducted to investigate the effect of RTC laws on violence. Most notably, research led by John Lott, Jr. suggests that RTC laws have led to significant reductions in violent crime.<sup>72</sup> But the research showing crime-reducing effects of RTC laws, including Lott's, has been carefully reviewed by a National Council of Research panel of experts, and others, and has been found to have serious flaws.<sup>73,74,75</sup> The most consistent finding across studies which correct for these flaws is that RTC laws are associated with an increase in aggravated assaults.<sup>76</sup> Using various statistical methods, estimates range from a one to nine percent increase in aggravated assaults as a result of RTC laws.<sup>77,78</sup>

### **Regulating the Design of Guns Can Save Lives**

Not all firearms are created equal. One characteristic of guns that is relevant to public safety, particularly in regard to mass shootings, is ammunition capacity. Large capacity magazines (LCM), typically defined as holding more than 10 rounds of ammunition, increase the number of rounds someone can fire without stopping to reload. An assault weapon is generally defined as a civilian version of a military style weapon. Assault weapons are typically capable of accepting LCMs.

Assault weapons and LCMs are common characteristics of guns discussed in policy debates because they are disproportionately used in mass shootings. Mass shootings involving assault weapons typically involve more victims per incident than mass shootings with other weapons.<sup>79</sup> Recent examples of firearms with LCM being used in mass shootings include Jared Lee Loughner's use of a Glock 9mm semi-automatic pistol, with a magazine holding 33 rounds of ammunition, to murder 6 and wound 13 others, including Congresswoman Gabrielle Giffords, in January 2011. The suspect in the mass shooting at a movie theater in Aurora, Colorado that left 12 dead and 58 injured used an assault rifle with a 100-round magazine. Weapons with LCMs were also used in the mass shootings at Virginia Tech University and Fort Hood, Texas. It is impossible to determine if these and other perpetrators of mass shootings would have been able to acquire assault weapons or LCMs had the weapons and magazines been banned. But if the perpetrators had used firearms that were not equipped with LCMs, it seems very likely that fewer people would have been injured and killed.

In September, 1994, a federal law went into effect which banned the manufacture, transfer and possession of certain assault weapons and all ammunition magazines that held more than10 rounds; but the law expired in 2004. The ban had important limitations. It allowed "copy cat" assault weapons with only slight differences from banned models, it allowed the import of rifles that could accept LCM, and, most importantly, it allowed for the "grandfathering" of assault weapons and LCMs manufactured before the effective date of the law. In contrast, the Australian government developed a process for the government to buy banned weapons from citizens when that country banned semi-automatic and pump-action rifles and shotguns in response to a mass shooting. In the decade following enactment of the policy, there was not a single mass shooting, and declines in homicide rates accelerated.<sup>80</sup>

Criminologist Christopher Koper led a study to evaluate the federal assault weapon and LCM ban which revealed the limitation of the law's impact.<sup>81</sup> Just prior to the ban going into effect, production of assault rifles and assault pistols surged. Nevertheless, the percentage of crime guns recovered by police which were assault weapons dropped 70 percent between 1992-1993 (just prior to the ban) and 2001-2002. But assault weapons accounted for only 6 percent of all crime guns prior to the ban. For assaults with LCMs, which were used in 13 to 20 percent of gun crimes in selected cities, there was no detectable change in criminal use following the ban. (In contrast, a separate study of firearms recovered by police in Virginia found that the percentage of firearms with LCMs dropped sharply following the federal ban of LCMs, and then rebounded when the ban expired.<sup>82</sup>) Koper and colleagues found no detectible effects of the law on gun violence. The researchers attribute this negative finding of the LCM ban's effect to several factors, including the wide availability of grandfathered LCMs, their relatively low cost, and criminals' high demand for LCMs.

The finding of this study underscores the need to be realistic about the likely impact of an assault weapons or LCM ban. Ammunition capacity of 10 or more rounds becomes relevant in only a small percentage of shootings. A study of shootings in Jersey City, NJ found that 10 or more rounds were fired in only 4.7 percent of the incidents.<sup>83</sup> Koper indicated that the lack of statistically significant findings does not mean that the law did not prevent a small percentage of the over 10,000 firearm-involved homicides and nonfatal woundings of nearly 50,000 people annually. Even if the ban eventually prevented only 1 of every 5 of the five percent of incidents in which LCM are relevant, that would translate into about 100 fewer homicides and 500 fewer people wounded by gunshots per year. Such effects would not be definitively detectable with national data, but would be nonetheless meaningful, given the magnitude of the problem. Further, focusing solely on the "body count" glosses over the considerable psychological trauma and other social costs resulting from mass shootings. We have decided to regulate the design of numerous consumer products, such as cribs and small, high-powered magnets, in order to prevent far fewer deaths than could be prevented with a ban of LCMs. Opponents of such bans do not have a compelling reason why law-abiding citizens need to have firearms with unlimited ammunition capacity.

Aside from ammunition capacity, other characteristics of firearms that are relevant to public safety include how easily the gun can be concealed, and how prone it is to misfire or fire unintentionally. Concealability and a tendency to misfire or fire unintentionally are two of the characteristics that define what some refer to as "junk guns" or "Saturday night specials." These

weapons are over-represented among crime guns after controlling for handgun purchaser demographics, gun dealer characteristics, and sale conditions.<sup>84</sup> Within a year following one gun dealer's decision to stop selling junk handguns, the number of guns sold by the dealer that were linked to crime showed a 73 percent reduction.<sup>48</sup> When Maryland banned the sale of junk handguns, researchers found that such guns were much less likely to be used in crime in Baltimore than in other cities,<sup>85</sup> and that the enactment of the law was associated with an estimated 8 to 11 percent reduction in gun homicides.<sup>86</sup>

Although unintentional or accidental shootings account for a small share of firearmrelated mortality and morbidity, these deaths and injuries are highly preventable through proper design of firearms. Some of these incidents occur because inexperienced gun handlers, often children, do not realize that a gun is loaded, or that a pistol can have a round loaded in the chamber to fire even after the ammunition clip is removed. Unintentional shootings of this type can be prevented by magazine safety disconnect devices and loaded chamber indicators, relatively inexpensive safety features already available on some handguns.<sup>87</sup> Guns can also be designed so that they cannot be fired by unauthorized users, and thus, prevent unintentional and self-inflicted shootings by underage youth, as well as some crimes committed with stolen guns.<sup>88</sup>

### There is Broad Public Support for Many Needed Reforms to Our Gun Laws

Much has been made of an apparent drop in public support for gun control in recent years. Much of this is based on findings from Gallup polls, in which respondents are asked a very general question about whether gun laws should be made stricter, less strict, or kept as they are. This may be a reasonable barometer of respondents' general attitudes toward guns and government regulation, but it tells us little about what specific policies people believe are in place, and which policies citizens support. A recent survey of gun owners found that more than half of respondents believed erroneously that background checks are required for all gun sales. In reality, most states limit background check requirements to persons purchasing firearms from a licensed gun dealer. This survey of gun owners also found: 1) 82 percent favored mandatory background checks for all firearms sales, not just for those by licensed dealers, 2) 68 percent supported laws mandating reporting of gun thefts, and 3) broad support of stricter standards for issuing permits to carry concealed firearms than are in place in most states.<sup>‡89</sup> Another poll from 2011 on specific gun policies found broad public support for a number of measures which either expand current prohibitions for potentially dangerous people (e.g., people on terrorist watch list, persons arrested for drug crimes), or enhance accountability, so that prohibited persons cannot access firearms. Although there is relatively less support for creating a national registry for all gun owners and their guns, and for banning high-capacity ammunition magazines, such policies still garner the support of 66 percent and 58 percent of all adults, respectively, along with a plurality of gun owners.<sup>90</sup> We suspect that support for adopting these reforms would be even greater if more people knew the facts about weaknesses in current gun laws, and the effectiveness of regulations that most would consider reasonable.

<sup>&</sup>lt;sup>‡</sup> Support for specific restrictions on issuing of concealed carry permits if applicant is: younger than 21 (69%), has been arrested for domestic violence (73%), been convicted of a misdemeanor crime of violence (78%), has not completed safety training for carrying concealed firearms (80%).

### **Conclusion**

The burden of gun violence on American society is substantial, whether measured in years of productive life lost, disability, fear, or economic costs. The toll is unprecedented among high-income nations. Weaknesses in current gun laws contribute to this burden by establishing low standards for legal gun ownership and significant loopholes in policies designed to keep guns from prohibited persons. When states expand firearm prohibitions to high-risk groups, and adopt comprehensive measures to prevent diversion of guns to prohibited persons, fewer guns are diverted to criminals, and there is less violence.

Some mistakenly believe that the Second Amendment to the U.S. Constitution would prohibit the kinds of legal reforms we believe are warranted. In 2008, in *District of Columbia v. Heller*,<sup>91</sup> the U.S. Supreme Court ruled that the Second Amendment protected an individual right to own guns, striking down Washington, D.C.'s law banning handgun possession in the home. However, the *Heller* decision also mentioned numerous types of presumptively valid gun laws, including "laws imposing conditions and qualifications on the commercial sale of arms." Since *Heller*, lower courts have overwhelmingly upheld the constitutionality of a wide range of gun laws other than handgun bans.<sup>92</sup>

Contrary to recent media reports, a large majority of the public, including gun owners, favors remedying many current weaknesses in our gun laws. There are real political hurdles to enacting new gun control laws, and the power of the gun lobby is substantial. But politicians who want to correct flaws in our current laws, which enable dangerous people to get guns, could do so knowing that there is broad support for those policies, the reforms are constitutional, and the policies would enhance public safety.

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## Exhibit 3

Ex.3 Page 30

## Absolute Risk of Suicide After First Hospital Contact in Mental Disorder

Merete Nordentoft, MD; Preben Bo Mortensen, MD; Carsten Bøcker Pedersen, MD

**Context:** Estimates of lifetime risk of suicide in mental disorders were based on selected samples with incomplete follow-up.

**Objective:** To estimate, in a national cohort, the absolute risk of suicide within 36 years after the first psychiatric contact.

**Design:** Prospective study of incident cases followed up for as long as 36 years. Median follow-up was 18 years.

**Setting:** Individual data drawn from Danish longitudinal registers.

**Participants:** A total of 176 347 persons born from January 1, 1955, through December 31, 1991, were followed up from their first contact with secondary mental health services after 15 years of age until death, emigration, disappearance, or the end of 2006. For each participant, 5 matched control individuals were included.

**Main Outcome Measures:** Absolute risk of suicide in percentage of individuals up to 36 years after the first contact.

**Results:** Among men, the absolute risk of suicide (95%

confidence interval [CI]) was highest for bipolar disorder, (7.77%; 6.01%-10.05%), followed by unipolar affective disorder (6.67%; 5.72%-7.78%) and schizophrenia (6.55%; 5.85%-7.34%). Among women, the highest risk was found among women with schizophrenia (4.91%; 95% CI, 4.03%-5.98%), followed by bipolar disorder (4.78%; 3.48%-6.56%). In the nonpsychiatric population, the risk was 0.72% (95% CI, 0.61%-0.86%) for men and 0.26% (0.20%-0.35%) for women. Comorbid substance abuse and comorbid unipolar affective disorder significantly increased the risk. The co-occurrence of deliberate self-harm increased the risk approximately 2-fold. Men with bipolar disorder and deliberate self-harm had the highest risk (17.08%; 95% CI, 11.19%-26.07%).

**Conclusions:** This is the first analysis of the absolute risk of suicide in a total national cohort of individuals followed up from the first psychiatric contact, and it represents, to our knowledge, the hitherto largest sample with the longest and most complete follow-up. Our estimates are lower than those most often cited, but they are still substantial and indicate the continuous need for prevention of suicide among people with mental disorders.

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LL MENTAL DISORDERS ARE associated with increased risk of suicide,1-6 and this risk is often reported as the increased relative risk or odds ratio for death by suicide by people with mental disorders who have contact with health services compared with those who do not. The absolute risk of death by suicide, often mentioned as lifetime risk of suicide after the onset of mental disorders, can be estimated as the percentage of a cohort expected to die by suicide before extinction. Although no studies have actually conducted lifetime follow-up, lifetime risk is mentioned in many scientific papers7,8 and textbooks.9 It has been estimated to be high, but these estimates have never previously been based on a large national sample with a prospective long-

term follow-up. One of the most cited reports is the 1977 review conducted by Miles.<sup>7</sup> This review estimated that 15% of persons affected with unipolar affective disorder would die by suicide, as well as 15% of persons with alcoholism and 10% of persons with schizophrenia. However, this review was based on rather small studies with selected samples and a rather short followup, and several authors later concluded that, for different reasons, Miles' estimates were most likely too high.<sup>10-14</sup> Later meta-analyses,<sup>12-14</sup> based on more sophisticated statistical methods and including some large long-term follow-up studies, found clearly lower figures. Inskip et al14 estimated the lifetime risk to be 6% for affective disorder, 7% for alcohol dependence, and 4% for schizophrenia. Bostwick and Pankratz<sup>12</sup> estimated the risk to be 4% for patients hospitalized for affective disorders and 8.6% for those hospitalized for affective disorder and suicidality. Palmer et al<sup>13</sup> estimated the lifetime risk to be 5.6% for schizophrenia. Recently, Dutta et al<sup>5</sup> estimated the lifetime risk of suicide to be 3.23% for patients 20 years after the first psychotic diagnosis.

Although the lifetime risk of suicide has been reported in many studies, most estimates were based on incomplete follow-up in selected samples or were based on rather short-term follow-up of patients with firsttime treated mental disorders. In addition, the lifetime morbid risk is not well defined from an epidemiological viewpoint. We will estimate the absolute risk of committing suicide within 36 years after the first onset of the disorder, using competing risks Cox regression to account for censoring emigration and death from other causes. Omitting such censoring will bias the estimated cumulative incidences upward. By using competing risks survival analyses,15 the absolute risks of suicide (or cumulative incidences) can be calculated as the percentages of persons in the population who had committed suicide at a given time since onset of the disorder of interest, taking into account that people may migrate or die of other causes.

For any dynamic population, the cumulative incidence of suicide is the best possible estimate of longterm absolute risk of suicide. We were able to use the unique Danish registers to estimate absolute cumulative risk of suicide for different mental disorders and to include a complete national sample of persons born after 1955 with follow-up to 51 years of age.

#### **METHODS**

#### STUDY POPULATION

The Danish Civil Registration System<sup>16</sup> was established in 1968, and all persons who are alive and living in Denmark are registered. Among many other variables, it includes information on personal identification number, sex, and date of birth; continuously updated information on vital status; and the personal identification number of parents. The personal identification number is used in all national registers, which enables accurate linkage between registers. Our study population included all persons born in Denmark from January 1, 1955, through December 31, 1991 (2.46 million people). A cohort of 176 347 persons who came into contact with secondary mental health services for the first time and 881 735 controls without any contact with mental health services were followed up prospectively for a maximum of 36 years, from 15 years through as old as 51 years (median follow-up, 18 years).

#### ASSESSMENT OF SUICIDE AND MENTAL ILLNESS

The study population was linked with the Danish Registers of Causes of Death<sup>17</sup> to obtain information about any history of suicide (codes 950-959 from the *International Statistical Classification of Diseases, 8th Revision [ICD-8]*, or codes X60-X84 from the *International Statistical Classification of Diseases, 10th Revision [ICD-10]*) and date of suicide, if any. The registry contains information for all residents who died in Denmark from 1970 through 2006. In Denmark, the legal regulation of death certification states that any case of sudden and unexpected death

shall be reported to the police, and the death certificate may only be issued after a medicolegal examination.

The study population was also linked with the Danish Psychiatric Central Register<sup>18</sup> to obtain information about mental illness. The Danish Psychiatric Central Register was computerized in 1969 and contains data on all admissions to Danish psychiatric inpatient facilities; from 1995, information on outpatient visits to psychiatric departments was included in the register. The register currently includes data on approximately 630 000 persons and 2.7 million contacts. From 1969 through 1993, the diagnostic system used was the Danish modification of the ICD-8,19 and from 1994, the ICD-10.20 Cohort members were categorized with a history of schizophrenia (ICD-8 code 295 or ICD-10 code F20), schizophrenialike psychoses (ICD-8 codes 297, 298.39, and 301.83 or ICD-10 codes F21-F29), bipolar affective disorder (ICD-8 codes 296.19 and 296.39 or ICD-10 codes F30 and F31), unipolar affective disorder (ICD-8 codes 296.09, 296.29, 296.89, 296.99, 298.09, 298.19, 300.49, and 301.19 or ICD-10 codes F32-F34, F38, and F39), substance abuse (ICD-8 codes 291, 294.30, 294.38, 303, and 304 or ICD-10 codes F10-F19), anorectic disorder (ICD-8 code 306.50 or ICD-10 code F50.0), and any mental illness (any ICD-8 or ICD-10 code) if they had been admitted to a psychiatric hospital or had been in outpatient care with one of these diagnoses. For each mental disorder, the date of onset was defined as the first day of the first contact (inpatient or outpatient) with the diagnosis of interest. The National Hospital Register was established in 1977, and information about all admissions to public hospitals in Denmark was prospectively recorded. Since 1995, outpatient visits were also registered. Because some patients with substance abuse disorders are treated only in somatic departments, we decided to include patients in the National Hospital Register<sup>21</sup> who had a diagnosis of substance use disorders (ICD-8 codes 291, 294.30, 294.38, 303, and 304 or ICD-10 codes F10-F19).

Identifying deliberate self-harm in Danish registers is rather complicated because procedures have changed, and some procedures are not well complied with. We have identified deliberate self-harm in the different periods with different algorithms. From 1977 to 1986, deliberate self-harm was identified as persons with the diagnoses classified in *ICD-8* codes E9500 through E9599 in the National Hospital Register or Danish Psychiatric Central Register. From 1987 to 1993, deliberate selfharm was identified as persons admitted with a "reason for contact code" of 4 in the National Hospital Register. After 1994, suicide attempts were identified as people fulfilling at least 1 of the following criteria in the National Hospital Register or Danish Psychiatric Central Register:

1. Reason for contact code of 4;

2. Any psychiatric diagnosis (*ICD-10* chapter F) and a comorbid diagnosis of poisoning with medication and biological compounds (*ICD-10* codes T36 through T50) or nonmedical compounds, excluding alcohol and poisoning from food (T52 through T60);

3. Any psychiatric disorder (*ICD-10* chapter F) and comorbid diagnosis reflecting lesions on the forearm, wrist, or hand (*ICD-10* codes S51, S55, S59, S61, S65, or S69);

4. Any contact with a hospital because of poisoning with weak or strong analgesics, hypnotics, sedatives, psychoactive drugs, antiepileptics, and antiparkinsonian drugs or carbon monoxide (*ICD-10* codes T39, T42, T43, and T58); and

5. Any somatic or psychiatric diagnosis X60 through X84.

The classification of deliberate self-harm was identical to that used previously.<sup>22,23</sup>

Analyses of deliberate self-harm were only possible beginning in 1977; therefore, the follow-up for these analyses is no longer than 30 complete years.

#### Table 1. Cumulative Incidence of Suicide Up to 36 Years After First Psychiatric Contact<sup>a</sup>

	Men			Women		
Disorder	No. of Suicides	No. Followed Up	Cumulative Incidence, % (95% CI)	No. of Suicides	No. Followed Up	Cumulative Incidence, % (95% CI)
Schizophrenia	422	10213	6.55 (5.85-7.34)	163	5796	4.91 (4.03-5.98)
Schizophrenialike disorders	413	11 798	5.90 (5.21-6.67)	236	9739	4.07 (3.28-5.04)
Bipolar affective disorder	97	2571	7.77 (6.01-10.05)	78	3356	4.78 (3.48-6.56)
Unipolar affective disorder	417	17 362	6.67 (5.72-7.78)	292	28 871	3.77 (3.05-4.66)
Substance abuse at psychiatric department	804	30 626	4.71 (4.24-5.23)	233	13 469	3.34 (2.80-3.98)
Substance abuse at somatic hospital	672	56 351	2.54 (2.20-2.93)	202	27 370	1.71 (1.40-2.09)
Anorectic disorder <sup>b</sup>	3	145	5.61 (1.46-21.65)	25	3559	2.62 (1.08-6.38)
Any mental illness	1679	80 621	4.33 (3.92-4.77)	740	95 726	2.10 (1.86-2.37)
No mental illness	747	403 105	0.72 (0.61-0.86)	199	478 630	0.26 (0.20-0.35)

Abbreviation: CI, confidence interval.

<sup>a</sup> Population includes the 2.46 million people born in Denmark from January 1, 1955, through December 31, 1991, and followed up from 15 years of age to 2006. Cumulative incidence measures the percentage of persons in the population who had committed suicide within 36 years after onset of the disorder of interest, taking into account that people may migrate or die of other causes.

<sup>b</sup>Cumulative suicide risk for men with anorectic disorder was based on only 3 suicides.

#### STUDY DESIGN AND STATISTICAL ANALYSIS

For each mental disorder, cohort members were followed up from their first hospital contact as inpatients or outpatients after 15 years of age until suicide, death from other causes, emigration from Denmark, disappearance, or December 31, 2006 (whichever came first). Because we aimed to study suicidal behavior among adolescents, we excluded from the analyses individuals who had their psychiatric disorder before 15 years of age. By selecting only persons born in 1955 and later, we ensured that the cohort consisted of incident cases, as the number of persons who had their first hospital contact owing to one of the mental disorders of interest before 15 years of age is very small.<sup>24</sup>

Competing risks survival analyses<sup>15</sup> allowed us to calculate the absolute risks of suicide (or cumulative incidences) as the percentages of persons in the population who had committed suicide at a given time since the onset of the disorder of interest, taking into account that people may migrate or die of other causes. These analyses were made for each sex and were subdivided according to the age at onset of the disorder of interest. In this report, our interest is the probability of suicide. This probability, also referred to as the *cumulative incidence*, is not a simple function of the incidence rate of suicide; rather, it is estimated as the weighted integral of the incidence rates, in which the weights equal the survival function. In this situation, people born in 1955 contribute to the estimation of the incidence rate from time 0 to time 36, whereas people born in 1991 only contribute to the estimation of the incidence rate until 1 year after the first psychiatric contact. Additional details can be found in Rosthøj et al.25

To estimate the cumulative incidence of suicide among people with no history of mental illness, we adopted a slightly alternative strategy. For each person with a history of any mental illness (as defined in the "Assessment of Suicide and Mental Illness" subsection), we randomly selected 5 people of the same sex and same birth date who had no history of mental illness (time matched). Using the described strategy, we followed up this healthy population (881 735 persons) to provide absolute suicide risks. Because this healthy population was selected at random among all 2.46 million people included in the study population, the estimates obtained represent the absolute risk of suicide among all 2.46 million people without a mental disorder. In addition, we performed analyses of comorbidity. Patients who had a diagnosis of substance abuse disorder plus any other mental disorder—at the same time or at different times underwent separate analyses. Similarly, we performed analyses of comorbidity for patients with unipolar affective disorder in combination with other psychiatric disorders and any history of hospital treatment after deliberate self-harm. This study was approved by the Danish Data Protection Agency.

#### RESULTS

The absolute risk of suicide according to diagnostic group is shown in Table 1. Among men, suicide risk was highest in bipolar disorder (7.77%; 95% confidence interval [CI], 6.01%-10.05%), followed by unipolar affective disorder (6.67%; 5.72%-7.78%), schizophrenia (6.55%; 5.85%-7.34%), schizophrenialike disorders (5.90%; 5.21%-6.67%), and substance abuse treated in a psychiatric department (4.71%; 4.24%-5.23%). Among women, the highest risk was found among women with schizophrenia (4.91%; 95% CI, 4.03%-5.98%), followed by bipolar disorder (4.78%; 3.48%-6.56%), schizophrenialike disorder (4.07%; 3.28%-5.04%), unipolar affective disorder (3.77%; 3.05%-4.66%), substance abuse treated in a psychiatric department (3.34%; 2.80%-3.98%), and anorexia (2.62%; 1.08%-6.38%). In the nonpsychiatric population, the risk was 0.72% (95% CI, 0.61%-0.86%) for men and 0.26% (0.20%-0.35%) for women. The estimate of the suicide risk for men with anorexia is based on small numbers of cases.

The cumulative incidence of suicide by time since the first psychiatric contact for each of the disorders investigated is shown in the **Figure** for men and women. The steepest increase in suicide incidence occurs during the first years after first contact. The cumulative incidences of suicide were virtually independent of age at onset of the different mental disorders (data not shown).

In **Table 2**, the cumulative incidence of suicide is presented for patients who had a diagnosis of a substance abuse disorder and a different additional mental disorders during the same contact or at different times. In all diagnostic groups, comorbidity with substance abuse disorder increased the cumulative incidence of suicide except among men with schizophrenia. In **Table 3**, the cumulative incidence of suicide is pre-



Figure. Cumulative incidence of suicide by time since the first psychiatric contact among men (A) and women (B).

sented for patients who had a diagnosis of a unipolar affective disorder and a different additional mental disorder. For all mental disorders, comorbid occurrence of unipolar affective disorder increased the cumulative incidence of suicide.

In **Table 4**, the cumulative incidence for patients who had attempted suicide at least once is presented in different diagnostic groups among men and women. Overall, across all diagnostic groups, deliberate self-harm doubled the risk. The highest cumulative incidence of suicide was found among men with bipolar disorder and deliberate self-harm (17.08%; 95% CI, 11.19%-26.07%).

#### COMMENT

To our knowledge, this study has the hitherto largest sample and includes a long-term follow-up of a complete national sample from 15 to 51 years of age. We found the absolute risk of suicide in different psychiatric disorders to vary from 2% to 8%, higher for men than for women and highest for men and women with bipolar disorder, unipolar affective disorder, schizophrenia, and schizophrenialike disorder. For both sexes, comorbid occurrence of substance abuse and unipolar affective disorders increased the absolute suicide risk, and cooccurrence of deliberate self-harm generally doubled the risk in each diagnostic group. The suicide risk increased steeply during the first few years after first contact with psychiatric services.

Although the absolute suicide risks identified in this study are high, they are clearly lower than the often-cited figures reported by Guze and Robins<sup>8</sup> and Miles.<sup>7</sup> References to those old, exaggerated estimates should be replaced by more recent and correct ones. Estimates of the cumulative incidences in the literature have often ignored the fact that people may emigrate or die of other causes.

The strengths of this study are the large and representative number of cases investigated, the long followup, and the fact that we accounted for emigration and death from other causes. Omitting such censoring in our sample would bias the absolute risks upward by approximately 10%.<sup>15,25</sup>

	Men			Women		
Disorder	No. of Suicides	No. Followed Up	Cumulative Incidence, % (95% Cl)	No. of Suicides	No. Followed Up	Cumulative Incidence, % (95% CI)
Schizophrenia	170	4955	5.88 (4.90-7.04)	59	1734	6.88 (4.86-9.74)
Schizophrenialike disorders	197	5607	6.26 (5.23-7.51)	82	2703	5.74 (4.41-7.49)
Bipolar affective disorder	42	1101	10.01 (6.40-15.66)	19	870	5.20 (2.81-9.60)
Unipolar affective disorder	180	6763	6.74 (5.24-8.67)	92	5439	7.12 (4.68-10.83)
Anorectic disorder <sup>b</sup>	1	23	5.56 (0.87-35.37)	11	441	4.95 (2.58-9.48)
Anv mental illness	862	34 539	4.60 (4.13-5.12)	270	17 733	3.26 (2.75-3.87)

Table 2. Cumulative Incidence of Suicide Up to 36 Years After the First Psychiatric Contact Among Individuals With Comorbid Substance Abuse<sup>a</sup>

Abbreviation: CI, confidence interval.

<sup>a</sup> Population includes the 2.46 million people born in Denmark from January 1, 1955, through December 31, 1991, and followed up from 15 years of age to 2006. Cumulative incidence measures the percentage of persons in the population who had committed suicide within 36 years after onset of the disorder of interest, taking into account that people may migrate or die of other causes.

<sup>b</sup>Cumulative suicide risk for men with anorectic disorder was based on only 1 suicide.

ARCH GEN PSYCHIATRY/VOL 68 (NO. 10), OCT 2011 WWW.ARCHGENPSYCHIATRY.COM 1061

Table 3. Cumulative Incidence of Suicide Up to 36 Years After the First Psychiatric Contact Among Individuals With Comorbid Unipolar Affective Disorder<sup>a</sup>

	Men			Women		
Disorder	No. of Suicides	No. Followed Up	Cumulative Incidence, % (95% CI)	No. of Suicides	No. Followed Up	Cumulative Incidence, % (95% Cl)
Schizophrenia	64	1698	7.02 (5.06-9.73)	46	1649	6.18 (4.30-8.89)
Schizophrenialike disorders	96	2521	9.20 (6.89-12.30)	95	3140	5.16 (4.05-6.57)
Bipolar affective disorder	60	1173	9.63 (7.10-13.06)	42	1915	5.03 (3.12-8.11)
Substance abuse at psychiatric department	159	5827	6.85 (5.34-8.79)	81	4085	7.39 (4.86-11.26)
Substance abuse at somatic hospital	94	3727	5.66 (3.68-8.69)	58	3177	5.61 (3.56-8.85)
Anorectic disorder <sup>b</sup>	1	23	5.59 (0.88-35.65)	5	644	3.77 (0.93-15.23)
Any mental illness	410	16 984	6.63 (5.67-7.75)	292	28 307	3.81 (3.08-4.72)

Abbreviation: CI, confidence interval.

<sup>a</sup>Population includes the 2.46 million people born in Denmark from January 1, 1955, through December 31, 1991, and followed up from 15 years of age to 2006. Cumulative incidence measures the percentage of persons in the population who had committed suicide within 36 years after onset of the disorder of interest, taking into account that people may migrate or die of other causes.

<sup>b</sup>Cumulative suicide risk for men with anorectic disorder was based on only 1 suicide.

## Table 4. Cumulative Incidence of Suicide Up to 36 Years After the First Psychiatric Contact Among Individuals Admitted After Deliberate Self-harm<sup>a</sup>

	Men				Women		
Disorder	No. of Suicides	No. Followed Up	Cumulative Incidence, % (95% CI)	No. of Suicides	No. Followed Up	Cumulative Incidence, % (95% CI)	
Schizophrenia	193	2801	10.26 (8.36-12.58)	111	2118	10.85 (8.43-13.95)	
Schizophrenialike disorders	215	3112	9.98 (8.35-11.93)	151	2994	8.00 (6.46-9.91)	
Bipolar affective disorder	58	651	17.08 (11.19-26.07)	47	991	9.39 (6.07-14.54)	
Unipolar affective disorders	223	4277	10.48 (8.24-13.32)	184	6567	6.51 (5.23-8.09)	
Substance abuse at psychiatric department	439	10 461	6.54 (5.82-7.34)	174	5999	5.04 (4.19-6.06)	
Substance abuse at somatic hospital	351	10 555	5.53 (4.68-6.53)	155	6968	4.03 (3.32-4.90)	
Anorectic disorder <sup>b</sup>	1	16	10.42 (1.79-60.55)	14	555	4.38 (2.48-7.75)	
Any mental illness	799	16274	8.10 (7.32-8.96)	450	17 993	4.57 (4.03-5.17)	

Abbreviation: CI, confidence interval.

<sup>a</sup> Population includes the 2.46 million people born in Denmark from January 1, 1955, through December 31, 1991, and followed up from 15 years of age to 2006. Cumulative incidence measures the percentage of persons in the population who had committed suicide within 30 years after onset of the disorder of interest, taking into account that people may migrate or die of other causes.

<sup>b</sup>Cumulative suicide risk for men with anorectic disorder was based on only 1 suicide.

The findings in our study are in agreement with the meta-analysis performed by Bostwick and Pankratz<sup>12</sup> and Palmer et al,<sup>13</sup> partly because Danish register-based studies contributed a large proportion of the patients and person-years included in their analyses. Our study population includes all the Danish patients included in the meta-analyses; in our study, they were followed up longer than in previous studies.

There are some limitations in a register-based study compared with a population-based survey. The study population includes only persons who have received some kind of treatment in psychiatric treatment facilities, and outpatient treatment was recorded only since 1995. However, most other studies have the same limitations.

Another limitation is that we were able to identify incident cases of mental illness only among people born in 1955 or later and to follow up these individuals until 2006, that is, people who had received a diagnosis of a mental illness before 51 years of age. We can only speculate whether the absolute risks reported are applicable to people with later onset of a mental disorder.

Based on our material, we cannot estimate lifetime risk because the cohort was followed up until, at most, 51 years of age. Also, because the design of the study exploits the advantages of including the longest possible historical period, there is a risk that changes in suicide risk occurred during the period investigated. Prior investigations have previously demonstrated that suicide rates for patients with schizophrenia,<sup>26</sup> affective disorder, and substance abuse<sup>27</sup> decreased and can be influenced by a range of conditions related to the treatment, as well as to other factors, such as availability of dangerous means.

The number of persons with bipolar disorder in our sample is much lower than the number of cases of schizo-

phrenia. The explanation for the smaller figures is that many cases classified as bipolar II disorder in *DSM* terms will not be classified as bipolar disorders in *ICD-8* and *ICD-10* and also that the incidence of bipolar disorder peaks at a later age<sup>28</sup> compared with schizophrenia.<sup>29</sup> In the present, rather young cohort, some individuals have not yet developed bipolar disorder.

All persons in this study were classified according to the clinical diagnosis given at first contact with mental health services after 15 years of age. Diagnostic switch between, for instance, schizophrenialike disorder and schizophrenia or a switch between unipolar affective disorder and bipolar disorder is therefore not taken into account. Theoretically, persons who later switched from one group to another could have a different risk than those who remained in the same group, thereby artificially leveling out differences between diagnostic groups. However, diagnostic switch cannot be taken into account without introducing survival bias (healthyworker effect).

Large prospective studies of first-onset cases with a long follow-up, such as the present study and the recent British 40-year follow-up of first-episode psychosis cases,<sup>30</sup> provide good estimates of suicide risk. However, an inherent problem with such studies is that, by the time the results become available, the risk for new patients with first-episode psychosis may have changed because of changes in treatment and other factors.<sup>31</sup> Since 2000, suicide rates in Denmark have been clearly lower than those in the previous decades, when many of the cases in the cohort died.<sup>31</sup>

We did find higher figures than Dutta et al<sup>5,30</sup> in their study of patients with first-episode psychosis, which can be partly explained by differences between the 2 countries in classification of suicide, with higher suicide rates in Denmark.<sup>32</sup> Other possible explanations are the higher proportion of outpatients in the British sample, differences in determination of cause of death, differences in access to data on previous treatment, or differences in the completeness of follow-up due to the Danish unique nationwide personal identifier, which ensures the most optimal conditions for selecting a true first-time-treated population and a complete follow-up. It is a limitation that the data are only available from Denmark, which might limit generalization of our findings to other countries. In the 1980s, Denmark had extraordinarily high suicide rates, most likely because of a large number of suicides with barbiturates.<sup>33</sup> Because some suicides in this study occurred during that period, these figures might not reflect conditions in other countries.

Despite these limitations, it is beyond doubt that the risk of suicide is high in all the investigated mental disorders, and suicide preventive measures should be a mandatory part of treatment programs, not only for affective disorders but also for schizophrenia and related disorders, for substance abuse, and for anorexia.<sup>34</sup> It is also evident that a history of deliberate self-harm markedly increases the risk of suicide across all diagnostic groups. In addition, as underlined in recent findings from a large Swedish study,<sup>35</sup> attempted suicide should be considered a very important risk factor among patients with different mental disorders.

The fact that the steepest increase in suicide risk occurs during the initial years after first contact with mental health services can serve as an argument for intensive early-intervention services. By establishing closer contact and closer monitoring of symptoms, we hope that such services can reduce suicide risk in this high-risk period and thereby ensure that the long-term risk of suicide may be influenced positively.

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## Exhibit 4

#### OPINION

# Why do many psychiatric disorders emerge during adolescence?

#### Tomáš Paus, Matcheri Keshavan and Jay N. Giedd

Abstract | The peak age of onset for many psychiatric disorders is adolescence, a time of remarkable physical and behavioural changes. The processes in the brain that underlie these behavioural changes have been the subject of recent investigations. What do we know about the maturation of the human brain during adolescence? Do structural changes in the cerebral cortex reflect synaptic pruning? Are increases in white-matter volume driven by myelination? Is the adolescent brain more or less sensitive to reward? Finding answers to these questions might enable us to further our understanding of mental health during adolescence.

Across cultures and centuries, adolescence has been noted as a time of dramatic changes in body and behaviour. Although most teenagers successfully navigate the transition from dependence on a caregiver to being a self-sufficient member of society, adolescence is also a time of increasing incidence of several classes of psychiatric illness, including anxiety and mood disorders, psychosis, eating disorders, personality disorders and substance abuse. The pathophysiology of these disorders is increasingly understood as arising from aberrations of the maturational changes that normally occur in the adolescent brain.

In this Perspective we address the neurobiological changes that occur during adolescence and discuss their possible relationship to the emergence of psychopathology. We focus on three major disorders, namely schizophrenia, substance-use disorders and affective/anxiety disorder, because our understanding of their developmental neurobiological basis has increased considerably in recent years.

#### Typical development of the adolescent brain

In the past 15 years there has been an impressive accumulation of knowledge about the development of the structure and the function of the human brain. Studies carried out with MRI<sup>1-4</sup> in children and adolescents have allowed investigators to chart

trajectories of grey- and white-matter volumes, cortical thickness and, more recently, other structural properties of white matter, such as fractional anisotropy and magnetizationtransfer ratio (MTR), as well as age-related changes in brain activity (BOX 1).

*Brain structure.* Most of the existing literature on age-related changes in brain structure has been reviewed in detail elsewhere<sup>5,6</sup>. Here we note only the most salient findings.

Volumes of cortical grey matter seem to increase during childhood, reaching peak levels at approximately the time of puberty onset, after which they gradually decline; this is the case for the frontal and parietal lobes but not for the temporal lobes<sup>7</sup>. Local volume of cortical grey matter declines during childhood and adolescence in most regions, with the slope of the decline varying from relatively gentle (for example, in the anterior portion of the superior temporal gyrus (STG)) to steep (for example, in the posterior portion of the STG) and, in some cases, displaying a nonlinear relationship with age; for example, between 10 and 20 years of age, an 'inverted-U-shaped' relationship between age and cortical grey matter has been found in the post-central gyrus, and a 'U-shaped' relationship has been found in the mid-dorsolateral frontal cortex<sup>8,9</sup> (FIG. 1).

Volumes of white matter show a rather clear linear increase throughout childhood and adolescence, with the maximum volumes often reached as late as the third decade of life<sup>10</sup>. It seems that the slope of the age-related increase is steeper in males than in females7,11. More recently, diffusion tensor imaging (DTI) has been used to assess whitematter changes in more detail in the human brain during childhood and adolescence. Overall, DTI studies reveal age-related decreases in the magnitude and increases in the directionality of water diffusion in a number of white-matter regions<sup>12-14</sup>, many of which are identical to those revealed by structural MRI studies, such as those of the arcuate fasciculus. Such changes in DTIderived measures may indicate ongoing maturation of axons and/or their myelin sheaths (see below).

Brain activity. The overall picture to be gleaned from the existing descriptive studies of age-related changes in brain activity is less coherent than that for structural changes. This is due to the fact that functional MRI (fMRI) studies usually focus on a particular brain function, and to the fact that the behavioural paradigms used to assess that brain function often differ across laboratories. It is also more challenging to interpret fMRI data than structural measurements, owing to the indirect nature of the fMRI signal (BOX 1) and the large number of potential confounders, such as levels of anxiety and arousal during scanning, varying task performance across participants, and the use of different cognitive strategies by different participants in the same task — all of these might interact with the effects of age. We will touch here on two sets of fMRI studies of adolescents that respectively focused on cognitive control (or executive functions) and on experiencing gains and losses of various rewards.

A number of the initial studies that investigated how task-related brain activity changes during development focused on executive functions, such as working memory and response inhibition. But, as we reviewed previously<sup>6</sup>, many such executive abilities are fully developed by the time a child enters adolescence<sup>6</sup>. On the other hand, certain aspects of executive function, such as

#### Box 1 | Neuroimaging

MRI has revolutionized the way we can study the structure and function of the brain in living human beings throughout the entire lifespan<sup>1</sup>. The principles of MRI are relatively straightforward: in most applications, the magnetic resonance signal results from the magnetic properties of hydrogen atoms, which form part of the most abundant substance in the human body, water. By placing the human body in a strong (0.5–7.0 T) static magnetic field ( $B_0$ ) and applying a brief pulse of electromagnetic energy, we can make the dipoles formed by the hydrogen nuclei rotate away from their axes and, in turn, measure the time it takes for the nuclei to 'relax' back to their original position. By slightly changing the static magnetic field at different positions along/across the  $B_0$ , we can establish the spatial origin of the signal and, eventually, create a three-dimensional image of the measurement. What is measured depends on the combination of various imaging parameters or, in the terminology of the MR physicists, on the acquisition sequence.

For imaging brain structure, the most common acquisition sequences include T1-weighted (T1W) and T2W images, diffusion-tensor images (DTI) and magnetization-transfer images (MT). The T1W and T2W images are typically used to quantify the volume of grey and white matter (global and regional) and to estimate the cortical thickness or other morphological properties of the cerebral cortex, such as its folding. Using DTI and MT imaging one can assess different properties of white matter, again in both a global and a regional manner. The various features of brain structure that can be extracted from these four types of images are described in the main text. In addition to the above sequences, less common but often even more informative acquisitions include T1 and T2 relaxometry (that is, measurement of the actual relaxation times<sup>2</sup>) and magnetic resonance spectroscopy<sup>3</sup>.

For imaging brain function, the most common MR parameter to measure is the so-called bloodoxygenation-level-dependent (BOLD) signal. The BOLD signal reflects the proportion of oxygenated and deoxygenated blood in a given brain region at a given moment. A strong correlation between the amount of synaptic activity and regional cerebral blood flow is the reason why the BOLD signal is a good, albeit indirect, measure of brain 'function' (REF. 4). In most functional MRI studies, one measures changes in BOLD signal in response to various sensory, motor or cognitive stimuli. Therefore, only brain regions that are likely to respond to such stimuli can be interrogated using a given paradigm.

planning time and delayed gratification, do improve significantly from mid-adolescence (~16 years of age) onward, as indicated by recent behavioural studies15. An fMRI study found age-related (between the ages of 7 and 22 years) increases in the bloodoxygenation-level-dependent (BOLD) signal in the prefrontal and parietal cortices during the performance of a working-memory task even after factoring out inter-individual differences in performance<sup>16</sup>. Similar BOLD increases were observed in these regions during the performance of a variety of tasks that involve some form of response inhibition, including the Stroop task<sup>17</sup>, the antisaccade task<sup>18</sup>, the stop task<sup>19</sup> and, to a certain extent, the go/no-go task<sup>20</sup> and the Eriksen flanker task<sup>21</sup>.

Adolescence has traditionally been associated with risk-taking and sensationseeking behaviour<sup>22</sup>. In this context, several investigators used fMRI to examine possible differences in brain activity between children, adolescents and young adults during the experience of gains or losses of various rewards. Owing to its role in reward and motivation<sup>23</sup>, the nucleus accumbens (or ventral striatum) was the focus of most of these studies. If adolescents were 'driven' by reward seeking, one would expect a heightened engagement of this structure

during tasks that involve reward seeking. This seemed to be the case in participants in some24,25 but not other26 studies. For example, one study<sup>26</sup> described an increase from early adolescence to young adulthood (12 to 28 years) in the BOLD signal in the nucleus accumbens during the anticipation of monetary gains; this was the case even when self-reported level of excitement in response to seeing anticipatory cues was taken into account. It is worthwhile to point out that in the same study, excitement correlated positively with the BOLD signal in the nucleus accumbens even when age was taken into account. This observation highlights the importance of considering various aspects of behaviour when interpreting fMRI findings.

Although functional imaging studies are beginning to illuminate the functional maturation of the neural circuits that are involved in, for example, executive functions and reward processing, future studies need to increase substantially sample sizes and enhance the behavioural characterization of subjects' performance in the scanner in order to learn more about brain–behaviour relationships during adolescence.

#### Interpretation of underlying mechanisms

The age-related changes in brain structure and function during adolescence described

above have been interpreted using various conceptual frameworks. Changes in synaptic pruning and myelination have been the most popular explanations for the structural findings in the adolescent brain, whereas age-related alterations in neural connectivity and neurotransmission might underlie the functional changes associated with adolescence. We will now address, in a critical manner, such mechanistic interpretations.

#### *Does adolescence involve changes in pruning and myelination?* MRI-based estimates of the volume of cortical grey matter and cortical thickness seem to decrease during

adolescence. This has often been interpreted as an indication of 'synaptic pruning', a process by which 'redundant' synapses that were overproduced in the early years of life are eliminated (see REF. 27 for a critical appraisal of "neural Darwinism").

The initial evidence for accelerated synaptic pruning during development came from post-mortem studies by Huttenlocher and colleagues, who described a decrease in the number of synapses in the human cerebral cortex during childhood and adolescence<sup>28,29,30</sup>. It should be noted, however, that these studies were limited by the low number of specimens that were available for the different stages of human development, especially the adolescent period. Furthermore, most of the data do not actually indicate accelerated pruning of synapses during adolescence; rather, they indicate a gradual decrease in synapse number that begins (in several cortical regions) in childhood. More-definite evidence of synapse elimination during adolescence was provided by studies carried out by Rakic and colleagues in non-human primates<sup>31,32</sup>. Using electron microscopy, they observed a dramatic decrease in the number of synapses in the monkey visual cortex, as well as in other cortical areas, during puberty (that is, between the age of 2.5 and 5 years), whether the data were expressed as number of synapses per neuron or as number of synapses per 1 mm<sup>3</sup> of neuropil (~45% loss). But it is unlikely that this decrease in synaptic density translates into a decrease in cortical volume: Bourgeois and Rakic commented that "changes in the density of synapses affect very little either the volume or surface of the cortex because the total volume of synaptic boutons ... is only a very small fraction of the cortical volume" and concluded that "...a decline of synaptic number during puberty should have a rather small effect on the overall volume of the cortex" (REF. 32). Finally, it is often assumed that age-related

changes in cortical grey matter, glucose metabolism and synaptic density follow similar developmental trajectories from birth to adulthood and, hence, reflect the same cellular events; this is clearly not the case, especially during adolescence (FIG. 1).

If the number of synapses per se is unlikely to change the cortical volume and/ or thickness, then what other cellular elements could affect it? Approximately 10% of the (mouse) cortex is occupied by glial cells and approximately 60% is occupied by neuropil, which consists of dendritic and axonal processes<sup>33</sup>. It is conceivable that a reduced number of synapses, and a corresponding decrease in metabolic requirements, would be accompanied by a reduction in the number of glial cells, leading to a decrease in the regional volume and/or thickness of cortical grey matter. But it is perhaps even more likely that the apparent loss of grey matter reflects an increase in the degree of myelination of intra-cortical axons. Myelination of intra-cortical fibres progresses gradually from birth to adulthood<sup>34,35</sup>. The more myelinated the fibres are, the less 'grey' the cortex would appear on regular T1-weighted images. Such a 'partialvolume' effect could result in an apparent loss of cortical grey matter<sup>6</sup>.

Given the well-documented histologybased increase in the degree of myelination of white-matter pathways during the first two decades of human life<sup>36</sup>, it is perhaps not surprising that any changes in the volume or density of white matter, as revealed by computational analyses of T1-weighted images, are attributed to changes in myelination. Again, assumptions based on previous knowledge influence the interpretation of new data. Quite often we read articles that report age-related changes in myelination only to realize that what had actually been measured were volumes of white matter. Is it only a matter of semantics or could other, myelination-independent processes affect the volume and/or other features of white matter? In one of our large studies of human adolescence, we have observed a dissociation between age-related changes in the volume of white matter and changes in the MTR (an indirect index of the amount of myelin in white matter)<sup>37</sup>. Although white-matter volume increased with age during male adolescence, MTR values decreased, indicating a decrease in the amount of myelin per unit of volume (FIG. 2).

If myelin does not increase, what could be driving the observed increase in whitematter volume in males? Our tentative answer is a change in axonal calibre: the



Figure 1 | Schematic representations of developmental trajectories in local volume of cortical grey matter, glucose metabolism and synaptic density. Plots of grey-matter density (top graph) are based on data by Gogtay et al.8 and illustrate the local grey-matter density in the mid-dorsolateral prefrontal cortex in red, in the angular gyrus of the parietal cortex in blue, in the posterior superior temporal sulcus of the temporal cortex in purple, and in the occipital pole in green. Plots of glucose metabolism (middle graph) are based on data by Chugani et al.<sup>109</sup> and provide information about the absolute values of local cerebral metabolic rate (LCMR) for glucose in the frontal (red), parietal (blue), temporal (purple) and occipital (green) cortices. Plots of synaptic density in the prefrontal (red) and visual (green) cortices (bottom graph) are based on data by Huttenlocher and de Courten<sup>28</sup> and Huttenlocher<sup>110</sup>, as re-plotted on a semi-logarithmic scale by Rakic et al.<sup>111</sup>. To facilitate the comparison across the different plots, the vertical line indicates age 15 years. Note the following features of the trajectories, especially between childhood and adulthood: for cortical grey matter, different trajectories are observed in different cortical regions; for glucose metabolism, the same trajectories are found in the four different lobes; the same trajectories are also found for synaptic density in the prefrontal and occipital cortices. Taken together, these plots indicate that it is unlikely that there is a direct relationship between the three sets of measures.

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Figure 2 | **Sexual dimorphism in the maturation of white matter during adolescence.** a | Age-related changes in the relative (brain-size corrected) volume of white matter summed across the frontal, parietal, temporal and occipital lobes. b | Age-related changes in mean-centred values of magnetization transfer ratio (MTR) in the lobar white matter; the MTR provides an indirect index of myelination. Note that the opposite developmental trajectories in volume and MTR in males suggest that age-related increases in white-matter volume during male adolescence are not driven by myelination. Graphs are based on data from REF. 37.

larger the calibre, the fewer axons fit into the same unit of imaged volume, resulting in a relative decrease in the myelination index<sup>37</sup>. Although more work is needed to confirm this initial observation, it serves as a reminder that most of the MRI studies are not specific enough to allow one to interpret their findings as reflecting a single neurobiological process such as myelination.

Overall, as tempting as it might be to interpret descriptive findings obtained from structural MRI using mechanistic neurobiological processes, such as synaptic pruning or myelination, the evidence that supports such interpretations is limited. There is a pressing need to acquire direct evidence of the processes that underlie the observed changes in grey- and whitematter volume during adolescence using experimental models, in which investigators can combine in vivo and ex vivo methods to bring together descriptive and mechanistic levels of analysis. Until this happens, we suggest that a more cautious and open-minded approach is warranted.

*Neural connectivity.* Two key features characterize the functional organization of the mammalian brain: specialization and integration. Clearly, the structural and functional maturation of the neural pathways that connect a set of specialized brain regions is therefore essential for the successful development of cognitive, motor and sensory functions from infancy through childhood and adolescence and into adulthood. There are many different 'connectivities'. Studies of anatomical connectivity allow one to detect, using injection of radioactive tracers into the brain of experimental animals, the efferent and afferent projections of small populations of neurons. This is not the same as anatomical 'connectivity' assessed with DTI-based tractography, as this technique does not allow one to identify point-to-point (or cell-to-cell) connections between distinct neural populations. Functional connectivity captures the correlation between the neural activity of a set of brain regions that are 'engaged' during a particular task or measured at rest. But such correlations do not provide information regarding the causality or directionality of inter-regional interactions. Effective connectivity attempts to address this issue either by manipulating brain activity in one region and evaluating the effect of such manipulation elsewhere, or by using mathematical models<sup>38</sup>.

An example of a study that investigated functional connectivity during childhood and adolescence is an investigation of memory encoding in subjects between 11 and 19 years of age<sup>39</sup>. The study showed an age-related decrease in the fMRI signal in the left medial temporal lobe of subjects viewing photographs of natural outdoor scenes, whereas no age-related change was found in the control condition in which subjects viewed the same scene over and over. The authors used voxel-wise regression analysis to identify brain regions in which the fMRI signal correlated with that measured in two subregions of the left medial temporal lobe, namely the hippocampus and the entorhinal cortex — structures that are known to participate in encoding new information. This analysis revealed an age-related increase in the correlation between activity in the left entorhinal cortex and activity in the left dorsolateral prefrontal cortex. This work nicely illustrates the importance of including analyses of functional connectivity in developmental studies: although the fMRI signal decreased with age in one of the memoryrelevant structures (the entorhinal cortex), the proposed interaction between this structure and other brain regions (the prefrontal cortex) actually increased with age.

Another study investigated functional connectivity in the context of possible neural substrates of resistance to peer influences (RPI) in early adolescence (10-year-old children)<sup>40</sup>. This study aimed to determine whether the probability with which an adolescent follows the goals set by peers or those set by themselves might depend on the interplay between three neural systems. First, the action-observation network, which is considered by many to be the neural substrate of imitation<sup>41</sup>; it consists of frontal and parietal regions that are involved in the preparation and execution of actions. In this network, so-called 'mirror neurons' in the inferior premotor cortex, the inferior frontal gyrus and the anterior inferior parietal lobe are active both when subjects perform a specific action themselves and when they observe another individual performing the same action<sup>41</sup>. Second, the biological-motion processing network<sup>42</sup> (also known as the superior temporal sulcus (STS) network), which has an important role in extracting socially relevant cues, such as those imparted by the movements of eyes or hands. Neurons in the STS respond selectively to the presentation of dynamic bodies, body parts or faces<sup>42</sup>. Third, the executive network<sup>43</sup>, which supports a number of cognitive processes that underlie decision making, working memory and the suppression of alternative programmes that would otherwise interfere with planned actions; it consists of a set of regions in the lateral and medial prefrontal cortex<sup>43</sup>. In the study, subjects lying in an MRI scanner were asked to watch brief video clips containing face or hand/arm actions that were executed in neutral or angry ways, while changes in fMRI signals were measured. Outside the scanner, the subjects completed an RPI questionnaire44. Children with high RPI scores showed stronger inter-regional activity correlations in brain activity across the three networks while watching angry hand actions than the children who had low RPI scores (FIG. 3).

This method identified activity correlations between areas that included both regions involved in action observation (the frontoparietal and temporo-occipital systems) and regions in the prefrontal cortex. Thus, what distinguished subjects with high and low resistance to peer influences was not the magnitude of the BOLD response in individual brain regions but the degree of functional connectivity between regions.

*Neurochemistry.* The efficacy of communication across neuronal networks depends crucially on the state of the various



Figure 3 | Functional connectivity correlates with resistance to peer influence. This figure shows functional connectivity, indexed by interregional correlations in functional MRI (fMRI) signals, during the observation of angry hand movements in children who differ in their resistance to peer influences (RPI). a | Correlations between the fMRI signal in a combination of brain regions during observation of angry and neutral hand movements and facial expressions and scores on the RPI questionnaire. **b** | Brain activity (brain score) during angry hand movements correlated strongly with RPI scores. c | Locations of brain regions in which the fMRI signal correlated with the RPI score during the observation of angry hand movements; only regions that are visible on the lateral surface of the left and right hemispheres are shown. d | Correlation matrices depicting inter-regional correlations of fMRI signals measured during the observation of angry hand movements in subjects with high (left) and low (right) RPI scores (subjects with RPI scores above and below the group median, respectively). The degree of interregional correlation (that is, functional connectivity) is higher in children with high RPI scores than in children with low RPI scores. e | Multidimensional scaling representations of the interregional correlations of the 25-dimensional matrix depicted in part d. Brain regions between which the fMRI signals (during the observation of angry hand movements) were strongly correlated are placed close together. Functional connectivity between regions is greater in children with high RPI scores (left graph) than in children with low RPI scores (right graph). CB1, cerebellum, right; CB2, cerebellum, right; CN, caudate nucleus, right; F01, premotor cortex, dorsal, left; F02, premotor cortex, dorsal, right; F03, premotor cortex, ventral, left; F04, premotor cortex, ventral, right; F05, frontal operculum, right; F06, cingulate motor area, left; F07, insula, anterior, left; F08, prefrontal cortex, ventrolateral, right; F09, prefrontal cortex, dorsolateral, left; F10, prefrontal cortex, dorsolateral, right; F11, prefrontal cortex, ventrolateral, left; F12, anterior cingulate cortex, right; F13, orbitofrontal cortex, lateral, left; F14, prefrontal cortex, medial; O01, fusiform gyrus, left; P01, posterior cingulate cortex; P02, precuneus, left; P03, parietal cortex, dorsolateral, right; P04, parietal cortex, dorsomedial, right; SC, superior colliculus, right; T01, superior temporal sulcus, middle, right; T02, superior temporal sulcus, posterior, right; T03, hippocampus, right. Figure reproduced, with permission, from REF.40 © (2007) Society for Neuroscience.



#### Box 2 | Basics of neurotransmission

Transmission of information from one neuron to the next involves several steps. Local excitatory and inhibitory postsynaptic potentials (EPSPs and IPSPs) are continuously being summed at the axonal hillock, and once a threshold value is reached an action potential is generated. The action potential then travels along the axon and, at the synapse, causes a release of neurotransmitters. The so-called conduction velocity is higher in myelinated axons than in non-myelinated axons and is also higher in large-diameter axons than in small-diameter axons<sup>104-106</sup>. Neurotransmitters are chemicals that either relay action potentials or modulate (for example, amplify) this process. Neurotransmitters include amino acids (for example, glutamate and GABA (γ-aminobutyric acid)), monoamines (for example, dopamine, serotonin and noradrenaline), acetylcholine and many neuropeptides (for example, oxytocin). Glutamate and GABA are the main excitatory and inhibitory neurotransmitters, respectively, and dopamine is one of the most studied neuromodulators. The action of a particular neurotransmitter is mediated by a receptor; a given neurotransmitter can bind to a number of receptor subtypes that are found in different brain regions, or different layers of the cerebral cortex, in different densities<sup>107,108</sup>. The complex interaction between the various neurotransmitters released at any given time at the synapse determines the number of EPSPs and IPSPs generated on the postsynaptic membrane and, in turn, the firing of the neuron.

the receptors for the transmitter. Owing to radiation concerns, however, PET cannot be used in healthy children or adolescents. Therefore, most of our knowledge of developmental changes in neurotransmitters is derived from post-mortem studies in human and non-human primates.

We now consider developmental changes in the dopaminergic system, which has often been conceptualized as underlying adolescent-specific changes in motivational behaviour<sup>45</sup>. The existing data are not entirely consistent with this view, however. In the monkey, levels of the catecholaminesynthesizing enzyme tyrosine hydroxylase (TH) do not change during postnatal development in cortical layers I and VI. In layer III, TH levels are highest during infancy (5–7 months of age) in the entorhinal cortex<sup>46</sup> and during puberty (2–3 years of age) in the prefrontal cortex<sup>47</sup>.

In humans, two recent post-mortem studies evaluated age-related changes in TH, catechol-O-methyltransferase (COMT) and a number of dopamine receptors in the human prefrontal cortex; COMT is a dopamine-metabolizing enzyme that is particularly important for dopaminergic transmission in the prefrontal cortex. No differences in COMT activity were found between infants (5-11 months of age), adolescents (14-18 years) and young adults (20-24 years)<sup>48</sup>— COMT activity increased only in adulthood (31-43 years)<sup>48</sup>. The second study showed that TH levels in the human prefrontal cortex were highest in neonates and by adolescence had declined to the levels observed in adults<sup>49</sup>. The same was true, in the same region, for expression of the dopamine D2 receptor gene, DRD2. By contrast, expression of **DRD1** was highest in adolescents (14-18 years) and young adults

(20–24 years) in all layers of the prefrontal cortex. Levels of  $\underline{DRD4}$  in the prefrontal cortex did not change with  $age^{49}$ . These findings illustrate that, contrary to prior assumptions, developmental changes in the different elements of dopaminergic transmission during adolescence are complex — very few, if any, of these elements peak during adolescence. As such, these age-related variations — in particular in the prefrontal cortex — are not likely to account for differences between adolescents and adults in motivation-related modulation of cortical activity.

#### Psychopathology and adolescence

The results of the National Comorbidity Survey Replication study, which entailed inperson household assessments of over 9,000 people representative of the US population (conducted from February 2001 to April 2003), indicated that the peak age of onset for any mental health disorder is 14 years<sup>50</sup>. Anxiety disorders, bipolar disorder, depression, eating disorder, psychosis (including schizophrenia) and substance abuse all most commonly emerge during adolescence50,51 (FIG. 4). The emergence of certain psychopathologies is probably related to anomalies or exaggerations of typical adolescent maturation processes acting in concert with psychosocial factors (for example, school and relationships) and/or biological environmental factors (for example, pubertal hormonal changes and drugs of abuse), as will be discussed later. Here we focus on schizophrenia, affective and anxiety disorders and substance-use disorders because they are among the most well-studied, common and disabling disorders that emerge during adolescence, and they serve to highlight aberrations in the key developmental domains of cognition, affect and motivational behaviour.

Schizophrenia. Schizophrenia is a common disorder, with a lifetime prevalence of approximately 1%. It typically begins in adolescence or early adulthood and is characterized by unusual beliefs and experiences, namely delusions and hallucinations (collectively termed positive symptoms), social withdrawal and flat affect (negative symptoms), and cognitive impairment, notably in executive functions. An early onset of schizophrenia, during or even before adolescence, is associated with more-severe impairments<sup>52</sup>. The emerging ability to think abstractly during adolescence permits the application of advanced reasoning to social and interpersonal processes. These abilities are critically impaired in patients with schizophrenia, which led Feinberg to propose a relationship between lateadolescence-onset schizophrenia and changes that occur during adolescent brain development<sup>53</sup>. For example, the number and the duration of delta-wave sleep periods normally decrease during healthy adolescence<sup>53</sup>. In adolescents and young adults with schizophrenia, this reduction in delta-wave sleep is even more pronounced<sup>54,55</sup>. Delta-wave sleep represents the summed synchronous electrical activities of large assemblies of cortical neurons. On the basis of these observations, Feinberg speculated that schizophrenia might be a consequence of an exaggeration of the typical synaptic elimination that takes place during adolescence<sup>53</sup>.

Subsequently, several lines of evidence have lent support to this hypothesis (that an "exaggeration of typical adolescent changes" has occurred in patients with schizophrenia)<sup>54</sup>. In addition to the exaggerated reductions in delta-wave sleep in adolescent patients with schizophrenia55, patients with schizophrenia have prominent reductions in the level of membrane phospholipid precursors in the prefrontal cortex<sup>56</sup>, in prefrontal metabolism57 and in volumes of grey matter in the frontal cortex<sup>58</sup>; all of these observations are consistent with an exaggeration of the changes that occur in typical development. In a rare case of childhoodonset schizophrenia (onset before the age of 12 years), which is phenomenologically similar to adolescent- or adult-onset schizophrenia, the typical decrease in frontal grey-matter volume that is seen in healthy subjects during adolescence was exaggerated fourfold<sup>58</sup>.

Direct evidence of a decrease in the number of synapses and other neural elements in schizophrenia comes from postmortem studies that indicated a decreased density of synaptic spines<sup>59</sup>, a reduction in neuropil<sup>60</sup> and decreased expression of the



Figure 4 | **Ranges of onset age for common psychiatric disorders.** Recent data from the National Comorbidity Survey Replication study<sup>50,112</sup>, a nationally representative epidemiological survey of mental disorders, suggest that approximately half of the population fulfil the criteria for one or other psychiatric disorder in their lifetimes. Most of those with a mental disorder have the beginnings of the illness in childhood or adolescence. Some anxiety disorders (such as phobias and separation anxiety) and impulse-control disorders begin in childhood, whereas other anxiety disorders (such as panic, generalized anxiety and post-traumatic stress disorder), substance disorders and mood disorders begin later, with onsets rarely before the early teens. Schizophrenia typically begins in late adolescence or the early twenties, with men having a somewhat earlier age of onset than women<sup>51</sup>. Psychiatric disorders with childhood or adolescent onsets tend to be more severe, are frequently undetected early in the illness and accrue additional co-morbid disorders, especially if untreated. It is therefore crucial to focus efforts on early identification and intervention.

synaptic marker synaptophysin<sup>61</sup>. Although this evidence supports a neurodevelopmental pathophysiology of schizophrenia, it does not provide indications regarding its aetiology. The cause of schizophrenia probably lies in the interplay between genetic and environmental factors, perhaps involving preand perinatal adverse events, a suboptimal postnatal environment during infancy and childhood, and biological stressors during adolescence.

Substance abuse. Adolescents are more likely to experiment with drugs. Substanceabuse disorders in adults typically begin during the teenage years; they can be preceded by behavioural disturbances and poor adjustment in childhood, as shown by recent results from the National Child Development Study<sup>62</sup>. An earlier onset of drug use predicts a greater severity of addiction problem<sup>63</sup> and might serve as a 'gateway' to the use of multiple substances later in life<sup>64</sup>.

Certain personality traits are important risk factors for substance use, including high levels of novelty seeking and low levels of harm avoidance<sup>65,66</sup>. Across a wide array of mammalian species, adolescents exhibit increased risk taking and novelty seeking and a greater valuation of social factors<sup>67,68</sup>. Although these characteristics foster independence from the natal family, they also increase the risk for harmful behaviours, including, in humans, substance use and abuse. Some investigators have speculated that risk-taking and reward-seeking behaviours in adolescents might be related to a heightened sensitivity for reward<sup>24</sup>. As discussed above, this notion has been supported by fMRI studies that found greater feedback-related activity using a monetaryreward task in reward circuitry, namely the nucleus accumbens, in adolescents<sup>25</sup>. But other studies found the opposite pattern, namely lower accumbens activity in response to monetary gains in adolescents than in young adults<sup>26</sup>. On the other hand, the activity of the medial-frontal circuitry, which is implicated in conflict monitoring and decision making, increases from adolescence to adulthood during fMRI tasks in which participants assume some risk of penalty in pursuit of an explicit reward. This developmental difference is less pronounced, however, when potential penalties in the task are severe69.

Compounding these social and behavioural risks is the possibility that adolescents have less-aversive biological responses to substances of abuse. In adolescent rats, nicotine, amphetamine and alcohol produce less-pronounced acute effects and milder withdrawal responses<sup>70,71</sup>. Under the influence of alcohol, for instance, adolescent rats are less sensitive to developing motor impairment<sup>72</sup>, getting a 'hangover' (REF. 73) or becoming sedated. These developmental differences might be related to immaturity of the developing GABA<sub>A</sub> receptor (γ-aminobutyric acid type A receptor) systems<sup>74</sup>.

By contrast to their possibly morerewarding and less-aversive responses, adolescents might be more prone to the deleterious effects of substance abuse. The hippocampus of adolescent rats is unusually susceptible to ethanol-induced inhibition of long-term potentiation, making the rats more sensitive to the memory-impairing effect of alcohol<sup>75</sup>. This effect (which occurs at alcohol concentrations as low as 5 mM — equivalent to a single drink in humans), seems to be largely mediated through alcohol's effect on NMDA (*N*-methyl-D-aspartate) receptors, occurs at the single-cell level and is not confined to the hippocampus<sup>76</sup>.

Clearly, some neural alterations that take place during adolescence predispose to risk, whereas others, such as memory impairments, might be actually the result of the abuse. Morphometric studies of humans support this notion. For instance, in youths with a family history of alcohol abuse the right amygdala is smaller even before the onset of problem drinking, whereas hippocampal volumes are reduced only after a history of alcohol use<sup>77,78</sup>.

Exposure to substances of abuse in adolescence might also increase the likelihood of addictive disorders emerging later in life. For example, exposure to nicotine during adolescence, but not in the post-adolescent period, increases the reinforcing effects of nicotine in a self-administration paradigm in adult rats<sup>79</sup>.

*Affective and anxiety disorders.* Affective disorders, such as major depression, are common and serious disorders of adolescence; adolescent onset is associated with more-severe and more-disabling forms of these illnesses<sup>80,81</sup>. Anxiety symptoms frequently precede depression in adolescence<sup>82</sup> and during childhood<sup>83</sup>.

Structural MRI studies of adolescents with anxiety and affective disorders have reported structural anomalies in the superior temporal gyrus, the ventral prefrontal cortex and the amygdala<sup>84-86</sup>. An fMRI study of depressed and anxious adolescents reported anomalous amygdala responses to social stimuli<sup>87</sup>. In another fMRI study, adults but not adolescents engaged the orbitofrontal cortex when asked to switch from an emotional assessment of a face (that is, "How afraid does it make you feel?") to a non-emotional one (that is, "How wide is the nose?")88. The abnormal engagement of brain regions to emotional facial expressions in adolescents might underlie an unrealistic appraisal of emotions and thereby predispose to anxiety and depression.

Hormonal changes that occur during adolescence are likely to account for at least part of the risk for mood and anxiety disorders. Indeed, an intriguing clue to the biology of depression, anxiety and panic disorders is the change from equal female–male prevalence prepuberty to a 2:1 female–male

prevalence after puberty. Epidemiological evidence indicates that it is only after Tanner stage III that the sex differences in the incidence of depression emerge<sup>89</sup>. The finding that pubertal status predicts the sex difference in prevalence better than age<sup>90,91</sup> suggests that sex hormones play a part in the pathophysiology of these disorders.

A recent mouse study that examined the effect of tetrahydroprogesterone (THP), a steroid derived from progesterone, provides a possible mechanism for this phenomenon<sup>92</sup>. This hormone is released during stress and has an anxiolytic effect that is mediated by the activation of GABA

receptors, which are also activated by alcohol and benzodiazepines. However, when it binds to a particular subtype of GABA<sub>A</sub> receptor, namely the  $\alpha 4\beta 2\delta$  receptor subtype, THP has the opposite effect to that of alcohol and benzodiazepines: it increases anxiety. The expression of the  $\alpha 4\beta 2\delta$  receptor in the CA1 region of the hippocampus surges after puberty and is accompanied by increased anxiety, as measured on an elevated plus maze. Moreover, blocking the formation of THP alleviated the increase in anxiety in adolescent mice<sup>92</sup>. Whether the effects of stress-related hormones on the brain can explain the difference in rates of

#### Glossary

#### Androgen insensitivity syndrome

(Also known as androgen resistance syndrome or testicular feminization.) An X-linked, recessive condition characterized by a complete or partial failure of virilization that is due to a mutation on the gene that encodes the androgen receptor.

#### Anti-saccade task

A task in which subjects are required to suppress the automatic response of making a saccade towards a target and, instead, produce an eye movement in the opposite direction.

#### Congenital adrenal hyperplasia

A group of autosomal-recessive disorders caused by mutations in the genes for the enzymes that are involved in steroid synthesis. The result of these mutations is excessive or deficient production of sex steroids.

#### Delta-wave sleep

A stage of non-rapid-eye-movement sleep characterized by slow, or delta, waves (0.5-4 Hz); the more delta waves there are, the deeper the sleep.

#### Diffusion tensor imaging

(DTI). An MRI-based technique that allows one to characterize the structural properties of white matter.

#### Eriksen flanker task

A task in which subjects have to respond to a stimulus that is flanked by other stimuli that may code an alternative response.

#### Familial male precocious puberty

An autosomal-dominant disorder that occurs in males and is characterized by the onset of puberty (testicular enlargement) before 4 years of age.

#### Founder effect

The loss of genetic variation when a new colony is established by a very small number of individuals from a larger population.

#### Fractional anisotropy

(FA). The directionality of the (fast) diffusion of water in the extracellular space around the axons (in most common acquisition protocols). The more unidirectional the water diffusion is in a given fibre tract, the higher the FA value in that location.

#### Go/no-go task

A task in which the subject must produce a motor response for one class of stimulus but withhold responding to other classes of stimuli.

#### Magnetization transfer ratio

(MTR). A measure used for assessing white-matter properties; it provides information on the macromolecular content and structure of the tissue. Given that the macromolecules of myelin are the dominant source of MT signal in white matter, one can use MTR as an index of myelination. Note, however, that myelin is not likely to be the sole factor influencing the MTR.

#### Neural Darwinism

A neurodevelopmental process in which the synapses that are used the most are kept whereas the least-used connections are destroyed ('pruned').

#### Stop task

A test of response inhibition. On each trial, a stimulus (for example, a leftward- or rightward-pointing arrow) is displayed on a screen, and the subject has to respond as soon as possible by pressing the corresponding (left or right) key, unless a second stimulus (for example, a sound) signals that the response has to be withheld.

#### Stroop task

A task in which the subject is asked to name the colour of ink in which a word is displayed. The task is easy when the ink colour is congruent with the printed word (for example, 'red' printed in red ink). The task becomes difficult when the ink colour is incongruent with the printed word (for example, 'red' printed in green ink).

#### STS network

A set of regions, located along the superior temporal sulcus, that are involved in processing biological motion induced by the movement of different body parts, such as the eyes, the face or the entire body.

#### Tanner stage III

One of the five stages of puberty. Without resorting to a physical exam, pubertal stages can be assessed using, for example, the Puberty Development Scale, which is an eight-item self-report measure of physical development based on the Tanner stages with separate forms for males and females. For this scale there are five categories of pubertal status: prepubertal, beginning pubertal, midpubertal, advanced pubertal and postpubertal.

#### XXY

(Klinefelter's syndrome). A genetic syndrome that affects males and is caused by the presence of two X chromosomes (resulting in a 47-chromosome karotype).

anxiety and depressive disorders between prepubescents and adults awaits further investigation.

In summary, robust changes in hormones and hormonal receptors, increasingly powerful emotional responses to social stimuli and rapid alterations in motivation and reward systems might underlie the onset of anxiety and depressive disorders during adolescence.

#### **Conclusions and future directions**

The relationship between typical changes in the adolescent brain and the onset of psychopathology is not a unitary phenomenon, but an underlying theme can be conceptualized as 'moving parts get broken'. Adolescence is characterized by major changes in the neural systems that subserve higher cognitive functions, reasoning and interpersonal interactions, cognitive control of emotions, risk-versus-reward appraisal and motivation. Not surprisingly, it is precisely these changes that, when suboptimal in timing or magnitude, increase the risk of cognitive, affective and addictive disorders. Understanding the basis of these disorders therefore requires a comprehensive knowledge of how the brain is put together. Many advances are being made, but a lot remains to be learnt.

An emerging theme from paediatric neuroimaging studies is that the journey of brain development is often as important as the destination. For example, IQ is predicted by the developmental trajectory of cortical thickness, not by the adult cortical thickness93. The large individual variability in brain anatomy and function calls for longitudinal study designs that capture the nuances of heterochronous developmental curves. The first phases of longitudinal studies have mapped developmental trajectories for typical development, but those of patients with psychiatric illnesses have been mapped to a lesser extent. The next phases should go beyond simply mapping brain growth and begin to discern the adverse as well as protective factors that influence those trajectories.

A common initial approach to assessing causal influences on brain development is to discern the relative effects of genetic and non-genetic factors. This is best addressed through comparisons of monozygotic and dizygotic twins. Results from an ongoing paediatric longitudinal neuroimaging project at the Child Psychiatry Branch of the National Institute of Mental Health indicate significant age-by-heritability interactions, with heritability of grey-matter volume generally decreasing with age and heritability of

white-matter volume generally increasing with age<sup>94</sup>. Heritability-by-age interactions might be related to the timing of gene expression, which in turn might relate to the timing of the onset of illness. Post-mortem human and animal studies indicate that 'developmental' genes have diverse effects at various stages of brain development. But differences in heritability in different age groups may also reflect the cumulative effect of experience on brain structure; depending on certain inherent traits (for example, musical talent or personality), it is only with time that specific experiences start to shape the brain.

Multivariate analyses of twin data indicate that a relatively small number of shared genetic and environmental factors account for a substantial portion of the variance across multiple neuroanatomic structures<sup>95</sup>. Ongoing studies of specific gene effects on brain maturation may help to sharpen our understanding of brain-development mechanisms and provide insight into the aetiologies of various pathologies. The Saguenay Youth Study, carried out in a geographically isolated population with a known founder effect, will facilitate our search for genes that influence brain and behaviour during adolescence%. Finally, genetics may also provide biologically relevant subtypes of neuropsychiatric disorders that are obscured in current diagnostic schemes.

The marked sex differences in age of onset, prevalence and symptomatology for nearly every neuropsychiatric disorder may provide important clues as to these disorders' pathophysiology. The most-obvious outward physical manifestations of puberty are caused by changing levels of hormones97. Perhaps this has contributed to the tendency to attribute all of the cognitive and behavioural changes of adolescence to 'raging hormones'. But the relationship between hormones, the brain and behaviour is complex, reciprocal and poorly understood. Steroid hormones affect neuronal activity and morphology throughout development. Most neurons have receptors for adrenal and gonadal hormones, and when these receptors are activated they can affect neuronal function. Short-term effects are mediated by membrane-bound receptors, whereas long-term effects alter gene expression through intracellular or nuclear receptors. Conversely, the dramatic hormonal changes of puberty are triggered by alterations in excitatory and inhibitory inputs to gonadotropin-releasing hormone neurons in the pituitary. Hormonal effects drive aggression and sexual interest, but their

impact on impulse control, logical problem solving and other cognitive tasks has not been well established.

Social and cultural factors for boys and girls are profoundly different, and the relationship of these differences to manifest pathology should be explored. In the biological realm, sex differences probably stem directly from different genes on the X and Y chromosomes or indirectly from the effects of different hormone levels. Studies of subjects with sex-chromosome variations (for example, XO, XXY, XXYY, XXX or XXXXY) or anomalous hormone levels (for example, owing to congenital adrenal hyperplasia, androgen insensitivity syndrome or familial male precocious puberty) will be useful for sorting out the relative contributions of gene and hormone effects. For instance, males with an extra X chromosome (XXY or Klinefelter's syndrome) have a high incidence of language disorders, ADHD and social-skill deficits that are reflected in differences in cortical thickness, consistent with reports in the literature for XY subjects with such disorders98. Girls with congenital adrenal hyperplasia, which is characterized by intrauterine exposure to high levels of testosterone, have an entirely different pattern of structural findings, indicating differential effects of sex chromosomes and hormones on the brain<sup>99</sup>.

Although neuroimaging is beginning to establish correlations between brain structure/physiology and behaviour, the link between typical behavioural changes and psychopathology has not been firmly established. For example, the neural circuitry that underlies 'moodiness' in an adolescent might not be the same as that which is involved in depression or bipolar disorder. Neuroimaging data can help in the development of neuroanatomical models of cognitive, affective and social processes that are based on findings from developmental psychology<sup>100</sup>. Imaging studies of healthy adolescents are also helping to construct ageappropriate structural and functional brain templates.

Newer imaging approaches are being developed. Magnetic resonance spectroscopy studies using strong magnetic fields can help to quantify neurotransmitter systems, such as glutamate and GABA systems, as well as markers of neurogenesis<sup>101</sup>. Combining multiple imaging modalities, such as structural MRI, fMRI, DTI, magnetization transfer imaging, electroencephalography or magnetoencephalography, in the examination of single individuals will enhance our ability to interpret the signals for each of the modalities. Being able to examine simultaneously inter-individual variation from cellular to macroscopic levels will be instrumental in bridging the gaps between genes, the brain and behaviour.

Studies of the neural substrates of adolescent behaviour and decision making will need to be better integrated with social and educational science. Laboratory studies of teenagers using hypothetical situations in calm environments without peer influence might have little relevance for understanding real-world decision making, which often occurs in the presence of peers and in the context of intense physical or emotional arousal and conflicting priorities<sup>102</sup>.

Many questions about adolescent brain development and its impact on disease can best be investigated in animal models. Modelling the adolescent phase in animals is useful for investigating the risk for addictive and other early-onset neuropsychiatric disorders79. Although there are no animal models that represent the full phenotypic spectrum of a psychiatric disorder, such as schizophrenia or depression, individual phenotypic components of disorders such as developmental alterations that might be associated with the illness - can be used to construct animal models that are aimed at unravelling disease mechanisms and that allow novel interventions to be tested103.

Another translational approach involves combined *in vivo* (for example, MRI) and post-mortem studies in animals; such studies are essential for clarifying the nature of the neurobiological changes that drive the MRI findings. Of immediate relevance will be studies that attempt to discern the degree to which changes in cortical grey matter, as detected by MRI, are related to dendritic arborization, intracortical myelination or the encroachment of white matter on the inner cortical border.

Adolescence is a time of substantial neurobiological and behavioural change. These changes are usually beneficial and optimize the brain for the challenges ahead, but they can also confer a vulnerability to certain types of psychopathology. The technologies to elucidate the relationship between specific neurobiological maturational processes and specific normative or pathological changes are already in place. Applying these tools to understand when and how deviations from typical development occur may enhance our ability to prevent or treat disorders that affect a substantial number of people.

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#### DATABASES

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#### SCIENCE AND SOCIETY

# Beyond polemics: science and ethics of ADHD

#### Ilina Singh

Abstract | What is attention-deficit hyperactivity disorder (ADHD)? Why are so many children being diagnosed with ADHD and prescribed medication? Are stimulant drugs an effective and safe treatment strategy? This article explores the current state of scientific research into ADHD and the key social and ethical concerns that are emerging from the sharp rise in the number of diagnoses and the use of stimulant drug treatments in children. Collaborations among scientists, social scientists and ethicists are likely to be the most promising route to understanding what ADHD is and what stimulant drugs do.

Attention-deficit hyperactivity disorder (ADHD) is one of the most common childhood psychiatric disorders in the world<sup>1</sup>. Its core symptoms are inattention, hyperactivity and impulsiveness. Most children are first diagnosed with ADHD when they reach school age<sup>2</sup> and approximately 75% of those diagnosed are male<sup>3</sup>. The most common forms of treatment for ADHD are the stimulants methylphenidate and amphetamine<sup>4</sup>.

Rising rates of ADHD diagnosis and stimulant drug use in children have led to a public debate over the validity of the diagnosis, the root causes of ADHD and the ethics of treating children with psychotropic drugs. There are three partially overlapping positions in the debate. First, that ADHD is primarily caused by a combination of biological factors. From this perspective, diagnosis is valid and drug treatment is justified because it corrects an underlying neurochemical imbalance that affects cognitive and motor functions. Second, that ADHD is caused by a combination of biological and social factors; the diagnosis does not yet adequately capture the heterogeneity and complexity of the disorder. This perspective accepts the utility of stimulant drug medication, but some

## Exhibit 5

Ex.5 Page 50 MENU

## Health

## Mental Health Disorder Statistics

Mental and Behavioral Health (https://www.hopkinsmedicine.org/health/mental-and-behavioral-health)

## Statistics related to mental health disorders

The following are the latest statistics available from the National Institute of Mental Health Disorders, part of the National Institutes of Health:

- Mental health disorders account for several of the top causes of disability in established market economies, such as the U.S., worldwide, and include: major depression (also called clinical depression), manic depression (also called bipolar disorder), schizophrenia, and obsessive-compulsive disorder.
- An estimated 26% of Americans ages 18 and older -- about 1 in 4 adults -- suffers from a diagnosable mental disorder in a given year.
- Many people suffer from more than one mental disorder at a given time. In particular, depressive illnesses tend to co-occur with substance abuse and anxiety disorders.
- Approximately 9.5% of American adults ages 18 and over, will suffer from a depressive illness (major depression, bipolar disorder, or dysthymia) each year.
  - Women are nearly twice as likely to suffer from major depression than men. However, men and women are equally likely to develop bipolar disorder.
  - While major depression can develop at any age, the average age at onset is the mid-20s.
  - With bipolar disorder, which affects approximately 2.6% of Americans age 18 and older in a given year -- the average age at onset for a first manic episode is during the early 20s.
- Most people who commit suicide have a diagnosable mental disorder -- most commonly a depressive disorder or a substance abuse disorder.

https://www.hopkinsmedicine.org/health/wellness-and-prevention/mental-health-disorder-st... 1/2/2020

- Four times as many men than women commit suicide. However, women attempt suicide more often than men.
- The highest suicide rates in the U.S. are found in Caucasian men over age 85. However, suicide is also one of the leading causes of death in adolescents and adults ages 15 to 24.
- Approximately 1% of Americans are affected by schizophrenia.
  - In most cases, schizophrenia first appears in men during their late teens or early 20s. In women, schizophrenia often first appears during their 20s or early 30s.
- Approximately about 18% of people ages 18- 54 in a given year, have an anxiety disorder in a given year. Anxiety disorders include: panic disorder, obsessive-compulsive disorder (OCD), post-traumatic stress disorder (PTSD), generalized anxiety disorder (GAD), and phobias (social phobia, agoraphobia, and specific phobia).
  - Panic disorder typically develops in late adolescence or early adulthood.
  - The first symptoms of OCD often begin during childhood or adolescence.
  - GAD can begin at any time, though the risk is highest between childhood and middle age.
  - Individuals with OCD frequently can have problems with substance abuse or depressive or eating disorders.
  - Social phobia typically begins in childhood or adolescence.

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Ex. 5 Page 52

https://www.hopkinsmedicine.org/health/wellness-and-prevention/mental-health-disorder-st... 1/2/2020

# Exhibit 6



#### <u>APHA > Policy Statements and Advocacy > Policy Statements > Policy Statement Database ></u> Reducing Suicides by Firearms

## **Reducing Suicides by Firearms**

Date: Nov 13 2018 | Policy Number: 20184

Key Words: Gun Violence, Mental Behavioral Health, Mental Health, Violence

#### Abstract

Suicide by firearm is a public health problem. In 2016, firearm suicides accounted for half of all suicide deaths in the United States. Access to a firearm, particularly during a time of increased risk for suicide (e.g., divorce, job loss), has been identified as a key factor increasing one's risk for completing suicide. States with higher rates of gun ownership have higher suicide rates than states with low gun ownership, whereas non-firearm suicide rates are comparable, indicating that firearm access drives overall suicide rates. The most promising evidence-based strategies to reduce access to firearms during a period of high risk are (1) temporary relocation of household firearms away from home when a family member is at risk for suicide, (2) safe storage at home if relocation is not possible, (3) working with leaders in the gun community to develop and implement messaging about the preceding two strategies that will be acceptable to gun owners, and (4) increasing screening for and counseling about access to guns by health professionals and other gatekeepers. Working with gun owners, industry, law enforcement, physical and mental health professionals, and researchers is important in decreasing firearm suicides via evidencebased strategies. Declines in firearm suicides do not require decreased gun ownership rates. A concerted social marketing approach can incorporate firearm suicide prevention into standard firearm safety messaging. Moreover, the entertainment industry can model firearm suicide prevention behaviors. Through these efforts, firearm safety can include suicide prevention in a manner fully consistent with the Second Amendment.

#### **Relationship to Existing APHA Policy Statements**

The following APHA policy statements are relevant to the current statement:

- APHA Policy Statement 7524(PP): Suicide Prevention
- APHA Policy Statement 9818: Handgun Injury Reduction

This statement supplements 7524 regarding the role of firearms in suicide. Statement 9818 is replaced by this statement.

#### **Problem Statement**

According to the Centers for Disease Control and Prevention (CDC), suicide rates in the United States have increased by nearly one third over the past 20 years, with half of U.S. states experiencing an increase above 30% during this period.[1] Rates have risen among both sexes, all racial/ethnic groups, and all age groups under 75 years, as well as in rural, suburban, and urban settings.[2,3] There were 44,965 suicide deaths in the United States in 2016, with an age-adjusted rate of 15.6 per 100,000.[4] Suicide was the second-leading cause of death among individuals 10–14, 15–24, and 25–34 years of age.[5] It was the 10th-leading cause of death among all age groups combined and is one of only three leading causes that are increasing in prevalence.[5,6]

While suicide affects all individuals, males are four times more likely than females to die by suicide,[4] and the prevalence of suicidal thoughts, planning, and attempts is significantly higher among younger age groups (18–29 years) than older age groups.[7] Racial/ethnic and other groups with high suicide rates include American Indians and Alaska Natives, rural populations, and active or retired military personnel.[8] Suicide also affects the health of others; when people die by suicide, their family, friends, and community often experience shock, anger, guilt, and depression.

Suicide is a public health problem both economically and physically. According to the Suicide Prevention Resource Center, the estimated average cost of a single suicide is \$1,329,553.[9] Nearly all of this cost (97%) is attributed to lost productivity, with the remaining 3% due to medical treatment. It has been reported that the total cost of suicides and suicide attempts is \$70 billion per year.[10]

Multiple risk factors for suicide exist, including a previous suicide attempt (the strongest predictor), a history of depression or other mental illness, alcohol or drug abuse, a family history of suicide or violence, physical illness, and a feeling of being alone.[11,12] However, as evidenced by the far greater prevalence of these factors than of suicide deaths, most people with one or more such risk factors do not go on to die by suicide.[13] While individuals with these risk factors are encouraged to obtain mental health treatment, many of those who die by suicide have no known record of such treatment,[1] indicating a need for additional strategies to increase the safety of people at risk. Reducing access to lethal means is one such strategy.

There is ample evidence that suicidality is transitory. Should a person survive a suicidal impulse, his or her prognosis is quite good. The results of a meta-analysis of nearly 100 studies of suicide attempters showed that 90% of attempters who survive do not go on to die by suicide.[14] In fact, many suicide attempts occur with little planning,[15,16] often in response to a short-term crisis.[17,18] However, if a person attempts suicide through a means that is highly lethal, such as a firearm, the odds of survival are quite low.[19]

One must not opt to make a suicide attempt using a highly lethal means such as a firearm if there is to be any opportunity to obtain mental health treatment or endure a painful shortterm crisis. In 2016, 51% of all suicide deaths in the United States (a total of 22,936 deaths) involved firearms, with an age-adjusted rate of 7.8 per 100,000.[4] In nearly every age group, firearms were the leading mechanism for suicide deaths (among 10- to 14-year-olds, they were the second-leading mechanism).[4] Access to firearms is a key risk factor for suicide.[20-23] Several studies have shown that rates of suicide are higher in states with higher levels of gun ownership (but not higher rates of suicide attempts) and that these heightened rates are driven by increases in firearm suicides.[21,22,24] Suicides by methods other than firearms are not significantly different in states with lower or higher overall suicide rates.[24] Multiple reviews offer strong evidence that rises in gun ownership prevalence are associated with increases in firearm suicides, which in turn lead to increases in the overall suicide rate.[25-27] Studies of gun prevalence and suicide rates typically control for multiple potential confounders such as psychological distress, substance use, poverty, education, and unemployment.[22,28,29] They also typically reveal that the relationship between household gun ownership and suicide rates holds for men, women, children 5 to 14 years old, and those in nearly every other age group.[30-32] Decreasing the number of firearm suicides would yield a significant reduction in the overall suicide rate in the United States.[20-22]

A note on terminology: It is recognized in this policy statement that the terms "firearm" and "gun" are not identical in the weapons they cover. Precisely speaking, this statement addresses suicide by firearm. However, in much of the scientific literature, mass media, and common speech, the term gun is used when firearm would be more accurate. Here the term firearm is primarily used; however, in some cases the term gun is used when quoting the literature or referring to programs that use similar language (e.g., gun violence restraining order).

#### **Evidence-Based Strategies to Address the Problem**

A number of international studies have indicated that when lethal means are made less available or less deadly, suicide rates by the method in question decline, and often (when the method is commonly used) suicide rates overall decline.[27,33] This has been demonstrated in multiple countries and with a variety of suicide methods: bridges, domestic gas, pesticides, medications, and firearms.[34-38] Decreasing access to firearms is likely to reduce suicide in the United States overall because firearms are the most commonly used method,[4] consistent with their ubiquity in certain regions of the country (personal gun ownership is estimated to be as high as 25% in the South)[39] and high case fatality rate (92% versus 78% for hanging and lower percentages for other methods).[19] In addition, the speed with which one can carry out suicide by firearm increases the odds of a fatal outcome during an unplanned attempt or one involving minimal planning: 25% of attempters make an attempt within 5 minutes of deciding to end their lives.[16] Creating safer environments for those at risk of suicide by reducing their access to highly lethal means is emphasized in the CDC's technical guidelines for preventing suicide.[8] Numerous U.S.-based studies indicate that the most promising evidence-based strategies to decrease firearm suicides involve reducing access to guns in the following ways: (1) relocation of household firearms away from the home when a family member is at risk for suicide, (2) safe storage at home if relocation is not possible, (3) working with leaders in the gun community to develop and implement messaging about the preceding two strategies that will be favorable to most gun owners, and (4) increasing screening for access to firearms by health professionals and other gatekeepers.

Relocation of household firearms away from home when a family member is at risk for suicide: Between 2003 and 2005, about 90% of all suicides in the Israeli Defense Force (IDF), a mandatory population-based army drafting all youths 18–21 years of age, were firearm suicides. Since many IDF soldiers go home on the weekends, the IDF changed its weapons policy in 2006 to require that firearms remain on base when soldiers take a weekend leave. After this policy change, the overall IDF suicide rate decreased by 40% in

2007–2008. Most of this decrease was due to a reduction in weekend firearm suicides; there was no significant change in weekday suicide rates.[38] A study of suicides in Switzerland showed that when the army halved the number of soldiers from 2003 to 2004 (leading to a decrease in gun availability nationwide), both firearm suicide and overall suicide (but not non-firearm suicide) rates dropped among military-aged men but not military-aged women or older men.[40]

These IDF and Switzerland data support nationwide research indicating that restricting firearm access is effective in decreasing overall suicide rates. Over a 3-year period (2000-2002), the 15 states with the highest household firearm ownership rate (47%) had almost twice as many suicides (14,809) as the six states with the lowest ownership rate (15%; 8,052 suicides). This difference in overall suicides was largely accounted for by the difference in firearm suicides (9,749 versus 2,606). Non-firearm suicides and the total populations of the two sets of states were comparable.[22] During a more recent 2-year period (2008-2009), these findings persisted.[24] If the relationship is causal, these results suggest that a 1% decrease in household firearm ownership could reduce the firearm suicide rate by 3.5% and the overall suicide rate by 1.5%, with even greater effects for adolescents.[22] By extrapolation, reducing household firearm ownership by 5% could prevent about 3,000 suicides per year in the United States.[4] This is not to say, however, that gun owners must sell their firearms in order to reduce firearm suicides. Suicide prevention professionals are having success working with gun owners to help them identify offsite storage options (e.g., firearm retailers, shooting ranges, pawn shops, law enforcement facilities, storage facilities, and family members and friends authorized to store firearms) that will keep loved ones safe while preserving gun ownership rights.[41,42] These options could be temporary or permanent depending on the gun owner and his or her family's assessment of the benefits and risks of bringing firearms back into the home.

Safe storage of firearms at home: A gun owner may not find temporary relocation of firearms outside the home to be feasible. Gun owners may wish to talk over tenable storage options with loved ones as well as with mental health clinicians or other health professionals. In the event firearms cannot be relocated, numerous studies indicate a reduced risk of firearm suicide when household guns are stored locked and unloaded, with ammunition locked separately.[23,43,44] Having a locked gun closet, keeping firearms unloaded, and locking ammunition or storing it in a different part of the house each reduces the risk of suicide by 55% to 73%.[23] One study revealed that the risk of suicide was three times greater among individuals in households with loaded firearms than among those in homes with unloaded guns. Households with guns stored in unlocked places were associated with more than twice the risk of suicide than households in which firearms were kept in a locked place, and homes with one or more handguns were associated with a risk of suicide almost twice as high as that in homes containing only long guns.[43] An evaluation of a community-based giveaway of gun storage devices indicated a 13% increase in safe storage of firearms.[45] Also, a review of firearm safe storage interventions showed that providing owners with a safety device such as a lock box significantly improved storage practices.[46] Safe storage of firearms in the home is a critical component of reducing firearm suicide.

Education of gun owners on the relationship between firearms and suicide: Since 2009, gun rights advocates and suicide prevention experts have been working together to develop and test messages for gun owners on the importance of temporarily relocating firearms outside the home when a household member is struggling with a mental health or substance abuse problem or going through a painful crisis such as a divorce or job loss. A study of firearm retailers in New Hampshire showed that 48% of retailers that received intervention materials were still using at least one of the materials several months later. Retailers who believed that there was a relationship between firearm accessibility and risk of suicide were more likely to be using intervention materials.[41] Changing social norms related to firearms and safety is critical in intervention efforts and requires spreading accurate information via trusted messengers. Success in changing norms has been shown elsewhere in the public health literature, illustrating the role of advocacy and grassroots efforts, especially when combined with a multicomponent strategy.

For example, campaigns have been effective in spreading messages about the physical dangers and legal consequences of drunk driving.[47] An especially successful organization is Mothers Against Drunk Driving (MADD). Through its widespread programs, services, and legislative efforts, MADD has influenced social norms related to drinking and driving such that the number of U.S. alcohol-related auto fatalities fell from 25,000 to 17,000 between 1982 and 2003, while the number of auto fatalities from crashes unrelated to alcohol increased from 18,000 to 25,000.[48,49] During that time, the entertainment industry worked with public health experts to promote the "designated driver" campaign on popular television shows.[50] The industry has also worked with public health experts to promote the group the use of checklists during surgery, educate the public about human papillomavirus (HPV) and HIV, reduce the occurrence of smoking in youth-rated movies, and publicize countless other public health issues.[51–53] The American Foundation for Suicide Prevention's recent partnership with the National Shooting Sports Foundation is an important step toward communicating effectively with gun owners about preventing firearm suicides.[54]

Increasing screening for access to firearms: Doctors, nurses, mental health providers, and other health professionals have a critical opportunity to increase awareness about the link between firearms and suicide among those living in homes with firearms. Many people considering suicide do not seek mental health services, so it is important for primary care providers, emergency department (ED) doctors, and other health providers to screen for access to firearms, even among patients without mental health concerns. [55,56] One study showed a threefold increase in actions intended to limit access to household firearms among parents who brought their children to an ED for mental health treatment and received counseling on firearm access relative to those who did not receive counseling in the ED.[57] A more recent study revealed an increase in safe storage of firearms after EDbased counseling.[58] In spite of this, another recent study showed that only 18% of suicidal patients presenting at an ED received a lethal means assessment, with only 8% of these individuals receiving actual counseling on how to reduce access to lethal means.[59] Surveys of providers have shown that only 50% usually screen for access to firearms among suicidal patients, even though most think it is important to do so.[60] There are resources available to educate providers on the importance of conducting lethal means counseling and how to do so in a way that is respectful of the right to own guns.[61]

Each of these four strategies would benefit greatly from funding to further examine effective methods of implementation, but suicide prevention research is woefully underfunded. Alcoholism receives five times more research funding for 50% more deaths, and breast cancer receives nearly seven times more funding for fewer annual deaths.[62]

#### **Opposing Arguments/Evidence**

Firearm suicide is a public health issue wrought with assumptions and misinformation. While suicide is a subject familiar to Americans, guns as a suicidal determinant are underestimated. A common assumption is that suicide is premeditated, giving a person ample time to obtain access to a highly lethal means such as a firearm. While some suicides are planned, many suicide attempts occur within less than an hour of thinking about them. [15,16] Depression and substance abuse are risk factors for suicide that a person may be managing long term, but the elevated risk of a suicidal attempt is often short and fleeting. [16,63] A person experiencing an acutely distressing incident may respond impulsively to suicidal thoughts.[63] If this person does not have access to a highly lethal means such as a firearm during this crisis, she or he is more likely to survive the crisis either by attempting suicide with a less lethal means and surviving or by opting not to make an attempt at all.[27]

It is often argued that someone suicidal cannot be stopped, regardless of what methods are readily available. This implies that someone who survives a suicide attempt will simply die by suicide in a subsequent attempt. While there are certainly some suicides that cannot be prevented, as previously mentioned, a review of nearly 100 studies determined that 90% of people who attempted suicide and survived did not go on to die by suicide later.[14] One is more likely to survive a suicide attempt if one does not use the most highly lethal means available to make that attempt. Firearm suicide is the suicide method with the highest case fatality rate, with over 90% of attempts resulting in death.[19] By contrast, case fatality rates for hanging, carbon monoxide poisoning, and drug poisoning are 61%, 42%, and 2%, respectively.[19] Most people who die by suicide have not made an earlier attempt.[64,65] Their choice of method does not leave room for a change of heart later, whereas other methods do, and we see the change of heart in the literature on those who survive suicide attempts. Unfortunately, a firearm typically results in death after only one attempt.[66]

Those opposed to reducing firearm access have pointed out that some countries have high suicide rates but low firearm ownership rates. The effectiveness of means restriction in suicide reduction is well documented across a variety of means. Australian suicide rates decreased as barbiturate access was restricted.[67] Suicide in Asia dropped once access to pesticide, the most common means of suicide in the region, was limited.[36] In the United Kingdom in the 1960s, carbon monoxide levels in domestic gas were reduced from 20% to nearly 0%, corresponding with a 30% decrease in the suicide rate.[35] Countries seeking to reduce their suicide rate should contemplate means reduction strategies for the methods most likely to effect a sizeable reduction in suicides among their population. In the United States, firearms are the most common method of suicide.[20,27,63]

When asked about offering gun storage, some retailers have expressed concern about liability risks should a death occur once the firearm has been returned to the owner.[68] Having discussed this issue with lawyers familiar with relevant case law, the authors are unaware of any evidence of legal liability for events that might occur when a gun is returned. [69]

Some would oppose reducing access to lethal means because this would make a firearm unavailable for self-defense. Each household must weigh the likelihood of needing a firearm for self-defense against the likelihood of that firearm being used in a suicide or other gun death. These odds may well change repeatedly over time and can be reassessed regularly.

There have also been challenges to the professional autonomy of doctors involving the powers of the state to limit the topics that physicians can discuss with patients. In Florida, an act that prohibited physicians from asking patients about firearm ownership and storage was ruled invalid by the courts.[70] While there is the possibility of additional attempts to gag medical professionals, this ruling supports the rights of physicians.

#### **Alternative Strategies**

In popular media regarding firearm violence, both self-directed and outward, mental health care is often raised as the most important method for preventing shooting deaths.[71] The importance of access to mental health treatment cannot be denied. However, a recent CDC report noted that 54% of suicide decedents in 27 states in 2015 did not have a known mental health condition.[1] If, for the sake of argument, it is assumed that those decedents had an undiagnosed mental illness, it must be acknowledged that a lethal suicide attempt cut short the available time for persuading them to seek help. The pervasive stigma against acknowledging mental health struggles, as well as the lack of a cure for mental illnesses, must also be noted. The mental health field cannot be solely responsible for the universe of people at risk for suicide. Support from public health can be offered around how to talk about firearms with patients and their loved ones, as well as around information about local storage options for families seeking alternatives to storing firearms at home. Furthermore, the majority of people living with mental illness will not attempt suicide, and predicting who among them will is a very inexact science.[71] It is important not to reduce their propensity to seek treatment by categorically infringing upon their rights (e.g., by prohibiting anyone who has ever been diagnosed with a mental illness from owning a firearm).[72,73] Moreover, research into the impact of mental health on suicide has demonstrated that firearm access has independent effects on risk above and beyond other covarying risk factors, including mental illness. [24]

There are two other ways to reduce access to guns among those at risk for suicide that are more controversial and lack sufficient evidence to currently promote their implementation as firearm suicide prevention strategies: gun violence restraining orders (or other legislative approaches) and smart gun technology.

Gun violence restraining orders allow family members, intimate partners, and law enforcement personnel to formally request confiscation of firearms that belong to someone at risk of hurting him- or herself or others. These orders are fairly new, and their efficacy is as yet undetermined regarding firearm suicide prevention.[74] It is worth noting that 30% to 40% of firearm sales take place in the secondary market, where sellers are unlicensed. meaning that law enforcement cannot rely on a database to know what guns are in the home to confiscate. [75] Furthermore, a detailed discussion of legislative approaches in general was left out of this policy statement for several reasons. First, studies of firearm legislation and suicide, even those controlling for gun ownership rates, have been unable to demonstrate an actual reduction in firearm availability stemming from such legislation.[27] Second, the U.S. gun stock is so large relative to the marginal firearm likely to be affected by a given piece of legislation that, similar to Azrael and Miller, [27] the authors of this statement are unwilling to assume that firearm exposures have changed sufficiently to impact suicide rates as a result of said legislation. Finally, legislating gun access stands to further deepen the divide between gun owners and non-gun owners. Instead, health professionals can support innovative work being done within suicide prevention to regard gun owners as part of the solution rather than part of the problem.[76–78] With more research, strategies to reduce firearm suicide deaths may involve contemplating a legislative agenda; however, gun owners may not be receptive. This would be counterproductive to efforts regarding the present policy.

Smart gun technology prevents unauthorized use of a firearm by rendering it inoperable for anyone other than the owner. This could potentially prevent many suicidal individuals from obtaining quick access to a firearm, especially in the home (e.g., adolescents). However, this technology is not sufficiently widespread to evaluate its impact on firearm suicide,[26] and support for such an approach among gun owners is limited.[79]

#### Action Steps

- 1. Mental health advocates and governing associations for mental health professionals should adopt and promote available guidelines for mental health providers on screening for guns in the home if a client/patient is having a psychiatric crisis or has a chronic mental illness.[58,61] State-level professional associations should equip mental health providers with information about local offsite storage options when available for families that need to temporarily or permanently relocate their firearms outside the home.
- 2. State associations affiliated with national organizations such as the American Academy of Pediatrics and the American Medical Association, or accreditors such as the Joint Commission and the American College of Surgeons, should adopt and promote guidelines promulgated by their parent organizations advocating screening for firearms in the home along with counseling so that lethal means reduction awareness can be spread in general office appointments and emergency departments and safe storage strategies can be discussed, in addition to provision of firearm safety devices when possible.

- 3. State public health agencies should collaborate with public safety and mental health agencies to advocate for an increase in the availability of temporary firearm storage outside the home. Law enforcement personnel, firearm retailers, range owners, instructors, and other leaders in the firearms community are important partners, as they can provide low-cost gun storage for clients and normalize the inclusion of suicide prevention in gun safety education.
- 4. Public safety agencies should work with the legal community to educate law enforcement personnel and retailers/range owners about their rights and protections should they offer firearm storage.
- 5. Public health and mental health agencies should collaborate with firearm owners to increase awareness of the importance of lethal means reduction when a loved one is at risk for suicide. It is important to include gun owners as part of the solution instead of assuming that they are opposed to safety measures when it comes to firearm suicides.
- 6. Public health agencies and advocates should collaborate with the entertainment industry as well as the news media and communications firms to support safe storage practices. Films, television shows, news stories, and so forth should provide venues to normalize messaging on gun access in instances in which there is concern about someone's risk for suicide (whether on the part of a parent, spouse, health provider, teacher, or other concerned stakeholder).
- 7. Public health agencies should collaborate with gun owners and suicide prevention professionals to advocate for increased public and private funding of firearm suicide research. A small number of private foundations and funders have shouldered an undue portion of the responsibility to fund firearm research efforts, as support from the federal government and large foundations is minimal. Public agencies should work with foundations and private funders to develop funding opportunities that support firearm suicide prevention.[78] Such efforts should include development and evaluation of firearm safety education that incorporates suicide prevention and development of social marketing campaigns around firearm suicide prevention.

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# Exhibit 7

# Suicide Mortality in the United States: The Importance of Attending to Method in Understanding Population-Level Disparities in the Burden of Suicide

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### **Keywords**

suicide methods, means restriction, firearms, case fatality ratio

### Abstract

Suicide mortality varies widely across age, sex, race, and geography, far more than does mortality from the leading causes of natural death. Unlike the tight correlation between cancer mortality and the incidence of cancer, suicide mortality is only modestly correlated with the incidence of suicidal acts and other established risk factors for suicidal behavior, such as major psychiatric disorders. An implication of this modest correlation is that the proportion of all suicidal acts that prove fatal (the case fatality ratio) must account for a substantial portion of the (nonrandom) variation observed in suicide mortality. In the United States, the case fatality ratio is strongly related to the availability of household firearms. Findings from ecologic and individual-level studies conducted over the past two decades illustrate the importance of accounting for the availability of highly lethal suicide mortality across populations.

### **INTRODUCTION**

In 2007, the most recent year for which data are available, nearly 100 Americans died each day by suicide, making suicide the eleventh-leading cause of death for Americans (26; see sidebar, Definition of Terms). For Americans 40 years of age and younger, suicide is the second leading cause of death, exceeded only by motor vehicle fatalities (26).

The age-adjusted mortality rate for suicide, 11.3 per 100,000 population, is approximately three times higher than the age-adjusted death rate from HIV/AIDS (3.7) and twice that from leukemia (5.0), but much lower than the mortality rate from heart disease (197.2) or from cancer as a whole (182.9) (24).

Compared with suicide rates among adults in other industrialized countries, the suicide rate among adults in the United States falls roughly in the middle (125). Among younger persons, however, suicide mortality is relatively high: For children under 15 years of age, for example, the overall suicide rate in the United States is twice that of the average of other industrialized Westernized countries, largely accounted for by a firearm suicide rate 11 times that of the average of these countries (65).

Age, sex, race, and other demographic characteristics—including marital status, income, educational attainment, and employment status—all influence suicide mortality (94).

### **DEFINITION OF TERMS**

Suicide, in this review, refers to death by suicide, suicide attempts to nonfatal suicidal acts, and suicidal acts to the sum of suicide and suicide attempts. Case fatality ratio (CFR) is an aggregate measure of the proportion of all suicidal acts that result in death. Method-specific CFRs refer to the probability of death given a suicidal act with a particular method. We use the term deliberate self-harm when referring to hospital data because the coding system for hospital data does not differentiate suicide attempts from nonsuicidal self-harm. The majority of hospital visits for deliberate self-harm are for suicide attempts (22). When we report data on suicide attempts, we are referring to survey-based, self-report data. Suicide rates are higher, for example, for white and Native Americans than for black, Hispanic, and Asian Americans (24, 94). For white men, suicide rates peak in midlife and again in very old age; for white women, rates peak in midlife (24, 94). For nonwhite men and women, suicide mortality is highest in early adulthood (24, 94). A consistent finding across numerous studies is that the strongest individual-level risk factor for completing suicide is having previously attempted suicide (12, 46, 62, 94, 106); other strong risk factors include psychiatric and substance abuse disorders (2, 6, 12, 21, 45, 46, 55, 62, 94, 106, 112, 126).

Although the great majority of suicides appear to be associated with a retrospectively diagnosable mental health or addictive disorder (2, 21, 55, 126), identifying in advance those individuals who will subsequently die by suicide has not been possible (44, 99, 100). Moreover, psychiatric and behavioral factors appear to explain little of the marked, systematic variation in suicide mortality observed across populations: Whereas psychiatric factors may help explain why a particular person died by suicide, they do not explain why, year after year, residents of some states die by suicide at three times the rate of residents of other states (26), why residents of rural areas in the United States are more than one and one-half times more likely to die by suicide compared with city dwellers (26) or why more than four times as many males (compared with females) die by suicide (26), even though females attempt suicide more often than do males (22, 94, 95).

This article reviews how questions such as these have been addressed in the recent empirical literature and examines how, as a result, a better understanding of the population-level determinants of suicide mortality has been achieved in recent years. The empirical focus is the distribution of suicide mortality in the United States, but pertinent international studies are adduced to elaborate conceptual insights and illustrate practical advances. The review concludes with an assessment of the implications of recent insights for future research, policy, and prevention efforts.

### INSIGHTS FROM JOHN WENNBERG AND THE FIELD OF INJURY PREVENTION

Research on small area variation, pioneered by John Wennberg in studies of medical practice and extended to work that explains systematic variation in nontraumatic causes of death (82, 118–121), has demonstrated the benefit of focusing on and teasing apart different systematic components of variation in mortality rates. These insights suggest that geographic (or other) variation in mortality rates can usefully be decomposed into variation in the incidence of disease or injury, and the probability of death given the disease or injury (e.g., the proportion of people with cancer who die from cancer in a given year, the proportion of motor vehicle collisions or suicidal acts that prove fatal).

This approach, sometimes referred to as the decomposition method, suggests that, with reference to understanding patterns of suicide mortality, it may prove useful to view the suicide mortality rate (SMR; suicides/population) as the literal product of the incidence of suicidal acts (ISA; suicidal acts/population) and the proportion of suicidal acts that result in death [the case fatality ratio (CFR); suicide deaths/ suicide acts]:

 $SMR = ISA^*CFR$ 

**CFR:** case fatality ratio

### ECOLOGIC PATTERNS OF SUICIDE IN THE UNITED STATES

In the United States, as in most countries, disparities in the burden of death by suicide are generally more pronounced across geography, age, sex, and race than over time, as a result of which the vast majority of ecological studies have focused on the former dimensions rather than the latter. Over the half-century between 1955 and 2005, for example, the suicide rate for the United States as a whole ranged between 10.2 and 12.8 per 100,000 population (124), whereas across the 50 states in any given year, suicide mortality varied more than threefold (Figure 1), a range nearly as large as the discrepancy in suicide rates by sex [18.4 per 100,000 among males, compared with 4.8 per 100,000 among females (24)].



Figure 1

Age-adjusted suicide rate by state of residence, 1999–2007. Rates are age-adjusted to the year 2000 standard. Source: CDC WONDER (24).

By contrast, geographical variation in mortality for other leading causes of death is more modest. For example, across the 50 states cancer mortality rates vary less than twofold [and the standard deviation is  $\sim 10\%$  of the mean, compared with suicide, for which the standard deviation is 30% of the mean (26)]. The distributions of mortality from cancer and suicide do, however, share one salient characteristic: A single broad subcategory of disease (or injury) accounts for a large proportion of all deaths and a substantial amount of the observed variation in mortality rates. For cancer, lung cancer accounts, on average, for approximately onethird of all cancer deaths (but fewer than onesixth of all cancers); for suicide, firearm suicides account for more than half of all suicide deaths (but fewer than 1% of all suicidal acts) (116).

Unlike the tight correlation between (*a*) the mortality rate for cancer, and (*b*) the incidence of new cases of cancer, suicide mortality is only modestly correlated with the incidence of suicidal acts. Indeed, rates of suicide attempts (and strong correlates of attempts, such as major psychiatric illness) and rates of death by suicide are often weakly, sometimes inconsistently, and occasionally perversely correlated across many dimensions, including geography (e.g., U.S. states), spatial constructs (e.g., the rural-urban continuum) (61, 62), time (59), and demographic characteristics (e.g., sex, age, and race) (3, 6, 29, 61, 62).

For example, the Spearman rank correlation coefficient relating the incidence of hospital discharges for deliberate self-harm (1) and mortality from suicide (24) across the 26 states for which data are available is 0.2. Adding suicide fatalities (24) to hospitalized acts of deliberate self-harm (to approximate the incidence of suicidal acts overall) increases the correlation with suicide mortality, but the association remains modest (correlation coefficient = 0.4). By contrast, the correlation coefficient between suicide mortality and the CFR for suicidal acts is 0.7, a correlation as strong as that between cancer incidence and cancer mortality across these same 26 states. A no-more-than-modest correlation between mortality due to suicide and suicide attempts is also apparent over time. For example, whereas suicide rates in the United States declined by more than 15% between 1990 and 2000 (81), and by  $\sim$ 25% among youth over this same time period (23, 81), suicide attempt rates remained virtually unchanged for both adults (59) and youth (25).

Publicly available data from the National Survey of Drug Use and Health, used by a recent study (92) that examined predictors of variation in suicide rates across the 50 states, also reveal only modest correlations between suicide mortality and measures of psychiatric and behavioral problems. For example, across the 50 states, correlation coefficients relating suicide mortality and (*a*) major depressive disorder, (*b*) drug or alcohol abuse or dependence, and (*c*) a composite measure of serious psychological distress were, respectively, 0.4, 0.2, and 0.2.

A partial explanation for the modest correlation between suicide mortality and these strong individual-level risk factors for suicide is suggested by the observations that (a) the great majority of people who have engaged in suicidal acts do not die by suicide either in the short term (116) or in the long term (98), and (b) the major determinant of the proportion of suicidal acts in a population that prove fatal is the distribution of highly lethal methods used in suicidal acts, which is itself not strongly related to the underlying base rate of suicidal behavior.

### DETERMINANTS OF THE CASE FATALITY RATIO

Because ecologic patterns of suicide mortality are not well explained by patterns of suicide attempt rates, variation in the proportion of suicidal acts that prove fatal (i.e., the CFR) must explain the residual systematic variation in death by suicide. For geographic and demographic subgroups, the CFR is determined largely by the distribution of methods used in suicidal acts, rather than by marked variation in method-specific case fatality across subgroups (28, 40, 56, 87, 113). For example, compared Annu. Rev. Public Health 2012.33:393-408. Downloaded from www.annualreviews.org Access provided by 38.88.13.122 on 04/05/19. For personal use only. with females, males are less likely to engage in suicidal behavior (and only slightly more likely to die by suicide when they use a given method), but three to four times more likely to die in a suicidal act because the methods that men use are far more lethal (56, 87, 113). The same pattern is observed across age groups: Younger people engage in suicidal behavior more frequently, but older people are far more likely to die in any suicidal act because the methods older people use are, on average, more lethal (87, 113). Likewise, white and black Americans appear to engage in suicidal acts at similar rates (and have similar method-specific CFRs), but suicide rates are nearly twice as high for whites because the overall CFR for whites is nearly twice that for blacks (113).

Using data that include only those cases of intentional self-harm that received medical evaluation in an emergency department or hospital, several studies have found enormous differences in the CFR for different methods used in suicidal acts. For example, 85–90% of self-injuries with a firearm and 60–70% of hanging/suffocations prove fatal, but only 2% or less of overdoses and self-injuries with a sharp instrument result in death (87, 113, 116) (**Table 1**). Even within the category of poisoning, CFRs vary considerably: Pesticides and other agricultural chemicals often used in suicidal acts in developing countries are, on average, more lethal than the psychotropic and analgesic medications often used in deliberate self-poisonings in industrialized countries (39). In the United States, a predominant determinant of the CFR across geographic and demographic subgroups, and over time, is the proportion of suicidal acts that are committed using firearms (87, 113).

### THE FIREARM-SUICIDE CONNECTION: EVIDENCE FROM ECOLOGIC AND INDIVIDUAL-LEVEL STUDIES

### **Cross-Sectional Studies**

Just as the distribution of mortality rates for lung cancer accounts for a significant proportion of the variation in mortality from cancer overall, suicide mortality rates in the United States appear to be driven largely by rates of firearm suicide (86, 88, 91, 92). For example, **Figure 2** shows that suicide rates in rural areas of the United States are considerably higher than in urban areas: The most urban areas have mortality rates of less than 10 per 100,000 residents, compared with rates of more than 14 per 100,000 residents in the most rural areas. Moreover, mortality from suicide across the

Table 1 Case fatality ratio by method of self-harm, United States, 2001. Listed here are all U.S. suicide deaths in 2001 and estimated visits to the emergency department based on a nationally representative sample of emergency departments. Source: Vyrostek et al.  $(116)^a$ 

Method	Fatal	Nonfatal	Total	Case fatality ratio
Firearm	16,869	2,980	19,849	85
Suffocation/hanging	6,198	2,761	8,959	69
Poisoning/overdose	5,191	215,814	221,005	2
Fall	651	1,434	2,085	31
Cut/pierce	458	62,817	63,275	1
Other	1,109	35,089	36,198	3
Unspecified	146	2,097	2,243	7
Total	30,622	322,992	353,614	9

<sup>a</sup>Case fatality ratio is the proportion of cases recorded in a year that are fatal. The emergency department (ED) estimate overstates ED-treated suicide attempts (because nonsuicidal self-harm cannot be disaggregated from actual suicide attempts), but it underestimates nonfatal attempts because it does not capture suicide attempts that do not result in medical care.



### Figure 2

Suicide rate per 100,000, United States, 1999–2007, aggregated by level of urbanization of the decedent's county of residence. Source: CDC WONDER (24).

rural-urban continuum is accounted for by differences in rates of firearm suicide, without compensatory changes in rates of nonfirearm suicide. Notably, most (60–62, 101, 102), though not all (104), studies have found that rates of suicidal acts and risk factors for suicidal behavior, such as depression, are not higher in rural areas compared with urban areas.

In 2001, the Behavioral Risk Factor Surveillance System (BRFSS) (27) obtained the first state-level estimates of firearm prevalence sufficiently precise to allow for empirical evaluation of the relation of firearm ownership to suicide for the nation as a whole. Prior to 2001, nationally representative ecologic studies relied on proxies for gun ownership or examined variation across larger areas of aggregation, such as the census regions. Despite the imprecision of the proxies used and the small number of observations across which variation in firearm ownership had been measured directly, studies consistently found a positive association between cross-sectional measures of firearm prevalence and firearm suicide (8, 57, 63, 70, 72, 79, 84-86, 110). Findings in these earlier

studies with respect to the association between firearm prevalence and rates of overall suicide, however, were mixed, depending largely on the way firearm prevalence was measured, as reviewed elsewhere (11, 90, 93). In the absence of more precise measures of firearm ownership, questions about the relation between household firearm prevalence and overall suicide mortality lingered, in part because questions were raised about whether the proxies used in ecologic studies produced associations that were biased away from the null (93). This criticism was put to rest once the BRFSS made its 2001 survey results available (27).

One of the first studies to exploit comprehensive state-level firearm prevalence data from the BRFSS examined the relationship between rates of household gun ownership and suicide across the 50 states for the period 2000–2002 (92). The study controlled for rates of serious psychological distress, along with rates of poverty, urbanization, unemployment, and drug and alcohol dependence and abuse. As in a smaller study of northeast states for which deliberate self-harm data were available (91),

398 Miller • Azrael • Barber



### Figure 3

Rate of suicide per 100,000 in 2004 for overall suicide, firearm suicide, and nonfirearm suicide, in the 50 states, by state-level prevalence of firearm ownership. Sources: CDC BRFSS (83), CDC WONDER (24).

this study found in both crude and multivariate analyses, among men and women and in every age group, that states with higher rates of household gun ownership had higher rates of firearm suicide and overall suicide. The study showed no association between firearm ownership rates and nonfirearm suicide (92).

A graphical depiction of the bivariate findings from the study is presented in **Figures 3**a and **3**b, updated to include data from the most recent year for which BRFSS firearm ownership data are available (2004) and suicide mortality data from the corresponding year. Trend lines depict the correlations between firearm prevalence and suicide mortality overall and by method. Firearm prevalence, as shown in the figure, correlates strongly with mortality due to suicide overall (correlation coefficient = 0.7) and with firearm suicide (correlation coefficient = 0.8), but not with nonfirearm suicide (correlation coefficient = 0.1).

### Longitudinal Studies

One approach to mitigating potential confounding by cross-sectional differences in psychopathology (or other markers of suicideproneness that might sort by geography) is to conduct longitudinal analyses. An early study using aggregate national data on firearm ownership, 1959–1984, found a significant bivariate relation between firearm ownership and firearm suicide, but no relation between firearm ownership and overall suicide (30). Firearm availability was measured by responses to Gallop and National Opinion Research Council (NORC) polls (the biannual General Social Survey, GSS). The variability in

Ex. 7

measured firearm ownership over time appears to have been largely a function of differences in the samples and methodologies used by Gallop and NORC (90), and the null finding, on that basis, may have been due to measurement error (90). Until recently most ecologic time-series studies abandoned direct measures of firearm prevalence because available measures were too imprecise (relative to secular changes in suicide rates) to power analyses adequately. Instead, most ecologic studies focused on the relation between passage of firearm legislation and subsequent rates of suicide (or geographic variation in firearm laws and cross-sectional variation in suicide rates) with mixed results (20, 32, 71, 73, 74, 76, 103, 117), as reviewed elsewhere (11, 91). One drawback to longitudinal studies on firearm legislation is that investigators did not measure whether firearm prevalence or exposure changed after legislation was enacted (11, 91).

During the 1990s, the percentage of Americans living in households with firearms declined far more than it had over the previous three decades (36). The ongoing General Social Survey, for example, found that compared with the 1960s, 1970s, 1980s, and early 1990s, when roughly one in two Americans lived in a home with a firearm, by 2000 that fraction had fallen to one in three. The first study to exploit this significant decline in household firearm ownership examined whether changes in firearm prevalence had been accompanied by significant changes in rates of suicide by firearms, by other means, and overall (88). The findings, illustrated below for the nation as a whole (Figure 4), were consistent with those from cross-sectional studies and case-control studies in that the time-series study found a significant relation between declines in household firearm ownership and declines in rates of suicide overall and by firearms, but not by nonfirearm methods. These results held for the population as a whole, for men, for women, and most markedly for children and young adults, even after adjustment for several potential confounders including rates of poverty, unemployment, per capita alcohol consumption, the age distribution of the population, and census region.



### Figure 4

Method-specific suicide rate per 100,000, United States, 1985–2007. Source: CDC WONDER (24).

### Individual-Level Studies

The literature reviewed thus far has focused on ecologic patterns of suicide mortality in the United States. Considered as a whole, these studies point to the strong association between the availability of firearms and suicide mortality. The empirical foundation for this assertion rests not only on analytic studies of aggregate data, but also on compelling evidence from individual-level studies, many of which have been conducted over the past two decades. These studies, the most pertinent of which are summarized below, address concerns about the ecologic fallacy by providing associations at the individual level analogous to those observed at the aggregate level.

Every U.S. case-control study in the peerreviewed literature that has examined the firearm-suicide connection has found that a firearm in the home is associated with an increased risk of suicide (10, 12–16, 31, 34, 47, 48, 58, 67, 68, 107, 122). The increase in risk is large, typically 2–10 times that of homes without firearms, depending on the sample population (e.g., adolescents versus older adults) and on the way in which the firearms are stored (48). The association between firearms in the home and the risk of suicide is due to a large increase in the risk of suicide by firearm that is not counterbalanced by a reduced risk of nonfirearm suicide.

Three additional findings from the casecontrol studies are notable. First, the higher risk of suicide in homes with firearms applies not only to the gun owner but also to the gun owner's spouse and children (12–15, 17, 18, 33, 34, 58, 122); the relative risk of suicide associated with a firearm in the home is greater for young people (11, 58, 90), and greatest for those without known psychopathology (13). Second, the presence of a firearm in the home, no matter how it is stored, is a risk factor for completed suicide (11, 58, 90). Third, how household guns are stored matters (11, 31, 48, 58, 90). One casecontrol study that focused on the role of firearm storage in suicide risk among younger people found that adolescent suicide was four times more likely in homes with a loaded, unlocked firearm, compared with homes where firearms were stored unloaded and locked (48).

Case-control studies adjust for potential confounding of the association between firearms and suicide by adjusting for underlying psychopathology among cases and controls. Other studies have used survey instruments designed to assess psychopathology to address directly the possibility that people who live in homes with firearms are inherently more suicidal than people who live in homes without firearms (7, 54, 89, 111). All these studies have found that people who live in homes with firearms are no more likely to have any of the major psychopathological disorders known to increase suicide risk, no more likely to have suicidal thoughts, and no more likely to have made suicide attempts, compared with people who live in homes without firearms.

## REDUCING ACCESS TO LETHAL MEANS

The literature reviewed above points to the potential for reducing the population-level suicide burden by reducing population-level access to lethal means. Natural experiments from other countries provide evidence of the dominant role that the ready availability of highly lethal, commonly used means can play in determining suicide mortality rates (4, 35, 41, 49, 51, 53, 66, 69, 77, 80). The most famous and dramatic largescale natural experiment that drew attention to the benefits of means restriction has been called the Coal Gas Story (50, 64). During the 1950s, nearly half of all suicides in the United Kingdom were domestic poisonings with the gas used in ovens and heaters. With the discovery of a new, less toxic and less expensive source of gas in the North Sea, the carbon monoxide content of domestic gas fell over the next 15 years to near-zero levels. Following what was, in effect, the detoxification of gas used in homes, rates of suicide by gas fell in effect to zero (e.g., in 1960 there were 2,499 suicides by domestic gas, whereas in 1977 there were 8). Rates of nongas suicide increased somewhat, but overall rates

of completed suicide dropped substantially (by  $\sim$ 30%) for both men and women.

A similar experiment occurred in Sri Lanka, where pesticides are the leading method of suicide. From 1950 to 1995, the suicide rate increased eightfold, owing almost entirely to increases in suicide with toxic pesticides, a byproduct of the agricultural revolution. In 1995 and again in 1998, the government banned several of the most highly toxic pesticides that were commonly used in Sri Lankan suicides. By 2005, the suicide rate had fallen by half (49). Declines in suicide were from poisoning suicides; nonpoisoning suicides did not decline, nor did attempts by poisoning. Secular trends in unemployment, alcohol misuse, divorce, overall pesticide use, and Sri Lanka's civil war did not appear to be associated with the decline. Agricultural output did not suffer, and the cost of production did not appear to increase (78). Western Samoa provides another case in point; there suicides-but not suicide attempts-closely tracked the introduction and later banning of paraquat on the island (9).

A recent study in Israel also found a substantial reduction in suicide rates after a policy that reduced access to lethal means was put into effect. After the policy change, which reduced soldiers' access to firearms on weekends, the total suicide rate in the Israel Defense Forces decreased by 40%. Most of this decrease was due to a decrease in weekend firearm suicides, with no significant change in weekday suicide rates or any compensatory increase in nonfirearm suicides (75).

The coal gas in the United Kingdom, the pesticides banned in Sri Lanka and Western Samoa, and the firearms in Israel and the United States share critical characteristics that make them logical targets for means restriction. First, they account for a large proportion of the total number of suicides in their respective countries. Second, they all have a relatively high probability of causing death when used in an attempt. Third, alternative methods readily available and commonly used in suicidal acts are less likely to prove fatal. In this context, it is worth noting that reducing ready access to methods with low CFR, such as over-the-counter medications (52) and methods that make up a very low proportion of total suicides locally [such as jumps from bridges (5)], may not produce a measurable impact on the overall suicide mortality rate for the population, though lives may be saved. Furthermore, when restrictions are only partial [such as erecting a barrier at one bridge and not at nearby bridges that afford similarly accessible lethal jumping opportunities (109)], it is more uncertain that suicides will be prevented.

The rationale for why reducing the ready availability of lethal means in the short run can save lives in the long run rests on three wellestablished clinical and epidemiologic observations. First, many suicidal acts have an impulsive component. For example, among people who made near-lethal suicide attempts, 24% took less than 5 min (108) between the decision to kill themselves and the actual attempt, and 70% took less than 1 h (T.R. Simon, personal communication). In a study of emergency department-treated suicide attempts, 40% reported less than 5 min from decision to action (123); in another study of people referred to a psychiatric hospital following an attempt, 48% reported acting on their ultimate decision to attempt suicide within 10 min (37).

Second, many suicidal crises are selflimiting. Such crises are often precipitated by an immediate stressor, such as the breakup of a romantic relationship, the loss of a job, or a run-in with police, even as an underlying vulnerability may be explained by a more chronic problem, such as a mental health, behavioral, or substance abuse problem (45, 53, 97). As the acute phase of the crisis passes, often so does the urge to attempt suicide. A recent study of college and university students, for example, found that among those who seriously considered suicide in the past year, approximately one in three reported that the suicidal episode lasted less than an hour (38).

Third, the great majority of people who survive a suicide attempt, including attempts that they expected to be lethal (such as a gunshot to the head or jumping in front of a train), do

402 Miller • Azrael • Barber

not go on to die by suicide. Indeed, a review of more than 90 studies that have followed suicide attempters over time (70 of which examined fatal outcomes) found that, on average, 7% (range: 5–11%) eventually died by suicide, 23% reattempted nonfatally, and 70% made no further attempts (98). Even studies that counted equivocal deaths as suicides, followed attempters for several decades, and focused on medically serious attempts (such as jumping in front of a train) found similarly low completion rates.

### SUMMARY AND IMPLICATIONS

Suicide mortality rates vary widely across geography, age, sex, race, and other sociodemographic dimensions, a disparity in burden that, at the population level, is more exaggerated than that for the leading causes of natural death. Unlike the tight correlation between the incidence of cancer and the incidence of death due to cancer, ecologic patterns of suicide mortality are more tightly correlated with the CFR than with the incidence of suicide attempts (or with the incidence of established risk factors for suicidal behavior, such as major depression or other measures of mental illness). Because this is a striking feature of suicide mortality patterns in the United States, research attempting to test the impact of putative causal factors on suicide mortality should employ the decomposition approach, championed by Wennberg, by specifying, a priori, and testing, a posteriori, whether an observed variation in suicide rates across population groups is the result of a change in the incidence of suicidal acts or a result of a change in the lethality of the act itself (and if so, in what way and to what measurable effect).

To date, however, ecologic studies of suicide mortality in the United States that have examined interventions that are presumed to affect suicidal behavior, rather than the CFR, have not applied the decomposition approach (e.g., ecologic studies of antidepressant prescription rates and suicide mortality) (19, 42, 43, 83, 96, 114). One practical reason for this lacuna is that suicide attempt data are not readily available for all 50 states. Nevertheless, even in the absence of attempt data, indirect assessment is still possible by examining whether suicide methods were affected differentially, and if so, in what way. As reviewed here, a plausible-and testable-alternative explanation for ecologic associations between suicide mortality in the United States and putative causal factors is that the factor is serving as a proxy for household firearm prevalence. By parsing suicide mortality by method, the plausibility of competing explanations can better be assessed.

More than ten years ago, in his Letter to the American People, then-Surgeon General David Satcher (115) asserted that suicide should be seen as a serious public health problem. In making this assertion, Satcher made the case that accounting for ecologic variation in suicide mortality is central to any public health approach to reducing the population-level burden of suicide. The ecologic and individuallevel studies reviewed herein illustrate the importance of considering access to highly lethal means in approaches to understanding population-level suicide patterns. These studies also point to the great, yet largely unrealized potential that means-reduction strategies hold out to save lives, not only in the short run, but also in the long run, not only for individuals, but also for populations that bear a disproportionately large burden of suicide mortality, even if they do not bear a disproportionately large burden of risk factors for suicidal behavior or engage in a disproportionately large number of suicidal acts.

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Ex. 7

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ix

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Volume 33, 2012

## Contents

### Symposium: Comparative Approaches to Reducing Health Disparities

Health Disparities Research in Global Perspective: New Insights and New Directions <i>Shiriki Kumanyika</i>
Health Inequalities: Trends, Progress, and Policy Sara N. Bleich, Marian P. Jarlenski, Caryn N. Bell, and Thomas A. LaVeist7
Conceptual Approaches to the Study of Health Disparities <i>Ana V. Diez Roux</i>
How Society Shapes the Health Gradient: Work-Related Health Inequalities in a Comparative Perspective <i>Christopher B. McLeod, Peter A. Hall, Arjumand Siddiqi, and Clyde Hertzman</i> 59
Disparities in Infant Mortality and Effective, Equitable Care: Are Infants Suffering from Benign Neglect? <i>Diane L. Rowley and Vijaya Hogan</i>
Clinical Care and Health Disparities B. Starfield, J. Gérvas, and D. Mangin

### Epidemiology and Biostatistics

Graham A. Colditz and Esther K. Wei	
of Cancer Mortality	
and Social and Physical Environmental	Determinants
Preventability of Cancer: The Relative Co	ntributions of Biologic
Sati Mazumdar, Frank Lotrich, and Ste	ven M. Albert 123
Neerja Chowdhary, Olivia I. Okereke, N	lary Amanda Dew, Stewart J. Anderson,
Churtes F. Reynous III, I im Chippers, Viki	fum I uter, There Consent, Timit Dius,
Charles F Revnolds III Pin Cuinters Vib	ram Patel Alex Cohen Amit Dias
of Major Depression in Older Adults	
Early Intervention to Reduce the Global E	lealth and Economic Burden
Amy H. Auchincloss, Samson Y. Gebreab, O	Christina Mair, and Ana V. Diez Roux 107
A Review of Spatial Methods in Epidemiol	logy, 2000–2010
	2000 2010

### Ex. 7

The Hurrider I Go the Behinder I Get: The Deteriorating International Ranking of U.S. Health Status <i>Stephen Bezruchka</i>
Unintentional Injuries: Magnitude, Prevention, and Control Sarah Stewart de Ramirez, Adnan A. Hyder, Hadley K. Herbert, and Kent Stevens
Environmental and Occupational Health
Community-Based Approaches to Controlling Childhood Asthma Noreen M. Clark
<ul> <li>Future Challenges to Protecting Public Health from</li> <li>Drinking-Water Contaminants</li> <li><i>Eileen A. Murphy, Gloria B. Post, Brian T. Buckley, Robert L. Lippincott,</i></li> <li><i>and Mark G. Robson</i></li></ul>
Speed Limits, Enforcement, and Health Consequences <i>Rune Elvik</i>
Toward a Systems Approach to Enteric Pathogen Transmission:From Individual Independence to Community InterdependenceJoseph N.S. Eisenberg, James Trostle, Reed J.D. Sorensen,and Katherine F. Shields239
Public Health Practice
Advertising of Prescription-Only Medicines to the Public: Does Evidence of Benefit Counterbalance Harm? <i>Barbara Mintzes</i>
Economic Evaluation of Pharmaco- and Behavioral Therapies for Smoking Cessation: A Critical and Systematic Review of Empirical Research <i>Jennifer Prah Ruger and Christina M. Lazar</i>
Policies for Healthier Communities: Historical, Legal, and Practical Elements of the Obesity Prevention Movement Samantha K. Graff, Manel Kappagoda, Heather M. Wooten, Angela K. McGowan, and Marice Ashe
Public Health and the Epidemic of Incarceration Dora M. Dumont, Brad Brockmann, Samuel Dickman, Nicole Alexander, and Josiah D. Rich

Quitlines and Nicotine Replacement for Smoking Cessation:         Do We Need to Change Policy?         John P. Pierce, Sharon E. Cummins, Martha M. White, Aimee Humphrey,         and Karen Messer         341
Systems Science Methods in Public Health: Dynamics, Networks, and Agents Douglas A. Luke and Katherine A. Stamatakis
Health Inequalities: Trends, Progress, and Policy Sara N. Bleich, Marian P. Jarlenski, Caryn N. Bell, and Thomas A. LaVeist7
Social Environment and Behavior
Health Disparities Research in Global Perspective: New Insights and New Directions <i>Shiriki Kumanyika</i>
Health Inequalities: Trends, Progress, and Policy Sara N. Bleich, Marian P. Jarlenski, Caryn N. Bell, and Thomas A. LaVeist7
Conceptual Approaches to the Study of Health Disparities Ana V. Diez Roux
How Society Shapes the Health Gradient: Work-Related Health Inequalities in a Comparative Perspective <i>Christopher B. McLeod, Peter A. Hall, Arjumand Siddiqi, and Clyde Hertzman</i> 59
Disparities in Infant Mortality and Effective, Equitable Care: Are Infants Suffering from Benign Neglect? <i>Diane L. Rowley and Vijaya Hogan</i>
Suicide Mortality in the United States: The Importance of Attending to Method in Understanding Population-Level Disparities in the Burden of Suicide <i>Matthew Miller, Deborah Azrael, and Catherine Barber</i>

### **Health Services**

From Small Area Variations to Accountable Care Organizations:
How Health Services Research Can Inform Policy
Harold S. Luft
Suicide Mortality in the United States: The Importance of Attending
to Method in Understanding Population-Level Disparities in the
Burden of Suicide
Matthew Miller, Deborah Azrael, and Catherine Barber

The Medicalization of Chronic Disease and Costs Kenneth E. Thorpe and Meredith Philyaw	409
The Methods of Comparative Effectiveness Research Harold C. Sox and Steven N. Goodman	425
Clinical Care and Health Disparities B. Starfield, J. Gérvas, and D. Mangin	89

### Indexes

Cumulative Index of Contributing Authors, Volumes 24–33	447
Cumulative Index of Chapter Titles, Volumes 24–33	452

### Errata

An online log of corrections to *Annual Review of Public Health* articles may be found at http://publhealth.annualreviews.org/

# Exhibit 8



Original article

# Type of Firearm Used in Suicides: Findings From 13 States in the National Violent Death Reporting System, 2005–2015

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*Article history:* Received November 27, 2018; Accepted March 25, 2019 *Keywords:* Firearms; Handguns; Suicide; Long guns

### ABSTRACT

**Purpose:** Although household firearm ownership has been consistently associated with increased suicide rates in the U.S., scant data speak to the type of gun used in U.S. suicides. We address this research gap using data from the National Violent Death Reporting System Restricted Access Data Set, 2005–2015, by describing the types of guns used in firearm suicides over time, by urban versus rural residence, for the population as a whole, and separately among adolescents, and by sex, race, and age.

**Methods:** The types of firearm used by 44,540 firearm suicide decedents in 13 states from 2005 to 2015 are based on individual-level data from the National Violent Death Reporting System and urban–rural classification schemes from the National Center for Health Statistics. Rates are calculated using population data from the National Center for Health Statistics' CDC WONDER online database.

**Results:** Between 2005 and 2015, suicide rates by handguns, but not long guns, increased markedly in both urban and rural counties. Among adolescents, handgun suicide rates doubled over the study period in both rural and urban areas; long gun suicide rates increased modestly. Although handguns were used in nearly three fourths of firearm suicides for the population as a whole, long gun use was relatively higher in rural counties and among adolescents. In rural counties, long guns were used in 51% of adolescent male suicides.

**Conclusions:** Suicide prevention efforts that advise gun-owning families to reduce access to household firearms should focus not only on handguns but also on long guns, especially in rural areas and among households with adolescents.

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### IMPLICATIONS AND CONTRIBUTION

Handguns are used in nearly three fourths of firearm suicides, but long guns constitute two fifths of the guns used in suicides by adolescents and in rural areas. More than half of firearm suicides among rural adolescent males are with rifles or shotguns. Clinical and other interventions to reduce suicide should incorporate this information into efforts focused on reducing access to lethal means, especially in rural areas and among families of adolescents.

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Household firearm ownership has been consistently associated with elevated suicide rates in the U.S. [1-15]. Medical associations and others involved in suicide prevention efforts, such as physicians and mental health practitioners, have limited data to draw from when advising gun owners whether suicide risk is chiefly related to handguns or also to long guns. One reason so little is known about the type of gun used in U.S.

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**Conflicts of interest:** The authors have no conflicts of interest to disclose. **Disclaimer:** The findings and conclusions of this study are those of the authors alone and do not necessarily represent the views of the Centers for Disease Control and Prevention or of participating National Violent Death Reporting System states.

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367

suicides, overall and by specific subgroups, is that official mortality data (the National Vital Statistics System) list gun type as unspecified in nearly two thirds of all firearm suicides [16].

A relatively new data system, the Centers for Disease Control and Prevention's National Violent Death Reporting System (NVDRS), supplements death certificate data with additional information about suicide deaths gathered from police and medical examiner/coroner reports. As such, NVDRS categorizes gun type far more comprehensively than does the National Vital Statistics System and has done so for over a dozen states since 2005. A 2002 pilot study for the NVDRS, for example, found that gun type was reported in all but 1 of 145 youth firearm suicides, and that long guns were used in 48% of firearm suicides under age 18 years [17]. A recent study using the NVDRS found that long guns were used in just under 50% of firearm suicides among those under age 21 years [18], consistent with the few prior peerreviewed studies of youth suicide using local data sources [19,20]. For example, a study of 59 youth aged <19 y who intentionally shot themselves with a firearm in Washington State between 1990 and 1995 [19] found that slightly more than half of the guns used in self-inflicted deaths and injuries were handguns (n = 33), about one-third were rifles (n = 18), and 9% (n = 5) were shotguns. By contrast, the earliest study to examine gun type in firearm suicide found that between 1983 and 1985 in Sacramento County, CA, among 235 firearm suicides, 88% of all firearms used were handguns. The authors note that the high proportion of handguns was perhaps due to the largely urban population of Sacramento County [21].

The present study uses data on over 44,000 firearm suicides from the NVDRS Restricted Access Data Set, 2005–2015. The size of the data set allows us to extend prior work on the types of guns used in suicides by examining patterns of gun type over time and urbanization, sex, race, and age group.

### Methods

This study examined individual-level data from the Centers for Disease Control and Prevention's NVDRS Restricted Access Data Set. The NVDRS is a state-based surveillance system now in 40 states providing comprehensive information on deaths classified as suicide, homicide, legal intervention, unintentional firearm injury, or injury of unknown intent. The NVDRS gathers information from sources including death certificates, coroner/ medical examiner reports, and police reports. The Division of Violence Prevention at the Center for Disease Control and Prevention has provided a detailed description of the NVDRS [22].

Sixteen states provided data to NVDRS throughout the period of 2005–2015 (Alaska, Colorado, Georgia, Kentucky, Maryland, Massachusetts, New Jersey, New Mexico, North Carolina, Oklahoma, Oregon, Rhode Island, South Carolina, Utah, Virginia, and Wisconsin). Three of these states—Georgia, Kentucky, and New Jersey—were excluded from this study, as the gun type used in firearm suicides was unknown in 22.5%, 36.5%, and 24.3% of incidents, respectively. We selected all firearms deaths that were coded as suicides (n = 45,811) in the remaining 13 states. Of these, a total of 3.6% (n = 1,645) were missing gun type information or coded as "unknown" or "other" for firearm type in the "firearm type" variable. However, of these cases, there were 374 cases for which another variable, the ICD-10 "Underlying Cause of Death" variable was coded either X72 (intentional self-harm by handgun) or X73 (intentional self-harm by rifle, shotgun, or larger firearm). These 374 cases were assigned to the handgun or

long gun category, as appropriate. As such, our data set of "known gun type" suicides included 44,540 suicide cases.

Each incident was classified into one of six urbanization categories according to the National Center for Health Statistics' Urban-Rural Classification Scheme for Counties. We collapsed the first four categories—large central metro, large fringe metro, medium metro, and small metro—into one urban category, and the remaining two—micropolitan and noncore—into one rural category. Average annual rates per 100,000 population (by age, sex, race, and rurality) were calculated using population data accessed from CDC Wonder [16]. Analyses were performed in 2018. The Harvard School of Public Health's Institutional Review Board determined that the study was exempt from human subject review.

### Results

As summarized in Table 1, among the 44,540 firearm suicides in our analytic data set, 73% were by handguns, and 27% by long guns (44% rifles and 56% shotguns). Handguns were used in most suicides in each subcategory except American Indian/Alaska Natives and rural adolescent males (not shown). Long gun use was relatively higher in rural settings compared with urban settings and among adolescents compared with adults. Females, African American/Blacks, and Asian/Pacific Islander were the groups least likely to use a long gun, with 87%, 84%, and 86% of decedents, respectively, having used a handgun.

The rate of handgun suicide—which was highest in rural counties—increased steadily from 2005 to 2015 in both urban and rural counties, for the population as a whole and among

#### Table 1

Gun type used in firearm suicides by urbanization of victim's county of residence, sex, race, and age group, 2005–2015, 13 states

	Handgun		Long gun			
	N	Rate/ 100,000	Row %	N	Rate/ 100,000	Row %
Total <sup>a</sup>	32,452	5.0	73	12,088	1.9	27
Type of county of residence						
Large metro <sup>b</sup>	4,169	4.2	81	1,008	1.0	19
Large fringe metro <sup>b</sup>	7,726	3.6	76	2,500	1.2	24
Medium metro <sup>b</sup>	9,666	5.7	75	3,219	1.9	25
Small metro <sup>b</sup>	3,631	5.6	71	1,514	2.3	29
Micropolitan <sup>c</sup>	4,200	6.7	69	1,906	3.0	31
Noncore	3,004	7.0	61	1,919	4.4	39
Sex						
Male	27,243	8.5	71	11,338	3.5	29
Female	5,209	1.6	87	750	.2	13
Age group (y)						
<18	671	.4 <sup>d</sup>	61	421	.3 <sup>d</sup>	39
18-24	2,916	4.5	66	1,479	2.3	34
25-44	9,492	5.4	72	3,671	2.1	28
45-64	11,946	7.0	73	4,369	2.6	27
65+	7,421	8.7	78	2,146	2.5	22
Race						
White	29,089	5.6	72	11,074	2.1	28
Black/African American	2,052	2.1	84	380	.4	16
American Indian/Alaska Native	304	2.1	47	345	2.4	53
Asian/Pacific Islander	266	1.0	86	45	.2	14

<sup>a</sup> Type of county was unknown in 78 cases, age unknown in eight cases, and decedents were not listed as one of the above four races in 1,026 cases.

<sup>o</sup> Suicides classified as urban.

<sup>c</sup> Suicides classified as rural.

<sup>d</sup> Rate calculated using 0- to 17-year-old population.

adolescents (Figure 1A,B). Among adolescents (Figure 1B), handgun suicide rates in rural and urban areas doubled from the beginning of the study period (2005–2007), when rates of handgun suicide were .67 per 100,000 in urban areas and .97 per 100,000 in rural areas, to the end of the study period (2014–2015), by which time rates of handgun suicide in urban and rural areas had increased to 1.36 and 2.09 per 100,000, respectively. Over the corresponding period, rates of adolescent suicide by long guns increased modestly in urban areas, from .42 to .57 per 100,000, and in rural areas, from 1.03 to 1.48 per 100,000. Over the same period, rates of long gun suicide for the population as a whole changed little (Figure 1A).

The firearm suicide rate (handgun and long gun rates combined) among males aged 10–17 years was substantially higher among rural versus urban adolescents (4.6 and 2.2 per 100,000, respectively; Figure 2). The handgun suicide rate among rural male adolescents was 1.6 times that of their urban counterparts, whereas the long gun suicide rate was 2.6 times higher. A total of 51% of rural male adolescent firearm suicides were by long guns. Even by the end of the study period (2014–2015), 44% of rural male adolescent firearm suicides involved long guns (not shown). Gun type did not vary greatly between rural and urban counties among adolescent female decedents.

Among males, long guns as a proportion of overall firearm suicides were highest among the young (Figure 3). Overall, long guns accounted for 39% of firearm suicides among 10- to 17-year-old men (in rural areas, half used long guns; in urban areas, closer to one-third). Among young adult men, the proportion of long gun suicides was 47% among 19-year-olds, 43% among 20-year-olds, 34% among 21-year-olds, and 29% among 22-year-olds. After age 22 years, long gun use among men did not exceed 35% for any single year of age.

In each of the 13 states, across all ages, a majority of firearm suicides involved handguns, ranging from 57% to 78%. Among youth aged 10–17 years, however, handguns were used in as many as 82% of firearm suicides (in Massachusetts) to as few as 31% (in Wisconsin; Table A1, Appendix).

### Discussion

Between 2005 and 2015, among our NVDRS states, the rate of suicide by handguns increased substantially in both rural and



**Figure 1.** (A) Rate of handgun and long gun suicide by year and urban-rural status among all ages, 2005–2015, 13 NVDRS states. (B) Rate of handgun and long gun suicide by year and urban-rural status among adolescents aged 10–17 years, 2005–2015, 13 NVDRS states. NVDRS = National Violent Death Reporting System.



Figure 2. Rate of handgun and long gun suicides among 10- to 17-year-olds by sex and urban-rural status, 2005–2015, 13 NVDRS states.

urban counties, with rates doubling among adolescents. These findings are consistent with ongoing changes in the composition of the U.S. gun stock. For example, data from a recent study found that although household gun ownership rates in the U.S. have remained relatively stable over the past two decades [23,24], almost four of five gun owners currently own at least one handgun, and millions of guns, the large majority of which are handguns, have been added to the U.S. gun stock over the past several decades [24]. That study also found that handguns, which are more often owned for protection than are long guns [24], are far more likely than long guns to be stored loaded and unlocked [25,26].

Our finding that handguns predominate among firearm suicides as a whole, but that long guns play a prominent role in adolescent and young adult male suicides, particularly in rural areas, reflects findings from two prior small studies that gathered information about gun type among firearm suicide decedents [19,20]. Because hunting and sport shooting with rifles and shotguns are often traditional parts of young rural males' upbringing, ready access to these guns may explain the disproportionate contribution long guns make to both the distribution of gun types we observed in rural compared with urban areas and the higher suicide rates among rural versus urban male adolescents reported in prior studies [19,20].

The role of long guns in adolescent suicides has ramifications for gun safety messages to teenagers' parents, particularly in rural areas. Pediatricians, primary care providers, mental health clinicians, hunting safety educators, and firearm safety instructors should emphasize that when a family member is at risk for suicide, preventing their access to *all* household firearms both handguns and long guns—is important, the latter particularly in rural areas. The importance of delivering this message about long guns to parents is underscored by the observation that a majority of guns used in suicides by youth belong to their parents [19,27], and by our finding (Figure 3) that among young adult males, the proportion of long gun suicides was approximately 40%–45% among suicide decedents under age 21 years and even higher for youth living in rural areas.

Our findings are based on the largest data set that includes information about the type of gun used in suicides ever analyzed. Nevertheless, interpreting our results should bear in mind the following limitations. First, our findings come from only 13 states, are limited to descriptive statistics of decedents only (and therefore should not be construed as predictors of method choice), and may not be representative of the nation as a whole. Second, although we examine patterns of gun type over time and by urbanization, sex, race, and age group, we do not describe how gun type varies by other characteristics collected by NVDRS, such as mental health diagnoses, mental health treatment, or evidence of a recent crisis. Third, we classify whether decedents lived in a rural or urban area based on their county of residence. Counties sometimes include both rural and urban areas, so in some cases, a decedent's county classification will not describe the urban or rural character of their specific city or town. Notwithstanding these limitations, clear strengths of NVDRS data are that firearm type is reported or can be easily determined in 97% of suicides, and that these data can be used to monitor trends over time and across jurisdictions.

Despite these limitations, findings from our study draw attention not only to recent increases in suicide rates by handguns but also to the substantial role that long guns continue to play in U.S. suicide, particularly among rural male youth. Our results underscore the importance of including both handguns and long guns in suicide prevention discussions aimed at reducing access to highly lethal suicide methods and help identify groups, such as hunter safety organizations, that might otherwise be overlooked as partners in suicide prevention efforts.



Figure 3. Long gun suicides as a percentage of total firearm suicides among male decedents by age and urban-rural status, 2005–2015, 13 NVDRS states.

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### **Supplementary Data**

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# Exhibit 9



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### Legal status and source of offenders' firearms in states with the least stringent criteria for gun ownership

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### ABSTRACT

**Background** Gun possession by high-risk individuals presents a serious threat to public safety. U.S. federal law establishes minimum criteria for legal purchase and possession of firearms; many states have laws disgualifying additional categories for illegal possession.

**Methods** We used data from a national survey of state prison inmates to calculate: 1) the proportion of offenders, incarcerated for crimes committed with firearms in 13 states with the least restrictive firearm purchase and possession laws, who would have been prohibited if their states had stricter gun laws; and 2) the source of gun acquisition for offenders who were and were not legally permitted to purchase and possess firearms.

**Results** Nearly three of ten gun offenders (73 of 253 or 28.9%) were legal gun possessors but would have been prohibited from purchasing or possessing firearms when committing their most recent offense if their states had stricter prohibitions. Offenders who were already prohibited under current law acquired their gun from a licensed dealer, where a background check is required, five times less often than offenders who were not prohibited (3.9% vs. 19.9%;  $\chi^2$ =13.31; p=0.001). Nearly all (96.1%) offenders who were legally prohibited, acquired their gun from a supplier not required to conduct a background check.

**Conclusions** Stricter gun ownership laws would have made firearm possession illegal for many state prison inmates who used a gun to commit a crime. Requiring all gun sales to be subject to a background check would make it more difficult for these offenders to obtain guns.

### INTRODUCTION

Gun violence has long been one of the most significant public safety and social problems in the USA. In the USA, in 2008, gun violence resulted in 12 179 homicides and an estimated 56 626 assaultive injuries serious enough to warrant a hospital emergency room visit.<sup>1</sup> Among high-income countries, the USA is unique in its extraordinarily high rate of homicides. This disparity is most striking for homicides committed with firearms where the US rate is 20 times higher than other high-income countries.<sup>2</sup>

Despite the magnitude of the problem, US gun policy rarely considers appropriate criteria for disqualifying someone from lawfully possessing a firearm. Federal law disqualifies certain groups of high-risk individuals from owning guns, including felons, fugitives, unlawful users of or those addicted to controlled substances, those who have been 'adjudicated as a mental defective' or committed to a mental institution, individuals who have been dishonourably discharged from the armed forces, persons subject to certain domestic violence restraining orders, persons less than the age of 18 years (for handguns) and domestic violence misdemeanants. Federal law does not set a minimum age requirement for the legal possession of long guns (ie, rifles and shotguns).<sup>3</sup>

Although the federal firearm prohibitions apply minimum standards for all US states, many states have enacted broader disqualifications for firearm possession including: a minimum age of 21 for all guns; convictions for some misdemeanour crimes involving violence, firearms or drugs; multiple convictions for alcohol-related offences; or convictions for serious crimes committed as a juvenile.<sup>4</sup>

Research supports the underlying premise of laws that widen exclusionary criteria for firearm possession: that some groups have higher rates of criminal offending than do those without a criminal history or other indicia of risk.<sup>5–9</sup> For example, Wintemute and colleagues found that individuals denied legal handgun purchase, as a result of a new California law expanding firearm prohibitions to include misdemeanants convicted of crimes of violence, were less likely to commit a new crime of violence than were demographically-matched Californian misdemeanants who had been approved for handgun sales during the years just prior to the new restrictions.<sup>9</sup> A study of homicide offenders in Illinois found that 42% would have been prohibited from possessing firearms as a result of a prior felony conviction; however, convictions for misdemeanours as an adult or more serious crimes as a juvenile were not reported.6

Under federal law, persons buying guns from licensed gun dealers must undergo a criminal history background check.<sup>10</sup> But federal law and the law of most states do not require firearm sellers who are not licensed gun dealers to verify that purchasers of firearms are legally qualified to possess a firearm such as through a background check.<sup>4</sup> Understanding how those with and without a criminal history acquire guns can also inform policies intended to keep guns from prohibited persons.

Prior research on firearm acquisition suggests that incarcerated adults often obtain their guns from casual sources such as from friends and family members, and 'off the street.'<sup>11-13</sup> To our knowledge, whether and to what extent the source varies based on the legal status of the purchaser has not been investigated.

Therefore, the goals of the current study are to: (1) identify the proportion of state prison inmates

incarcerated for gun-related offences in states with the least strict standards for firearm purchase and possession who would have been prohibited from possessing firearms if laws in their states had included additional exclusion criteria and (2) describe how these inmates acquired their firearms.

### METHODS

### Data

This study used data from the most recent (2004) Survey of Inmates in State Correctional Facilities (SISCF), a nationallyrepresentative survey of state prison inmates administered by the Bureau of the Census for the US Department of Justice.<sup>14</sup> The 2004 SISCF consisted of computer-assisted personal interviews conducted between October 2003 and May 2004. Inmates were asked about a broad range of topics including: demographic characteristics; offences for which they were currently serving time; prior criminal history; gun possession and use; prior drug and alcohol use and treatment; and physical and mental health status. In the 2004 survey, 14 499 inmates were interviewed. Of those eligible to participate in the study, 89.1% participated.

Additional information about data collection and analysis methodology for the SISCF is available from the University of Michigan's Inter-university Consortium for Political and Social Research.<sup>15</sup> Prior research using data from the SISCF include studies on incarcerated women, veterans and parents.<sup>16–18</sup> No reported studies have used SISCF data on inmates who used firearms in their most recent crimes.

### Study sample

To focus on the potential effects of broadening state laws regarding firearm restrictions, we limited our analysis to offenders currently serving time for an offence committed with a firearm in states that, as of 2004, did *not* have laws prohibiting persons in the following five groups from purchasing or possessing a firearm: (1) persons less than 21 years of age; (2) persons convicted of a serious juvenile offence; (3) violent misdemeanants; (4) drug misusers; and (5) alcohol abusers. To identify states meeting these criteria, we consulted the Bureau of Justice Statistics 2004 Survey of State Procedures Related to Firearm Sales,<sup>19</sup> supplemented by legal research to confirm some state laws.

Because domestic violence misdemeanants are already prohibited from purchasing or possessing firearms under federal law,<sup>20–22</sup> we included states with laws that prohibited domestic violence misdemeanants if the states did not also prohibit other violent misdemeanants from purchasing or possessing firearms. In addition, although federal law restricts firearm purchase or possession for drug misusers, the law's definition of a drug misuser does not provide objective criteria that can be implemented via a background check, limiting its practical use.<sup>23</sup> We excluded states with separate legal restrictions on possession of firearms by those convicted of serious offences, not technically classified as felonies, when committed by a juvenile.

Nine states-Arkansas, Idaho, Louisiana, Michigan, Missis-New sippi, Montana, Hampshire, Vermont and Wyoming-lacked all five types of expanded firearm disqualifications. Four additional states-Georgia, Maine, New Mexico and Wisconsin-lacked these expanded disgualifications with some exceptions. For example, New Mexico had a minimum age law stating that handgun possession is unlawful by persons <19-years-old<sup>24</sup> and Wisconsin restricted individuals convicted of a felony as a juvenile only if the offence occurred on or after 21 April 1994.<sup>25</sup> We excluded a total of 12 cases meeting these exceptions, because they were already prohibited from firearm

purchase and possession under state law. The final sample consisted of 13 states, though there were no inmates meeting our case definition in two states (New Hampshire, Wyoming; see table 1).

### Measures

To determine whether offenders had a firearm while committing the crime for which they were currently incarcerated, SISCF interviewers asked, 'Did you use, carry or possess a weapon when the (...offense...) occurred?' If the answer was 'yes,' the interviewer asked, 'What kind of weapon was it?' Offenders who said they used a firearm were included in our analyses. Offenders who reported using a firearm in their current crime were asked follow-up questions, including questions about the type of gun(s) (eg, handgun, shotgun, rifle), how and where they obtained the gun, whether they fired it, and their reasons for having it.

SISCF interviewers also asked the offenders a series of questions about their prior arrests and convictions leading to probation or incarceration. Those who had been convicted and sentenced to probation or incarceration were asked about the type of offence, length of sentence, and whether they were sentenced as a juvenile or as an adult for up to 10 prior probations and 10 prior incarcerations. Offence information for juvenile convictions leading to probation and no incarceration was not collected in the SISCF.

To examine the potential for current and expanded disqualifications to curtail gun crime, we categorised offenders into the following groups based on their prior criminal convictions: (1) those who would have no firearm disqualification even under stricter state laws (described below); (2) those who were disqualified under current federal law; and (3) those who were legal firearm possessors under current federal law, but who would have been prohibited in states with stricter standards.

We further categorised offenders in the third group-those who might be impacted if the laws in their states were changed—based on whether they fell into any of the following categories: (1) age 18-20 years at incarceration for their current offence if that offence involved a handgun; (2) less than age 21 years at incarceration for their current offence if that offence involved a long gun; (3) committed a prior serious crime as a juvenile (<18-years-old); (4) conviction for a violent or firearms-related misdemeanour; (5) convictions for two or more drugrelated misdemeanours; and (6) convictions for two or more alcohol-related misdemeanours. These laws were chosen because each is in effect in at least some states.<sup>19</sup> Violent and firearmrelated misdemeanours included convictions for a simple assault or a weapons offence. Drug-related misdemeanours included convictions for driving under the influence of drugs, possession or use of marijuana and unspecified drug-related offences (but did not include drug-related offences involving heroin, powder cocaine or crack cocaine which are generally felonies). Alcoholrelated misdemeanours included DUI/DWI convictions or convictions for public drunkenness.

### Analysis

We first calculated the proportion of offenders who would have been legally prohibited from purchasing or possessing firearms if their states had a variety of stricter laws. We then examined the method and source of firearm acquisition for offenders and calculated  $\chi^2$  statistics to identify any significant differences between offenders who were currently prohibited versus offenders who were not prohibited from purchasing and possessing firearms.

Table 1 Demographic and offence characteristics of state prison inmates incarcerated for an offence committed with a firearm in 13 states (n=253)

	n (%)
Demographic characteristics	
Sex	
Male	234 (92.5)
Female	19 (7.5)
Age when sentenced for current offence (years)	
14—17	48 (19.0)
18—20	58 (22.9)
21-24	46 (18.2)
25—29	35 (13.8)
30 and older	66 (26.1)
Race/ethnicity	
Non-Hispanic Black	169 (66.8)
Non-Hispanic White	63 (24.9)
Hispanic	9 (3.6)
Uther	12 (4.7)
Education (n=251)	105 (70 7)
Less than high school	185 (/3./)
High School or equivalent	41 (10.3)
Iviore than high School Maxital status (s. 252)	25 (10.0)
Maintai status (n=252)	177 (70.0)
Never married	1// (/0.2)
Divorced/separated/widowed	48 (19.1)
Warried	27 (10.7)
	120 (52 /)
Part time/occasional	24 (0.8)
	24 (5.0)
Unemployed: not looking for work	52 (13.0) 61 (24.8)
State of current offense	01 (24.0)
Arkansas	21 (8.3)
Georgia	64 (25.3)
Idaho	5 (2.0)
Louisiana	39 (15.4)
Maine	1 (0.4)
Michigan	67 (26.5)
Mississippi	27 (10.7)
Montana	5 (2.0)
New Hampshire	0 (0)
New Mexico	13 (5.1)
Vermont	1 (0.4)
Wisconsin	10 (4.0)
Wyoming	0 (0)
Current offences* †	
Violent offences	
Murder/voluntary non-vehicular manslaughter	86 (34.0)
Robbery	75 (29.6)
Aggravated assault/assault on police officer	32 (12.6)
Other violent acts	6 (2.4)
Property offences	
Burglary	6 (2.4)
Other property offences	3 (1.2)
Drug offences	
Trafficking	15 (5.9)
Possession or use	7 (2.8)
Public order offences	
Weapons offences	19 (7.5)
Parole/probation violation or contempt	2 (0.8)
Other public order offences	2 (0.8)
	Continued

 Table 1
 Continued

	n (%)
Type of gun used in current offense‡	
Handgun	204 (80.6)
Rifle	30 (11.9)
Shotgun	25 (9.9)
Other firearm	4 (1.6)

\*For inmates currently incarcerated for more than one offence, only the most serious is included here.

†All offence categories include attempted and completed offences.

<sup>‡</sup>Percentages do not sum to 100 because 10 respondents used more than one type of gun in their current offence.

### RESULTS

The overall SISCF sample of 50 states included 14499 inmates, 2046 of whom used a gun in the crime for which they were incarcerated. The distribution of the total sample of gun users was similar to the 13 states in our sample with regard to crime type, type of gun, sex, education, marital status and employment status. Our 13-state sample had a somewhat higher proportion of younger (age 14-17 years) and non-Hispanic Black offenders than for all 50 states.

### Sample characteristics

Our initial sample consisted of 281 offenders who were incarcerated for offences involving firearms from the 13 states with the most lenient firearm restrictions (no stricter than existing federal law). Due to missing or insufficiently specific information about the nature of the prior convictions, 28 offenders were excluded from the analyses for a final sample of 253. The majority of the respondents came from Georgia, Louisiana, Michigan, Mississippi and New Mexico. Some of the more populous US states (eg, California, New York, Texas) were excluded from our analysis because they did not meet our legal inclusion criteria.

Three-quarters (n=190) of offenders committed their current offence (ie, the offence for which they were serving time when the interview occurred) in their state of residence. All offenders were sentenced as adults and age at sentencing for the current incarceration ranged from 14 to 55 years with a mean of 25 years. A majority of the offenders were male subjects, non-Hispanic Black, had not completed high school, were employed in the month before they were incarcerated and had never been married (table 1).

### **Current offences**

More than three-quarters (n=199) of the offenders were serving time for a violent offence at the time of the SISCF interview. In all, 43% of these violent offenders were incarcerated for an attempted or completed murder, or voluntary non-vehicular manslaughter (table 1). The remainder of the sample was incarcerated for property, drug or public order offences (all involving firearms).

Although fewer than half (44.3%) of the offenders reported that they fired a gun while committing the current crime, most (83.4%) identified one or more other or additional reasons for possessing the gun, including using the gun to scare the victim(s) (42.7%), or for self-protection (32.4%).

### Legal status for firearm possession prior to firearm offence leading to current incarceration

Inmates were categorised into three mutually-exclusive groups based on their actual or potential legal status for firearm possession (table 2). In all, 31% (n=78) of offenders would not

	n (%)
May possess even under stricter standards	78 (30.8)
No prior arrests or convictions and offender age $\ge$ 21 years	28 (11.1)
Prior arrests but no convictions and offender age $\geq$ 21 years	34 (13.4)
Prior non-disqualifying misdemeanour convictions, and no convictions for serious juvenile offence, and offender age $\ge$ 21 years	16 (6.3)
Prohibited under current state or federal laws	102 (40.3)
Prior adult (≥18 years) felony conviction(s) or dishonourable discharge	69 (27.3)
Offender age $<$ 18 years at sentencing and used handgun in current offence	33 (13.0)
Would be prohibited only under stricter standards*	73 (28.9)
Handgun offender age 18–20 years at sentencing for current offence	43 (17.0)
Long gun offender age $1-20$ years at sentencing for current offence	17 (6.7)
Prior conviction for serious juvenile offence	13 (5.1)
Prior conviction for firearms or violent misdemeanour	9 (3.6)
Prior conviction for 2+ drug misdemeanours	2 (0.8)
Prior conviction for 2+ alcohol misdemeanours	1 (0.4)

Table 2	Firearm p	orohibition	status o	f state	prison	inmates	incarcerated	for	offence	committed	with
firearm in	13 states	(n=253)									

\*These subcategories are *not* mutually exclusive.

have been disqualified from firearm possession based on prior convictions or minimum age even if their states had laws prohibiting the legal purchase and possession of firearms by persons <21-years-old, persons with a conviction for a serious juvenile offence, violent misdemeanants, and drug and alcohol misusers.

In the second group, 40% (n=102) of offenders were already prohibited from legal firearm possession under current state or federal law and, thus, would be unaffected by the implementation of the stricter firearm prohibition standards we considered.

The third group consists of 73 offenders (28.9%) who were not prohibited under current standards, but would have been prohibited if their states adopted stricter standards similar to those already in place in a number of other states. Most of this group (58.9% and 17.0% of all firearm offenders, n=43) would have been prohibited if their state had a law that raised the minimum age to possess a handgun to 21 years. An additional 17 offenders would have been prohibited if their state passed a law restricting possession to all firearms, including long-guns, for persons <21 years. If persons convicted of a serious crime as a juvenile were to become prohibited, it would have been illegal for 13 offenders (5.1% of all firearm offenders) to purchase or possess a firearm. Nine offenders (3.6% of all firearm offenders) would also have been disgualified if their states had prohibited persons convicted of a violent or firearms-related misdemeanour from purchasing or possessing a firearm. Two offenders would have been prohibited if states were to restrict firearm purchase and possession for those with two or more drug-related misdemeanours and one offender would be prohibited if the same restriction were applied to alcohol-related misdemeanours.

### How and where criminals obtained their firearms

About eight of every 10 offenders reported using a handgun (vs rifle or shotgun) in the offence for which they were serving time. Half of the offenders reported that they had bought the gun used in the crime (table 3). The second most common method of gun acquisition—cited by fewer than one in five offenders—was borrowing or holding the gun for someone. Regardless of how they obtained the gun, friends and family members were the most common source (34.0%), followed by drug dealers or other black market sources (30.4%). Only 13.4% got the gun directly from a gun store or pawnshop where federal law requires

prospective firearm purchasers to pass a background check. It is important to recognise, however, that table 3 represents only the most recent acquisition of a specific gun: it does not indicate whether the gun *ever* passed through a particular distribution channel (eg, a gun show).

There were few differences between the groups of offenders with regard to how and where they got the gun used in their most recent offence. More than half (55.6%) of offenders for whom firearm purchase and possession was legal under current standards (adding the 45 inmates who would be legal even under stricter standards with the 39 inmates who would be prohibited only under stricter standards) bought or traded for the gun used in their most recent crime compared with two-fifths (39.2%) of offenders who were prohibited under current state or federal law  $(\chi^2 = 6.56; p \le 0.01)$ . Offenders who were prohibited from purchasing and possessing a gun under current law acquired their gun from a licensed dealer, where a background check would be required, five times less often than offenders who were not prohibited (3.9% vs 19.9%;  $\chi^2$ =13.31; p≤0.001). Similarly, nearly all (96.1%) offenders who were legally prohibited from possessing a firearm acquired their gun from a supplier not required to conduct a background check.

### DISCUSSION

Our findings indicate that 40% of offenders incarcerated for committing crimes with a gun in the 13 US states with the least strict standards for legal firearm purchase and possession were in possession of the gun illegally. If these states had adopted more restrictive standards like those in place in a number of other states, an additional 29% of the persons incarcerated for committing a crime with a firearm would have been legally prohibited from possessing a firearm at the time of their current offence. The vast majority of these individuals—nearly a quarter of the entire sample of firearm offenders—would have been prohibited if the minimum legal age for possessing any type of firearm was 21 years. An additional 9.9% would have been legally prohibited from firearm possession as a result of convictions for serious crimes as a juvenile or for misdemeanours involving violence, firearms, drugs or alcohol.

Nearly one in five offenders was <18-years-old at the time they were sentenced for the current offence; 41.9% were less than age 21 when sentenced. An even greater proportion would

	Total (n=253) N (%)	Legal even under stricter standards (n=78) n (%)	Prohibited under current state or federal law (n=102) n (%)	Would be prohibited only under stricter standards (n=73) n (%)
How gun was got				
Stole	8 (3.2)	0 (0)	4 (3.9)	4 (5.5)
Borrowed	44 (17.4)	12 (15.4)	17 (16.7)	15 (20.6)
Bought/traded	124 (49.0)	45 (57.7)	40 (39.2)	39 (53.4)
Given as gift	21 (8.3)	8 (10.3)	9 (8.8)	4 (5.5)
Other	23 (9.1)	4 (5.1)	13 (12.8)	6 (8.2)
Don't know (DK)/refused	33 (13.0)	9 (11.5)	19 (18.6)	5 (6.9)
Where gun was got				
Gun store or pawnshop	34 (13.4)	24 (30.8)	4 (3.9)	6 (8.2)
Gun show	1 (0.4)	0 (0)	0 (0)	1 (1.4)
Friend/family member	86 (34.0)	25 (32.1)	35 (34.3)	26 (35.6)
Street/black market	77 (30.4)	14 (18.0)	36 (35.3)	27 (37.0)
Burglary	1 (0.4)	0 (0)	1 (1.0)	0 (0)
Other	21 (8.3)	6 (7.7)	8 (7.8)	7 (9.6)
DK/refused/skipped+	33 (13.0)	9 (11.5)	18 (17.7)	6 (8.2)

 Table 3
 Source of gun used in current offence by state prison inmates incarcerated for offence committed with firearm in 13 states, by firearm prohibition status\*

\*If inmate used more than one gun in current offence, response pertains to the most recently acquired gun.

†Respondents who refused to disclose how they got the gun were not subsequently asked where they got it.

have fallen into the <18 group if we had data on offenders' age at the time the offence occurred rather than age at incarceration. These findings underscore the importance of minimum-age restrictions for firearms possession and disqualifications for serious offences committed as juveniles, even if the duration of these disqualifications is limited.

It is also important to consider the political feasibility of any new restrictions on access to firearms. In a 1998 survey, a large majority of respondents—including the majority of gun owners—favoured laws that would restrict guns from various categories of misdemeanants including assault and battery without a lethal weapon or serious injury, driving under the influence of alcohol, and carrying a concealed weapon without a permit.<sup>26</sup> Although public support was strong for a variety of firearm laws, firearm restrictions based on criminal history may be among the most politically feasible.<sup>23</sup> <sup>27</sup> Each firearm policy considered in this study is currently law in at least some states.

Although setting appropriate standards for legal firearm ownership is important, it is equally important to make sure that databases used to screen gun purchasers and ascertain legal status for gun possession are up-to-date so that prohibited individuals can be identified. For example, juvenile convictions must be recorded in an accessible database so that they are picked up in background checks in order for prohibitions for serious offences committed as a juvenile to be useful in restricting the legal purchase and possession of firearms in this high-risk group.

Relatively few offenders purchased their guns directly from licensed firearms dealers. Only 3.9% of individuals disqualified based on current federal or state prohibitions and 3.8% who were <21-years-old at the time of their incarceration obtained their gun from a licensed firearms dealer. Presumably most, if not all, of these prohibited individuals purchased their firearm prior to becoming a prohibited person. Among individuals who appeared to be legally qualified to purchase firearms, only one in five (19.9%) obtained their firearm directly from a licensed firearm dealer, perhaps to avoid having their firearms transactions recorded and therefore traceable to the purchaser. Given offenders' preferences for new firearms, 13 28 it is noteworthy how criminals avoid the regulated gun market of licensed sellers and prefer the largely unregulated market involving unlicensed sellers where new guns may be harder to obtain. The lack of regulation of firearm sales by unlicensed sellers is likely to

significantly limit the government's ability to keep firearms from prohibited individuals.<sup>28</sup> Requiring all gun sales to be subject to a background check, and holding sellers accountable for failure to do so, are policies that could address this problem.<sup>29</sup>

To our knowledge, this is the first study to use data on gun offenders' age and criminal histories to examine the potential benefits of strengthening the criteria for legal firearm possession. Nonetheless, it is subject to several limitations. The data used in this analysis come from inmates' self-report. As such, they share the limitations inherent to all self-report data (eg, recall and social desirability bias). And although the data were drawn from a nationally-representative survey of state prison inmates, they are not necessarily representative of state prison populations. In addition, the 13 states in our sample may not have the same distribution of offenders as in all 50 states. For example, the five states with the most offenders in our sample may be more urban, on average, than the USA as a whole. We chose states for inclusion in the sample based on their laws in 2004, the year the SISCF survey took place. These laws may be different from the laws that were in effect at the time the offenders were convicted for their prior offences, though it is rare for laws prohibiting certain persons from owning guns, based on criminal history, to be repealed. Moreover, we were unable to determine whether the guns used in the current crimes were obtained in the state in which the crime was committed. This is particularly relevant for considering criteria for firearm purchase rather than possession.

The numbers of offenders with prior misdemeanour convictions are likely undercounted because we did not have status information about juveniles sentenced to probation nor did we have information about persons who were convicted but not sentenced to probation or incarceration (eg, those sentenced only to pay a fine). It is also possible (though unlikely) that some of the offenders with a prior felony had their gun rights reinstated. Finally, it is also important to remember that this is a prison population. As such, our findings may not generalise to offenders who avoid imprisonment.

However, our sample comes from a large national survey of state prison inmates and contains extensive information on their prior criminal history. In addition, we have focused on the population that is most likely to be affected by the policy changes we considered by including only offenders who used a firearm in their current offence.
- Guns in the hands of high-risk individuals present a serious threat to public safety.
- Among high-income countries, the USA is unique in its extraordinarily high rate of firearm homicides.
- US federal law establishes minimum criteria for who may legally purchase and possess firearms; state laws vary widely in this regard.

#### What this study adds

- This study is the first to use data on incarcerated gun offenders' age and criminal histories to examine the potential benefits from strengthening the criteria for legal firearm possession.
- Nearly three of every 10 gun offenders in the 13 US states with the least stringent criteria for legal gun ownership would have been prohibited from purchasing or possessing a firearm when they committed their most recent offence if their states had more restrictive laws in place.
- Offenders for whom access to firearms was legal under current standards were five times more likely to have obtained their gun from a gun store or pawnshop than were offenders who were prohibited under current state or federal law.

Our findings indicate that stricter gun ownership laws in states with the lowest standards would have made firearm possession illegal for many who used a gun to commit a crime. We are uncertain about the degree to which stricter legal standards for firearm possession might deter criminal gun possession and use. But, adding barriers for the acquisition of guns by high-risk persons is an underused potential intervention.

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#### Competing interests None.

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# Exhibit 10

Ex.10 Page 98

## State Gun Laws and Pediatric Firearm-Related Mortality

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**BACKGROUND:** Firearms are the second leading cause of pediatric death in the United States. There is significant variation in firearm legislation at the state level. Recently, 3 state laws were associated with a reduction in overall deaths from firearms: universal background checks for firearm purchases, universal background checks for ammunition purchases, and identification requirement for firearms. We sought to determine if stricter firearm legislation at the state level is associated with lower pediatric firearm-related mortality.

**METHODS**: This was a cross-sectional study in which we used 2011–2015 Web-based Injury Statistics Query and Reporting System and Census data. We measured the association of the (1) strictness of firearm legislation (gun law score) and (2) presence of the 3 aforementioned gun laws with pediatric firearm-related mortality. We performed negative binomial regression accounting for differences in state-level characteristics (population-based race and ethnicity, education, income, and gun ownership) to derive mortality rate ratios associated with a 10-point change in each predictor and predicted mortality rates.

**RESULTS:** A total of 21 241 children died of firearm-related injuries during the 5-year period. States with stricter gun laws had lower rates of firearm-related pediatric mortality (adjusted incident rate ratio 0.96 [0.93–0.99]). States with laws requiring universal background checks for firearm purchase in effect for  $\geq$ 5 years had lower pediatric firearm-related mortality rates (adjusted incident rate ratio 0.65 [0.46–0.90]).

**CONCLUSIONS:** In this 5-year analysis, states with stricter gun laws and laws requiring universal background checks for firearm purchase had lower firearm-related pediatric mortality rates. These findings support the need for further investigation to understand the impact of firearm legislation on pediatric mortality.

abstract

WHAT'S KNOWN ON THIS SUBJECT: Firearm-related injuries are the second leading cause of pediatric death in the United States, yet there is significant variation in firearm legislation at the state level.

WHAT THIS STUDY ADDS: States with stricter firearm legislation, specifically legislation regarding universal background checks for firearms, had lower firearm-related mortality rates in children.

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Page 99

Firearm injury is the second leading cause of traumatic death and the third leading cause of death overall among children in the United States.<sup>1</sup> The United States has the highest rate of firearm-related injuries in children relative to other industrialized countries.<sup>1,2</sup> Of note,  $\sim$ 7 US children die of firearm-related injuries daily.<sup>3</sup>

When compared with other highincome countries, the United States has the highest rate of gun ownership, the weakest gun laws, and the highest rate of firearm-related deaths in children.<sup>4–6</sup> Firearm legislation varies at the state level and regulations differ with respect to the presence or absence of laws for firearm purchase, ownership, and carriage.<sup>7</sup> Each year, the Brady Campaign to Prevent Gun Violence gathers an expert panel to objectively assess and rate state firearm legislation on the basis of a series of 33 different gun policies.8 Additionally, authors of a recent study found 3 state laws in particular to be strongly associated with a reduction in firearm-related deaths among children and adults combined: universal background checks for firearm purchase, universal background checks for ammunition purchase, and identification requirement for firearms.<sup>9</sup>

Authors of several previous studies have described lower rates of pediatric suicide, homicide, firearm carriage, and firearm-related morbidity in states with strict gun laws.<sup>10-15</sup> We performed this study to test the hypothesis that stricter firearm legislation at the state level is associated with lower pediatric firearm-related mortality rates.

#### **METHODS**

#### **Study Design and Data Source**

This was a repeated cross-sectional study using the 2011–2015 Webbased Injury Statistics Query and Reporting System (WISQARS). WISQARS is a publicly available, interactive, online, de-identified database that provides fatal injury data in the United States from the Centers for Disease Control and Prevention by broad demographic characteristics and cause of injury.<sup>3</sup> These data were used to select firearm-related deaths per year for those aged  $\leq 21$  years by state, except in states with <10 annual firearmrelated deaths where the counts were suppressed. These data were matched to comparable state population data for all children aged  $\leq 21$  years. Although the intent of injury may differ across the pediatric age group, we chose to focus this study across the entire pediatric age spectrum because the primary purpose of this analysis was to measure the relationship between a comprehensive score of state-based firearm legislation (which may impact children from infancy through young adulthood) and firearm-related mortality. This study was exempt from institutional review board approval because of the use of publicly available de-identified data.

#### **Outcome Variable**

The primary outcome was firearmrelated mortality rate in children. Deaths were identified by using *International Classification of Diseases, 10th Revision* codes W32–W34, X72–X74, X93–X95, Y22–Y24, Y35.0, and \*U01.4 to specify firearm-related mortality. Statespecific firearm-related mortality rates were calculated by using respective US 2011–2015 census data.

#### **Exposure Variables**

The primary exposure variable was gun law score based on the 2011–2015 Gun Law Scorecards from the Brady Campaign to Prevent Gun Violence. States can receive a maximum of 100 points, based on points awarded for having consistently strong laws. The higher the state gun law score, the stricter the firearm legislation. In 2013, states began losing points for laws considered to weaken public safety. Because states could lose points, negative scores were possible.<sup>8</sup> To facilitate statistical modeling, scores were inflated by a constant of 40 to prevent negative values while preserving the original scale.

Secondary exposure variables included individual laws previously associated with lower mortality rates in the total population of adults and children. These included the following 3 laws: (1) universal background checks for firearm purchase, (2) universal background checks for ammunition purchase, and (3) identification requirement for firearms (microstamping, ballistic fingerprinting).<sup>9</sup> States were categorized into the following groups on the basis of 2015 laws: states having no law, law in effect for <5 years, or law in effect for  $\geq$ 5 years.<sup>16,17</sup>

#### **Confounding Variables**

We used the following state-level data from the 2011-2015 US Census to adjust for characteristics previously associated with firearmrelated mortality: population-based race and ethnicity proportions, percent of the population with college education, and percent of the population living below the poverty threshold.<sup>2,4,18-20</sup> We adjusted for gun ownership using 2013 data from YouGov, an Internet-based market research company, as reported in a study by Kalesan et al. $^{\bar{21}}$  States were dichotomized as having low or high gun ownership on the basis of the median value of the percentage of gun ownership.

#### **Data Analysis**

We used standard descriptive statistics to summarize the characteristics of the study population and calculate the overall and state-level firearm-related mortality per 100 000 US children. After determining that the data were

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too dispersed for Poisson modeling, we used negative binomial multiple regression models to measure the associations of state gun law scores (primary exposure variable) and the presence of the 3 aforementioned laws (secondary exposure variables) with firearm-related mortality rates among children. Four separate models incorporated population-level adjustments for state-level proportions by race and ethnicity, education level, household income, and gun ownership. Variance estimates were adjusted to account for clustering by state across the study years.

Because states with <10 firearmrelated deaths among children had suppressed mortality rates, we performed a sensitivity analysis using the mean number of firearm-related deaths over the 5-year study period to estimate an annual mortality rate for states with suppressed data. We compared these results with our primary analysis to assess the robustness of our results. We report incident (mortality) rate ratios (IRRs) and predicted mortality rates with 95% confidence intervals (CIs). We used the "margins" command in Stata version 12.0 (Stata Corp, College Station, TX) to derive predicted mortality rates associated with a 10-point change in the gun law score or in the proportion by race and ethnicity, education level, poverty level, and gun ownership. Similarly, when measuring the impact of the 3 aforementioned laws, we separately calculated predicted mortality rates for states that did not have the law present, states that had the law in effect for <5 years, and states that had the law in effect for  $\geq 5$  years.

#### RESULTS

From 2011 through 2015, there were 21241 firearm-related deaths among US children reported in WISQARS (~4250 deaths per year). This translates to an annual firearm-related mortality rate of 4.65 per 100 000 US children. The majority of firearm-related deaths were assault related (61.6%) and occurred among males (87.3%) and 18- to 21-year-old individuals (68.7%) (Table 1).

State-specific mortality rates ranged from 1.1 to 18.1 per 100 000 children. State gun law scores ranged from -39 to +81, and after scaling, 1 to 121, with higher scores indicating stricter gun laws. Gun ownership ranged from 5.2% to 61.7% (median value: 32.2%).

In unadjusted analysis, the association between the gun law score and pediatric firearm-related mortality demonstrated that for every 10-point increase in the gun law score (eg, stricter firearm legislation), the firearm-related mortality rate among children decreased by 8% (IRR 0.92 [95% CI 0.89-0.96]). Sensitivity analysis, in which we used the mean mortality rate over the 5year period as the annual mortality rate for states that had suppressed mortality data (DE, HI, ME, NH, RI, SD, VT, and WY), revealed similar results (IRR 0.92 [95% CI 0.88-0.96]).

Table 2 reveals the results of the fully adjusted model. In this fully adjusted model, every 10-point increase in gun law score decreases the firearmrelated mortality rate in children by 4% (adjusted incident rate ratio [aIRR] 0.96 [95% CI 0.93-0.99]). Figure 1 illustrates the relationship between the gun law score and firearm-related mortality in children after population-level adjustments by race and ethnicity, education level, household income, and gun ownership. As illustrated in Fig 1, predicted firearm-related mortality decreases as firearm laws get stronger.

Table 3 reveals the relationship between specific laws and firearmrelated mortality in children. A summary of the presence of these laws by state can be found in the Supplemental Information. In 2015, 7 states had laws requiring universal background checks for firearm purchases that had been in effect for  $\geq$ 5 years, 5 states had these laws for <5 years, and 38 states did not have such laws. After population-level adjustments, states that had these laws in effect for  $\geq$ 5 years had

 TABLE 1 Characteristics of Study Population, 2011–2015

Demographic	N (%) (Total N = 21241)	Rate of Firearm-Related Mortality per 100 000 US Children	
Age group, y			
≤12	1141 (5.4)	0.4	
13–17	5517 (26.0)	5.3	
18–21	14 583 (68.7)	16.5	
Sex			
Male	18 544 (87.3)	7.9	
Female	2697 (12.7)	1.2	
Race			
White	11 133 (52.4)	3.2	
African American	9471 (44.6)	12.4	
Other	637 (3.0)	1.8	
Hispanic ethnicity			
Yes	3407 (16.0)	3.2	
No	17 786 (83.7)	5.1	
Unknown	48 (0.2)	—	
Intent			
Assault	13082 (61.6)	2.9	
Suicide	7217 (40.0)	1.6	
Unintentional	696 (3.3)	0.2	
Undetermined	251 (1.2)	0.1	

—, not applicable.

TABLE 2 Association of State Gun Law Scores With Firearm-Related Mortality Rates, 2011–2015

	aIRR (95% CI) <sup>a</sup>
Gun law score	0.96 (0.93-0.99) <sup>b</sup>
High gun ownership (referent = low gun ownership)	0.96 (0.83-1.12)
Percent of population with African American race	1.16 (1.07-1.25) <sup>b</sup>
Percent of population with Hispanic ethnicity	0.98 (0.91-1.05)
Percent of population with a college education	0.72 (0.57–0.90) <sup>b</sup>
Percent of population living below the poverty level	0.79 (0.51-1.22)

<sup>a</sup> For every 10-point increase in the gun law score (eg, stricter firearm legislation), the firearm-related mortality rate among children decreases by 4%.

 $^{\rm b}$  Statistical significance at P value  $<\!.05;$  adjusted for year and clustered by state.

a predicted mortality rate of 3.80 (2.67-4.94) per 100 000 children compared with 5.88 (5.25-6.52) per 100 000 children in states that did not have such laws (aIRR 0.65 [95% CI 0.46–0.90]). The majority of states (n = 47) did not have laws requiring universal background checks for ammunition purchases in 2015. After population-level adjustment, the 1 state that had laws regarding universal background checks for ammunition purchase in effect for <5 years had a lower firearm-related mortality rate than states that did not have such laws (aIRR 2.18 [CI 0.52-3.84] per 100 000 children compared with aIRR 5.69 [CI 5.17-6.22] per 100 000 children;

aIRR 0.38 [CI 0.19–0.82]); however, this association was not significant when compared with the 2 states that had such laws for  $\geq$ 5 years. Only 2 states had laws requiring firearm identification in 2015, and there was no statistically significant difference in mortality rates between the 2 states that had these laws versus the states that did not.

#### DISCUSSION

This study supports the hypothesis that states with stricter firearmrelated legislation have lower rates of pediatric firearm-related deaths compared with states with less strict firearm legislation. This association



FIGURE 1 Gun law score and predicted pediatric firearm-related mortality rates, 2011–2015.

persists after adjustment for gun ownership and other sociodemographic variables. We found that of the 21 241 children who died of firearm-related injuries from 2011 through 2015, rates of firearmrelated death were lower in states that had higher (more strict) gun law scores and in states that had laws requiring universal background checks for firearm purchases.

Our findings reveal an important association between firearm legislation and pediatric firearmrelated mortality. This association was strong even after adjustment for rates of gun ownership. These data suggest that strict firearm legislation may be protective of children even in areas of high gun ownership.

Our results are consistent with previous studies that revealed lower rates of firearm-related injury in states with stricter firearm laws in a hospitalized population.<sup>13,18</sup> Safavi et al<sup>13</sup> found lower pediatric hospitalization rates in states with stricter firearm legislation. Simonetti et al<sup>18</sup> demonstrated that stricter firearm legislation is associated with lower hospital discharge rates for firearm-related injuries in a combined adult and pediatric population in 18 states. Similarly, authors of other studies have observed an association between firearm-related mortality and strictness of firearm legislation or specific firearms laws across 50 states.<sup>9,19,22</sup> For instance, Fleegler et al<sup>22</sup> demonstrated that states with more firearm laws had lower rates of firearm fatalities in a population of adults and children. In an exhaustive review of the literature, Lee et al<sup>19</sup> found that stronger gun policies were associated with lower rates of firearm homicide in the United States. Furthermore, authors of a 2015 international review of 130 studies concluded that the implementation of firearm restrictions is associated with reductions in firearm deaths in the combined population of adults and children.<sup>23</sup>

TABLE 3 Specific Firearm Legislation in 2015 and Pediatric Firearm-Related Mortality Rates

Law	No. States	Predicted Mortality Rate <sup>a</sup> (95% CI)	Adjusted IRR <sup>b</sup> (95% CI)
Universal background checks for firearm			
purchase			
Not present	38	5.88 (5.25-6.52)	Referent
Present $<$ 5 y	5	5.25 (3.53-6.96)	0.89 (0.63-1.27)
Present $\geq$ 5 y	7	3.80 (2.67-4.94)	0.65 (0.46-0.90)
Universal background checks for			
ammunition purchase			
Not present	47	5.69 (5.17-6.22)	Referent
Present $<$ 5 y	1	2.18 (0.52-3.84)	0.38 (0.19-0.82)
Present $\geq$ 5 y	2	3.65 (1.94-5.36)	0.64 (0.39-1.03)
Identification requirement for firearms			
Not present	48	5.59 (5.03-6.15)	Referent
Present <5 y	0	—	—
Present ≥5 y	2	5.89 (2.86-8.91)	1.05 (0.63-1.77)

—, not applicable. ª Per 100 000 children.

<sup>b</sup> Population-level adjustments by race and ethnicity, education level, household income, and gun ownership.

In general, firearm legislation impacts overall mortality in adults; states with higher numbers of laws regulating firearms have lower rates of overall firearm mortality as well as fewer suicides and homicides than states with fewer total laws.<sup>22</sup> Additionally, laws enforcing strict waiting periods before firearm purchases, universal background checks, restrictions to carrying guns in public, and mandated gun locks were associated with lower adult suicides.<sup>24</sup> Studies such as these suggest that specific laws may have particular efficacy in preventing firearm mortality. Kalesan et al<sup>9</sup> studied 25 different regulations related to firearms and found that 3 laws were associated with a decrease in overall firearm mortality: universal background checks for firearm purchases, background checks for ammunition purchases, and a requirement of identification on the firearm (microstamping or ballistic fingerprinting). In our study, which was specific to children, we found that states with laws requiring universal background checks for firearm purchases had lower firearmrelated mortality. The presence of these laws was associated with a >35% lower rate of firearm-related mortality, even after adjustment for socioeconomic factors and gun ownership. Few states had laws

regarding background checks for ammunition purchases or identification requirements for firearms; therefore, the sample was too small to draw conclusions on the impact. Furthermore, laws regarding firearm identification faced challenges at the state level in both California and Maryland. In 2013, California expanded its firearm identification laws and was the first state to pass a microstamping requirement for all new handguns. However, the law faced multiple legal challenges, and gun manufacturers refused to sell new handguns in the state to avoid this requirement. Additionally, in Maryland, a ballistics fingerprinting program that had been in place for almost 15 years was repealed in 2015.<sup>17</sup> Therefore, it may be too early to study the impact of microstamping or ballistics identification on preventing firearmrelated injury.

Although many state and federal laws are passed with the intent to reduce firearm-related morbidity and mortality, the nuances of differential implementation can make it difficult to elucidate the effectiveness of these laws individually or as a whole. In previous studies, mostly focused on the general population rather than specifically on children, authors have

suggested that there are lower firearm-related deaths in states with lower gun ownership<sup>25,26</sup> in states with specific laws on safer firearm storage practices,<sup>20,27–29</sup> and in states with background check requirements for firearm or ammunition purchase.9,19 Alternatively, authors of other studies have used composite scores, such as the gun law score, to measure differences in firearm-related injury and mortality. In these studies, authors found lower rates of firearmrelated injury and death in states with more restrictive firearm legislation.<sup>13,18,22</sup> However, these studies were limited to either a hospitalized population or a population consisting of both adults and children. We used a combined approach in our study in which we evaluated the association of firearmrelated mortality among children with strictness of firearm legislation using the gun law score as well as the presence of the 3 laws previously noted by Kalesan et al<sup>9</sup> to be associated with lower rates of firearm mortality across all ages. We also studied these trends over a 5-year study period rather than limiting our analysis to just 1 year. In addition, we were able to assess the impact of firearm legislation after adjustment for gun ownership.

Evidence-based policy to drive legislative change suggests that a combination of laws may be the most effective to reduce firearm-related injury and death. Moreover, the American Academy of Pediatrics affirms that the most effective method for preventing pediatric firearm-related injuries is a multilateral approach, advocating for legislation that reduces firearm availability and imposes stricter requirements regarding child access, safety, and design.<sup>2</sup> This approach requires more detailed data sources with information on the acquisition of firearms, types of firearms, and enforcement of firearm laws.

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The findings of this study build on previous literature and help provide compelling data that an evidencebased, data-driven, public health approach to firearm legislation may be successful in reducing firearmrelated injury in children. Legislation to decrease injury from other obvious public health hazards, such as motor vehicle collisions and secondhand smoke exposure, has shown that the adoption of restrictive laws (eg, seat belts, use of car seats, limits on where an individual can smoke, etc) results in lower injury rates.<sup>30,31</sup> For instance, as a result of the evidencebased approach taken to reduce mortality from motor vehicle collisions, motor vehicle-related mortality rates have decreased from 9.8 per 100 000 children in 2007 to 6.1 in 2015.<sup>32</sup> In contrast, firearmrelated crude mortality has not changed, with 5.4 per 100 000 children in 2007 to 5.2 in 2015.32 Thus, an evidence-driven approach, based on more comprehensive data sources, is needed to inform decisionmaking to reduce childhood injury and death from firearms.

There are several potential limitations to this study. First, because this is a repeated crosssectional study, we are unable to establish causality between the strictness of firearm legislation and state-based mortality. However, given that the study was conducted over a 5-year period, we believe this adds to the robustness of our findings. Second, the Gun Law Scorecard is not a validated measure of strictness of firearm legislation. However, many studies have used the Gun Law Scorecard to assess "strictness" of firearm legislation at the state level.<sup>10,13,18,33</sup> We are unaware of any validated scoring system for firearm legislation, but given the comprehensiveness of the Gun Law Scorecard and its use in published literature, it is a reasonable means to compare levels of firearm strictness. Third, we used the 2013 YouGov survey to estimate gun ownership in our models. Although this survey provides the most recent estimate of gun ownership in the United States, it is possible that this estimate is inaccurate. Nevertheless, it likely reflects relative patterns in gun ownership because its estimates approximate those derived from the Centers for Disease Control and Prevention 2002 Behavioral Risk Factor Surveillance System and has been used in previously published studies.<sup>9,34</sup> Furthermore, although we assessed the presence or absence of certain firearm legislation, we were unable to assess the effectiveness of the enforcement of these laws. In addition, when the presence of specific gun laws was evaluated, the effect of other coexistent laws was not adjusted for in the multivariable

model. Lastly, these analyses were limited strictly to firearm-related deaths rather than firearm-related injuries, which underestimates the burden of firearm-related morbidity among children.

#### CONCLUSIONS

We found that states with stricter firearm legislation had lower rates of firearm-related death in children. This association remained after population-based adjustment for sociodemographic factors and gun ownership. Furthermore, states with laws requiring universal background checks for firearm purchase also had lower rates of pediatric firearmrelated deaths. These results support the need for more robust research related to the impact of firearm legislation on firearm-related injury and death in children. Implementation of evidence-based policies and legislation is required to reduce firearm-related injury in children.

#### **ABBREVIATIONS**

aIRR: adjusted incident rate ratio CI: confidence interval IRR: incident rate ratio WISQARS: Web-based Injury Statistics Query and Reporting System

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### **Supplemental Information**

#### SUPPLEMENTAL TABLE 4 States With Laws Requiring Universal Background Checks for Firearm Purchases

State	Year
California	1991
Connecticut	1999
Hawaii	2006
Illinois	1991
Massachusetts	1991
New Jersey	1991
Rhode Island	1991
Colorado	2013
Delaware	2013
New York	2013
Washington	2014
Oregon	2015

#### SUPPLEMENTAL TABLE 5 States With Laws Requiring Universal Background Checks for Ammunition Purchases

State	Year
Connecticut	2013
Illinois	1991
Massachusetts	1991

#### SUPPLEMENTAL TABLE 6 States With Laws Requiring Firearm Identification (Microstamping or Ballistics Fingerprinting)

State	Year(s)
California	2007
Maryland	2000–2015
New York	2002–2012

#### State Gun Laws and Pediatric Firearm-Related Mortality Monika K. Goyal, Gia M. Badolato, Shilpa J. Patel, Sabah F. Iqbal, Kavita Parikh and Robert McCarter *Pediatrics* 2019;144; DOI: 10.1542/peds.2018-3283 originally published online July 15, 2019;

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# PEDIATRICS®

State Gun Laws and Pediatric Firearm-Related Mortality Monika K. Goyal, Gia M. Badolato, Shilpa J. Patel, Sabah F. Iqbal, Kavita Parikh and Robert McCarter *Pediatrics* 2019;144; DOI: 10.1542/peds.2018-3283 originally published online July 15, 2019;

The online version of this article, along with updated information and services, is located on the World Wide Web at: http://pediatrics.aappublications.org/content/144/2/e20183283

Data Supplement at: http://pediatrics.aappublications.org/content/suppl/2019/07/12/peds.2018-3283.DCSupplemental

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# Exhibit 11

### Firearm Legislation Stringency and Firearm-Related Fatalities among Children in the US

Check for updates

Sriraman Madhavan, MS, Jordan S Taylor, MD, Julia M Chandler, MD, Kristan L Staudenmayer, MD, FACS, Stephanie D Chao, MD, FACS

BACKGROUND:	Firearm-related injuries are the second leading cause of pediatric deaths in the US. We sought
	to evaluate the effectiveness of both state child access prevention (CAP) laws and gun regu-
	lations on pediatric firearm mortality. We hypothesized that states with more stringent
	hrearm legislation had lower pediatric hrearm mortality.
STUDY DESIGN:	We used 2014-2015 firearm mortality data from the Web-Based Injury Statistics Query and
	and CAP laws. State-level covariates were obtained from government sources, including the
	Bureau of Labor Statistics and the Department of Education. Spearman rank correlations and
	linear regression were used to determine the relationship between overall pediatric firearm
	mortality and gun regulations. We also examined the relationship between gun regulations
	and firearm-related homicides and suicides.
<b>RESULTS:</b>	Annually, there were approximately 2,715 pediatric firearm fatalities among children; 62.1%
	were homicides and 31.4% suicides. There was a moderate negative correlation between states'
	firearm legislation stringency and overall pediatric firearm mortality ( $\rho = -0.66$ ; p < 0.001),
	and between CAP laws and firearm suicide rates ( $\rho = -0.56$ ; p < 0.001). After controlling for
	poverty, unemployment, substance abuse, and the number of registered firearms, the association
	between meanin legislation stringency and overall pediatric meanin mortanty remained signif- icant ( $p = 0.0/i$ ). The association between CAP laws and freerow suicide rate remained signif
	icant after controlling for socioeconomic factors, registered firearms, and other firearm
	legislation ( $p = 0.04$ ).
CONCLUSIONS:	Strict gun legislation and CAP laws are associated with fewer pediatric firearm fatalities and
	firearm suicides, respectively, though no such association was identified with pediatric firearm
	homicides. Although more studies are needed to determine causality, state-level legislation
	could play an important role in reducing pediatric firearm-related deaths. (J Am Coll Surg
	2019;229:150–157. © 2019 by the American College of Surgeons. Published by Elsevier
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Firearm-related injuries and fatalities among children are important public health problems in the US. Mass shootings continue to occur with regrettable frequency and

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Mr Madhavan and Dr Taylor contributed equally to this work.

Abstract presented at the American College of Surgeons 104th Annual Clinical Congress, Scientific Forum, Boston, MA, October 2018.

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Correspondence address: Jordan S Taylor, MD, Department of Surgery, Division of Pediatric Surgery, 300 Pasteur Dr, Always Building M116, Stanford University, Stanford, CA 94305. email: jtaylor4@stanford.edu dominate news headlines, serving as grim reminders of the consequences of firearm violence on children. In 2018, there were more than 85 incidents of gunfire on school grounds, including the especially deadly shootings at Marjory Stoneman Douglas High School (Parkland, FL) and Santa Fe High School (Santa Fe, TX) that left 27 dead, 27 injured, and hundreds of thousands of teens speaking out to demand legislative change.<sup>1-3</sup> Less well publicized, but even more staggering, are the pediatric lives lost each year due to suicide, homicide, or accidental death. Firearms were the second leading cause of death among children in the US in 2014, resulting in more pediatric deaths than cancer and heart disease combined.<sup>4</sup> Firearms contribute substantially to pediatric suicide and homicide rates. In 2014, firearms accounted for

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71% of all homicides and 41% of all suicides among children.<sup>5</sup> In 2010, the combined medical and work-loss cost for all fatal firearm injuries among children was estimated to be more than \$4.8 billion in the US.<sup>5</sup> In addition to the direct health and fiscal impacts of firearm injuries, there are significant long-term psychological effects on survivors and family members. Nearly 1 in 25 children have witnessed a shooting in the past year.<sup>6</sup>

Federal and state lawmakers are responsible for legislating public policy on firearms, though the effect of these policies on public health is often unclear and can be difficult to evaluate. Several studies have examined the association of overall firearm-related fatalities for all ages with state-level firearm legislation, but with varying results. Kwon and colleagues,<sup>7</sup> analyzing data from 1990, found that firearm legislation might have a very mild effect on the number of gun-related deaths, and socioeconomic variables have a significant impact. In their analysis of firearm fatalities between 2007 and 2010, Fleegler and colleagues<sup>8</sup> found the absolute number of firearm laws in a state is inversely correlated with firearm fatalities. Price and colleagues<sup>9</sup> found a strong association between restrictiveness of gun laws and firearm suicide, but little association with firearm homicide. However, these studies did not focus on the pediatric population.

Many states have enacted child-focused firearm legislation, known as child access prevention (CAP) laws intended to keep firearms away from youth. Several studies have examined the effect of CAP laws on unintentional injury or death in children and have suggested limited reductions in unintentional injury. These reductions have been limited largely to states that impose felony prosecution on CAP violations.<sup>10-12</sup> Webster and colleagues<sup>13</sup> found a modest reduction in adolescent suicides was associated with CAP laws; however, to our knowledge, no studies have examined the combined effects of CAP laws and overall state firearm legislation stringency on pediatric firearm mortality.

Previous research on the impact of firearm laws have focused largely on adults, or adults and children combined. To date, the few pediatric studies completed have largely evaluated the impact of firearm legislation on pediatric injuries or hospital admissions, with reference to in-hospital mortality, but not all pediatric firearm-related fatalities. In this article, we evaluate the association between CAP laws and state gun regulations on firearm mortality rates among children in 2014 and 2015. We also analyzed other statelevel factors that have previously been associated with mortality, including socioeconomic variables and neighboring states' firearms legislation.<sup>9</sup> We hypothesized that states with more stringent legislation and CAP laws would correlate with a lower incidence of firearm-related fatalities among children.

#### **METHODS**

#### Firearm-related injury and mortality

Firearm-related mortality data from 2014 and 2015 for children (ages 0 to 19 years) were accessed from the Web-Based Injury Statistics Query and Reporting System (WISQARS).<sup>5</sup> The WISQARS provides injury-related deaths and mortality rates derived from the National Vital Statistics System maintained by the Centers for Disease Control and Prevention National Center for Health Statistics. The WISQARS stratifies fatalities based on intent of injury (ie unintentional, homicide, suicide, or undetermined), mechanism of injury (eg firearm, drowning, or poisoning), sex, age, race/ethnicity, and state of residence of the injured person. The WISQARS uses the following ICD-10 codes to determine firearmrelated fatalities: W32 to W34, X72 to X74, X93 to X95, Y22 to Y24, Y35.0, and U01.4. To prevent inadvertent disclosure of identifiable cases, no data are reported if totals are <10 in tabulations for a specific geographic region.

#### State legislation data

The state-level variation in overall gun legislation stringency was quantified using the 2014 Brady Scorecard (Brady score) compiled annually by the Brady Campaign to Prevent Gun Violence since 2007.<sup>14</sup> In brief, the Brady score ranks 50 states based on a series of 33 gun policies, state gun death rates, and state crime gun export rates. Scores range from -100 for the least restrictive states to +100 for the most restrictive states.

We also considered state laws that are specifically designed to protect children from accessing firearms, or CAP laws. In 2014, the Brady score did not account for CAP laws, so this category of laws offered a second metric to measure child-specific gun legislation. Data on CAP laws by state were obtained from the Giffords Law Center, which tabulated CAP laws in effect in 2014.<sup>15</sup> The CAP scores (0 to 2 points) were assigned to each state, with 1 point given for presence of laws that address children gaining access to guns, and 1 point for any law that requires safe storage or firearm locks for guns in a home.

#### **State-level factors**

Socioeconomic and demographic factors that have been correlated previously with firearm-related violence<sup>9</sup> were included in the analysis. These variables include unemployment rates, poverty level, percent urbanization, alcohol

dependence, tobacco and marijuana use among teens, and high school graduation rates. Unemployment rates were obtained from the 2015 US Bureau of Labor Statistics<sup>16</sup>; state poverty rates and Supplemental Poverty Measures were taken from the US Census Bureau.<sup>17</sup> State public high school graduation rates for the 2014 to 2015 school year were obtained from the US Department of Education.<sup>18</sup> Health-related data (alcohol use, tobacco use, and marijuana use) were obtained from the 2014-2015 National Surveys on Drug Use and Health.<sup>19</sup> The surveys capture the percentage of 12- to 17-year-old individuals who have used marijuana in the past month or have had alcohol dependence in the past year, as defined by the Diagnostic and Statistical Manual of Mental Disorders, 4th edition.<sup>20</sup> In 2016, the Bureau of Alcohol, Tobacco, Firearms, and Explosives traced more than 60,000 firearms that were illegally trafficked across state lines, and many more are trafficked unnoticed.<sup>21</sup> To partially account for this, we examined the average Brady scores of neighboring states as separate state-level factors that could influence pediatric firearm mortality rates. Finally, the number of registered firearms per 100,000 children in each state was obtained from a 2014 Alcohol, Tobacco, Firearms, and Explosives report as a surrogate for total number of firearms in each state.<sup>22</sup>

#### **Data analysis**

The primary outcomes measures were annual pediatric firearm mortality rates, homicide rates, and suicide rates per 100,000 children, ages 0 to 19 years. We calculated simple descriptive statistics and performed correlation analysis of each state-level factor against our outcomes measures. We computed the Spearman's p statistic to estimate a rank-based measure of association between the state-level factors and the outcomes measures. Interpretation of correlation coefficients were defined by Mukaka,<sup>23</sup> with 0 to 0.3 considered negligible, 0.3 to 0.5 (-0.3)to -0.5) considered low positive (negative) correlations, 0.5 to 0.7 (-0.5 to -0.7) considered moderate positive (negative) correlations, and >0.7 (<-0.7) considered high or very high positive (negative) correlations. A p value < 0.05 was considered statistically significant. Data analysis was performed using R (R Foundation for Statistical Computing).

Multiple linear regression models were used to evaluate the relationship between the outcomes measures (overall pediatric firearm mortality rate, firearm-related homicide rate, and firearm-related suicide rate) and state-level factors. This allowed for evaluation of the association of firearm regulations and CAP laws with each of the outcomes measures, relative to other independent variables like socioeconomic factors.

#### RESULTS

There is considerable state-to-state variation in the stringency of firearm legislation (Fig. 1). In 2014, California had a Brady score of +76, and Arizona had a score of -39. The number of firearm-related fatalities among children and teens in the US has been relatively stable from 2008 to 2015, with an average of 2,715 victims per year (Fig. 2), and an overall pediatric firearm mortality rate of 3.28 per 100,000 children. However, there is significant variation in the pediatric firearm mortality rates between states, ranging from 0.45 deaths per 100,000 in Hawaii, to 7.55 deaths per 100,000 in Alaska. We found that an average of 62.1% of all the fatalities were homicides, and 31.4% were suicides; the remaining were categorized as unintentional firearm deaths, legal interventions, or deaths due to undetermined intent.

In unadjusted analysis, higher state gun legislation stringency (Brady score) was moderately associated with lower rates of firearm-related deaths among children and teens ( $\rho = -0.66$ ; p < 0.001) (Fig. 3). States in the highest quartile of Brady scores have an annual pediatric firearm mortality rate of 2.563 per 100,000 population compared with states in the lowest quartile, where the mortality rate is almost twice as high at 5.005 per 100,000. A low negative association was found between CAP laws and firearmrelated death rates ( $\rho = -0.31$ ; p = 0.03).

There was a moderate association between higher pediatric firearm mortality rates and lower Brady scores in the neighboring states in unadjusted analysis ( $\rho = -0.50$ ; p < 0.001). Non-legislative factors were also significantly associated with pediatric firearm mortality. There was a moderately positive association between state-wide unemployment and pediatric firearm homicide rates ( $\rho = 0.55$ ; p < 0.001). Similarly, there was a moderately positive correlation between teen tobacco use and adolescent firearm suicide rates ( $\rho = 0.50$ ; p < 0.001). No such association was found with marijuana use or alcohol dependence and any of our outcomes measures (Fig. 4).



**Figure 1.** Brady scores and child access prevention (CAP) scores for 2014; higher scores indicate stricter overall and child-specific firearm legislation in that state, respectively.



Figure 2. Pediatric firearm mortality rates in children (ages 0 to 19 years) in the US by year and by intent, 2000-2015.

After controlling for socioeconomic factors (poverty, unemployment, graduation rates, percent urbanization, alcohol dependence, tobacco use, and marijuana use) and number of registered firearms, the association between Brady scores and pediatric firearm mortality rates remained significant ( $\beta = -0.018$ ; 95% CI -0.035 to -0.0006; p = 0.04). In adjusted analysis, other variables that maintained a significant relationship with pediatric firearm mortality rates included the state's unemployment rate ( $\beta = 0.81$ ; 95% CI 0.35 to 1.27; p < 0.001) and the neighboring states' Brady scores ( $\beta = -0.022$ ; 95% CI -0.038 to -0.005; p = 0.01). The association between CAP laws and pediatric firearm mortality was not significant after controlling for Brady score and number of registered firearms.

We next evaluated the association between laws and suicide and homicide rates separately. In unadjusted analyses, Brady scores had low negative associations with both firearm homicide rates ( $\rho = -0.42$ ; p = 0.01) and firearm suicide rates ( $\rho = -0.49$ ; p = 0.001) among children. The presence of CAP laws was moderately associated with fewer firearm suicides among children ( $\rho = -0.56$ ; p < 0.001). States that had a CAP score of 2 had a pediatric firearm suicide rate of 0.633 per year per 100,000 children, although states that lack both access laws or laws regulating firearm suicide rate of 2.573 per year per 100,000 children (Fig. 5). The negative association between the presence of CAP laws and firearm homicide rates was negligible.

When controlling for unemployment rates, poverty rates, and the number of registered firearms, the correlation between Brady score and firearm homicide rates was no longer statistically significant. Similarly, the relationship between Brady score and firearm suicide rates was not statistically significant after controlling for the same state-level factors. However, even after controlling for socioeconomic factors, Brady score, and number of registered firearms, the association between presence of CAP laws and firearm suicide rates remained significant ( $\beta = -0.22$ ; 95% CI -0.440 to -0.003; p = 0.04).

#### DISCUSSION

Our study found that stricter state firearm legislation as quantified by the Brady scorecard was significantly associated with fewer firearm-related fatalities in children and teens. The CAP laws were similarly and significantly associated (moderate negative) with decreased firearm-related suicide rates, but not with overall firearm-related mortality. These associations remained significant after accounting for socioeconomic factors. The Brady scorecard had low negative associations with firearm homicide and suicide rates individually that were not statistically significant after accounting for socioeconomic factors. Likewise, the CAP score had low negative and negligible associations with overall firearm mortality and homicide rates, respectively, which were also not significant on multivariate analysis.

This research contributes to the national dialogue on how to address firearm-related fatalities, particularly in children. There are more than 300 federal gun laws that regulate the sale, transport, and possession of firearms in the US<sup>5,24</sup>; however, there is great variability on the state and local levels in the implementation of firearm regulations and little research on the effect of these laws. When compared with countries with similar economic



**Figure 3.** Relationship between Brady score (2014) and firearmrelated mortality rate by state and region.

development and political structure, the US is the most dangerous of nations for children, with pediatric deaths from firearm injuries as a leading contributor. Among the 23 wealthiest developed nations, the US accounts for approximately 30% of the overall population, but >90% of deaths due to firearms among children aged younger than 15 years.<sup>25</sup> A child is 82 times more likely to die from firearm-related injuries in the US than in other similar nations.<sup>26</sup> Fowler and colleagues<sup>27</sup> estimated nearly 19 children per day die or are treated for gunshot wounds in the US. The US has higher rates of homicide, suicide, and unintentional injury by firearm than any other industrialized nation.<sup>28</sup>

To our knowledge, this is the first study to examine the relationship between firearm legislation stringency and pediatric firearm fatalities using both overall stringency and child-specific legislation. To date, a few studies have examined firearm legislation stringency and pediatric firearm injuries. Safavi and colleagues<sup>29</sup> dichotomized states into strict firearm law states or non-strict firearm law states, based on child-specific legislation using Brady Campaign data and data pulled from individual state resources. Their group found that non-strict states had a higher mean firearm injury rate per 1,000 pediatric trauma patients. Non-strict states in their analyses increased the mean firearm injury rate by 3.75 compared with strict firearm law states. Tashiro and colleagues<sup>30</sup> compared legislative stringency with inpatient pediatric hospital admissions. Using Brady scores to classify states as either lenient or strict, they found that more hospital admissions due to firearm-related injuries occurred in lenient states. They identified an overall pediatric inhospital mortality of 7% from firearm injuries during their study period. Propensity score-matched analysis found mortality was higher in lenient states (7.5%) vs strict states (6.5%). Our study findings of a moderate negative correlation between pediatric fatality and increased legislative stringency are consistent with these previously noted overall trends in pediatric firearmrelated injuries. We note that when firearm legislation is treated as a continuous variable, rather than dichotomous, there is a dose-dependent correlation: states with more stringent firearm legislation are associated with decreased pediatric firearm mortality. We found the states with the least stringent laws had an annual pediatric firearm mortality rate twice that of states with the most stringent laws. Additionally, we found suggestion that the cumulative stringency of laws of neighboring states might have a similar relationship, as exhibited by the low negative correlation with pediatric firearm mortality.

Of note, studies examining the effect of legislative stringency on firearm injuries have found the most common mechanism of injury is typically assault, followed by accidental or undetermined, and trailed distantly by self-

Variable	Correlation with Firearm Mortality Rate (0-19 yrs)	Spearman's rank correlation coefficient	Correlation with	
			Firearm Homicide rate	Firearm Suicide Rate
Brady score	Moderate negative	ρ = -0.662, P < 0.001	Low negative	Low negative
Neighbors' Brady score	Low negative	ρ=-0.497, P<0.001	Low negative	Negligible
Unemployment rate	Low positive	p = 0.463, P = 0.003	Moderate positive	Negligible
CAP score	Low negative	p = -0.314, P = 0.03	Negligible	Moderate negative
Tobacco use %	Low positive	ρ = 0,382, P < 0.001	Low positive	Moderate positive
High school graduation rate	Low negative	p = -0.330, P = 0.02	Negligible	Low negative
Urban population %	Low negative	p = -0.338, P = 0.02	Low negative	Low negative
Firearms registration rate	Low positive	ρ = 0.426, P = 0.003	Negligible	Low positive
Supplemental poverty rate	Negligible	p = 0.202, P = 0.172	Low positive	Negligible
Marijuana use %	Negligible	p = -0.247, P = 0.100	Negligible	Negligible
Alcohol dependence %	Negligible	p = -0.152, P = 0.274	Negligible	Negligible

**Figure 4.** State-level factors and their correlation with the outcome measures. Spearman's rank correlation coefficient ( $\rho$ ) and p values are given for the primary end point, overall pediatric firearm mortality. All variables treated as continuous. Child access prevention (CAP) scores hold values of 0, 1, or 2. Tobacco use and marijuana use reflect percentage of survey respondents who indicated use within the last 30 days; participants were 12 to 17 years old.

Ex. 11 Page 114



**Figure 5.** Firearm suicide rates by presence of any child access prevention law (ie laws imposing criminal liability when a minor child gains access to a firearm, laws preventing people from providing firearms to minors, and safe storage requirements that apply to all firearms) and safe storage or gun lock requirements (ie laws concerning locking devices or storage in certain circumstances, or with certain types of guns). Five states (CA, CT, MD, MA, NY) have state-mandated standards for locking devices.

inflicted/suicide, with suicide typically accounting for just 3% of the cohorts.<sup>30,31</sup> Consequently, many studies have focused on the effect of firearms legislation on unintentional injury.<sup>11,12</sup> In our study, we found that suicides accounted for 31.4% of national pediatric firearm fatalities. The leading cause of fatality remains assault. This underscores the lethality of firearms when the intent is to cause harm. The case-fatality rate (ie proportion of cases resulting in death) is highest for suicides. Fowler and colleagues<sup>27</sup> reported that from 2012 to 2014, average annual case fatality rate was 74% for firearm-related suicides, 14% for firearm-related assault, and 6% for unintentional firearm injuries. In unadjusted analyses, we found that higher Brady scores and CAP laws were negatively associated (moderate-strength correlation) with fewer firearm-related suicide deaths. States without CAP laws had a 4-fold higher firearm-related suicide rates compared with states with the most extensive CAP laws (CAP score = 2). This association remained significant, even after accounting for potential confounders, such as gun ownership rates.

The use of firearms has consistently been one of the most common methods for suicide in adolescents in the US, accounting for 42.6% of adolescent suicides from 2000 to 2016.5 Numerous case-control studies have demonstrated that the presence of firearms in the home substantially increases the risk of adolescent suicide.<sup>32-34</sup> A separate case-control study in 2005 found that safe gun storage practices are associated with a decreased risk of teen suicide and unintentional firearm injuries.<sup>35</sup> Our analysis found only a low negative association between overall state firearm legislation stringency and suicide; however, CAP laws were more significantly associated (moderate correlation) with decreased suicide rates, suggesting that they can play an important role in reducing pediatric firearm suicide. Firearm storage and lock requirements can provide enough of a barrier to adolescents who are contemplating suicide, which is often an impulsive decision in this age group. Studies have demonstrated that teens who attempt suicide deliberate for as few as 10 minutes or less.<sup>36,37</sup> Preventing access to lethal means like firearms, even without adapting broader firearm legislation, can help reduce pediatric suicide rates across the country.

Between 2002 and 2007, there was a 17% increase in firearm homicide rates among children; from 2007 to 2014, there was a 60% increase in child firearm suicide rates.<sup>27</sup> Although we did find a significant association (moderate negative correlation) between firearm legislation stringency and overall pediatric mortality after accounting for socioeconomic factors, the correlation did not remain significant when considering homicide deaths alone. The CAP scores similarly had a negligible association with pediatric firearm homicide rates that was not statistically significant. Firearm homicide rates, however, are thought to be multifactorial and more closely associated with socioeconomic variables. Price and colleagues9 found that firearm legislation was associated with only 10% of the variation in homicide rates; the majority of the variation was ascribed to firearm ownership rates and socioeconomic factors. Other studies have noted significant variations in pediatric firearm homicide rates associated with racial disparities, drug and alcohol abuse, and poverty.38,39 Our study similarly found many socioeconomic variables, including unemployment rates, percent urbanization, poverty rates, and teen tobacco use, were associated with firearm homicide rates in unadjusted analysis. Firearm-related homicide in children remains a complex, multifaceted problem. Given the recent trends, more research is needed to identify meaningful ways to reduce firearm-related homicides among children.

There are several limitations to this study. As we evaluated only state-level information, these results are not generalizable at the individual person level. Our study examined a limited time period, and a longitudinal study can provide additional details about important time trends and draw stronger conclusions on the effect of firearm legislation on pediatric firearm mortality. This study was not able to control for differences in enforcement of state firearm legislation. There might be additional state-level variables or socioeconomic factors that were not captured by our identified confounders. Some of the state-level variables, like tobacco use and marijuana use, were gathered from a subset of the pediatric population (ages 12 to 17 years). Extrapolating data from subsets of the population and applying it to the entire group can cause error in our analysis, including our positive correlation between tobacco use and firearm-related suicide rates. However, suicide by any mechanism in children younger than 10 years is exceedingly rare,40 therefore, we are confident that the correlation is valid, despite the limitation in the data set. Overall state gun death rates are a component of the Brady scores, which can correlate to pediatric firearm mortality rates independently. However, the death rates contribute only 10% to the Brady scores and include both adults and children, minimizing the possible correlation to pediatric firearm mortality rates independently. Lastly, when analyzing independent predictors, we attempted to control for the number of firearms in each state with the number of registered firearms, which is a limited subset of the total number of firearms, as only certain types of firearms are required to be registered. Furthermore, each state has separate and variable laws related to what type of firearm must be registered in that state. Future work will aim to address some of these limitations by examining pediatric firearm-related deaths and state-level legislation over time.

#### CONCLUSIONS

156

Stricter state firearm legislation and CAP laws are associated with lower overall pediatric firearm mortality rates and pediatric firearm suicide rates, respectively. Additional state legislation could play an important role in reducing firearm fatalities in the pediatric population, particularly by reducing the number of suicides.

#### **Author Contributions**

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# Exhibit 12

### The Impact of Right-to-Carry Laws and the NRC Report: Lessons for the Empirical Evaluation of Law and Policy

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For over a decade, there has been a spirited academic debate over the impact on crime of laws that grant citizens the presumptive right to carry concealed handguns in publicso-called right-to-carry (RTC) laws. In 2005, the National Research Council (NRC) offered a critical evaluation of the "more guns, less crime" hypothesis using county-level crime data for the period 1977–2000. Seventeen of the eighteen NRC panel members essentially concluded that the existing research was inadequate to conclude that RTC laws increased or decreased crime. The final member of the panel, though, concluded that the NRC's panel data regressions supported the conclusion that RTC laws decreased murder. We evaluate the NRC evidence and show that, unfortunately, the regression estimates presented in the report appear to be incorrect. We improve and expand on the report's county data analysis by analyzing an additional six years of county data as well as state panel data for the period 1977–2006. While we have considerable sympathy with the NRC's majority view about the difficulty of drawing conclusions from simple panel data models, we disagree with the NRC report's judgment that cluster adjustments to correct for serial correlation are not needed. Our randomization tests show that without such adjustments, the Type 1 error soars to 40–70%. In addition, the conclusion of the dissenting panel member that RTC laws reduce murder has no statistical support. Finally, our article highlights some important questions to

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consider when using panel data methods to resolve questions of law and policy effectiveness. Although we agree with the NRC's cautious conclusion regarding the effects of RTC laws, we buttress this conclusion by showing how sensitive the estimated impact of RTC laws is to different data periods, the use of state versus county data, particular specifications, and the decision to control for state trends. Overall, the most consistent, albeit not uniform, finding to emerge from both the state and the county panel data models conducted over the entire 1977–2006 period with and without state trends and using three different models is that aggravated assault rises when RTC laws are adopted. For every other crime category, there is little or no indication of any consistent RTC impact on crime. It will be worth exploring whether other methodological approaches and/or additional years of data will confirm the results of this panel data analysis. (*JEL* K49, K00, C52)

#### 1. Introduction

The debate on the impact of "shall-issue" or "right-to-carry" (RTC) concealed handgun laws on crime—which has now raged on for over a decade demonstrates one of the many difficulties and pitfalls that await those who try to use observational data to estimate the effects of controversial laws.<sup>1</sup> John Lott and David Mustard initiated the "more guns, less crime" (MGLC) discussion with their widely cited 1997 article arguing that the adoption of RTC laws has played a major role in reducing violent crime. However, as Ayres and Donohue (2003b) note, Lott and Mustard's period of analysis ended just before the extraordinary crime drop of the 1990s. They concluded that extending Lott and Mustard's data set beyond 1992 undermined the MGLC hypothesis. Other studies have raised further doubts about the claimed benefits of RTC laws (e.g., see Black and Nagin, 1998; Ludwig, 1998).

But even as the empirical support for the Lott-Mustard thesis was weakening, its political impact was growing. Legislators continued to cite this work in support of their votes on behalf of RTC laws, and the MGLC claim has been invoked often in support of ensuring a personal right to have handguns under the Second Amendment. In the face of this scholarly and political ferment, in 2003, the National Research Council (NRC) convened a committee of top experts in criminology, statistics, and economics. Its purpose was to evaluate the existing data in hopes of reconciling the various methodologies and

<sup>1.</sup> The term "RTC laws" is used interchangeably with "shall-issue laws" in the guns and crime literature.

findings concerning the relationship between firearms and violence, of which the impact of RTC laws was a single, but important, issue. With so much talent on board, it seemed reasonable to expect that the committee would reach a decisive conclusion on this topic, and put the debate to rest.

The bulk of the NRC report on firearms, which was finally issued in 2005, was uncontroversial. The chapter on RTC laws, however, proved to be extremely contentious. Citing the extreme sensitivity of point estimates to various panel data model specifications, the NRC report failed to narrow the domain of uncertainty about the effects of RTC laws. Indeed, it may have broadened it. However, while the NRC report concluded there was no reliable statistical support for the MGLC hypothesis, the vote was not unanimous. One dissenting committee member argued that the committee's own estimates revealed that RTC laws did in fact reduce the rate of murder. Conversely, a different member went even further than the majority's opinion by doubting that *any* econometric evaluation could illuminate the impact of RTC laws.

Given the prestige of the committee and the conflicting assessments of both the substantive issue of RTC laws' impact and the suitability of empirical methods for evaluating such laws, a reassessment of the NRC's report would be useful for researchers seeking to estimate the impact of other legal and policy interventions. Our systematic review of the NRC's evidence—its approach and findings—also provides important lessons on the perils of using traditional observational methods to elucidate the impact of legislation. To be clear, our intent is not to provide what the NRC panel could not—that is, the final word on how RTC laws impact crime. Rather, we show how fragile panel data evidence can be, and how a number of issues must be carefully considered when relying on these methods to study politically and socially explosive topics with direct policy implications.

The outline of this article is as follows. Section 2 offers background on the debate over RTC laws, and Section 3 describes relevant aspects of the NRC report in depth. Section 4 enumerates the critical flaws of the key results in the NRC report. Sections 5 and 6 explore two key econometric issues where the NRC panel may have erred—whether to control for state-specific trends and whether to adjust standard errors to account for serial or within-group correlation. Section 7 extends the analysis through 2006, and Section 8 offers improvements to the NRC model by revising the regression specification in accordance with past research on crime. Section 9 discusses the issue of whether the impact of RTC laws can be better estimated using

county- or state-level data. Section 10 delves further into three issues in this debate that merit special attention: the problem of omitted variable bias in assessing the impact of RTC laws (and in particular, the difficult-to-measure effect of the crack epidemic), the plausibly endogenous adoption of RTC legislation, and the relatively untouched issue of how RTC laws affect gun violence in particular. Section 11 offers concluding comments on the current state of the research on RTC laws, the difficulties in ascertaining the causal effects of legal interventions, and the dangers that exist when policy makers can simply pick their preferred study from among a wide array of conflicting estimates.

#### 2. Background on the Debate

In a widely discussed 1997 article, "Crime, Deterrence, and Right-to-Carry Concealed Handguns," John Lott and David Mustard (1997) argued, based on a panel data analysis, that RTC laws were a primary driving force behind falling rates of violent crime. Lott and Mustard used county-level crime data (including county and year fixed effects, as well as a set of control variables) to estimate the impact of RTC laws on crime rates over the time period 1977–92. In essence, Lott and Mustard's empirical approach was designed to identify the effect of RTC laws on crime in the ten states that adopted them during this time period. Using a standard difference-in-difference model, the change in crime in the ten RTC states is compared with the change in crime in non-RTC states. The implicit assumption is that the controls included in the regression will explain other movements in crime across states, and the remaining differences in crime levels can be attributed to the presence or absence of the RTC laws.

Lott and Mustard estimated two distinct difference-in-difference-type models to test the impact of RTC laws: a dummy variable model and a trend, or "spline," model<sup>2</sup>. The "dummy model" tests whether the average crime level in the pre-passage period is statistically different from the post-passage crime level (after controlling for other factors). The "spline model" measures whether crime *trends* are altered by the adoption of RTC laws. Lott and Mustard noted that the

<sup>2.</sup> In the "dummy model," RTC laws are modeled as a dummy variable that takes on a value of 1 in the first full year after passage and retains that value thereafter (since no state has repealed its RTC law once adopted). In the "trend model," RTC laws are modeled as a spline variable indicating the number of years post-passage.

spline approach would be superior if the intervention caused a reversal in a rising crime rate. Such a reversal could be obscured in a dummy variable model that only estimates the average change in crime between the pre- and post-passage periods. An effective RTC law might show no effect in the dummy model if the rise in the pre-passage crime rate and the fall in the post-passage rate were to leave the average "before" and "after" crime levels the same.

In both regression models, Lott and Mustard included only a single other criminal justice explanatory variable—county-level arrest rates—plus controls for county population, population density, income, and thirty-six(!) categories of demographic composition. As we will discuss shortly, we believe that many criminological researchers would be concerned about the absence of important explanatory factors such as the incarceration rate and the level of police force.

Lott and Mustard's results seemed to support the contention that laws allowing the carry of concealed handguns lead to less crime. Their estimates suggested that murder, rape, aggravated assault, and overall violent crime fell by 4–7% following the passage of RTC laws. In contrast, property crime rates (auto theft, burglary, and larceny) were estimated to have increased by 2–9%. Lott and Mustard thus concluded that criminals respond to RTC laws by substituting violent crime with property crime to reduce the risk that they would be shot (since, according to them, victims are more often absent during the commission of a property crime). They also found that the MGLC contention was strengthened by the trend analysis, which ostensibly suggested significant decreases in murder, rape, and robbery (but no significant increases in property crime).

From this evidence, Lott and Mustard (1997) concluded that permissive gun-carrying laws deter violent crimes more effectively than any other crime reduction policy: "concealed handguns are the most cost-effective method of reducing crime thus far analyzed by economists, providing a higher return than increased law enforcement or incarceration, other private security devices, or social programs like early education." They went even further by claiming that had the remaining non-RTC states enacted such legislation, over 1,400 murders and 4,100 rapes would have been avoided nationwide, and that each new handgun permit would reduce victim losses by up to \$5,000.

#### 2.1. The Far-Reaching Impact of MGLC

The first "MGLC" article and Lott's subsequent research (and pro-gun advocacy) have had a major impact in the policy realm. Over the past decade,

politicians as well as interest groups such as the National Rifle Association have continually trumpeted the results of this empirical study to oppose gun control efforts and promote less restrictive gun-carrying laws. Lott relied on his own research to advocate for the passage of state-level concealed-carry gun laws, testifying on the purported safety benefits of RTC laws in front of several state legislatures, including Nebraska, Michigan, Minnesota, Ohio, and Wisconsin (Ayres and Donohue, 2003b).

The impact of the Lott-Mustard article can also be seen at the federal level. In 1997, ex-Senator Larry Craig (R-Idaho) introduced the Personal Safety and Community Protection Act with Lott's research as supporting evidence. This bill was designed to allow state nonresidents with valid handgun permits in their home state to possess concealed firearms (former football athlete Plaxico Burress sought to invoke this defense when he accidentally shot himself in a Manhattan nightclub with a gun for which he had obtained a Florida permit). According to Craig, Lott's work confirmed that positive externalities of gun carrying would result in two ways: by affording protection for lawabiding citizens during criminal acts and by deterring potential criminals from ever committing offenses for fear of encountering an armed response.<sup>3</sup> Clearly, Lott's work has provided academic cover for policy makers and advocates seeking to justify the view—on public safety grounds—that the Second Amendment confers a private right to possess handguns.

#### 2.2. Questioning MGLC

Immediately after the publication of the Lott–Mustard article, scholars started raising serious questions about the theoretical and empirical validity of the MGLC hypothesis. For example, Zimring and Hawkins (1997) claimed that the comparison of crime between RTC and non-RTC states is inherently misleading because of factors such as poverty, drugs, and gang activity, which vary significantly across gun-friendly and non-gun-friendly

<sup>3. 143</sup> CONG. REC. S5109 (daily ed. May 23, 1997) (statement of Sen. Craig). The bill was again introduced in 2000 by Congressman Cliff Stearns (R-Florida), who also cited Lott's work. 146 CONG. REC. H2658 (daily ed. May 9, 2000) (statement of Rep. Stearns). Indeed, this proposed legislation, now derisively referred to as "Plaxico's Law," is a perennial favorite of the NRA and frequently introduced by supportive members of Congress (Collins, 2009).

states (and are often difficult to quantify). To the extent that the relatively better crime performance seen in shall-issue states during the late 1980s and early 1990s was the product of these other factors, researchers may be obtaining biased impact estimates. Underscoring this point, Ayres and Donohue (2003b) pointed out that crime rose across the board from 1985 to 1992, and most dramatically in non-RTC states. Since the Lott-Mustard data set ended in 1992, it could not capture the most dramatic reversal in crime in American history. Figures 1–7 depict the trends of violent and property crimes over the period 1970–2007. For each of the seven crimes, the fifty states (plus DC) fall into four groupings: non-RTC states, states that adopted RTC laws over the period 1985-88 ("early adopters"), those that adopted RTC laws over the period 1989-91 ("mid-adopters"), and those that adopted RTC laws over the period 1994-96 ("late adopters"). The crime rate shown for each group is a within-group average, weighted by population. The figures corroborate Ayres and Donohue's point: crime rates declined sharply across the board beginning in 1992. In fact, there was a steady upward trend in crime rates in the years leading up to 1992, most distinctly for rape and aggravated assault. Moreover, the average crime rates in non-RTC states seemed to have dropped even more drastically than those in RTC states, which suggests that crime-reducing factors other than RTC laws were at work.

Ayres and Donohue (2003b) also recommended the use of a more general model, referred to as the "hybrid model," which essentially combined the dummy variable and spline models, to measure the immediate and long-run



**Figure 1.** Murder Trends in RTC versus Non-RTC States—Weighted Average of Murder Rates per 100,000 Residents (1970–2007).



**Figure 2.** Rape Trends in RTC versus Non-RTC States—Weighted Average of Rape Rates per 100,000 Residents (1970–2007).

impact of RTC laws on crime. Since the hybrid model nests both the dummy and spline models, one can estimate the hybrid and generate either of the other models as a special case (depending on what the data show). This exercise seemed to weaken the MGLC claim. Their analysis of the county data set from 1977–1997 using the Lott-Mustard specification (revised to measure state-specific effects) indicated that RTC laws in aggregate *raised* total crime costs by as much as \$524 million.

Just as Lott had identified a potential problem with the dummy model (it might understate a true effect if crime followed either a V-shaped or an



**Figure 3.** Assault Trends in RTC versus Non-RTC States—Weighted Average of Assault Rates per 100,000 Residents (1970–2007).



**Figure 4.** Robbery Trends in RTC versus Non-RTC States—Weighted Average of Robbery Rates per 100,000 Residents (1970–2007).

inverted V-shaped pattern), there is a potential problem with models (such as the spline and the hybrid models) that estimate a post-passage linear trend. Early adopters of RTC laws have a far more pronounced impact on the trend estimates of RTC laws than later adopters since there may only be a few years of post-passage data available for a state that adopts RTC laws close to the end of the data period. If those early adopters were unrepresentative of low-crime states, then the final years of the spline estimate would suggest a dramatic drop in crime, not because crime had in fact fallen in adopting states but because the more representative states had dropped out of the estimate (since there would be no post-passage data after, say, three years for



Figure 5. Auto Theft Trends in RTC versus Non-RTC States—Weighted Average of Auto Theft Rates per 100,000 Residents (1970–2007).



**Figure 6.** Burglary Trends in RTC versus Non-RTC States—Weighted Average of Burglary Rates per 100,000 Residents (1970–2007).

a state that had adopted the RTC law only three years earlier, but there would be such data for Maine, Indiana, and North Dakota, which were the earliest RTC adopters). We recognize that each model has limitations, and present the results of all three in our tables below.<sup>4</sup>

#### **3.** Findings of the NRC

The sharply conflicting academic assessments of RTC laws specifically and the impact of firearms more generally, not to mention the heightened political salience of gun issues, prompted the NRC to impanel a committee of experts to critically review the entire range of research on the relationships between guns and violence. The blue-chip committee, which included prominent scholars such as sociologist Charles Wellford (the committee chair), political scientist James Q. Wilson, and economists Joel Horowitz, Joel Waldfogel, and Steven Levitt, issued its wide-ranging report in 2005.

While the members of the panel agreed on the major issues discussed in eight of the nine chapters of the NRC report, the single chapter devoted to

<sup>4.</sup> We note that in the latest version of his book, Lott (2010) criticizes the hybrid model, but he fails to appreciate that the problem with the hybrid model—and with the spline model he prefers—is that they both yield estimates that are inappropriately tilted down as the more representative states drop out of the later years, which drive the post-passage trend estimates. An apples-and-apples comparison that included the identical states to estimate the post-passage trend would not suggest a negative slope. This is clear in Figure 1 and Table 1 of Ayres and Donohue (2003b).



Figure 7. Larceny Trends in RTC versus Non-RTC States—Weighted Average of Larceny Rates per 100,000 Residents (1970–2007).

exploring the causal effects of RTC laws on crime proved to be quite contentious. After reviewing the existing (and conflicting) literature and undertaking their own evaluation of Lott's county-level crime data, seventeen of the eighteen committee members concluded that the data provided no reliable and robust support for the Lott-Mustard contention. In fact, they believed the data could not support any policy-relevant conclusion. In addition, they claimed they could not estimate the true impact of these laws on crime because (1) the empirical results were imprecise and highly sensitive to changes in model specification and (2) the estimates were not robust when the data period was extended eight years beyond the original analysis (through 2000), a period during which a large number of states adopted the law.

One can get an inkling of the NRC majority's concern about model sensitivity by examining Table 2a (which we will discuss in detail in Section 4.2), which reports estimates from the NRC report on the impact of RTC laws on seven crimes. The estimates are based on the Lott and Mustard (1997) dummy and spline models and county data for the period 1997–2000. The vastly different results produced by the two models gave the majority considerable pause. For example, if one believed the dummy model, then RTC laws considerably *increased* aggravated assault and robbery, while the spline model suggested RTC laws *decreased* the rate of both of these crimes.

The tension created by conflicting estimates was epitomized by the intrapanel dissention, as two members of the committee wrote separately on the NRC's evaluation of RTC laws. One sought to refute the majority's skepticism, and one sought to reinforce it. Noted political scientist James Q. Wilson offered the lone dissent to the committee's report, claiming that Lott and Mustard's MGLC finding actually held up under the panel's reanalysis. Specifically, Wilson rejected the majority's interpretation of the regression estimates seen in Table 2a. Although the panel noted that the RTC impact estimates disagreed across their two models (dummy and spline) for six of the seven crime categories, Wilson emphasized the similar finding of murder rate declines in the two models. The agreement in the murder estimates led him to heartily endorse the MGLC view. Indeed, after dismissing articles that had cast doubt on the MGLC hypothesis (such as Black and Nagin, 1998), on the grounds that they were "controversial," Wilson concluded: "I find the evidence presented by Lott and his supporters suggests that RTC laws do in fact help drive down the murder rate, though their effect on other crimes is ambiguous" (NRC, 2005, p. 271).

The committee penned a response to Wilson's dissent (separate from its overall evaluation of RTC legislation), which stressed that the only disagreement between the majority and Wilson (throughout the entire volume on gun issues) concerned the impact of RTC laws on murder. They noted that, while there were a number of negative estimates for murder using the Lott-Mustard approach, there were also several positive estimates that could not be overlooked. In addition, even the results for murder failed to support the MGLC contention when restricting the period of analysis to five years or less after law adoption.<sup>5</sup> The important task was to try to reconcile these contradictions—and the panel majority believed that was not possible using the existing data.

Committee member (and noted econometrician) Joel Horowitz was the ardent skeptic, and not without merit. Horowitz joined the refutation of Wilson but also authored his own appendix discussing at length the difficulties of measuring the impact of RTC laws on crime using observational rather than experimental data.<sup>6</sup> He began by addressing a number of flaws in the panel data approach. First, if factors other than the adoption of the RTC law change but are not controlled for in the model, then the resulting estimates would not effectively isolate the impact of the law (we demonstrate the

<sup>5.</sup> The importance of this restriction on the post-passage data was mentioned earlier: As states dropped out of the post-passage data, the estimated impact of RTC laws became badly biased (since one was no longer deriving the estimated effect from a uniform set of states).

<sup>6.</sup> While his chapter is directed at the analysis of RTC laws, Horowitz's comments applied to an array of empirical studies of policy that were discussed throughout the entire NRC volume.

likelihood of this possibility in Section 10). Second, if crime increases before the adoption of the law at the same rate it decreases after adoption, then a measured zero difference would be misleading. The same problem arises for multiyear averages. Third, the adoption of RTC laws may be a *response* to crime waves. If such an endogeneity issue exists, the difference in crime rates may merely reflect these crime waves rather than the effect of the laws. Lastly, as even Lott (2000) found in his data, RTC states differ noticeably from non-RTC states (e.g., RTC states are mainly Republican and had low but rising rates of crime). It would not be surprising if these distinctive attributes influence the measured effect of RTC laws. In this event, looking at the impact of RTC laws in current RTC states may not be useful for predicting impact if they are adopted in very different states.

Ideally, states would be randomly selected to adopt RTC laws, thereby eliminating the systematic differences between RTC states and non-RTC states. In the absence of such randomization, researchers introduce controls to try to account for these differences, which generates debate over which set of controls is appropriate. Lott (2000) defended his model by claiming that it included "the most comprehensive set of control variables yet used in a study of crime" (p. 153). We show here that this claim is gravely outdated. Moreover, Horowitz noted that not only are the data limited for these variables, it is also possible to control for too many variables-or too few. He pointed out that Donohue (2003) found a significant relationship between crime and future adoption of RTC legislation, suggesting the likelihood of omitted variable bias and/or the endogenous adoption of the laws. Horowitz concludes by noting that there is no test that can determine the right set of controls: "it is not possible to carry out an empirical test of whether a proposed set of X variables is the correct one ... it is largely a matter of opinion which set [of controls] to use" (NRC, 2005, p. 307). Noting the likelihood of misspecification in the evaluation of RTC laws, and that estimates obtained from a misspecified model can be highly misleading, he concluded that there was little hope of reaching a scientifically supported conclusion based on the Lott-Mustard/NRC model.

#### 3.1. The Serious Need for Reassessment

The story thus far has been discouraging for those hoping for illumination of the impact of legislation through econometric analysis. If the NRC majority is right, then years of observational work by numerous researchers, topped off with a multiyear assessment of the data by a panel of top scholars, were not enough to pin down the actual impact of RTC laws. However, given that the panel only presented estimates based on the Lott-Mustard (1997) approach (except for a sparse model with no covariates, which we describe in Section 4), it is possible the committee overlooked quantitative models and potentially useful evidence that could have influenced their view on the topic. If Horowitz is right, then the entire effort to estimate the impact of state RTC policies from observational data is doomed. Indeed, there may be simply too much that researchers do not know about the proper structure of econometric models of crime. Notably, however, the majority did not join Horowitz in the broad condemnation of all observational microeconometrics for the study of this topic. Perhaps a model that better accounts for all relevant, exogenous, crime-influencing factors and secular crime trends could properly discern the effects of RTC laws. As we show below, a number of plausible explanations and factors were excluded from the committee's examination.

#### 4. Attempts to Replicate the NRC Findings

Previous research on guns and crime has shown how data and methodological flaws can produce inaccurate conclusions. In a follow-up to their initial 2003 *Stanford Law Review* article, Ayres and Donohue (2003a) showed how coding errors can yield inaccurate estimates of the effect of RTC laws on crime. Commenting on a study in support of the MGLC premise by Plassman and Whitley (2003), Ayres and Donohue (2003a) described numerous coding flaws. After correcting these errors, the evidence supporting the MGLC hypothesis evaporated.

#### 4.1. Panel Data Models with No Covariates

Since the NRC panel based their reported estimates on data provided by John Lott, we thought it prudent to carefully examine the NRC committee's own estimates. We first attempt to replicate the results of the report using the NRC 1977–2000 county data set, which the committee supplied to us. We begin with the committee's no-controls model, which, apart from the dummy and trend variables, only includes year and county fixed effects. The reported NRC estimates are presented in Table 1a, and the first two rows of Table 1b show our efforts at replicating them. While the estimates of the dummy variable model are reasonably close, the trend estimates are not at all
comparable: The sign on the estimates in the spline model switches when going from Table 1a to Table 1b for all crimes except auto theft. Table 1b also includes our own estimates from the more flexible version of these specifications—the hybrid model—which combines the dummy and trend approaches. In other words, taken at face value, Table 1b tells us that crime clearly worsened for six or seven crime categories after the passage of RTC laws, regardless of whether one used the dummy variable, spline, or hybrid models.

**Table 1a.** Estimated Impact of RTC Laws—Published NRC Estimates—No Controls, All Crimes, 1977–2000 (County Data)<sup>a</sup>

	Murder	Rape	Aggravated	Robbery	Auto	Burglary	Larceny
	(%)	(%)	Assault (%)	(%)	Theft (%)	(%)	(%)
1. Dummy variable	-1.95	17.91***	12.34***	19.99***	23.33***	19.06***	22.58***
model	1.48	1.39***	0.90***	1.21***	0.85***	0.61***	0.59***
2. Spline model	0.12	-2.17***	-0.65***	-0.88***	0.57***	-1.99***	-0.71***
	0.32	0.30***	0.20***	0.26***	0.19***	0.13***	0.13***

<sup>a</sup>Estimations include year and county fixed effects, and are weighted by county population. Robust standard errors are provided beneath point estimates.

\*Significant at 10%; \*\*significant at 5%; \*\*\*significant at 1%.

	Murder	Rape	Aggravated	Robbery	Auto	Burglary	Larceny
	(%)	(%)	Assault (%)	(%)	Theft (%)	(%)	(%)
1. Dummy variable	-2.58	18.40***	12.60***	19.70***	22.80***	19.00***	22.60***
model	1.87	2.29***	1.40***	1.75***	1.69***	1.24***	1.08***
2. Spline model	-0.57*	2.36***	1.52***	2.43***	3.17***	2.23***	3.01***
	0.34*	0.39***	0.25***	0.31***	0.30***	0.24***	0.22***
3. Hybrid model							
Post-passage	-0.06	16.20***	11.90***	17.40***	16.80***	17.70***	18.50***
dummy	2.33	2.22***	1.69***	1.88***	1.86***	1.34***	1.20***
Trend effect	$-0.56 \\ 0.43$	0.58 0.40	0.22 0.30	0.51 0.35	1.32*** 0.35***	0.28 0.27	0.98*** 0.25***

**Table 1b.** Estimated Impact of RTC Laws—Using NRC County Data—No Controls, All Crimes, 1977—2000<sup>a</sup>

<sup>a</sup>Estimations include year and county fixed effects, and are weighted by county population. Robust standard errors are provided beneath point estimates.

\*Significant at 10%; \*\*significant at 5%; \*\*\*significant at 1%.

	Murder (%)	Rape (%)	Aggravated Assault (%)	Robbery (%)	Auto Theft (%)	Burglary (%)	Larceny (%)
1. Dummy	-2.20	27.80***	16.40***	19.50***	23.90***	22.80***	28.10***
variable model	1.87	3.53***	2.16***	2.06***	2.27***	2.06***	2.29***
2. Spline model	0.68**	4.65***	4.31***	3.18***	4.72***	5.06***	6.02***
	0.28**	0.46***	0.26***	0.27***	0.28***	0.25***	0.27***
3. Hybrid model							
Post-passage	-7.99***	12.00***	-3.50	8.91***	5.50**	1.44	3.26
dummy	2.19***	3.08***	2.72	2.32***	2.70**	2.60	2.98
Trend effect	1.34*** 0.33***	3.66*** 0.37***	4.60*** 0.32***	2.44*** 0.30***	4.27*** 0.32***	4.94*** 0.31***	5.75*** 0.35***

**Table 1c.** Estimated Impact of RTC Laws—Using Updated 2009 County-Level Data—No Controls, All Crimes, 1977–2000 (without 1993 Data)<sup>a</sup>

<sup>a</sup>Estimations include year and county fixed effects, and are weighted by county population. Robust standard errors are provided beneath point estimates.

\*Significant at 10%; \*\*significant at 5%; \*\*\*significant at 1%.

We contacted the committee to see if we might be able to understand why the efforts at replication were failing, but the files for reproducing their results and tables had not been retained.<sup>7</sup> Thus, we thought it wise to analyze county-level data by constructing our own data set, which we will refer to as the "updated 2009 data set." We create the same variables found in Lott's data—crime rates, demographic composition, arrest rates, income, population, and population density—and extend our new set as far forward as the data are available—2006 (the NRC data ended in 2000).<sup>8</sup> This data extension also gives us an opportunity to explore how the NRC's results are

<sup>7.</sup> In an attempt to reconcile the divergence, we initially speculated that perhaps the NRC committee did not weight its panel data regressions by county population as we do throughout, but this turned out not to explain the difference. Our best guess is that the NRC did weight the regression by population since they essentially adopted the Lott and Mustard (1997) approach. We also determined that the NRC data set was missing all county identifiers for 1999 and 2000, so we speculated that this might explain the results (since data for any year with a missing country identifier would be omitted from the regression). Again, we could not replicate the NRC spline model results of Table 1a, whether we included all years of data or dropped 1999 and 2000.

<sup>8.</sup> We also add 0.1 to all zero crime values before taking the natural log in our county-level data set, as the NRC did.

affected when using the most current data available. As we will see in Section 7, the additional years of data will also enable us to estimate the effect of six additional state adoptions of RTC laws, not present in the NRC analysis: Michigan (2001), Colorado (2003), Minnesota (2003), Missouri (2003), New Mexico (2003), and Ohio (2004).<sup>9</sup>

We obtained our crime data from the University of Michigan's Interuniversity Consortium for Political and Social Research, which maintains the most comprehensive collection of Uniform Crime Reports (UCR) data. Unfortunately, county-level crime data for 1993 are currently unavailable. The National Archive of Criminal Justice Data recently discovered an error in the crime data imputation procedure for 1993 and, for this reason, has made 1993 data inaccessible until the error has been corrected. Thus, for all of the following tables with estimates using our updated data, we are missing values for 1993.

Table 1c reproduces Table 1b using our own newly constructed data set (with 1993 omitted). In the case of every crime-model permutation, the use of this new data set further weakened the crime-reducing effects of RTC laws.<sup>10</sup> The bottom line is that (1) we cannot replicate the NRC no-controls estimates of Table 1a whether we use our own newly constructed county data or the data used by the NRC committee and (2) the best estimates in the no-controls model overwhelm-ingly show that all crime was *higher* after RTC laws adoptions.

# 4.2. Panel Data Models with Covariates

After failing to replicate the NRC "no-covariates" model, we next undertook the same replication exercise with the "covariates" model, which adds to the county and year fixed effects model the following Lott-Mustard explanatory variables: arrest rate, county population, population density, real per capita income variables, and thirty-six variables designed to capture the

<sup>9.</sup> Kansas and Nebraska adopted RTC laws in 2006, which is too late to be captured in our analysis, since we assume a state to be an "RTC state" beginning in the first full year after a law's passage.

<sup>10.</sup> Table 1c differs from Table 1b in two respects—it uses our new data set instead of the NRC, and it omits 1993 data. To see how important the 1993 omission is, we reproduced Table 1b (using the NRC data) dropping that year, which turned out to have little effect on the estimates.

county's demographic composition.<sup>11</sup> Although we have already noted Lott's claim that this is "the most comprehensive set of control variables yet used in a study of crime," in fact, this set of variables omits many important influences on crime, which we will reintroduce in Section 8.

To be clear about our approach, we use annual county-level crime data (and later, state-level data) for the United States from 1977 through either 2000 (to conform to the NRC report) or 2006 (the last year for which data are available). We explore the impact of RTC laws on seven Index I crime categories by estimating the reduced-form regression:

$$Y_{it} = \eta \text{RTC}_{jt} + \alpha_i + \theta_t + \beta_{jt} + \gamma X_{ijt} + \epsilon_{it}, \qquad (1)$$

where the dependent variable  $Y_{it}$  denotes the natural log of the individual violent and property crime rates for county *i* and year *t*. Our explanatory variable of interest—the presence of an RTC law within state *j* in year *t*—is represented by RTC<sub>*jt*</sub>. The exact form of this variable shifts according to the three variations of the model we employ (these include the Lott-Mustard dummy and spline models, as well as the Ayres and Donohue hybrid model).<sup>12</sup>

The variable  $\alpha_i$  indicates county-level fixed effects (unobserved county traits) and  $\theta_t$  indicates year effects. As we will discuss below, there is no consensus on the use of state-specific time trends in this analysis, and the NRC report did not address this issue. Nevertheless, we will explore this possibility, with  $\beta_{jt}$  indicating state-specific trends, which are introduced in selected models. Since neither Lott and Mustard (1997) nor the NRC (2005) examines state

<sup>11.</sup> The NRC uses the Lott-Mustard method of calculating arrest rates, which is the number of arrests for crimes divided by the contemporaneous number of crimes. Econometrically, it is inappropriate to use this contemporaneous measure since it leaves the dependent variable on both sides of the regression equation (a better approach would lag this variable one year, as discussed in Ayres and Donohue, 2009). Another issue about the arrest rates is unclear: The NRC report does not indicate whether it uses the individual Index I crime categories to compute arrest rates, or alternatively, if they use the broad categories of violent and property crimes, as has been used in recent articles (Moody and Marvell, 2008). We adopt this latter approach for all tables in this article, although we also explored the possibility of arrest rates for individual crimes. Regardless of which arrest rate we used, our estimates still diverged considerably from the estimates presented by the NRC.

<sup>12.</sup> As noted previously, in the dummy variable approach, the RTC variable is a dichotomous indicator that takes on a value of 1 in the first full year that a state *j* has an RTC law. In the spline model, the RTC variable indicates the number of post-passage years. The hybrid specification contains both dummy and trend variables.

trends, this term is dropped when we estimate their models. The term  $X_{ijt}$  represents a matrix of observable county and state characteristics thought by researchers to influence criminal behavior. The components of this term, however, vary substantially across the literature. For example, while Lott uses only "arrest rates" as a measure of criminal deterrence, we discuss the potential need for other measures of deterrence, such as incarceration levels or police presence, which are measured at the state level.

In Tables 2a–c, we follow the same pattern as that of Tables 1a–c: We begin by showing the NRC published estimates (Table 2a) and then show our effort at replication using the NRC data set (Table 2b). We then show the estimates obtained from our reconstruction of the county data set from 1977 through 2000 (Table 2c, which omits 1993 data).<sup>13</sup> The basic story that we saw above with respect to the no-covariates model holds again: We cannot replicate the NRC results using the NRC's own data set (compare Tables 2a and b), and omitting 1993 data does not make a substantive difference. Once again, our Table 2c estimates diverge wildly from the Table 2a estimates, which appeared in the NRC report. As we will see in a moment, the results that Professor Wilson found to be consistent evidence of RTC laws reducing murder (see Table 2a) were probably inaccurate (see Table 2c).

	Murder	Rape	Aggravated	Robbery	Auto	Burglary	Larceny
	(%)	(%)	Assault (%)	(%)	Theft (%)	(%)	(%)
1. Dummy variable	-8.33***-	-0.16	3.05***	3.59***	12.74***	6.19***	12.40***
model	1.05***	0.83	0.80***	0.90***	0.78***	0.57***	0.59***
2. Spline model	-2.03*** -	-2.81***	-1.92***	-2.58***	-0.49***	-2.13***	-0.73***
	0.26***	0.20***	0.20***	0.22***	0.13***	0.19***	0.13***

**Table 2a.** Estimated Impact of RTC Laws—Published NRC Estimates—Lott-Mustard Controls, All Crimes, 1977–2000<sup>a</sup>

<sup>a</sup>Estimations include year and county fixed effects, and are weighted by county population. Robust standard errors are provided beneath point estimates. The control variables (adopted from the Lott-Mustard model) include arrest rate, county population, population density, per capita income measures, and thirty-six demographic composition measures indicating the percentage of the population belonging to a race-age-gender group. \*Significant at 10%; \*\*significant at 5%; \*\*\*significant at 1%.

<sup>13.</sup> Once again, we explored whether omitting 1993 data had an impact on the results, and again our Table 2 estimates looked quite similar when 1993 data were dropped.

	Murder (%)	Rape (%)	Aggravated Assault (%)	Robbery (%)	Auto Theft (%)	Burglary (%)	Larceny (%)
1. Dummy variable model	-3.80* 2.14*	10.50*** 2.18***	11.20*** 1.55***	11.20*** 1.81***	16.80*** 1.54***	11.00*** 0.98***	17.60*** 0.86***
2. Spline model	$-0.61 \\ 0.38$	1.38*** 0.36***	1.91*** 0.25***	1.63*** 0.32***	2.61*** 0.29***	1.62*** 0.19***	3.12*** 0.17***
3. Hybrid model Post-passage dummy	-2.51 2.63	9.77*** 2.28***	7.01*** 1.76***	9.02*** 1.92***	12.20*** 1.74***	8.92*** 1.06***	9.72*** 0.94***
Trend effect	$-0.30 \\ 0.47$	0.18 0.36	1.05*** 0.27***	0.53 0.33	1.11*** 0.34***	0.52** 0.22**	1.92*** 0.19***

**Table 2b.** Estimated Impact of RTC Laws—Using NRC Data—with Lott-Mustard Controls, All Crimes, 1977–2000<sup>a</sup>

<sup>a</sup>Estimations include year and county fixed effects, and are weighted by county population. Robust standard errors are provided beneath point estimates. The control variables (adopted from the Lott-Mustard model) include arrest rate, county population, population density, per capita income measures, and thirty-six demographic composition measures indicating the percentage of the population belonging to a race-age-gender group.

\*Significant at 10%; \*\*significant at 5%; \*\*\*significant at 1%.

**Table 2c.** Estimated Impact of RTC Laws—Using Updated 2009 County-Level Data—with Lott-Mustard Controls, All Crimes, 1977–2000 (without 1993 Data)<sup>a</sup>

	Murder (%)	Rape (%)	Aggravated Assault (%)	Robbery (%)	Auto Theft (%)	Burglary (%)	Larceny (%)
1. Dummy variable model	-3.80** 1.87**	9.82*** 2.74***	8.96*** 1.34***	5.44*** 1.45***	13.60*** 1.40***	4.36*** 0.95***	12.90*** 0.88***
2. Spline model	$-0.26 \\ 0.28$	0.48 0.33	1.10*** 0.18***	0.26 0.21	1.50*** 0.19***	0.30** 0.15**	1.16*** 0.14***
3. Hybrid model Post-passage dummy	-3.98* 2.22*	11.40*** 2.62***	6.34*** 1.48***	6.39*** 1.66***	10.60*** 1.57***	4.53*** 1.05***	11.80*** 0.94***
Trend effect	0.04 0.33	-0.38 0.30	0.63*** 0.20***	-0.23 0.25	0.70*** 0.22***	-0.04 0.16	0.28* 0.15*

<sup>a</sup>Estimations include year and county fixed effects, and are weighted by county population. Robust standard errors are provided beneath point estimates. The control variables (adopted from the Lott-Mustard model) include arrest rate, county population, population density, per capita income measures, and thirty-six demographic composition measures indicating the percentage of the population belonging to a race-age-gender group.

\*Significant at 10%; \*\*significant at 5%; \*\*\*significant at 1%.

### 4.3. Potential Problems with the NRC Models and Data

Before turning to the implications of the errors in the NRC estimates, we note a few small errors in the NRC data that we corrected in all our tables. First, we identified an extraneous demographic variable that caused a substantial number of observations to drop from the NRC data set (over 20,000).<sup>14</sup> We do not know if the committee dropped this variable before conducting its analysis, but we drop it in our own analysis.<sup>15</sup> Second, Philadelphia's year of adoption is coded incorrectly—as 1995 instead of 1996. Third, Idaho's year of adoption is coded incorrectly—as 1992 instead of 1991. Fourth, the area variable, which is used to compute county density, has missing data for years 1999 and 2000.<sup>16</sup>

The major differences in Table 2a (the NRC committee's estimates) and Table 2c (what we think is the best estimate of what the NRC intended to present) are profound enough that they might well have changed the nature of the report. Recall that Wilson had looked at the NRC's results (Table 2a) and decided that since the dummy and spline estimates were both consistent and statistically significant for only one crime—murder—these were the only estimates that should be accepted. But applying this same logic to the Table 2c estimates would lead to the drastically different conclusion that for four crimes—aggravated assault, auto theft, burglary, and larceny—Table 2c provides uniform evidence that

<sup>14.</sup> The variable is called "ppnpermpc." We stumbled into using this variable as we tried to incorporate Lott and Mustard's thirty-six demographic variables, which denote the percentage of each county's population that falls into each of six age-groups based on three racial categories for men and for women. Twelve of these variables begin with the prefix "ppn," which will then be included in the analysis if one uses a STATA command that groups together all variables with this common "ppn" prefix. For example, "ppnm2029" indicates the percentage of a county population that is male and neither white nor black. We do not know how the ppnpermpc variable fits into this grouping (or even if it is meant be a part of this group of variables). The mean value of this variable is -3.206657, with the individual observations ranging from -12.05915 to 4.859623. While the other ppn variables reflect some sort of percentage, the mean negative value obviously indicates that this variable is not a percentage.

<sup>15.</sup> We found that whether or not we include this variable, we cannot replicate the NRC's results (in Table 2a).

<sup>16.</sup> Because the NRC area numbers are the same for a county across all years, we fill in this gap by simply using the 1998 values for these two years. (However, we note that area should not be constant across all years, as the Census updates these data every decade.) We include complete, updated area data in our new data set.

RTC laws *increase* crime (while the evidence for the other crimes is mixed). One might go further and say that all the Table 2c dummy and spline estimates show crime *increases*, except for murder.

Although we speculate that Table 2c reflects where the NRC panel should have ended up if it had wanted to repeat Lott and Mustard's county data analysis, there is actually far more that the committee could have done to go beyond Table 2c to test the validity of the MGLC premise. We emphasize, though, that this is not necessarily a strong criticism of the NRC majority since it concluded (in our view, correctly) that the evidence was already too fragile to draw strong conclusions, and further support for this assessment would merely have been cumulative. Nevertheless, we now turn to some avenues of inquiry that Wilson might have considered before adopting the Lott and Mustard (1997) conclusion vis-à-vis murder.

# 5. Debate over the Clustering of Standard Errors

# 5.1. Is Clustering Necessary?

To this point we have said little about the important question of estimating the standard errors in panel-data regressions. The estimates presented thus far follow the NRC in providing heteroskedasticity-robust standard errors. Research has found, though, that the issue of whether to "cluster" the standard errors has a profound impact on assessments of statistical significance. This issue gained prominence beginning primarily with a 1990 article by Brent Moulton. Moulton (1990) pointed to the possible need for the clustering of observations when treatments are assigned at a group level. In such cases, there is an additive source of variation that is the same for all observations in the group, and ignoring this unique variation leads to standard errors are not needed (Lott, 2004), claiming that county-level fixed effects implicitly control for state-level effects, and therefore, clustering the standard errors on state is unnecessary.

On this point, the NRC committee (2005) sided with Lott, stating that "there is no need for adjustments for state-level clustering" (p. 138). However, we *strongly* believe the committee was mistaken in this decision. One must account for the possibility that county-level disturbances may be correlated within a state during a particular year by clustering the standard errors by state. There is also a second reason for clustering that the NRC report did not address. Specifically, serial correlation in panel data can lead to major underestimation of standard errors. Indeed, Bertrand, Duflo, and Mullainathan (2004) point out that even the Moulton correction alone may be insufficient for panel data estimators that utilize more than two periods of data due to autocorrelation in both the intervention variable and the outcome variable of interest. Wooldridge (2003, unpublished manuscript), as well as Angrist and Pischke (2009), suggest that clustering the standard errors by state (along with heteroskedasticity-robust standard errors) will help address this problem, and at least provide a lower bound on the standard errors.

# 5.2. Using Placebo Laws to Test the Impact of Clustering

Our reading of the influential literature on this issue suggests to us that clustering would make a major difference in the results generated by the Lott and Mustard models that the NRC report adopted in its analysis. But who is correct on the clustering issue—Lott, Mustard, and the NRC panel on the one hand, or Angrist, Pischke, and several other high-end applied econometricians on the other? To address this important question, we run a series of placebo tests. In essence, we randomly assign RTC laws to states, and reestimate our model iteratively (1,000 times), recording the number of times that the variable(s) of interest are "statistically significant." For this experiment, we use our most flexible model: the hybrid model (that incorporates both a dummy and a trend variable) with the controls employed by the NRC.

We run three versions of this test. First, we first generate a placebo law in a random year for all fifty states and the District of Columbia. Once the law is applied, it persists for the rest of our data period, which is how laws are coded in the original analysis. In our second test, we apply a placebo law in a random year to the thirty-two states that actually implemented RTC laws during the period we are analyzing. The remaining nineteen states assume no RTC law. Finally, we randomly select thirty-two states to receive a placebo law in a random year. The results of these three tests are presented in Table 3a.

Given the random assignment, one would expect to reject the null hypothesis of no effect of these randomized "laws" roughly 5% of the time if the standard errors in our regressions are estimated correctly. Instead, the table reveals that the null hypothesis is rejected 50–70% of the time for murder and robbery with the dummy variable and even more frequently with the trend variable (60–74%). Clearly, this exercise suggests that the standard errors used in the NRC report are far too small.

		Dummy Variable (%)	Trend Variable (%)
1. All 50 states + DC	Murder	50.2	67.4
	Robbery	56.7	65.6
2. Exact 32 states	Murder	64.2	71.9
	Robbery	59.8	67.2
3. Random 32 states	Murder	57.8	59.9
_	Robbery	70.6	74.2

**Table 3a.** Hybrid Model—Percentage of Significant Estimates (at the 5% Level)—Using Updated 2009 County-Level Data—Lott-Mustard Controls, without Clustered Standard Errors, 1977–2006 (without 1993 Data)<sup>a</sup>

<sup>a</sup>Simulation based on NRC with-controls model, which, similar to above estimations, includes year and county fixed effects, and weighting by county population. The control variables (adopted from the Lott-Mustard model) include arrest rate, county population, population density, per capita income measures, and thirty-six demographic composition measures indicating the percentage of the population belonging to a race-age-gender group.

**Table 3b.** Hybrid Model—Percentage of Significant Estimates (at the 5% Level)—Using Updated 2009 County-Level Data—Lott-Mustard Controls, with Clustered Standard Errors, 1977–2006 (without 1993 Data)<sup>a</sup>

		Dummy Variable (%)	Trend Variable (%)
1. All 50 states $+$ DC	Murder	8.9	11.5
	Robbery	8.1	8.1
2. Exact 32 states	Murder	10.0	11.0
	Robbery	9.2	7.1
3. Random 32 states	Murder	11.2	13.5
	Robbery	10.3	8.8

<sup>a</sup>Simulation based on NRC with-controls model, which, similar to above estimations, includes year and county fixed effects, and weighting by county population. The control variables (adopted from the Lott-Mustard model) include arrest rate, county population, population density, per capita income measures, and thirty-six demographic composition measures indicating the percentage of the population belonging to a race-age-gender group.

Table 3b replicates the exercise of Table 3a, but now uses the cluster correction for standard errors (on state). Table 3b suggests that clustering standard errors does not excessively reduce significance, as the NRC panel feared. In fact, the percentages of "significant" estimates produced in all three versions of the test still lie well beyond the 5% threshold. Similar results are found when we replicate Tables 3a and b while employing the dummy model instead of the hybrid model (we do not show those results here). All these tests show that if we do not cluster the standard errors, the likelihood of obtaining significant estimates is astonishingly (and unreasonably) high. The conclusion we draw from this exercise is that clustering is clearly needed to adjust the standard errors in these panel data regressions. Accordingly, we will use this clustering adjustment for all remaining regressions in this article.

#### 5.3. Does Clustering Influence the Results?

To get a sense of how clustering would have changed the NRC's estimates, we run the NRC model with standard errors clustered on state using our county-level data. Table 4 shows that clustering the standard errors in this model eliminates most of the statistical significance we saw in Table 2c (the same model but without clustering). Importantly, the significance of the negative coefficients for murder disappears. On this basis, one might suspect that had this set of results been used, the conclusions of the panel may have been quite different. These estimates—which we believe are now more accurate—provide no support for the claim that RTC laws reduce crime and, in fact, reveal evidence that aggravated assault, auto theft, and larceny all rise by between 9 and 14%. While this might suggest that RTC laws *increase* crime, the auto theft and larceny results do not readily comport with any plausible theory about the impact of RTC laws, and so we would proceed

**Table 4.** Estimated Impact of RTC Laws—Using Updated 2009 County-Level Data—with Lott-Mustard Controls, with Clustered Standard Errors, All Crimes, 1977–2000 (without 1993 Data)<sup>a</sup>

	Murder	Rape	Aggravated	Robbery	Auto	Burglary	Larceny
	(%)	(%)	Assault (%)	(%)	Theft (%)	(%)	(%)
1. Dummy variable	-3.80	9.82	8.96*	5.44	13.60**	4.36	12.90***
model	6.25	11.20	5.33*	5.53	5.83**	3.58	3.97***
<ol> <li>Spline model</li> <li>Hybrid model</li> </ol>	$-0.26 \\ 0.80$	0.48 1.22	1.10 0.81	0.26 0.85	1.50* 0.83*	0.30 0.50	1.16 0.82
Post-passage dummy	-3.98	11.40	6.34	6.39	10.60*	4.53	11.80***
	7.08	10.20	4.43	5.69	6.18*	3.92	2.95***
Trend effect	0.04 0.89	-0.38 0.86	0.63 0.76	-0.23 0.81	0.70 0.77	$-0.04 \\ 0.49$	0.28 0.65

<sup>a</sup>Estimations include year and county fixed effects, and are weighted by county population. Robust standard errors are provided beneath point estimates. The control variables (adopted from the Lott-Mustard model) include arrest rate, county population, population density, per capita income measures, and thirty-six demographic composition measures indicating the percentage of the population belonging to a race-age-gender group. \*Significant at 10%; \*\*significant at 5%; \*\*\*significant at 1%.

with caution in interpreting those results (even if we had more confidence in the Lott-Mustard model than we do given the concern over omitted variables).<sup>17</sup>

# 6. Debate over the Inclusion of Linear Trends

An important issue that the NRC did not address was whether there was any need to control for state-specific linear trends. Inclusion of state trends could be important if, for example, a clear pattern in crime rates existed before a state adopted an RTC law that continued into the post-passage period. In contrast, there is also a potential danger in using state-specific trends if their inclusion inappropriately extrapolates a temporary swing in crime long into the future. Lott and Mustard (1997) never controlled for state-specific trends in analyzing handgun laws, while Moody and Marvell (2008) always controlled for these trends. Ayres and Donohue (2003b) presented evidence with and without such trends.

Table 5 replicates the NRC's full model (with the appropriate clustering adjustment) from Table 4 while adding linear state trends to this county-data model. Strikingly, Table 5 suggests that RTC laws increase aggravated assault by roughly 3% each year, but no other statistically significant effect is observed. Thus, the addition of state trends eliminates the potentially problematic result of RTC laws increasing property crimes, which actually increases our confidence in these results. Certainly, an increase in gun carrying and prevalence induced by an RTC law could well be thought to spur more aggravated assaults. Nonetheless, one must at least consider whether the solitary finding of statistical significance is merely the product of running seven different models, is a spurious effect flowing from a bad model, or reflects some other anomaly (such as changes in the police treatment of

<sup>17.</sup> Lott and Mustard offered a crime substitution theory based on a view that if RTC laws reduced robbery (because criminals feared encountering armed victims), the criminals might turn to property crimes that were less likely to result in armed resistance. Note, though, that Table 4 gives no support for a robbery reduction effect, so the premise of the crime substitution story is not supported.

					·		
	Murder	Rape	Aggravated	Robbery	Auto	Burglary	Larceny
	(%)	(%)	Assault (%)	(%)	Theft (%)	(%)	(%)
1. Dummy variable	-6.17	-10.80	3.00	-5.31	0.21	-5.19	-0.40
model	5.31	8.27	3.60	5.66	5.85	3.55	3.04
2. Spline model	-1.21	-2.64	3.02**	-0.06	0.82	0.00	1.18
	1.46	3.48	1.23**	2.26	1.27	1.29	1.12
<ol> <li>Hybrid model</li> <li>Post-passage</li> <li>dummy</li> </ol>	-5.14	-8.28	-0.64	-5.69	-0.83	-5.63	-1.95
	5.07	5.65	3.79	6.28	5.99	3.95	3.25
Trend effect	-0.87	-2.09	3.06**	0.32	0.88	0.38	1.31
	1.43	3.28	1.29**	2.42	1.30	1.40	1.19

 
 Table 5. Estimated Impact of RTC Laws—Using Updated 2009 County-Level
 Data-with Lott-Mustard Controls, with Clustered Standard Errors and State Trends, All Crimes, 1977–2000 (without 1993 Data)<sup>a</sup>

<sup>a</sup>Estimations include year and county fixed effects, and are weighted by county population. Robust standard errors are provided beneath point estimates. The control variables (adopted from the Lott-Mustard model) include arrest rate, county population, population density, per capita income measures, and thirty-six demographic composition measures indicating the percentage of the population belonging to a race-age-gender group. \*Significant at 10%; \*\*significant at 5%; \*\*\*significant at 1%.

domestic violence cases, which could confound the aggravated assault results).<sup>18</sup>

# 7. Extending the Data through 2006

Thus far, we have presented panel data regression results for the period 1977– 2000. Since more data are now available, we can further test the strength of the MGLC premise over time by estimating the NRC Lott-Mustard covariates specification on data extended through 2006. Table 6a presents our estimates (with clustering), which can be compared with Table 4 (which also clusters the standard errors in the main NRC model, but is estimated on the shorter time period).

<sup>18.</sup> We tested this theory by creating a new right-hand side dummy variable that identified if a state passed legislation requiring law enforcement officials to submit official reports of all investigated domestic violence cases. Eight states have passed this legislation of which we are aware: Florida (1984), Illinois (1986), Louisiana (1985), New Jersey (1991), North Dakota (1989), Oklahoma (1986), Tennessee (1995), and Washington (1979). We included this dummy variable when running both the NRC specification (through 2000) and our preferred specification (through 2006), and found that this dummy indicator of domestic violence reporting statutes did not undermine the finding that RTC laws increase aggravated assaults.

**Table 6a.** Estimated Impact of RTC Laws—Using Updated 2009 County-Level Data—with Lott-Mustard Controls, with Clustered Standard Errors, All Crimes, 1977–2006 (without 1993 Data)<sup>a</sup>

	Murder (%)	Rape (%)	Aggravated Assault (%)	Robbery (%)	Auto Theft (%)	Burglary (%)	Larceny (%)
1. Dummy variable model	-5.44 5.91	10.40 13.20	11.40** 4.84**	3.10 4.47	14.40** 6.65**	7.48* 3.85*	12.90*** 3.96***
2. Spline model	$-0.28 \\ 0.60$	0.61 1.03	1.05 0.69	0.39 0.54	0.99 0.61	0.44 0.43	1.07** 0.51**
3. Hybrid model							
Post-passage	-5.35	9.77	8.39**	1.69	12.60**	6.99*	10.10***
dummy	6.05	12.00	3.48**	5.43	5.91**	3.99*	3.68***
Trend effect	-0.02 0.61	0.14 0.74	0.65 0.63	0.30 0.65	0.39 0.47	0.10 0.44	0.59 0.49

<sup>a</sup>Estimations include year and county fixed effects, and are weighted by county population. Robust standard errors are provided beneath point estimates. The control variables (adopted from the Lott-Mustard model) include arrest rate, county population, population density, per capita income measures, and thirty-six demographic composition measures indicating the percentage of the population belonging to a race-age-gender group. \*Significant at 10%; \*\*significant at 5%; \*\*\*significant at 1%.

**Table 6b.** Estimated Impact of RTC Laws—Using Updated 2009 County-Level Data—with Lott-Mustard Controls, with Clustered Standard Errors and State Trends, All Crimes, 1977–2006 (without 1993 Data)<sup>a</sup>

	Murder	Rape	Aggravated	Robbery	Auto	Burglary	Larceny
	(%)	(%)	Assault (%)	(%)	Theft (%)	(%)	(%)
1. Dummy variable	-4.45	-13.00	3.44	-0.22	3.81	-0.77	1.51
model	4.44	8.14	3.13	5.48	4.84	3.53	3.10
2. Spline model	-0.96 0.96	-4.51 3.74	1.72* 0.94*	$-0.95 \\ 1.60$	-0.91 1.10	-0.82 1.04	$-0.66 \\ 0.87$
<ol> <li>Hybrid model</li> <li>Post-passage</li> <li>dummy</li> </ol>	-3.98	-10.70	2.53	0.31	4.36	-0.32	1.89
	4.55	7.01	3.09	5.55	4.67	3.64	3.08
Trend effect	$-0.86 \\ 0.98$	-4.26 3.69	1.66* 0.93*	-0.96 1.62	$-1.01 \\ 1.08$	-0.82 1.07	$-0.70 \\ 0.89$

<sup>a</sup>Estimations include year and county fixed effects, and are weighted by county population. Robust standard errors are provided beneath point estimates. The control variables (adopted from the Lott-Mustard model) include arrest rate, county population, population density, per capita income measures, and thirty-six demographic composition measures indicating the percentage of the population belonging to a race-age-gender group. \*Significant at 10%; \*\*significant at 5%; \*\*\*significant at 1%.

This comparison reveals that the additional six years of data somewhat strengthen the evidence that RTC laws *increase* aggravated assault, auto theft, burglary, and larceny. Table 6b simply adds state trends to the Table 6a models, which can then be compared to Table 5 (clustering, state trends, and 1977–2000 data). Collectively, these results suggest that the added six years of data do not appreciably change the results from the shorter period. The inclusion of state trends on the longer data set renders all estimates insignificant except for the evidence of marginally significant *increases* in aggravated assault.

#### 8. Revising the Lott-Mustard Specification

We have already suggested that the Lott-Mustard specification that the NRC employed is not particularly appealing along a number of dimensions. The most obvious problem—omitted variable bias—has already been alluded to: the Lott and Mustard (1997) model had no control for incarceration, which Wilson considered to be one of the most important influences on crime in the last twenty years. In addition to a number of important omitted variables, the Lott-Mustard model adopted by the NRC includes a number of questionable variables, such as the highly dubious ratio of arrests to murders, and the thirty-six (highly collinear) demographic controls.<sup>19</sup>

To explore whether these specification problems are influencing the regression estimates, we revise the NRC models in a number of ways. First, we drop the flawed contemporaneous arrest rate variable and add in two preferable measures of state law enforcement/deterrence: the incarceration rate and the rate of police.<sup>20</sup> Second, we add two additional controls to capture economic conditions: the unemployment rate and the poverty rate, which are also state-level variables. Finally, mindful of Horowitz's admonition that the Lott-Mustard model might have *too many* variables (including demographic controls that are arguably irrelevant to the relationship between the guns and crime, and may have a spurious, misleading effect), we decided not to follow the NRC in using the thirty-six demographic controls employed by Lott-Mustard. Instead, we adhered to the more customary practice in the econometrics of crime and controlled only for the demographic groups considered to be most

<sup>19.</sup> For extended discussion on the abundant problems with this pseudo arrest rate, see Donohue and Wolfers (forthcoming).

<sup>20.</sup> We also estimated the model with the arrest rate (lagged by one year to avoid endogeneity concerns), and the results were qualitatively similar.

involved with criminality (as offenders and victims), namely the percentage of black and white males between ages ten and thirty years in each county.<sup>21</sup>

The results with this new specification are presented in Tables 7a and b (which correspond to Tables 6a and b estimated using the Lott-Mustard specification). In particular, one sees a strong adverse shift for murder. Note that had the NRC panel used our preferred specification while maintaining its view that neither clustering nor controls for state trends are needed, then we would have overwhelming evidence that RTC laws increase crime across every crime category. We do not show these regression results since we are convinced that clustering is needed, although of course when we cluster in Table 7a, the point estimates remain the same (while significance is drastically reduced).

It would indeed be a troubling state of the world if the NRC view on clustering (and linear trends) were correct, for in that event, RTC laws would increase every crime category other than murder by 20–40% (the dummy model) or increase it by 2–4% every year (the spline model)—all at the 0.01 level.<sup>22</sup> In fact, the version of Table 7a in which the standard errors are not adjusted by clustering generates a finding that RTC laws increase murder at the 0.10 level in the spline model and at the 0.05 level in the trend term of the hybrid model. When we do cluster, however, as shown in Table 7a, we are left with large positive point estimates but far fewer significant results: Nonetheless, this more reasonable specification suggests that RTC laws increase aggravated assault, robbery, and larceny. Interestingly, adding state trends in Table 7b wipes out all statistical significance.

This discussion again highlights how critical the choices of clustering and state trends are to an assessment of RTC laws. Using neither, the data suggest these laws are harmful. With only clustering, RTC laws show (marginally significant) signs of increases for two violent crime categories as well as for larceny. In our preferred specification (without state trends), the effect of RTC laws on murder seems to basically be zero. With both clustering

<sup>21.</sup> To test the robustness of this specification to alternations in the demographic controls used, we also estimated the following models: Only black men between ages ten and forty years; black, white, and Hispanic men between ages ten and forty years; only black men between ages ten and thirty years; black and white men between ages ten and thirty years; and black, white, and Hispanic men between ages ten and forty years. The results were again qualitatively similar across our tests.

<sup>22.</sup> These results are not presented here since standard errors clustered on state are clearly needed. The authors can provide these results upon request.

	Murder	Rape	Aggravated	Robbery	Auto	Burglary	Larceny
	(%)	(%)	Assault (%)	(%)	Theft (%)	(%)	(%)
1. Dummy variable	-0.44	21.30	21.60	19.30	24.80	26.60	29.50
model	7.13	19.40	19.00	14.50	21.10	22.40	26.00
2. Spline model	0.31	2.34	3.16	2.64*	3.12	3.59	4.20
	0.79	1.83	1.89	1.46*	2.11	2.27	2.61
3. Hybrid model							
Post-passage	-2.72	12.60	7.40	7.92	12.00	11.10	10.90
dummy	6.96	15.40	15.80	12.10	16.80	18.20	20.50
Trend effect	0.45	1.70	2.78*	2.24*	2.51	3.03	3.64*
	0.81	1.39	1.62*	1.27*	1.74	1.94	2.15*

**Table 7a.** Estimated Impact of RTC Laws—Using Updated 2009 County-Level Data—with Preferred Controls, with Clustered Standard Errors, All Crimes, 1977–2006 (without 1993 Data)<sup>a</sup>

<sup>a</sup>Estimations include year and county fixed effects, and are weighted by county population. Robust standard errors are provided beneath point estimates. The control variables for this "preferred" specification include incarceration and police rates (lagged one year to avoid potential endogeneity issues), unemployment rate, poverty rate, county population, population density, per capita income, and six demographic composition measures. \*Significant at 10%; \*\*\*significant at 5%; \*\*\*significant at 1%.

Data-with Preferr	ed Con	trols, w	ith Clustere	d Standa	rd Errors a	and State	Trends,
All Crimes, 1977–2006 (without 1993 Data) <sup>a</sup>							
Ν	Iurder	Rape	Aggravated	Robbery	Auto	Burglary	Larceny
	(07)	(07)	A coult $(07)$	(07)	That $(07)$	(07)	(07)

Table 7b. Estimated Impact of RTC Laws—Using Updated 2009 County-Level

	Murder	Rape	Aggravated	Robbery	Auto	Burglary	Larceny
	(%)	(%)	Assault (%)	(%)	Theft (%)	(%)	(%)
1. Dummy variable	-3.11	-15.50	0.02	1.15	1.89	-3.98	-3.22
model	4.81	10.80	9.70	7.25	9.89	10.90	12.50
2. Spline model	-0.41	-6.69	0.61	-0.82	-0.97	-1.92	-2.25
	1.31	4.77	2.44	2.28	2.66	2.83	3.15
3. Hybrid model Post-passage dummy	-2.97 5.08	-13.00 9.98	-0.22 10.30	1.48 7.64	2.29 10.40	-3.25 11.50	-2.35 13.10
Trend effect	-0.35	-6.46	0.61	-0.85	-1.01	-1.87	-2.21
	1.35	4.76	2.54	2.35	2.76	2.96	3.29

<sup>a</sup>Estimations include year and county fixed effects, and are weighted by county population. Robust standard errors are provided beneath point estimates. The control variables for this "preferred" specification include incarceration and police rates (lagged one year to avoid potential endogeneity issues), unemployment rate, poverty rate, county population, population density, per capita income, and six demographic composition measures. \*Significant at 10%; \*\*significant at 5%; \*\*\*significant at 1%.

and state trends, all statistically significant effects are wiped out. The only conclusion from both the NRC/Lott-Mustard model and our preferred specification (on county data) is that there is no robust evidence that RTC laws provide any net benefits, and there is a greater likelihood that RTC laws may cause either some or a great deal of harm.

#### 9. State versus County Crime Data

In their initial study, Lott and Mustard (1997) tested the MGLC hypothesis by relying primarily on county-level data from the FBI's UCR.<sup>23</sup> These FBI reports present yearly estimates of crime based on monthly crime data from local and state law enforcement agencies across the country. The NRC report followed Lott and Mustard in this choice and presented regression estimates using only county data. Unfortunately, according to criminal justice researcher Michael Maltz, the FBI's county-level data are highly problematic.

The major problem with county data stems from the fact that law enforcement agencies voluntarily submit crime data to the FBI. As a result, the FBI has little control over the accuracy, consistency, timeliness, and completeness of the data it uses to compile the UCR reports. In a study published in the *Journal of Quantitative Criminology*, Maltz and Targonski (2002) carefully analyzed the short-comings in the UCR data set and concluded that UCR county-level data are unacceptable for evaluating the impact of RTC laws. For example, in Connecticut, Indiana, and Mississippi, over 50% of the county-level data points are missing crime data for more than 30% of their populations (Maltz and Targonski, 2002). In another thirteen states, more than 20% of the data points have gaps of similar magnitude. Based on their analysis, Maltz and Targonski (2002) concluded that:

County-level crime data cannot be used with any degree of confidence .... The crime rates of a great many counties have been underestimated, due to the exclusion of large fractions of their populations from contributing to the crime counts. Moreover, counties in those states with the most coverage gaps have laws permitting the carrying of concealed weapons. How these shortcomings can be compensated for is still an open question ... it is clear, however, that in their current condition, county-level UCR crime statistics cannot be used for evaluating the effects of changes in policy. (p. 316–17)

Because of the concerns raised about county-level crime data, it is prudent to test our models on state-level data. According to Maltz and Targonski (2003), state-level crime data are less problematic than county-level data because the

<sup>23.</sup> Lott and Mustard present results based on state-level data, but they strongly endorse their county-level over their state-level analysis: "the very different results between state- and county-level data should make us very cautious in aggregating crime data and would imply that the data should remain as disaggregated as possible" (Lott and Mustard, 1997, p. 39).

FBI's state-level crime files take into account missing data by imputing all missing agency data. County-level files provided by National Archive of Criminal Justice Data, however, impute missing data only if an agency provides at least six months of data; otherwise, the agency is dropped completely (Maltz, 2006). As with our estimations using county-level data, we compiled our state-level data from scratch, and will refer to it as "Updated 2009 State-Level Data."

Unsurprisingly, the regression results reproduced using state-level data are again different from the NRC committee's estimates using county-level data. This is shown in Table 8a, which presents the results from the NRC's specification (the Lott-Mustard model) on state data, with the cluster adjustment.<sup>24</sup> Table 8b simply adds state trends. When we compare these state-level estimates to the county-level estimates (using the updated 2009 county-level data set), we see that there are marked differences. Considering the preceding discussion on the reliability—or lack thereof—of county data, this result is unsurprising. Importantly, state-level data through 2006 show not a hint of statistically significant evidence that RTC laws reduce murder.<sup>25</sup> None of the state results is robust to the addition or exclusion of state linear trends.

Tables 9a and b below repeat Tables 8a and b, but use the model with our preferred set of explanatory variables instead of the Lott and Mustard (1997) model. The main question raised by these estimations is whether state trends are needed in the regression models. If not, there is evidence that RTC laws increase assault and larceny. If state trends are needed, some muddiness returns but RTC laws appear to increase aggravated assault, while declines in rape are marginally significant.

# **10.** Additional Concerns in the Evaluation of Legislation Using Observational Data

We now turn to three critical issues that must be considered when using panel data to evaluate the impact of legislation and public policy (and gun

<sup>24.</sup> Our placebo test on county data showed that standard errors needed to be adjusted by clustering. In Appendix A, we again find that clustering is needed for state data. Thus, all our state-level estimates include clustering.

<sup>25.</sup> We also estimate the model on data through 2000 (the last year in the NRC report), though those results are not shown here. The results similarly do show not any statistically significant evidence that RTC laws reduce murder. Moreover, we also estimate the NRC's no-controls model on the state-level data. See Appendix B for these results.

	Murder (%)	Rape (%)	Aggravated Assault (%)	Robbery (%)	Auto Theft (%)	Burglary (%)	Larceny (%)
1. Dummy variable model	-4.94 3.61	-5.04** 2.29**	1.44 4.11	-6.96** 2.90**	0.31 3.98	-4.97** 2.22**	2.32 1.58
2. Spline model	$-0.03 \\ 0.54$	-0.49 0.33	0.80 0.66	$-0.16 \\ 0.60$	$-0.87^{**}$ $0.42^{**}$	$-0.44 \\ 0.45$	0.40 0.29
3. Hybrid model Post-passage dummy	-5.62 4.25	-3.77 2.36	-1.69 3.26	-7.41** 3.59**	4.00 4.88	-3.92* 2.03*	1.03 1.80
Trend effect	0.19 0.58	$-0.35 \\ 0.36$	0.86 0.64	0.12 0.64	-1.02** 0.50**	-0.29 0.46	0.36 0.32

**Table 8a.** Estimated Impact of RTC Laws—Using Updated 2009 State-Level Data—with Lott-Mustard Controls, with Clustered Standard Errors, All Crimes, 1977—2006<sup>a</sup>

<sup>a</sup>Estimations include year and county fixed effects, and are weighted by county population. Robust standard errors are provided beneath point estimates. The control variables (adopted from the Lott-Mustard model) include arrest rate, county population, population density, per capita income measures, and thirty-six demographic composition measures indicating the percentage of the population belonging to a race-age-gender group. \*Significant at 10%; \*\*significant at 5%; \*\*\*significant at 1%.

**Table 8b.** Estimated Impact of RTC Laws—Using Updated 2009 State-Level Data—with Lott-Mustard Controls, with Clustered Standard Errors and State Trends, All Crimes, 1977–2006<sup>a</sup>

	Murder	Rape	Aggravated	Robbery	Auto	Burglary	Larceny
	(%)	(%)	Assault (%)	(%)	Theft (%)	(%)	(%)
1. Dummy variable	-3.32	-3.33	-1.12	-3.36	2.64	-1.93	1.21
model	3.47	2.20	2.78	3.04	2.71	1.37	1.07
2. Spline model	0.42	0.34	2.49***	0.46	-1.95***	0.35	0.39
	0.82	0.88	0.61***	1.00	0.72***	0.79	0.60
3. Hybrid model Post-passage	-3.83	-3.78	-3.33	-3.90	4.51	-2.33	0.92
dummy	3.58	2.42	2.84	3.10	2.85	1.62	1.28
Trend effect	0.61	0.54	2.67***	0.66	-2.19***	0.47	0.35
	0.81	0.92	0.63***	1.00	0.77***	0.83	0.64

<sup>a</sup>Estimations include year and county fixed effects, and are weighted by county population. Robust standard errors are provided beneath point estimates. The control variables (adopted from the Lott-Mustard model) include arrest rate, county population, population density, per capita income measures, and thirty-six demographic composition measures indicating the percentage of the population belonging to a race-age-gender group. \*Significant at 10%; \*\*significant at 5%; \*\*\*significant at 1%.

	Murder	Rape	Aggravated	Robbery	Auto	Burglary	Larceny
	(%)	(%)	Assault (%)	(%)	Theft (%)	(%)	(%)
1. Dummy variable	-2.93	-0.62	5.05	5.36	7.03	2.24	6.72**
model	3.94	3.76	3.71	4.28	6.05	3.00	2.98**
2. Spline model	-0.16	-0.44	1.09*	0.64	0.45	0.00	0.57
	0.61	0.54	0.60*	0.75	0.62	0.39	0.46
3. Hybrid model Post-passage dummy	-2.75 3.75	1.71 3.52	0.15 3.56	3.09 4.74	6.29 5.49	2.82 3.21	5.22* 3.05*
Trend effect	$-0.04 \\ 0.63$	$-0.52 \\ 0.56$	1.09* 0.63*	0.50 0.83	0.17 0.56	-0.13 0.43	0.34 0.50

Table 9a. Estimated Impact of RTC Laws-Using Updated 2009 State-Level Data -with Preferred Controls, with Clustered Standard Errors, All Crimes, 1977–2006<sup>a</sup>

<sup>a</sup>Estimations include year and county fixed effects, and are weighted by county population. Robust standard errors are provided beneath point estimates. The control variables for this "preferred" specification include incarceration and police rates (lagged one year to avoid potential endogeneity issues), unemployment rate, poverty rate, county population, population density, per capita income, and six demographic composition measures. \*Significant at 10%; \*\*significant at 5%; \*\*\*significant at 1%.

	Murder (%)	Rape (%)	Aggravated Assault (%)	Robbery (%)	Auto Theft (%)	Burglary (%)	Larceny (%)
1. Dummy variable model	0.54 2.72	-3.61* 1.83*	-2.03 3.05	2.40 3.67	8.17* 4.16*	1.51 2.18	1.89 1.83
2. Spline model	0.83 0.87	0.08 0.79	3.10** 0.81**	0.51 1.29	$-1.84^{**}$ $0.82^{**}$	$-0.22 \\ 0.88$	$-0.15 \\ 0.74$
3. Hybrid model Post-passage dummy	0.11 2.86	-3.70* 1.96*	-3.68 3.15	2.17 3.96	9.26** 4.24**	1.65 2.41	1.99 1.97
Trend effect	0.83 0.89	0.19 0.79	3.21*** 0.82***	0.44 1.35	$-2.11^{**}$ $0.84^{**}$	$-0.27 \\ 0.91$	$-0.20 \\ 0.77$

Table 9b. Estimated Impact of RTC Laws—Using Updated 2009 State-Level Data-with Preferred Controls, with Clustered Standard Errors and State Trends, All Crimes, 1977–2006<sup>a</sup>

<sup>a</sup>Estimations include year and county fixed effects, and are weighted by county population. Robust standard errors are provided beneath point estimates. The control variables for this "preferred" specification include incarceration and police rates (lagged one year to avoid potential endogeneity issues), unemployment rate, poverty rate, county population, population density, per capita income, and six demographic composition measures.

\*Significant at 10%; \*\*significant at 5%; \*\*\*significant at 1%.

laws in particular). First, we discuss the possibility of difficult-to-measure omitted variables, and how such variables can shape estimates of policy impact. We are particularly concerned with how the crack epidemic of the 1980s and 1990s may bias results in the direction of finding a beneficial effect. Second, we explore pre-adoption crime trends in an attempt to examine the plausibly endogenous adoption of RTC legislation. Finally, given that the intent of right-to-carry legislation is to increase gun-carrying in law-adopting states, we explore whether these laws may have had a particular effect on gun-related assaults (which is the one crime category that has generated somewhat consistent results thus far).

# 10.1. Further Thoughts on Omitted Variable Bias

As discussed above, we believe it is likely that the NRC's estimates of the effects of RTC legislation are marred by omitted variable bias. In our attempt to improve (at least to a degree) on the original Lott-Mustard model, we included additional explanatory factors, such as the incarceration and police rates, and removed extraneous variables (such as unnecessary and collinear demographic measures). We recognize, however, that there are additional criminogenic influences for which we cannot fully control. In particular, we suspect that a major shortcoming of all the models presented is the inability to account for the possible influence of the crack cocaine epidemic on crime.<sup>26</sup>

<sup>26.</sup> Although Lott and Mustard (1997) do make a modest attempt to control for the potential influence of crack cocaine through the use of cocaine price data based on the U.S. Drug Enforcement Administration's STRIDE data, we find their approach wanting for both theoretical and empirical reasons. First, a control for crack should capture the criminogenic influence of the crack trade on crime. We know that prior to 1985, there was no such influence in any state and that after some point in the early to mid-1990s this criminogenic influence declined strongly. Since there is little reason to believe that cocaine prices would be informative on the criminogenic influence of crack in particular geographic areas, it is hard to see how the cocaine price data could be a useful control. Second, the data that Lott and Mustard use are themselves questionable. Horowitz (2001) argues forcefully that STRIDE data are not a reliable source of data for policy analyses of cocaine. The data are mainly records of acquisitions made to support criminal investigations in particular cities, and are not a random sample of an identifiable population. Moreover, since the STRIDE data are at the city level, we are not sure how this would be used in a county-level analysis. The data were collected for twenty-one cities, while there are over three thousand counties in the United States. In addition, the data are missing for 1988 and 1989, which are crucial years in the rise of the crack epidemic in poor urban areas. Lott and Mustard drop those years of analysis when including cocaine prices as a control.

Many scholars now suggest that rapid growth in the market for crack cocaine in the late 1980s and the early 1990s was likely one of the major influences on increasing crime rates (and violent crimes in particular) during this period (Levitt, 2004). Moreover, the harmful criminogenic effect of crack was likely more acute in urban areas of states slow to adopt RTC laws. Meanwhile, many rural states adopted such laws during this era. If this was indeed the case, this divergence between states could account for much of the purported "crime-reducing" effects attributed by Lott and Mustard to gun laws (which were then supported by scholars such as James Q. Wilson). The regression analysis would then identify a relationship between rising crime and the failure to adopt RTC legislation, when the actual reason for this trend was the influence of crack (rather than the passage of the RTC law).

We now explore how results from our main models vary when we restrict the analysis to the time periods before and after the peak of the American crack epidemic. According to Fryer et al. (2005), the crack problem throughout most of the country peaked at some point in the early 1990s. Coincidentally, the original Lott-Mustard period of analysis (1977–1992) contains years that likely represent the height of crack-induced crime problem. With this in mind, we run our main regressions after breaking up our data set into two periods: the original Lott-Mustard period of analysis (1977–1992) and the post–Lott-Mustard period (1993–2006). We first present the results for the era that includes the crack epidemic (1977–1992) on our preferred model. We run these regressions (with clustered standard errors) on state-level data, with and without state trends. These results are presented in Tables 10a and b. We then estimate the same models on the post-crack period (see Tables 11a and b).

Note that the regression results in Table 10 from the initial Lott-Mustard sixteen-year time period (1977–1992) do suggest that rape, robbery, and aggravated assault are dampened by RTC laws if state trends are not needed and that murder may have declined if state trends are needed. If we look at the following fourteen-year period from 1993 to 2006 in Table 11, however, the conclusion flips around: Now, there is evidence that all four violent crimes *rose* when states adopted RTC laws. This evidence supports the theory that the Lott-Mustard finding was likely the result of the crime-raising impact of crack in non-RTC states.

Figure 8 depicts a measure of crack prevalence for the period 1980–2000 in the five states with the greatest crack problem as well as the five states with the

	Murder (%)	Rape (%)	Aggravated Assault (%)	Robbery (%)	Auto Theft (%)	Burglary (%)	Larceny (%)
1. Dummy variable model	-3.69 3.81	-12.10*** 3.41***	-6.55 4.66	-4.85 4.07	7.28 4.73	-3.73 2.45	0.12 1.52
2. Spline model	-0.88 1.44	$-2.87^{***}$ $0.80^{***}$	0.52 1.70	-2.28*** 0.72***	0.51 1.13	$-0.34 \\ 0.83$	$-0.10 \\ 0.33$
3. Hybrid model Post-passage dummy	-2.32 4.70	-7.59** 3.01**	-11.80** 5.64**	1.08 5.32	9.07* 4.61*	-4.37 3.87	0.54 1.82
Trend effect	-0.56 1.67	-1.83*** 0.59***	2.13 1.47	-2.42** 1.08**	$-0.73 \\ 0.85$	0.26 0.97	$-0.17 \\ 0.42$

**Table 10a.** Estimated Impact of RTC Laws—Using Updated 2009 State-Level Data—with Preferred Controls, with Clustered Standard Errors, All Crimes, 1977–1992<sup>a</sup>

<sup>a</sup>Estimations include year and county fixed effects, and are weighted by county population. Robust standard errors are provided beneath point estimates. The control variables for this "preferred" specification include incarceration and police rates (lagged one year to avoid potential endogeneity issues), unemployment rate, poverty rate, county population, population density, per capita income, and six demographic composition measures. \*Significant at 10%; \*\*significant at 5%; \*\*\*significant at 1%.

**Table 10b.** Estimated Impact of RTC Laws—Using Updated 2009 State-Level Data—with Preferred Controls, with State Trends and Clustered Standard Errors, All Crimes, 1977–1992<sup>a</sup>

	Murder	Rape	Aggravated	Robbery	Auto	Burglary	Larceny
	(%)	(%)	Assault (%)	(%)	Theft (%)	(%)	(%)
1. Dummy variable	-5.61	-4.14	-2.02	-3.78	-0.04	-3.05	1.28
model	3.57	3.61	3.70	4.25	3.84	2.23	1.96
2. Spline model	-5.41** 2.45**	0.27 1.11	$-0.05 \\ 1.17$	-4.35* 2.48*	-1.62 2.20	-2.36 1.43	0.37 1.15
3. Hybrid model							
Post-passage	2.47	-6.67*	-2.89	3.08	3.17	0.18	1.16
dummy	4.31	3.52*	5.10	6.91	4.98	4.26	2.02
Trend effect	-6.01**	1.88	0.65	-5.10	-2.38	-2.41	0.09
	2.51**	1.18	1.84	3.30	2.64	2.11	1.26

<sup>a</sup>Estimations include year and county fixed effects, and are weighted by county population. Robust standard errors are provided beneath point estimates. The control variables for this "preferred" specification include incarceration and police rates (lagged one year to avoid potential endogeneity issues), unemployment rate, poverty rate, county population, population density, per capita income, and six demographic composition measures. \*Significant at 10%; \*\*\*significant at 5%; \*\*\*significant at 1%.

	Murder	Rape	Aggravated	Robbery	Auto	Burglary	Larceny
	(%)	(%)	Assault (%)	(%)	Theft (%)	(%)	(%)
1. Dummy variable	3.12	-3.47	1.36	3.64	2.46	3.58	0.27
model	3.61	2.47	3.54	4.89	4.50	2.57	2.74
2. Spline model	1.11* 0.63*	-0.21 0.68	1.91** 0.74**	1.78** 0.87**	$-0.30 \\ 0.80$	0.35 0.71	0.08 0.55
3. Hybrid model Post-passage dummy	2.36 3.82	-3.35 2.46	0.03 4.05	2.42 4.73	2.70 4.33	3.37 2.57	0.22 2.76
Trend effect	1.09*	-0.17	1.91**	1.75**	-0.34	0.31	0.08
	0.64*	0.67	0.76**	0.87**	0.77	0.70	0.55

 
 Table 11a. Estimated Impact of RTC Laws—Using Updated 2009 State-Level
 Data-with Preferred Controls, with Clustered Standard Errors, All Crimes, 1993-2006<sup>a</sup>

<sup>a</sup>Estimations include year and county fixed effects, and are weighted by county population. Robust standard errors are provided beneath point estimates. The control variables for this "preferred" specification include incarceration and police rates (lagged one year to avoid potential endogeneity issues), unemployment rate, poverty rate, county population, population density, per capita income, and six demographic composition measures. \*Significant at 10%; \*\*significant at 5%; \*\*\*significant at 1%.

Table 11b. Estimated Impact of RTC Laws—Using Updated 2009 State-Level Data—with Preferred Controls, with State Trends and Clustered Standard Errors, All Crimes, 1993–2006<sup>a</sup>

	Murder	Rape	Aggravated	Robbery	Auto	Burglary	Larceny
	(%)	(%)	Assault (%)	(%)	Theft (%)	(%)	(%)
1. Dummy variable	3.12	0.27	2.38	3.81	2.83	0.89	0.33
model	3.62	2.66	2.59	3.33	3.39	2.19	1.83
2. Spline model	-1.99	2.61**	4.34***	-0.17	-5.53*	-0.71	-1.49
	2.00	1.16**	1.53***	1.89	2.77*	1.74	1.31
3. Hybrid model Post-passage dummy	4.04 3.87	-0.75 2.46	0.79 2.40	4.04 3.48	5.12 3.43	1.20 2.29	0.93 1.98
Trend effect	-2.44 2.10	2.69** 1.14**	4.25** 1.61**	-0.62 1.95	-6.10** 2.99**	$-0.84 \\ 1.80$	-1.59 1.42

<sup>a</sup>Estimations include year and county fixed effects, and are weighted by county population. Robust standard errors are provided beneath point estimates. The control variables for this "preferred" specification include incarceration and police rates (lagged one year to avoid potential endogeneity issues), unemployment rate, poverty rate, county population, population density, per capita income, and six demographic composition measures. \*Significant at 10%; \*\*significant at 5%; \*\*\*significant at 1%.



Source: Authors' calculations based on the crack index of Fryer et al. (2005).



least crack, according to Fryer et al. (2005). Figure 9 shows the murder rates over time for these two sets of states. We see that crime rose in the high-crack states when the crack index rises in the mid-to-late 1980s, but that the crack index does not turn down in those states at the time crime started to fall.





Figure 9. Murder Rates in the Five Most and the Five Least Crack-Affected States.

Apparently, the rise of the crack market triggered a great deal of violence but once the market stabilized, the same level of crack consumption could be maintained while the violence ebbed.

Of course, omitting an appropriate control for the criminogenic influence of crack is problematic if the high-crack states tend not to adopt RTC laws and the low-crack states tend to adopt. This is in fact the case: All the five "high-crack" states are non-RTC states during this period, whereas four of the five "low-crack" states are RTC states (all four adopted an RTC law by 1994).<sup>27</sup> The only exception is Nebraska, a state that did not adopt an RTC law until 2007, which is outside the scope of our current analyses.<sup>28</sup>

	Year of RTC		
State	Law Adoption	Murder Rate	Crack Index
Indiana	1980	6.53	0.17
Maine	1985	2.53	-0.04
North Dakota	1985	1.29	0.01
South Dakota	1986	2.10	-0.03
Florida	1987	11.73	0.67
Virginia	1988	7.90	0.65
Georgia	1989	12.28	0.92
Pennsylvania	1989	5.73	0.65
West Virginia	1989	5.65	0.32
Idaho	1990	3.56	0.30
Mississippi	1990	11.65	0.25
Oregon	1990	4.85	0.76

Table 12.	Population-Weighted	Statistics of	f RTC-Adopting	States between
1977 and 1	1990 <sup>a</sup>			

Notes: Source-Fryer et al. (2005) and Bureau of Justice Statistics (2009).

<sup>a</sup>The crack index data come from the Fryer et al. (2005) study, which constructs the index based on several indirect proxies for crack use, including cocaine arrests, cocaine-related emergency room visits, cocaine-induced drug deaths, crack mentions in newspapers, and Drug Enforcement Administration drug busts. The article does suggest that these values can be negative. The state with the lowest mean value of the crack index over our data period is Maine (-0.04) and the state with the highest mean value is New York (1.15). (The article does suggest that the crack index values can be negative.)

<sup>27.</sup> New Mexico, one of the five highest crack states, adopted its RTC law in 2003. Wyoming and Montana adopted RTC laws in 1994 and 1991, respectively. North Dakota and South Dakota adopted their laws prior to the start of our data set (pre-1977), although the dates are contested (Lott and Mustard, 1997; Moody and Marvell, 2008).

<sup>28.</sup> In fact, out of the ten states with the lowest crack cocaine index, seven adopted an RTC law by 1994. The exceptions are Nebraska, Minnesota (2003), and Iowa (no RTC law).

Moreover, as Table 12 reveals, the twelve states that adopted RTC laws during the initial Lott-Mustard period (1977–1992) had crack levels substantially below the level of the five high-crack states shown in Figures 8 and 9. None of the RTC adopters shown in Table 12 has an average crack index value that even reaches 1, while Figure 9 reveals that the high-crack states had a crack level in the neighborhood of 4 or 5.

In other words, over the initial Lott-Mustard period of analysis (ending in 1992), the criminogenic influence of crack made RTC laws look beneficial since crack was raising crime in non-RTC states. In the later period, crime fell sharply in the high-crack states, making RTC states look bad in comparison. Therefore, the effects estimated over this entire period will necessarily water down the initial Lott-Mustard results. The hope is that estimating the effect over the entire period will wash out the impact of the omitted variable bias generated by the lack of an adequate control for the effect of crack.

# 10.2. Endogeneity and Misspecification Concerns

To this point, our analysis has remained within the estimation framework common to the NRC/Lott-Mustard analyses, which implicitly assumes that passage of RTC legislation in a given state is an exogenous factor influencing crime levels. Under this assumption, one can interpret the estimated coefficient as an unbiased measure of RTC laws' collective impact.

We probe the validity of this strong claim by estimating a more flexible yearby-year specification, adding pre- and post-passage dummy variables to the analysis.<sup>29</sup> Pre-passage dummies can allow us to assess whether crime trends shift in unexpected ways prior to the passage of a state's RTC law. Autor, Donohue, and Schwab (2006) point out that when analyzing the impact of state-level policies using panel data, one would ideally see lead dummies that are near zero. The graphs that we present below, though, suggest the possible presence of systematic differences between RTC law adopters that can complicate or thwart the endeavor of obtaining clean estimates of the impact of RTC laws.

Figures 10–13 present the results from this exercise in graphical form. Using our preferred model as the base specification, we introduce dummies for the eight years preceding and the first eight years following adoption. We

<sup>29.</sup> In Appendix C, we further analyze the issue of misspecification and model fit by analyzing residuals from the regression analysis.



Figure 10. Normalized Year-by-Year Estimates of the Impact of RTC Laws on Murder.

Notes: Estimations include year and county fixed effects, state trends, and are weighted by county population. The control variables include incarceration and police rates, unemployment rate, poverty rate, county population, population density, per capita income, and six demographic composition measures.

first estimate this regression for each violent crime category over the full sample of RTC states. However, because of the presence of one state that adopted its RTC law just three years after our data set begins, and eight states that adopted laws within the five years before our data set ends, we have nine



**Figure 11.** Normalized Year-by-Year Estimates of the Impact of RTC Laws on Rape. Notes: Estimations include year and county fixed effects, state trends, and are weighted by county population. The control variables include incarceration and police rates, unemployment rate, poverty rate, county population, population density, per capita income, and six demographic composition measures.



Figure 12. Normalized Year-by-Year Estimates of the Impact of RTC Laws on Assault.

Notes: Estimations include year and county fixed effects, state trends, and are weighted by county population. The control variables include incarceration and police rates, unemployment rate, poverty rate, county population, population density, per capita income, and six demographic composition measures.

states that cannot enter into the full set of pre- and post-adoption dummy variables. Because Ayres and Donohue (2003a) showed that the year-by-year estimates can jump wildly when states drop in or out of the individual year estimates, we also estimate the year-by-year model after dropping out



Figure 13. Normalized Year-by-Year Estimates of the Percent Change in Robbery.

Notes: Estimations include year and county fixed effects, state trends, and are weighted by county population. The control variables include incarceration and police rates, unemployment rate, poverty rate, county population, population density, per capita income, and six demographic composition measures.

the earliest (1980) and latest (post-2000) law-adopting states. In this separate series of regressions, our estimates of the full set of lead and lag variables are based on the set of all twenty-five adopters between 1985 and 1996.<sup>30</sup>

Unfortunately, the graphs raise concerns about the presence of endogenous adoption that complicate our thinking about the influence of RTC laws on violent crime. If one looks at the four lines in Figure 10, one sees four different sets of year-by-year estimates of the impact of RTC laws on murder. The lines have been normalized to show a zero value in the year of adoption of an RTC law. Let us begin with the bottom line (looking at the right-hand side of the figure) and the line just above it. The lower line represents the naive year-by-year estimates from the preferred model estimated on the 1977–2006 period, while the line just above it drops out the early and late adopters, so that the estimated year-by-year estimates are based on the "clean" sample of twenty-five adopters for which complete data are available from eight years prior to adoption through eight years after adoption. One immediately sees that the trimmed estimates are different and less favorable to the MGLC hypothesis, as evidenced by the higher values in the post-passage period. They also look superior in the pre-passage period in that on average the pre-passage dummies are closer to zero for the trimmed set of estimates (the mean of the pre-passage dummies is x for the trimmed estimate and Y for the naive estimate).<sup>31</sup>

How should we interpret these trimmed sample estimates? One possibility is to conclude that on average the pre-passage estimates are reasonably close to zero and then take the post-passage figures as reasonable estimates of the true effect. If we do this, none of the estimates would be statistically significant, so one could not reject the null hypothesis of no effect. But note that the pre-passage year-to-year dummies show an oscillating pattern that is not altogether different from what we see for the post-passage values. Without

<sup>30.</sup> The states that drop out (with dates of RTC law passage in parentheses) include Indiana (1980), Michigan (2001), Colorado (2003), Minnesota (2003), Missouri (2003), New Mexico (2003), Ohio (2004), Kansas (2006), and Nebraska (2006).

<sup>31.</sup> Note that this bias in favor of a deterrent effect for murder would also be operating in the aggregate estimates, further suggesting that the true aggregate estimates would be commensurately less favorable for the deterrence hypothesis than the ones we presented earlier in this article—and in all other articles providing unadjusted aggregate estimates.

the odd drop when moving from Year 4 to Year 5 and subsequent rise in values through Year 8, the zero effect story would seem more compelling, but perhaps the drop merely reflects a continuation of the pre-passage oscillations, which are clearly not the product of the passage of RTC laws.<sup>32</sup>

Perhaps what is most important is not the oscillations but rather the trend just prior to passage. This might suggest that rising crime in fact increases the likelihood that a state would adopt an RTC law. In particular, since murder is typically the crime most salient in the media, we suspect it has the greatest effect on implementation of purported crime control measures such as RTC legislation. Of course, this would suggest an endogeneity problem that would also likely lead to a bias in favor of finding a deterrent effect. The mechanism driving this bias would presumably be that rising crime strengthens the National Rifle Association's push for the law, and the mean reversion in crime would then falsely be attributed to the law by the naive panel data analysis (incorrectly premised on exogenous RTC law adoption). Post-adoption murder rates again decline—often to within the neighborhood of prelaw levels. We do, however, uncover some interesting findings when estimating (more cleanly) the year-by-year effects on the twenty-five states for which we have observations across the full set of dummy variables.

Another striking feature we note is the strong influence of Florida and Georgia on our estimates of the impact of RTC laws on murder and rape. When we remove these two states, the post-adoption trend lines for murder and rape shift upward substantially. Moreover, when dropping them from the set of RTC states that already excludes the early and late adopters—still leaving us with twenty-three RTC states to analyze—we see that murder increases in each post-adoption year except one. As previous articles have noted, Florida experienced enormous drops in murder during the 1990s that may have been completely unrelated to the passage of its RTC policy. Donohue (2003) points out that the 1980 Mariel boatlift temporarily added many individuals prone to committing crimes to Florida's population, causing a massive increase in crime in Florida during the 1980s. Thus, it is plausible that the massive 1990s crime reductions in Florida were not driven by the

<sup>32.</sup> The ostensible pronounced drop in murder five years after adoption (exists for the full data set, as well, but it is part of a continuing downward trend in murder that simply reaches a trough five years after passage).

adoption of the state's RTC law but rather a return to traditional population dynamics that were less prone to violent crime (again, a reversion to the mean). This is important to consider given the strong downward pull of Florida on aggregate murder rates.

The line based on dropping Florida and Georgia from the trimmed sample would suggest that for the twenty-three other states, the impact of RTC laws on murder was highly pernicious—and increasingly so as the sharp upward trend in the last three years would suggest. Again a number of interpretations are possible: (1) Florida and Georgia are unusual and the best estimate of the impact of RTC laws comes from the trimmed sample that excludes them (and the early and late adopters); (2) there is heterogeneity in the impact of RTC laws, so we should conclude that the laws help in Florida and Georgia, and tend to be harmful in the other twenty-three states; and (3) omitted variables mar the state-by-state estimates but the aggregate estimates that include Florida and Georgia may be reasonable if the state-by-state biases on average cancel out.

Note that Figure 11, which presents the comparable year-by-year estimates of the impact of RTC laws on rape, shows a similar yet even more extreme pattern of apparent spikes in crime leading to adoption of RTC laws followed by a substantial amount of mean reversion. The somewhat unsettling conclusion from Figures 10 and 11 is that RTC laws might look beneficial if one only had data for four or five years, but this conclusion might be substantially reversed if a few additional years of data were analyzed. Taken as a whole, these two figures show the sensitivity of the estimates to both the time period and sample of states that are analyzed.

Further casting doubt on the possibility that drops in murder and rape could be attributed to the passage of RTC laws, a dramatically different picture emerges from our year-by-year analysis of these laws' impact on assault and robbery rates. The general story here seems to be that assault increases markedly over the time period after law passage, which squares with our results discussed in the previous sections. One observes positive coefficient changes that are initially modest, but these increase dramatically and uniformly over the second half of the post-passage period. Moreover, in contrast to the year-by-year murder and rape estimates, assault trends are not demonstrably different when we alter the sample to exclude early and late adopters, as well as Florida and Georgia. The pattern is generally unaffected by sample, giving us some confidence that RTC laws may be having an adverse impact on the rate of assault. Robbery rates similarly increase over time after the passage of RTC laws, although not as dramatically.

Something to consider, however, is how one should interpret the assault trends in light of the murder trends just discussed. If, for example, the decline in murder to pre-law levels after RTC laws' passage is nothing more than a "mean reversion" effect, it is conceivable that the apparent increase in assault simply represents mean reversion in reverse (from relatively low to high). It is important to note, however, that while assault does return to its pre-law levels a few years after passage, the coefficients continue to rise dramatically, with no hint of any subsequent mean regression. Thus, a more plausible way to interpret the near uniform increases in assault coefficients is that aggravated assault did actually increase over time with the passage of RTC legislation, which strongly undercuts the "MGLC" thesis. Interestingly, the robbery data (Figure 13) suggest either a pernicious effect similar to that on aggravated assault (particularly for the trimmed estimates dropping only early and late adopters) or a strong upward trend in crime, starting well before passage, that might be taken as a sign of the absence of any impact of RTC laws on robbery.

# 10.3 Effects of RTC Laws on Gun-related Assaults

Thus far in our analysis, we have yet to consider whether RTC laws affect aggravated assaults committed with a firearm differently than aggravated assaults overall. This is important to consider given that the 1990s witnessed huge movements in reported assaults due to cultural shifts around the issue of domestic violence. Many of these crimes would not have involved guns, making it possible that our results above suggesting increased rates of assault in RTC states are actually a statistical artifact of changing crime-reporting norms. For this reason, gun-related aggravated assaults may be an arguably more reliable statistic for measuring RTC laws' impact than overall aggravated assaults.

To test this possibility, we estimate our preferred regression using gunrelated aggravated assaults as the dependent variable (both with and without state-specific trends) in Table 13 below. Comparing these new results with the assault estimates in Tables 9a and 9b above, our bottom-line story of how RTC laws increases rates of aggravated assault does not change much when limiting our analysis to assaults involving a gun. Without state trends, we see large positive estimates, some of which are significant at the 10% level. With

	Without State Trends (%)	With State Trends (%)
1. Dummy variable model:	15.50*	0.67
	8.11*	7.48
2. Spline model:	2.23*	5.64*
*	1.27*	3.12*
3. Hybrid model:		
Postpassage dummy	7.76	-2.19
	7.76	7.13
Trend effect	1.90	5.71*
	1.28	3.08*

Table 13. Estimated Impact of RTC Laws on Gun-related Aggravated
Assaults-Using Updated 2009 State-Level Data-With Preferred Controls,
With Clustered Standard Errors, All Crimes, 1977–2006 <sup>a</sup>

<sup>a</sup>Estimations include year and county fixed effects and are weighted by county population. Robust standard errors are provided beneath point estimates. The control variables for this "preferred" specification include: incarceration and police rates (lagged one year to avoid potential endogeneity issues), unemployment rate, poverty rate, county population, population density, per capita income, and six demographic composition measures.

\*Significant at 10%; \*\*significant at 5%; \*\*\*significant at 1%.

state trends, we again see some significant evidence that gun-related aggravated assault rates are increased by RTC legislation. These results solidify our overall confidence in the array of estimates we present above that suggests that RTC laws raise rates of aggravated assault.

# **11. Conclusions**

In this article, we have explored the NRC panel's 2005 report detailing the impact of RTC gun laws on crime. Using the committee's models as a starting point for our analysis, we highlight the importance of thoroughly considering all the possible data and modeling choices. We also highlight some issues that should be considered when evaluating the NRC report.

Data reliability is one concern in the NRC study. We corrected several coding errors in the data that were provided to us by the NRC (which had originally been obtained from John Lott). Accurate data are essential to making precise causal inferences about the effects of policy and legislation—and this issue becomes particularly important when we are considering topics as controversial as firearms and crime control. We attempted to mitigate any uncertainty over data reliability by re-collecting the data. However, when attempting to replicate the NRC specifications—on both the NRC's and

our own newly constructed data sets—we consistently obtained point estimates that differed substantially from those published by the committee.

Thus, an important lesson for both producers and consumers of econometric evaluations of law and policy is to understand how easy it is to get things wrong. In this case, it appears that Lott's data set had errors in it, which then were transmitted to the NRC committee for use in evaluating Lott and Mustard's hypothesis. The committee then published tables that could not be replicated (on its data set or a new corrected data set), but which made at least Professor James Q. Wilson think (incorrectly it turns out—see our Tables 2a–c) that running Lott-Mustard regressions on both data periods (through 1992 and through 2000) would generate consistently significant evidence that RTC laws reduce murder. This episode suggests to us the value of making publicly available data and replication files that can re-produce published econometric results. This exercise can both help to uncover errors prior to publication and then assist researchers in the process of replication, thereby aiding the process of ensuring accurate econometric estimates that later inform policy debates.

A second lesson is that the "best practices" in econometrics are evolving. Researchers and policy makers should keep an open mind about controversial policy topics in light of new and better empirical evidence or methodologies. Case in point: The NRC report suggested that clustering standard errors on the state level in order to account for serial correlation in panel data was not necessary to ascertain the impact of RTC laws on crime. However, most applied econometricians nowadays consider clustering to be advisable in the wake of a few important articles, including one in particular by Bertrand, Duflo, and Mullainathan (2004) on difference-in-differences estimation. The evidence we present corroborates the need for this standard error adjustment. Our placebo tests showed that standard errors are greatly understated without clustering, and we believe strongly that this adjustment is vital for both county-level and state-level analyses of gun laws and crime. Otherwise, statistical significance is severely exaggerated and significant results are detected where none in fact exists.

A third lesson relates to the potential flaws in the Lott-Mustard (and by extension, the NRC) approach and specification. Issues—such as the inclusion of state-specific linear trends, the danger of omitted variable bias, and the choice of county-level over state-level data, all of which the NRC neglected to discuss—clearly have enough impact on the panel data
estimates to influence one's perception of the MGLC theory and thus warrant closer examination. These issues were not all arcane (although many were, such as the need to control for state trends). By now, empirical researchers should be well acquainted with omitted variable bias, and the increases in the prison and police populations were known major factors influencing the pattern of U.S. crime in recent decades (Wilson, 2008). Yet, the Lott-Mustard model—adopted by the NRC—had no control for incarceration or police!<sup>33</sup> On that basis alone, Wilson might well have hesitated before accepting the MGLC hypothesis on the basis of the Lott-Mustard or NRC results. Yet, Lott, with at best questionable support for his view that RTC laws reduce murder, now claims that Wilson, one of the most eminent criminologists of our time, supports his position (Lott, 2008). Clearly, the consequences of embracing fragile empirical evidence can be severe.

Granted, much of the work of applied econometricians is of the sort that was set forth by the NRC as evidence on the impact of RTC laws. The committee, though, found this evidence inadequate to reach a conclusion, doubtless because the results seemed too dependent on different modeling choices. But Horowitz is even more nihilistic, essentially rejecting all applied econometric work on RTC legislation, as indicated by his following independent statement in an appendix to the NRC's (2005) report:

It is unlikely that there can be an empirically based resolution of the question of whether Lott has reached the correct conclusions about the effects of right-to-carry laws on crime. (p. 304)

Of course, if there can be no empirically based resolution of this question, it means that short of doing an experiment in which laws are randomly assigned to states, there will be no way to assess the impact of these laws. The econometrics community needs to think deeply about what the NRC report and the Horowitz appendix imply for the study of legislation using panel data econometrics and observational data.

Finally, despite our belief that the NRC's analysis was imperfect in certain ways, we agree with the committee's cautious final judgment on the

<sup>33.</sup> The Lott-Mustard model omitted a control for the incarceration rate (which is indicated implicitly—though not explicitly—in the notes to each table of the NRC report, which listed the controls included in each specification).

effects of RTC laws: "with the current evidence it is not possible to determine that there is a causal link between the passage of right-to-carry laws and crime rates." Our results here further underscore the sensitivity of guns crime estimates to modeling decisions.<sup>34</sup> If one had to make judgments based on panel data models of the type used in the NRC report, one would have to conclude that RTC laws likely increase the rate of aggravated assault. Further research will be needed to see if this conclusion survives as more data and better methodologies are employed to estimate the impact of RTC laws on crime.

### Appendix A Using Placebo Laws to Test the Impact of Clustering in the State Data

Using state-level data, we again conduct our experiment with placebo laws to examine the effects of clustering the standard errors. As seen in Tables A1–4, we find results similar to those generated with our county data: Without clustering, the Type 1 error rates are often an order of magnitude too high or worse for our murder and robbery regressions (see Tables A1 and A3). In fact, even *with* clustered standard errors (Tables A2 and A4), the rejection of the null hypothesis

**Table A1.** Hybrid Model—Percentage of Significant Estimates (at the 5% Level)—Using Updated 2009 State-Level Data—Lott-Mustard Controls, without Clustered Standard Errors, 1977–2006 (without 1993 Data)

		Dummy Variable (%)	Trend Variable (%)
1. All 50 states $+$ DC	Murder	47.1	67.2
	Robbery	46.0	61.7
2. Exact 32 states	Murder	48.5	57.3
	Robbery	51.2	71.1
3. Random 32 states	Murder	49.3	64.2
	Robbery	50.0	66.0

<sup>34.</sup> For a quick and clear sense of how sensitive estimates of the impact of RTC laws are, see Appendix D, where we visually demonstrate the range of point estimates we obtain throughout our analysis.

		Dummy Variable (%)	Trend Variable (%)
1. All 50 states + DC	Murder	18.5	22.6
	Robbery	12.5	15.4
2. Exact 32 states	Murder	17.1	19.4
	Robbery	15.2	20.3
3. Random 32 states	Murder	22.0	22.7
	Robbery	16.3	18.2

**Table A2.** Hybrid Model—Percentage of Significant Estimates (at the 5% Level)—Using Updated 2009 State-Level Data—Lott-Mustard Controls, with Clustered Standard Errors, 1977–2006 (without 1993 Data)

**Table A3.** Dummy Variable Model—Percentage of Significant Estimates (at the 5% Level)—Using Updated 2009 State-Level Data—Lott-Mustard Controls, without Clustered Standard Errors, 1977–2006 (without 1993 Data)

		Dummy Variable (%)
1. All 50 states + DC	Murder	44.3
	Robbery	46.7
2. Exact 32 states	Murder	50.3
	Robbery	49.4
3. Random 32 states	Murder	51.9
	Robbery	50.8

**Table A4.** Dummy Variable Model—Percentage of Significant Estimates (at the 5% Level)—Using Updated 2009 State-Level Data—Lott-Mustard Controls, with Clustered Standard Errors, 1977–2006 (without 1993 Data)

		Dummy Variable (%)
1. All 50 states + DC	Murder	18.0
	Robbery	14.1
2. Exact 32 states	Murder	16.0
	Robbery	16.4
3. Random 32 states	Murder	22.7
	Robbery	14.3

(that RTC laws have no significant impact on crime) occurs at a relatively high rate. This finding suggests that, at the very least, we should include clustered standard errors to avoid unreasonably high numbers of significant estimates.

### Appendix B Panel Data Models Over the Full Period with no Covariates

The NRC panel sought to underscore the importance of finding the correct set of covariates by presenting panel data estimates of the impact of RTC without covariates but including county and year fixed effects. For completeness, this appendix presents these same estimates for the preferred models (with and without state trends) on both county and state data for the period from 1977 to 2006. If one compares the results from these four tables with no controls with the analogous tables using the preferred model for the same time period, one sees some interesting patterns. For example, if we compare the county results without state trends from both our preferred specification (Table 7a) and the no-controls specification (Table B1), we see that the results are quite similar in terms of magnitude and direction, although adding in our suggested covariates seems to both dampen the coefficients and reduce their significance. The basic story from our analysis is again strengthened: There seems to be virtually no effect of RTC laws on murder, while if there is *any* RTC effect on other crimes generally, it is a crimeincreasing effect. The results are slightly less similar when we compare those from the models that include state trends (Tables 7b and B2). While we see that estimates are similar for murder, rape, robbery, and auto theft, the estimates for assault, burglary, and larceny change in either magnitude or direction (or both) when adding controlling factors to the model. In general, though, we only see decreases when adding state trends to either specification, and even then, the results are much too imprecise to make causal inferences. When we shift to a comparison of the state-level results, we again see similarities between the preferred and no-controls specifications. When looking at the results without state trends, we see that the estimates are very similar in terms of direction, although the no-controls estimates are often larger in magnitude and more

	Murder	Rape	Aggravated	Robbery	Auto	Burglary	Larceny
	(%)	(%)	Assault (%)	(%)	Theft (%)	(%)	(%)
1. Dummy variable	$-0.55 \\ 8.30$	33.10	27.30	25.50*	33.50	35.90	38.00
model		22.60	18.90	14.60*	21.50	22.00	25.50
2. Spline model	0.35	3.35*	3.20*	2.86**	3.42*	3.85*	4.27*
	0.76	1.94*	1.66*	1.36**	2.01*	2.00*	2.29*
3. Hybrid model							
Post-passage	-3.48	21.40	14.30	14.30	21.40	21.50	21.30
dummy	8.07	18.70	16.90	12.70	17.60	18.90	21.60
Trend effect	0.54	2.17*	2.41*	2.07*	2.24	2.66*	3.09*
	0.72	1.25*	1.27*	1.08*	1.48	1.54*	1.69*

**Table B1.** Estimated Impact of RTC Laws—Using Updated 2009 County-Level Data—No Controls, with Clustered Standard Errors, All Crimes, 1977–2006<sup>a</sup>

<sup>a</sup>Estimations include year and county fixed effects, and are weighted by county population. Robust standard errors are provided beneath point estimates.

\*Significant at 10%; \*\*significant at 5%; \*\*\*significant at 1%.

	Murder	Rape	Aggravated	Robbery	Auto	Burglary	Larceny
	(%)	(%)	Assault (%)	(%)	Theft (%)	(%)	(%)
1. Dummy variable	-2.80	-13.10	5.02	3.10	5.58	1.50	2.98
model	5.03	10.60	9.31	7.71	9.47	10.50	11.70
2. Spline model	-0.54	-4.74	1.95	-0.37	-0.14	-0.78	-0.80
	1.23	4.06	2.30	2.33	2.52	2.45	2.61
3. Hybrid model Post-passage dummy	-2.52 5.22	-10.50 10.10	3.94 10.20	3.35 8.27	5.73 10.20	1.97 11.40	3.48 12.80
Trend effect	-0.48	-4.52	1.87	-0.44	-0.26	-0.82	-0.87
	1.27	4.07	2.42	2.42	2.63	2.61	2.80

**Table B2.** Estimated Impact of RTC Laws—Using Updated 2009 County-Level Data—No Controls, with State Trends and Clustered Standard Errors, All Crimes, 1977–2006<sup>a</sup>

<sup>a</sup>Estimations include year and county fixed effects, and are weighted by county population. Robust standard errors are provided beneath point estimates.

\*Significant at 10%; \*\*significant at 5%; \*\*\*significant at 1%.

statistically significant. When doing a similar comparison of the specifications that now adds in state trends, we also see similar results for nearly all crimes. The exception is aggravated assault, for which we see that our preferred specification produces more

	Murder	Rape	Aggravated	Robbery	Auto	Burglary	Larceny
	(%)	(%)	Assault (%)	(%)	Theft (%)	(%)	(%)
1. Dummy variable	-1.79	8.33	11.70**	20.00**	24.70**	18.30***	16.60***
model	7.54	8.22	4.62**	7.90**	11.60**	6.69***	4.04***
2. Spline model	0.08	0.78	1.47**	1.98**	2.03*	1.73**	1.63***
	0.88	0.90	0.64**	0.96**	1.17*	0.72**	0.46***
3. Hybrid model							
Post-passage	-3.22	5.90	5.36	13.30*	19.60**	12.70**	11.00***
dummy	6.96	5.81	3.82	7.36*	9.00**	4.96**	3.69***
Trend effect	0.26	0.45	1.17*	1.24	0.90	0.99*	1.00**
	0.89	0.71	0.63*	0.96	0.86	0.56*	0.42**

**Table B3.** Estimated Impact of RTC Laws—Using Updated 2009 State-Level Data—No Controls, with Clustered Standard Errors, All Crimes, 1977–2006<sup>a</sup>

<sup>a</sup>Estimations include year and county fixed effects, and are weighted by county population. Robust standard errors are provided beneath point estimates.

\*Significant at 10%; \*\*significant at 5%; \*\*\*significant at 1%.

**Table B4.** Estimated Impact of RTC Laws—Using Updated 2009 State-Level Data—No Controls, with State Trends and Clustered Standard Errors, All Crimes, 1977–2006<sup>a</sup>

	Murder (%)	Rape (%)	Aggravated Assault (%)	Robbery (%)	Auto Theft (%)	Burglary (%)	Larceny (%)
1. Dummy variable model	-0.31 3.73	-4.66** 2.00**	0.62 3.36	3.43 4.92	8.38 5.28	1.10 2.93	0.92 2.37
2. Spline model	0.78 0.93	$-0.54 \\ 0.92$	2.46*** 0.91***	0.29 1.39	-0.16 1.71	$-0.20 \\ 0.80$	$-0.46 \\ 0.63$
<ol> <li>Hybrid model Post-passage dummy</li> </ol>	-0.80 3.67	-4.39** 2.03**	-0.90 3.37	3.30 5.30	8.63 5.17	1.25 3.23	1.24 2.55
Trend effect	0.80 0.93	$-0.44 \\ 0.91$	2.48*** 0.92***	0.21 1.43	-0.39 1.70	-0.23 0.84	-0.49 0.67

<sup>a</sup>Estimations include year and county fixed effects, and are weighted by county population. Robust

standard errors are provided beneath point estimates.

\*Significant at 10%; \*\*significant at 5%; \*\*\*significant at 1%.

negative estimates for the dummy model (although this result is not particularly precise). Again, when the comparison is taken as a whole, support is lacking for the view that RTC laws lead to reductions in crime.

### Appendix C Trimming the Sample to Address Questions of Model Fit

Given our concerns about how well the guns crime econometric models fit all 50 U.S. states (plus DC), we decided to examine the residuals from various regressions models. For example, one potentially important issue is whether one should include linear state trends in our models. To further explore this issue, we examined the variance of the residuals for the aggravated assault regression estimates using our preferred models on state data for the period through 2006-both with and without state trends.<sup>35</sup> In particular, we found that the residual variance was high for smaller states, even when we do not weight our regressions by population.<sup>36</sup> We explored how these "high-residual variance" states (defined from the aggravated assault regressions on our preferred model through 2006) might be influencing the results. We estimated our preferred model (both with and without state trends) after removing the 10% of states with the highest residual variance. This step is also repeated after removing the highest 20% of states in terms of residual variance. Our full-sample results for our preferred specification (which includes clustered standard errors, and is run over the entire time period) are shown in Tables 11a and b (without and with state trends, respectively). The results from our two trimmed set of states are presented below. Tables C1 and C2 should be compared to Table 11a (no state trends) and Tables C3 and C4 should be compared to Table 11b (adding in state trends). Removing highresidual variance states (based on the aggravated assault regressions) has little impact on the story told in Table 11a (no state trends): There was no hint that RTC laws reduce crime in Table 11a and this message comes through again in Tables C1 and C2. All three of these tables show at least some evidence that RTC laws increase aggravated

<sup>35.</sup> Since our most robust results across the specifications in this article were for aggravated assault, we focused specifically on the residuals obtained using assault rate as the dependent variable.

<sup>36.</sup> We removed the population weight for this exercise because it is likely that when regressions are weighted by population, the regression model will naturally make high-population states fit the data better. As a result, we expect that residuals for smaller states will be higher. We find, however, that the results are qualitatively similar even when we obtain the residuals from regressions that include the population-weighting scheme.

	Murder (%)	Rape (%)	Aggravated Assault (%)	Robbery (%)	Auto Theft (%)	Burglary (%)	Larceny (%)
1. Dummy variable	-3.53	-0.98	4.33	5.04	6.80	1.38	5.75*
model	4.02	3.95	3.15	4.41	6.27	3.05	2.96*
2. Spline model	-0.13	-0.50	1.16**	0.66	0.57	0.01	0.57
	0.62	0.56	0.57**	0.77	0.63	0.39	0.47
3. Hybrid model							
Post-passage	-3.69	1.65	-1.21	2.53	5.26	1.69	3.94
dummy	3.80	3.69	3.22	4.98	5.80	3.30	2.98
Trend effect	0.04	-0.58	1.21*	0.55	0.34	-0.07	0.40
	0.64	0.58	0.60*	0.86	0.58	0.43	0.50

**Table C1.** Estimated Impact of RTC Laws—Using Updated 2009 State-Level Data—with Preferred Controls, with Clustered Standard Errors, All Crimes, 1977–2006, Dropping States with Highest Residual Variance (Top 10%: MT, ME, WV, NH, TN)<sup>a</sup>

<sup>a</sup>Estimations include year and county fixed effects, and are weighted by county population. Robust standard errors are provided beneath point estimates. The control variables for this "preferred" specification include incarceration and police rates (lagged one year to avoid potential endogeneity issues), unemployment rate, poverty rate, county population, population density, per capita income, and six demographic composition measures. \*Significant at 10%; \*\*significant at 5%; \*\*\*significant at 1%.

**Table C2.** Estimated Impact of RTC Laws—Using Updated 2009 State-Level Data—with Preferred Controls, with Clustered Standard Errors, All Crimes, 1977–2006, Dropping States with Highest Residual Variance (Top 20%: MT, ME, WV, NH, TN, NE, VT, HI, OH, KY)<sup>a</sup>

	Murder	Rape	Aggravated	Robbery	Auto	Burglary	Larceny
	(%)	(%)	Assault (%)	(%)	Theft (%)	(%)	(%)
1. Dummy variable	-4.99	-0.28	3.94	5.80	8.13	2.86	6.75**
model	4.23	4.28	2.40	4.97	6.60	3.20	3.23**
2. Spline model	-0.16 0.66	$-0.50 \\ 0.59$	$0.84* \\ 0.47*$	0.90 0.83	0.71 0.70	0.29 0.37	0.71 0.50
3. Hybrid model							
Post-passage	-5.38	2.53	0.15	2.09	6.16	1.91	4.39
dummy	3.93	3.95	3.05	5.54	6.13	3.64	3.37
Trend effect	0.09	-0.61	0.83	0.81	0.43	0.21	0.52
	0.68	0.61	0.54	0.92	0.66	0.43	0.55

<sup>a</sup>Estimations include year and county fixed effects, and are weighted by county population. Robust standard errors are provided beneath point estimates. The control variables for this "preferred" specification include incarceration and police rates (lagged one year to avoid potential endogeneity issues), unemployment rate, poverty rate, county population, population density, per capita income, and six demographic composition measures. \*Significant at 10%; \*\*significant at 5%; \*\*\*significant at 1%.

	Murder (%)	Rape (%)	Aggravated Assault (%)	Robbery (%)	Auto Theft (%)	Burglary (%)	Larceny (%)
1. Dummy variable model	1.17 2.95	-3.56 2.16	-0.13 2.82	2.28 3.75	7.82** 3.26**	1.31 2.03	1.77 1.66
2. Spline model	0.80 0.91	0.15 0.81	2.83*** 0.82***	0.32 1.37	-2.01** 0.83**	$-0.31 \\ 0.91$	-0.21 0.79
<ol> <li>Hybrid model Post-passage dummy</li> </ol>	0.73 3.12	-3.71 2.32	-1.77 2.80	2.14 4.04	9.13*** 3.23***	1.51 2.31	1.93 1.84
Trend effect	0.77 0.95	0.27 0.83	2.89*** 0.84***	0.25 1.42	-2.29*** 0.83***	$-0.35 \\ 0.95$	-0.27 0.83

**Table C3.** Estimated Impact of RTC Laws—Using Updated 2009 State-Level Data—with Preferred Controls, with State Trends and Clustered Standard Errors, All Crimes, 1977–2006, Dropping States with Highest Residual Variance (Top 10%: MT, NH, VT, WV, KY)<sup>a</sup>

<sup>a</sup>Estimations include year and county fixed effects, and are weighted by county population. Robust standard errors are provided beneath point estimates. The control variables for this "preferred" specification include incarceration and police rates (lagged one year to avoid potential endogeneity issues), unemployment rate, poverty rate, county population, population density, per capita income, and six demographic composition measures. \*Significant at 10%; \*\*\*significant at 5%; \*\*\*significant at 1%.

**Table C4.** Estimated Impact of RTC Laws—Using Updated 2009 State-Level Data—with Preferred Controls, with State Trends and Clustered Standard Errors, All Crimes, 1977–2006, Dropping States with Highest Residual Variance (Top 20%: MT, NH, VT, WV, KY, NE, NV, SD, ND, DE, IN)<sup>a</sup>

	Murder (%)	Rape (%)	Aggravated Assault (%)	Robbery (%)	Auto Theft (%)	Burglary (%)	Larceny (%)
1. Dummy variable model	2.09 2.97	-2.88 2.29	-1.35 2.78	4.63 3.44	8.94*** 3.18***	1.42 2.14	2.41 1.68
2. Spline model	0.92 0.97	0.25 0.83	2.42*** 0.80***	0.63 1.44	$-2.11^{**}$ $0.88^{**}$	$-0.43 \\ 0.99$	-0.12 0.83
3. Hybrid model Post-passage dummy	1.69 3.09	-3.03 2.40	-2.50 2.83	4.39 3.71	10.00*** 3.18***	1.63 2.40	2.50 1.87
Trend effect	0.88 1.01	0.32 0.84	2.48*** 0.81***	0.53 1.50	-2.35** 0.87**	-0.47 1.02	$-0.18 \\ 0.87$

<sup>a</sup>Estimations include year and county fixed effects, and are weighted by county population. Robust standard errors are provided beneath point estimates. The control variables for this "preferred" specification include incarceration and police rates (lagged one year to avoid potential endogeneity issues), unemployment rate, poverty rate, county population, population density, per capita income, and six demographic composition measures. \*Significant at 10%; \*\*significant at 5%; \*\*\*significant at 1%.

assault. Removing the high-residual variance states from the models with state trends does nothing to shake the Table 11b finding that RTC laws increase aggravated assault. The somewhat mixed results for auto theft seen in Table 11b also remain in Tables C3 and C4. Of the states dropped from Table C1, the following four states adopted RTC laws during the 1977–2006 period (with date of adoption in parentheses): Montana (1991), Maine (1985), West Virginia (1989), and Tennessee (1994). Of the additional states dropped from Table C2, the following four states adopted RTC laws during the 1977–2006 period (with date of adoption in parentheses): Ohio (2004), Kentucky (1996), Indiana (1980), and Oklahoma (1995).<sup>37</sup> Results from Table C3 come from dropping similar RTC states to Table C1, although Kentucky (1996) is dropped rather than Tennessee, and New Hampshire (1959) is dropped rather than Maine.<sup>38</sup> Finally, in addition to the five RTC states that were dropped in Table C3, Table C4 dropped the following four RTC states: Nevada (1995), South Dakota (1986), North Dakota (1985), and Indiana (1980).

### **Appendix D**

### Summarizing Estimated Effects of RTC Laws Using Different Models, State Versus County Data, and Different Time Periods

This appendix provides graphical depictions of sixteen different estimates of the impact of RTC laws for the dummy and spline models for specific crimes using different data sets (state and county), time periods (through 2000 or through 2006), and models (Lott-Mustard versus our preferred model and with and without state trends). For example, Figure D1 shows estimates of the impact on murder using

<sup>37.</sup> In implementing our protocol of dropping high–residual variance states, we examined the residuals of the dummy and spline models separately to identify the high-variance states. While they match across models for three of the four tables, in the case of Table C4, the ordinal rank of the states in terms of residual variance were slightly different for the dummy versus the spline model. For this table, Indiana had the 9th highest residual variance when looking at the dummy model results, while North Dakota had the 11th highest variance. For the spline results, the residual variance ranks of these two states were reversed. Thus, for this table, we dropped both states to estimate our regressions.

<sup>38.</sup> The dropped states are slightly different between Tables C1 and C3, as well as between Tables C2 and C4, because the state ranks based on residual variances differed when the models were run with and without state trends.



Figure D1. Various Murder Estimates (Dummy Model).



Figure D2. Various Murder Estimates (Spline Model).



Figure D3. Various Rape Estimates (Dummy Model).



Figure D4. Various Rape Estimates (Spline Model).



Figure D5. Various Assault Estimates (Dummy Model).



Figure D6. Various Assault Estimates (Spline Model).



Figure D7. Various Robbery Estimates (Dummy Model).



Figure D8. Various Robbery Estimates (Spline Model).



Figure D9. Various Auto Theft Estimates (Dummy Model).



Figure D10. Various Auto Theft Estimates (Spline Model).



Figure D11. Various Burglary Estimates (Dummy Model).



Figure D12. Various Burglary Estimates (Spline Model).



Figure D13. Various Larceny Estimates (Dummy Model).



Figure D14. Various Larceny Estimates (Spline Model).

the dummy model, designed to capture the average effect of RTC laws during the post-passage period. The first bar in each of the eight groupings corresponds to county-level estimates; the second bar corresponds to state-level estimates, for a total of sixteen estimates per figure. The value of the figures is that they permit quick visual observation of the size and statistical significance of an array of estimates. Note, for example, that none of the estimates of RTC laws on murder in either Figure D1 or D2 is significant at even the 0.10 threshold. This sharp contrast to the conclusion drawn by James Q. Wilson on the NRC panel is in part driven by the fact that all the estimates in this appendix come from regressions in which we adjusted the standard errors by clustering. In contrast to the wholly insignificant estimates for murder, the estimates of the impact of RTC laws on aggravated assault in Figure D6 are generally significant as indicated by the shading of the columns, where again no shading indicates insignificance, and the shading darkens as significance increases (from a light gray indicating significance at the 0.10 level, slightly darker indicating significance at the 0.05 level, and black indicating significance at the 0.01 level). Note that the overall impression from Figure D6 is that RTC laws increase aggravated assault. Even in Figure D6, though, one can see that some of the estimates differ between county- and state-level data and tend to be strongest in state data controlling for state trends.

Figure D5, which provides estimates of the effect of RTC laws on aggravated assault using the dummy model (rather than the spline model of Figure D6), reveals that the conclusion that RTC laws increase aggravated assault is model dependent: If the dummy model is superior, and if we confine our attention to the complete 1977–2006 data set, the conclusion that RTC laws increase aggravated assault only holds in the Lott-Mustard county data model. In Figure D14, the state-level estimates of the preferred specifications (without state trends) through 2000 and 2006 are essentially zero (no impact), so only the county-level estimates show up in the graph.

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# Exhibit 13

Ex.13 Page 187

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## Shooting Down the More Guns, Less Crime Hypothesis

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### Shooting Down the "More Guns, Less Crime" Hypothesis

### Ian Ayres\* & John J. Donohue III\*\*

INTRODUC	ΤΙΟΝ	1194
I. THEORE	TICAL ISSUES CONCERNING THE EFFECT OF INCREASED CARRYING	
OF CONCE.	ALED HANDGUNS	1203
II. Empiri	CAL ISSUES IN ESTIMATING THE EFFECT OF INCREASED CARRYING	
OF CONCE.	ALED HANDGUNS	1206
A.	Introducing Explanatory Variables into the Panel Data Model	1217
1.	Model specification.	1218
2.	Control variables	1222
В.	Comparing the Results Using the Zheng and Lott Controls	1223
C.	Sensitivity of the Lott Results to Time Period and Inclusion of	
Demogr	caphic Controls	1230
D.	Problems with Unequal Years of Data from Early and Late	
Adopter	s in the Pre-Post Comparison	1240
E.	Problems with Endogenous State Adoption	1255
III. Movn	NG FROM THE STATE TO THE COUNTY DATA	1259
A.	Lott's County Data Analysis for 1977-1992	1260
1.	The dummy variable model	1260
2.	Lott's trend (or spline) model	1263
3.	The hybrid model testing for main and trend effects	1264
В.	Extending Lott's County Data Through 1997	1265
С.	Replicating Table 10 While Controlling for State Trends	1267
IV. ESTIM	IATING STATE-SPECIFIC PASSAGE EFFECTS	1270
V. SOME	INTERPRETATIONS AND SPECULATIONS	1285
А.	Illustrating a Methodology for Deriving State-Specific Predictions	1289
В.	The Resulting State-Specific Predictions	1293

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1193

CONCLUSION	1296
APPENDIX I: CODING THE TIMING AND STATUS OF SHALL-ISSUE LAWS	1297
APPENDIX II: DATA SOURCES	1299

### FIGURES

FIGURE 1A: ROBBERY RATES FOR STATES BY PASSAGE OF SHALL ISSUE LAW,
WEIGHTED BY STATE POPULATION (VERNICK'S CODING)
FIGURE 1B: VIOLENT CRIME RATES FOR STATES BY PASSAGE OF SHALL ISSUE
LAW, WEIGHTED BY STATE POPULATION (VERNICK'S CODING) 1209
FIGURE 1C: MURDER RATES FOR STATES BY PASSAGE OF SHALL ISSUE LAW,
WEIGHTED BY STATE POPULATION (VERNICK'S CODING)
FIGURE 1D: RAPE RATES FOR STATES BY PASSAGE OF SHALL ISSUE LAW,
WEIGHTED BY STATE POPULATION (VERNICK'S CODING)
FIGURE 1E: AGGRAVATED ASSAULT RATES FOR STATES BY PASSAGE OF SHALL
ISSUE LAW, WEIGHTED BY STATE POPULATION (VERNICK'S CODING) 1212
FIGURE 1F: PROPERTY CRIME RATES FOR STATES BY PASSAGE OF SHALL ISSUE
LAW, WEIGHTED BY STATE POPULATION (VERNICK'S CODING) 1213
FIGURE 2: HOW DIFFERENT MODELS ESTIMATE THE LAW'S EFFECT WHEN THE
Path of Crime Is an Inverted V 1220
FIGURE 3A: VIOLENT CRIME—NORMALIZED EFFECT BY YEAR RELATIVE TO
ADOPTION (VERNICK'S CODING) 1246
FIGURE 3B: MURDER—NORMALIZED EFFECT BY YEAR RELATIVE TO
ADOPTION (VERNICK'S CODING) 1247
FIGURE 3C: RAPE—NORMALIZED EFFECT BY YEAR RELATIVE TO ADOPTION
(VERNICK'S CODING) 1248
FIGURE 3D: AGGRAVATED ASSAULT—NORMALIZED EFFECT BY YEAR
RELATIVE TO ADOPTION (VERNICK'S CODING) 1249
FIGURE 3E: ROBBERY—NORMALIZED EFFECT BY YEAR RELATIVE TO
Adoption (Vernick's Coding) 1250
FIGURE 3F: PROPERTY CRIME—-NORMALIZED EFFECT BY YEAR RELATIVE TO
ADOPTION (VERNICK'S CODING) 1251
FIGURE 3G: AUTO THEFT—NORMALIZED EFFECT BY YEAR RELATIVE TO
Adoption (Vernick's Coding) 1252
FIGURE 3H: BURGLARY—NORMALIZED EFFECT BY YEAR RELATIVE TO
ADOPTION (VERNICK'S CODING) 1253
FIGURE 31: LARCENY—NORMALIZED EFFECT BY YEAR RELATIVE TO
Adoption (Vernick's Coding) 1254
FIGURE 4: IMPACT OF SHALL-ISSUE LAW ON VIOLENT CRIME (JURISDICTION-
SPECIFIC DUMMY MODEL WITH STATE TRENDS, COUNTY DATA) 1273
FIGURE 5: IMPACT OF SHALL-ISSUE LAW ON MURDER (JURISDICTION-
SPECIFIC DUMMY MODEL WITH STATE TRENDS, COUNTY DATA) 1274
FIGURE 6: IMPACT OF SHALL-ISSUE LAW ON PROPERTY CRIME (JURISDICTION-
SPECIFIC DUMMY MODEL WITH STATE TRENDS, COUNTY DATA) 1275
FIGURE 7: FIVE-YEAR ANNUALIZED AVERAGE DOLLAR IMPACT OF SHALL-
ISSUE LAWS OVER ALL CRIME TYPES 1283

### TABLES

TABLE 1: THE ESTIMATED IMPACT OF SHALL ISSUE LAWS ON CRIME, STATE	
DATA, USING STATE AND YEAR EFFECTS ONLY (VERNICK'S CODING)	1215
TABLE 2A: THE ESTIMATED IMPACT OF SHALL ISSUE LAWS ON CRIME,	
ZHENG'S STATE DATA, 1977-1999 (VERNICK'S CODING)	1225
TABLE 2B: THE ESTIMATED IMPACT OF SHALL ISSUE LAWS ON CRIME	
Controlling for State Trends, Zheng's State Data, 1977-1999	
(VERNICK'S CODING)	1226
TABLE 3A: THE ESTIMATED IMPACT OF SHALL ISSUE LAWS ON CRIME, STATE	
DATA, 1977-1999 (VERNICK'S CODING), USING INCARCERATION RATES	1228
TABLE 3B: THE ESTIMATED IMPACT OF SHALL ISSUE LAWS ON CRIME	
Controlling for State Trends, State Data, 1977-1999	
(VERNICK'S CODING), USING INCARCERATION RATES	1229
TABLE 4A: THE ESTIMATED IMPACT OF SHALL ISSUE LAWS ON CRIME, STATE	
DATA, 1991-1999 (VERNICK'S CODING), USING INCARCERATION RATES	1232
TABLE 4B: THE ESTIMATED IMPACT OF SHALL ISSUE LAWS ON CRIME	
Controlling for State Trends, State Data, 1991-1999	
(VERNICK'S CODING), USING INCARCERATION RATES	1233
TABLE 5A: THE ESTIMATED IMPACT OF SHALL ISSUE LAWS ON CRIME,	
STATE DATA, 1977-1999 (VERNICK'S CODING), USING INCARCERATION	
RATES, ADDING ON DEMOGRAPHIC CONTROLS TO DUMMY	
VARIABLE MODEL	1234
TABLE 5B: THE ESTIMATED IMPACT OF SHALL ISSUE LAWS ON CRIME	
Controlling for State Trends, State Data, 1977-1999	
(VERNICK'S CODING), USING INCARCERATION RATES, ADDING ON	
DEMOGRAPHIC CONTROLS TO DUMMY VARIABLE MODEL	1235
TABLE 6A: THE ESTIMATED IMPACT OF SHALL ISSUE LAWS ON CRIME,	
STATE DATA, 1977-1999 (VERNICK'S CODING), USING INCARCERATION	
RATES, INCLUDING ONLY BLACK AND WHITE MALES OVER AGE 10 AS	
Demographic Controls	1237
TABLE 6B: THE ESTIMATED IMPACT OF SHALL ISSUE LAWS ON CRIME	
Controlling for State Trends, State Data, 1977-1999	
(VERNICK'S CODING), USING INCARCERATION RATES, INCLUDING ONLY	
BLACK AND WHITE MALES OVER AGE 10 AS DEMOGRAPHIC CONTROLS	1238
TABLE 7: YEARS AND STATES INCLUDED IN THE 1977-1999 TIME PERIOD	
RELATIVE TO SHALL-ISSUE LAW ADOPTION, STATE DATA (VERNICK'S	
CODING OF THE SHALL-ISSUE DUMMY)	1242
TABLE 8A: THE ESTIMATED IMPACT OF SHALL-ISSUE LAWS ON CRIME,	
STATE DATA, 1977-1999 (VERNICK'S CODING), USING INCARCERATION	
RATES, ADDING IN DUMMIES FOR YEARS MORE THAN 8 YEARS	
BEFORE OR 3 YEARS AFTER SHALL-ISSUE LAW ADOPTION	1243
TABLE 8B: THE ESTIMATED IMPACT OF SHALL-ISSUE LAWS ON CRIME	
Controlling for State Trends, State Data, 1977-1999	
(VERNICK'S CODING), USING INCARCERATION RATES, ADDING IN	
DUMMIES FOR YEARS MORE THAN 8 YEARS BEFORE OR 3 YEARS	
AFTER SHALL-ISSUE LAW ADOPTION	1244

TABLE 9: TWO-STAGE LEAST SQUARES ESTIMATES OF THE IMPACT OF	
SHALL-ISSUE LAWS, STATE DATA (LOTT'S CODING)	1258
TABLE 10: THE ESTIMATED IMPACT OF SHALL-ISSUE LAWS ON CRIME,	
COUNTY DATA (LOTT'S CODING)	1262
TABLE 11: THE ESTIMATED IMPACT OF SHALL-ISSUE LAWS ON CRIME	
CONTROLLING FOR STATE TRENDS COUNTY DATA (LOTT'S CODING)	1269
TABLE 12: THE JURISDICTION-SPECIFIC ESTIMATED IMPACT OF SHALL-ISSUE	
LAWS ON CRIME, DUMMY MODEL CONTROLLING FOR STATE TRENDS IN	
CRIME, COUNTY DATA (LOTT'S CODING)	1272
TABLE 13: THE JURISDICTION-SPECIFIC ANNUALIZED FIVE-YEAR IMPACT OF	
SHALL-ISSUE LAWS ON CRIME, LINEAR HYBRID MODEL CONTROLLING FOR	
STATE TRENDS IN CRIME, COUNTY DATA (LOTT'S CODING)	1279
TABLE 14: ESTIMATES OF DOLLAR IMPACT OF CRIME (IN MILLIONS OF	
DOLLARS*) BASED ON JURISDICTION-SPECIFIC ANNUALIZED FIVE-YEAR	
IMPACT, HYBRID MODEL WITH STATE TRENDS (LOTT'S CODING)	1282
TABLE 15: ESTIMATED TOTAL DOLLAR IMPACT OF 24 SHALL-ISSUE LAWS	
(IN MILLIONS OF DOLLARS)	1285
TABLE 16: EXPLAINING THE ESTIMATED PERCENTAGE IMPACT ON CRIME OF	
THE 24 SHALL-ISSUE JURISDICTIONS	1292
APPENDIX TABLE 1: DIFFERENCES IN CODING DATES OF PASSAGE OF	
Shall-Issue Laws	1300
APPENDIX TABLE 2A: AYRES AND DONOHUE'S EXPANDED LOTT DATASET:	
LIST OF VARIABLES AND SUMMARY STATISTICS	1301
APPENDIX TABLE 2B: ZHENG'S DATASET: LIST OF VARIABLES AND	
SUMMARY STATISTICS	1302
APPENDIX TABLE 3A: THE ESTIMATED IMPACT OF SHALL ISSUE LAWS ON	
CRIME, STATE DATA, 1977-1999 (LOTT'S CODING), USING	
INCARCERATION RATES (ANALOGOUS TO TABLE 3A, BUT WITH	
LOTT'S CODING)	1303
APPENDIX TABLE 3B: THE ESTIMATED IMPACT OF SHALL ISSUE LAWS ON	
CRIME CONTROLLING FOR STATE TRENDS, STATE DATA, 1977-1999	
(LOTT'S CODING), USING INCARCERATION RATES (ANALOGOUS TO	
TABLE 3B, BUT WITH LOTT'S CODING)	1304
APPENDIX TABLE 4A: THE ESTIMATED IMPACT OF SHALL ISSUE LAWS ON	
CRIME, STATE DATA, 1991-1999 (LOTT'S CODING), USING INCARCERATION	
RATES (ANALOGOUS TO TABLE 4A, BUT WITH LOTT'S CODING)	1305
APPENDIX TABLE 4B: THE ESTIMATED IMPACT OF SHALL ISSUE LAWS ON	
CRIME CONTROLLING FOR STATE TRENDS, STATE DATA, 1991-1999	
(LOTT'S CODING), USING INCARCERATION RATES (ANALOGOUS TO	
TABLE 4B, BUT WITH LOTT'S CODING)	1306
APPENDIX TABLE 5A: THE ESTIMATED IMPACT OF SHALL-ISSUE LAWS ON	
CRIME, STATE DATA, 1977-1999 (LOTT'S CODING), USING INCARCERATION	
KATES, ADDING IN DUMMIES FOR YEARS MORE THAN 8 YEARS BEFORE OR 3	
Y EARS AFTER SHALL-ISSUE LAW ADOPTION (ANALOGOUS TO TABLE 5A,	1000
BUT WITH LOTT'S CODING)	1307

APPENDIX TABLE 5B: THE ESTIMATED IMPACT OF SHALL-ISSUE LAWS ON
CRIME CONTROLLING FOR STATE TRENDS, STATE DATA, 1977-1999
(LOTT'S CODING), USING INCARCERATION RATES, ADDING IN DUMMIES
FOR YEARS MORE THAN 8 YEARS BEFORE OR 3 YEARS AFTER SHALL-
Issue Law Adoption (Analogous to Table 5b, but with
LOTT'S CODING)1308
APPENDIX TABLE 6: ESTIMATING THE EFFECT OF SHALL-ISSUE LAWS USING
LOTT'S TIME TREND SPECIFICATION FOR SELECTED YEARS 1309
APPENDIX TABLE 7: THE ESTIMATED IMPACT OF SHALL ISSUE LAWS ON
CRIME, JURISDICTION-SPECIFIC HYBRID MODEL WITH STATE TRENDS
APPENDIX TABLE 8: PREDICTION RESULTS

#### INTRODUCTION

In a remarkable paper published in 1997, John Lott and David Mustard managed to set the agenda for much subsequent dataset work on the impact of guns on crime in America by creating a massive dataset of crime across all U.S. counties from 1977 through 1992 and by amassing a powerful statistical argument that state laws enabling citizens to carry concealed handguns had *reduced* crime.<sup>1</sup> The initial paper was followed a year later by an even more comprehensive and sustained argument to the same effect in a book solely authored by John Lott entitled *More Guns, Less Crime* (now in its second edition).<sup>2</sup> The work by Lott and Mustard has triggered an unusually large set of academic responses, with talented scholars lining up on both sides of the debate.<sup>3</sup> Indeed, a panel of the National Academy of Sciences has been convened to sort through the now large body of conflicting studies.

2. JOHN R. LOTT, JR., MORE GUNS, LESS CRIME: UNDERSTANDING CRIME AND GUN-CONTROL LAWS (2d ed. 2000).

<sup>1.</sup> John R. Lott, Jr. & David B. Mustard, *Crime, Deterrence, and Right-to-Carry Concealed Handguns*, 26 J. LEGAL STUD. 1 (1997). A law that allows a citizen to carry a concealed handgun if he or she can demonstrate a need to a government official is a discretionary, or "may-issue," law. The "shall-issue" laws are designed to eliminate discretion on the part of governmental officials by requiring them to issue a permit to carry concealed handguns unless specific and easily verifiable factors dictate otherwise. Essentially, these "concealed-carry laws" enable adults without serious criminal records or identified mental illness to carry concealed handguns in virtually all public places. For a listing of the states that have shall-issue laws (according to two different sets of coding), see Appendix Table 1.

<sup>3.</sup> Among the articles that are supportive of the more guns, less crime thesis are Bruce L. Benson & Brent Mast, *Privately Produced General Deterrence*, 44 J.L. & ECON. 1 (2001); Stephen G. Bronars & John R. Lott, Jr., *Criminal Deterrence, Geographic Spillovers, and the Right to Carry Concealed Handguns*, 88 AM. ECON. REV. 475 (1998); Carlisle E. Moody, *Testing for the Effects of Concealed Weapons Laws: Specification Errors and Robustness*, 44 J.L. & ECON. 799 (2001); David B. Mustard, *The Impact of Gun Laws on Police Deaths*, 44 J.L. & ECON. 635 (2001); John R. Lott, Jr. & William M. Landes, *Multiple Victim Public Shootings, at* http://papers.ssrn.com/sol3/papers.cfm?abstract\_id=272929 (last modified June 10, 2001).

Articles that raise doubts about the Lott and Mustard findings include Dan A. Black &

But in the world of affairs rather than ideas, it did not take long for the National Rifle Association (NRA) and politicians across the country to seize upon the work of Lott and Mustard to oppose efforts at gun control and advance the cause of greater freedom to carry guns. For example, in the same year that the initial article was published, Senator Larry Craig (R-Idaho) introduced The Personal Safety and Community Protection Act, which was designed to facilitate the carrying of concealed firearms by nonresidents of a state who had obtained valid permits to carry such weapons in their home state. Senator Craig argued that the work of John Lott showed that arming the citizenry via laws allowing the carrying of concealed handguns would have a protective effect for the community at large because criminals would find themselves in the line of fire.<sup>4</sup> On May 27, 1999, Lott testified before the House Judiciary Committee that the stricter gun regulations proposed by President Clinton either would have no effect or would actually cost lives,<sup>5</sup> and a number of Republican members of Congress have since included favorable references in their speeches to Lott's work.<sup>6</sup> Moreover, Lott has also testified in support of concealed gun laws before several state legislatures, including Nebraska (1997), Michigan (1998), Minnesota (1999), Ohio (2002), and Wisconsin (2002).<sup>7</sup>

This past summer, Lott's work was favorably cited in a letter to Attorney General John Ashcroft, signed by eighteen state attorneys general, in support of Ashcroft's decision to interpret the Second Amendment as protecting the right of individuals to bear arms. The letter concluded with the following statement:

As the chief law enforcement officers of our respective states, we wish to make one final point that is outside the scope of constitutional analysis.

Daniel S. Nagin, Do Right-to-Carry Laws Deter Violent Crime?, 27 J. LEGAL STUD. 209 (1998); Hashem Dezhbakhsh & Paul H. Rubin, Lives Saved or Lives Lost? The Effects of Concealed-Handgun Laws on Crime, 88 AM. ECON. REV. 468 (1998); Mark Duggan, More Guns, More Crime, 109 J. POL. ECON. 1086 (2001); Jens Ludwig, Concealed-Gun-Carrying Laws and Violent Crime: Evidence from State Panel Data, 18 INT'L REV. L. & ECON. 239 (1998); Franklin Zimring & Gordon Hawkins, Concealed Handguns: The Counterfeit Deterrent, RESPONSIVE COMMUNITY, Spring 1997, at 46.

4. 143 CONG. REC. S5109 (daily ed. May 23, 1997) (statement of Sen. Craig). Although Congress has not yet adopted this legislation, it was reintroduced in 2000 by Congressman Cliff Stearns (R-Florida), who also specifically cited Lott's work. 146 CONG. REC. H2658 (daily ed. May 9, 2000) (statement of Rep. Stearns).

5. Gun Regulations Can Cost Lives: Hearing Before the Subcomm. on Crime of the House Comm. on the Judiciary, 106th Cong. (1999) (statement of John R. Lott, Jr.).

6. See, e.g., 146 CONG. REC. S349 (daily ed. Feb. 7, 2000) (statement of Sen. Craig); 145 CONG. REC. H8645 (daily ed. Sept. 24, 1999) (statement of Rep. Doolittle).

7. Of these states, only Michigan has adopted a right-to-carry law—the law took effect on January 1, 2001. See Michigan's New Concealed Weapon's Law: Referendum Petition Drive Under Way, FOCUS (Michigan Catholic Conference, Lansing, Mich.), Jan. 2001, at 1, available at http://www.micatholicconference.org/pdf/FOCUS\_Jan\_2001.pdf. The Wisconsin Assembly adopted the proposed law supported by Lott on February 26, 2002, but the Senate has not yet passed the bill. See A.B. 675, 95th Leg. Sess., Reg. Sess. (Wis. 2001), available at http://www.legis.state.wi.us/2001/data/AB675hst.html. Simply put, your position on the Second Amendment is a sound public policy decision. There is an increasing amount of data available to support the claim that private gun ownership deters crime. That evidence comes both from the United States (particularly as highlighted in the empirical research of John Lott) and from abroad.<sup>8</sup>

Lott has also drawn upon his scholarly work to become a major popular commentator in the wake of various instances of violence and mayhem. After sixteen individuals were killed in a school shooting in Germany in April 2002, Lott attacked the strict gun control measures in Europe claiming that "[t]he problem with such laws is that they take away guns from law-abiding citizens, while would-be criminals ignore them, leaving potential victims defenseless."9 After the attacks of September 11, 2001, Lott similarly argued that "fears of having guns on planes are misplaced. The special, high-velocity handgun ammunition used on planes packs quite a wallop but is designed not to penetrate the aluminum skin of the plane." Noting that "[s]tates that pass concealed handgun laws experience drops in violent crimes, especially in multiple victim shootings-the type of attack most associated with terrorism," Lott argued that "[t]he use of guns to stop terrorists shouldn't be limited to airplanes. We should encourage off-duty police, and responsible citizens, to carry guns in most public places. Cops can't be everywhere."<sup>10</sup> Clearly, Lott's message has been widely heard. This Article will explore whether the message is in fact true.

In addition to their statistical work, Lott and Mustard have also compiled a large body of anecdotal evidence concerning instances where law-abiding citizens have used guns to capture or thwart dangerous criminals. It may well be the case that many advocates of gun control have been inattentive to the possible benefits—in terms of protection or psychological comfort—that have at times been achieved by those lawfully carrying concealed weapons. Among the many anecdotes in Lott's book designed to emphasize this point, consider the case of Suzanna Hupp, who was sitting in a cafeteria in Killeen, Texas in 1991 when a gunman crashed his car into the restaurant and began shooting the patrons. Although Hupp had a gun in her car, which she believes might have been used to stop the killer, her parents and twenty-one others died in the

<sup>8.</sup> Letter from Bill Pryor, Alabama Attorney General, to John Ashcroft, United States Attorney General (July 8, 2002), *available at* http://www.ago.state.al.us/ag\_items.cfm? Item=81.

<sup>9.</sup> John R. Lott, Jr., Blown Away: Gun Control Misfires in Europe, What's Behind the Massacres in Germany, France and Switzerland?, WSJ.com Opinion Journal (May 4, 2002), at http://www.opinionjournal.com/extra/?id=105002026. Supporters of tighter controls on guns note, however, that "among rich countries, the U.S. far surpassed any other country in firearm-related deaths—with 30,419 reported in 1998, or 11.3 per 100,000 people. That compared with 83 gun-related deaths in Japan and 197 in the United Kingdom during the same period, with rates of 0.1 and 0.3 per 100,000, respectively." Rachel Zimmerman, Study Finds Violence Took 1.6 Million Lives in 2000, WALL ST. J., Oct. 3, 2002, at D5.

<sup>10.</sup> John R. Lott, Jr., Only Guns Can Stop Terrorists, WALL ST. J., Sept. 28, 2001, at A14.

massacre. Following the horrific event, she verbally attacked those who had "legislated me out of the right to protect my family members."<sup>11</sup> Going on to become a legislator herself, Hupp now always carries a firearm, and she is trying to extend the right to carry concealed handguns (now legal in Texas in part due to her efforts) to the few remaining places where they are banned, such as in churches, university campuses, and public schools.

Of course, we do not know whether Hupp would have been carrying her gun on her person that day if she had had the legal right to do so (the fact that she carries it now is not dispositive on that question), or if she would have been able to save her parents or others (rather than just become another casualty by staying and fighting instead of escaping out a shattered window to safety, as she did). It is clear, though, that her loss was terrible, and having a concealedcarry law in effect at the time *might* have reduced the carnage in that situation (or might have added one or more other victims).

While Lott and Mustard have energetically catalogued the situations in which armed citizens have protected themselves or others, they never acknowledge cases on the other side of the ledger where the presence of guns almost certainly led to killings.<sup>12</sup> In the end, one must acknowledge that there are both costs and benefits to either allowing or prohibiting the carrying of handguns, and the task for the scholar is to try to determine which effects dominate.

The existence of a widely cited study based on the statistical analysis of a massive dataset that is invoked in both political and popular circles as an argument against most forms of gun control suggests that careful scrutiny of the empirical evidence is warranted. Lott and Mustard based their analysis on the current state-of-the-art technique of micro-econometric evaluation—a panel data model with fixed effects.<sup>13</sup> That is, Lott and Mustard began by collecting

13. There are actually two "fixed effects." The first is a dummy variable for each county or state that is designed to reflect any unvarying trait that influences crime in that county or state yet is not captured by any of the other explanatory variables. The second is a set of "year fixed effects," which are dummy variables included for each year of the dataset to capture any national influence on crime that is not captured in any of the other explanatory

<sup>11.</sup> Hector Tobar, To Texas Lawmaker, Guns Are the Answer, L.A. TIMES, Mar. 16, 2001, at A21.

<sup>12.</sup> For example, the nightmare scenario for those asserting the value of defensive use of guns is not mentioned: the case of the Japanese exchange student, Yoshihiro Hattori, who was on his way to a Halloween party in October 1992 when he mistakenly approached the wrong house and was shot to death by the homeowner, Rodney Peairs. Peairs, who was later found civilly liable for the boy's wrongful death after an acquittal at his criminal trial, was at home with his family when Hattori and his American host mistakenly rang the doorbell in search of the party. Peairs's wife answered and, apparently frightened by the costume, yelled to her husband to get his gun. Peairs shot Hattori dead after warning him to "freeze," a phrase the young man apparently did not understand. A Baton Rouge, Louisiana judge awarded more than \$650,000 in damages to Hattori's parents, saying there was "no justification whatsoever" for the killing of the 16-year-old boy. Adam Nossiter, *Judge Awards Damages in Japanese Youth's Death*, N.Y. TIMES, Sept. 16, 1994, at A12.

data over a period of years (1977-1992) for individual states and counties across the United States, and then used panel data regression techniques to estimate the effect of the adoption of shall-issue laws, controlling for an array of social, economic, and demographic factors.<sup>14</sup> In earlier work, we commented on concerns that we had about model reliability based on Lott's analysis of 1977-1992 data evaluating the effect of the adoption of shall-issue laws in ten states.<sup>15</sup> We opined on the potential theoretical and empirical infirmities in that analysis, and noted the value in further study given that more state adoptions and the passage of time would likely either strengthen Lott's case if it were true or weaken it if it were false. Having extended the state data through 1999 and the county dataset through 1997, we are now able to test that prediction. We conclude that Lott and Mustard have made an important scholarly contribution in establishing that these laws have not led to the massive bloodbath of death and injury that some of their opponents feared. On the other hand, we find that the statistical evidence that these laws have reduced crime is limited, sporadic, and extraordinarily fragile. Minor changes of specifications can generate wide shifts in the estimated effects of these laws, and some of the most persistent findings-such as the association of shall-issue laws with increases in (or no effect on) robbery and with substantial increases in various types of property crime—are not consistent with any plausible theory of deterrence. Indeed, the probabilistic underpinnings of statistical analysis suggest that running regressions for nine different crime categories to see if there is any measurable impact on crime will, by chance alone, frequently generate estimates that on their face are "statistically significant." Therefore, it

14. Lott, in his book and in a variety of articles, also supports his more guns, less crime thesis with other types of evidence. For example, he collected data at the city and state levels to test whether jurisdictions of different sizes exhibited a reduction in crime when a shall-issue law was adopted. See LOTT, supra note 2, at 169, 190. Lott was also able to secure permit data from 10 states that he used to test whether counties with more concealed handgun permits have larger reductions in crime. Id. at 178. In addition, Lott has concluded from other work that counties located next to states passing shall-issue laws experience an increase in crime just as the passing states experience a decrease. Bronars & Lott, supra note 3, at 479. Although our disaggregated analysis below will cast doubt on this finding, it should still be noted that any estimate of the overall effect on crime from these laws will be biased toward making them look more beneficial than they are if they cause crime to shift from passing to nonpassing states. We do not have these additional types of data and therefore do not analyze them in this Article.

15. Ian Ayres & John J. Donohue III, Nondiscretionary Concealed Weapons Laws: A Case Study of Statistics, Standards of Proof, and Public Policy, 1 AM. L. & ECON. REV. 436 (1999).

variables, but which might be expected to affect all jurisdictions equally. A full list of the variables included in the regressions (other than year and county dummies) and their summary statistics is included in Appendix Table 2. Panel data is data that varies both across time and across jurisdictions. A fixed effects regression adds separate controls for every time period and for every individual jurisdiction and sees whether, after controlling for these individual jurisdiction and time effects, some other characteristic was associated with crime. *See* WILLIAM H. GREENE, ECONOMETRIC ANALYSIS 303, 466 (2d ed. 1993) (describing fixed effects regressions on panel data).

may well be the case that the scattered negative coefficients for various violent crime categories, which on their face suggest that crime decreases with passage of shall-issue laws, should be thought of as statistical artifacts.<sup>16</sup> 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.

The remainder of this Article is divided into five Parts. Part I analyzes the theory underlying Lott's empirical project, discussing the ways in which shallissue laws could dampen-or increase-crime. Part II delves into a host of methodological issues that a researcher must confront in estimating the impact of law on crime. Using a state dataset with several additional years of information, this section demonstrates that while the simplest regression models suggest that crime has tended to increase more when states adopt shallissue laws, these results vary over different time periods (yielding the opposite results before crime began dropping faster in the 1990s in the nonadopting states) and are surprisingly sensitive to inclusion of seemingly extraneous righthand side control variables. While the Lott and Mustard model based on state data gives some support for the view that shall-issue laws can lower rates of murder and rape, better models undermine this conclusion. The most robust findings from the most up-to-date state data are that various property crimes rise with passage, although the absence of a compelling theoretical justification for this result raises concerns about the predictive validity of the models. Part III turns to Lott's own county dataset to assess the extent to which the "more guns, less crime" result persists in less-constrained specifications with additional years of data. Part IV explores even less-constrained regressions, in particular estimating state-specific effects, and concludes that the core finding of more guns, less crime is reversed once the statistically preferable statespecific regression models are used. Part V discusses a hierarchy of possible conclusions to emerge from our empirical work and provides an illustration of how state-specific regression models can potentially provide more nuanced policy recommendations across states than are possible with more customary aggregated models.

<sup>16.</sup> While a primer in regression analysis is beyond the scope of this Article, a crime regression would traditionally regress a measure of crime as a left-hand side variable onto a linear combination of right-hand side attributes (called control variables) that are assumed to be exogenous influences on crime. In such a regression, a negative coefficient on a variable indicating that a jurisdiction had a shall-issue law in effect would tend to indicate that the effect of the law was associated with a reduction in crime.

As Milton Friedman stated: "I have long had relatively little faith in judging statistical results by formal tests of statistical significance. I believe that it is much more important to base conclusions on a wide range of evidence coming from different sources over a long period of time." Daniel Hamermesh, *The Craft of Labormetrics*, 53 INDUS. & LAB. REL. REV. 363, 376 n.11 (2000).

### I. THEORETICAL ISSUES CONCERNING THE EFFECT OF INCREASED CARRYING OF CONCEALED HANDGUNS

Given the massive extent of gun ownership in this country, coupled with the fact that the United States is exceptional in only one aspect of its crime problem—its high rate of lethal violence—it might at first appear that guns must be a part of the problem. But over the last decade, a number of scholars have offered theoretical and empirical support for the notion that allowing lawabiding citizens to carry concealed handguns (unobservable to potential criminals) can deter criminal behavior.<sup>17</sup> The theory is that criminals will be willing to arm themselves whether or not this is lawful, so that laws designed to restrain gun ownership and carrying will only serve to protect criminals, who will have a lessened fear of encountering armed resistance to their criminal designs. Allow law-abiding individuals to carry guns, so the theory goes, and the costs of engaging in criminal activity will rise, thereby dampening the amount of crime.

Lott and Mustard's empirical project is grounded in the important theoretical insight that hidden precautions by potential victims can generate powerful general deterrence effects. Visible precautions by potential victims may simply tend to displace crime toward victims who take less precaution, while unobservable precautions (silent alarms, gasoline kill switches, Lojack) make potential criminals generally more reluctant to commit crime. Thus, while the conventional wisdom focuses on the danger that more guns pose to the citizenry, the new critique emphasizes the protective effect that spills over from those who carry concealed weapons. Because criminals cannot know in advance who is armed with a concealed weapon, their risk goes up in an encounter with any potential victim. Note, then, that even though the open carrying of handguns might only divert criminals from potential victims with guns to those without them, legalizing the concealed carrying of weapons holds out at least the potential of reducing crime rather than merely shifting its incidence.

The first rejoinder to this view is that shall-issue laws allow anyone of a certain age without an officially documented problem of mental health or criminal record to secure a permit to carry a concealed weapon; this is not a particularly exacting standard. A moment's reflection on one's own acquaintances would likely suggest the names of numerous angry or intemperate individuals who could pass the "shall-issue" test even though the prospect of their carrying a concealed weapon would not be likely to enhance one's sense of personal security. Still, Lott and Mustard have a fairly good reply to this point: The number of crimes committed by those individuals who have obtained permits appears to be rather small (although it is doubtless higher

<sup>17.</sup> See GARY KLECK, POINT BLANK: GUNS AND VIOLENCE IN AMERICA (1991); Daniel D. Polsby, *The False Promise of Gun Control*, ATLANTIC MONTHLY, Mar. 1994, at 57.

than official records would suggest as the identity of the perpetrator in a substantial proportion of crimes is never discovered).

But even if no one securing a concealed-carry permit ever used it to commit a crime, there are still a number of avenues by which the passage of a concealed-carry law could stimulate crime. First, even if the adoption of a shall-issue law increased the riskiness of criminal activity and thereby dampened the number of criminals, it might also *increase* the number of criminals who decided to carry weapons themselves (by hypothesis, illegally) and also might *increase* the speed at which a criminal decides to shoot or disable potential victims (as the presence of armed victims increases the cost of hesitation once a criminal engagement has been launched). Therefore, the number of murders and aggravated assaults can rise if criminals respond to shall-issue laws by packing more heat and shooting quicker. Arming the citizenry can encourage an arms race, leading more criminals to carry even higher-powered weapons and to discharge them more quickly when threatened.<sup>18</sup>

Second, even when no criminal act is initially contemplated, the injection of a gun into an angry dispute, perhaps in lawful defense, might escalate a minor dispute into a criminal homicide or a serious wounding.<sup>19</sup> As an earlier president of the Connecticut Chiefs of Police Association stated, "We are concerned about the increasing availability of handguns and the ease with which a person can get a pistol permit. . . . [A] permit is dangerous in the hands of a neophyte who goes to a bar and shows off his phallic symbol to the

<sup>18.</sup> John J. Donohue III & Steven D. Levitt, Guns, Violence, and the Efficiency of Illegal Markets, 88 AM. ECON. REV. 463 (1998).

<sup>19.</sup> For an illustration of this occurrence, consider the story of Skip Olson, 58, and his roommate and friend of 25 years, Michael Jurisin, 50. The two Palo Alto, California residents were fighting about rent payments when Jurisin took out a handgun. Olson grabbed Jurisin's gun and shot him in the back of the head on February 17, 1998. Olson was later convicted of second-degree murder. One suspects that if neither man had owned a gun, no one would have been murdered. Of course, one might object that this example (and the aforementioned Hattori case, supra note 11) show only that having a gun around can lead to needless killings, but not that concealed-carry laws lead to needless killings. Killings in the home will still occur even without shall-issue laws, since no permit is needed to have guns there in most states (although shall-issue laws may increase the attractiveness of guns and lead to more guns being in the home as well as on the highway). Perhaps a better example, then, is the recent triple murder followed by a suicide in Arizona, where a veteran with a concealed handgun permit in Arizona, who was performing poorly in a nursing program, chose to murder the teachers who gave him low grades. While one suspects that the teachers were correctly identifying the student's limited capacity for success in a healing profession, they were certainly victims of his ability to carry concealed handguns to the school. John Broder, Arizona Gunman Chose Victims in Advance, N.Y. TIMES, Oct. 30, 2002, at A20. Still, one suspects that the killer was the sort of person who would have had access to guns in any event, and thus, given his intent to kill, might not have been dissuaded from doing so even if Arizona law had prohibited carrying concealed weapons. Ultimately, one hopes that the statistical evidence can answer the question of which set of effects the competing anecdotes illustrate is more common.

boys."<sup>20</sup> Indeed, there was a bit of a scandal in Connecticut in 1977 when it was revealed that Michael O'Brien-deemed by the federal organized crime strike force special prosecutor as one of the "two most important criminals in the Hartford area" and convicted for racketeering, extortion, and gamblinghad obtained a right to carry a concealed weapon with the support of letters of recommendation from certain major political figures in the state.<sup>21</sup> This suggests that those who are able to secure handgun permits are not always model citizens, and that at least some criminals find it useful to have the legal right to carry weapons.

Third, with some estimates suggesting that as many as one million or more guns are stolen each year, we know that putting more guns in the hands of the law-abiding population necessarily means that more guns will end up in the hands of criminals.<sup>22</sup> In fact, with guns being a product that can be easily carried away and quickly sold at a relatively high fraction of the initial cost, the presence of more guns can actually serve as a stimulus to burglary and theft.<sup>23</sup> Even if the gun owner had a permit to carry a concealed weapon and would never use it in furtherance of a crime, is it likely that the same can be said for the burglar who steals the gun?

Fourth, allowing citizens to carry concealed weapons imposes burdens on police in that they must ascertain whether the gun is being carried legally. Officers of the Illinois State Police have indicated that their job would be complicated if private citizens were permitted to carry guns as they would need to spend time confirming whether the guns were being legally carried.<sup>24</sup> As it stands now in Illinois, anyone caught with a gun in public is violating state law and can be immediately brought into custody without the need for further investigation, which the state police believe has been a powerful tool for taking criminals off the streets.<sup>25</sup> According to James Jacobs, "[t]he possibility of ratcheting up street-level policing to seize more unlawful guns [perhaps through new technologies that can allow police to detect guns from some

20. Lincoln Millstein, Police Toughen Criteria for Getting Gun Permit, HARTFORD COURANT, Jan. 15, 1978, at 1.

21. Andrew Kreig, Pair Gets 3 to 10 Years in Prison in Racketeering Case, HARTFORD COURANT, July 6, 1977 at 3.

22. Mark Duggan reports that nearly 500,000 guns are stolen in the United States every year. Duggan, supra note 3, at 1094. Others estimate the figure to be as much as three times that number.

23. See Philip J. Cook & Jens Ludwig, Guns and Burglary, in EVALUATING GUN POLICY: EFFECTS ON CRIME AND VIOLENCE 74, 75-76 (Philip J. Cook & Jens Ludwig eds., 2003).

24. Donohue has served as a consultant to the Illinois State Police on matters relating to a claim of racial profiling.

25. Lott also identifies another mechanism by which shall-issue laws might induce crime: They may embolden citizens to frequent high-crime areas (or enter areas at night) that would be avoided were they not carrying a gun, thereby increasing possible exposure to criminal acts. While we would lament the increased crime in this situation, the greater willingness to move about would be a benefit associated with the right to carry weapons.

1205

distance away] is complicated by the passage of state 'shall-issue' laws ....<sup>26</sup> Finally, accidental deaths and suicides are obviously aided by the presence of guns, and these costs could conceivably outweigh any benefits of shall-issue laws in reducing crime.<sup>27</sup> Extensive empirical study is needed to assess the relative magnitudes of the likely conflicting effects of a state's decision to permit citizens to carry concealed weapons.

### II. EMPIRICAL ISSUES IN ESTIMATING THE EFFECT OF INCREASED CARRYING OF CONCEALED HANDGUNS

It is probably useful to begin our empirical investigation by giving a sense of the nature of the difficulties in trying to uncover concealed-carry laws' ultimate impact on crime. To this end, look at Figure 1, which shows the pattern of robbery over the period from 1977 through 1999 in four groups of states: the four states that had shall-issue laws prior to 1977, the eight adopters between 1977 and 1989, the seventeen adopters between 1990 and 1999, and the twenty-two states (including the District of Columbia) that have never adopted a shall-issue law.<sup>28</sup> Robbery is a good place to start our inquiry because it is committed in public more than any other crime, and should be the crime most likely to decline if the Lott and Mustard story of deterrence has any plausibility.<sup>29</sup> The first thing one notices in this Figure is that there is a broadly similar pattern for the four groups (particularly for the top three state groupings): Robbery rates rose until about 1980 then dipped, began rising yet again starting in roughly the mid-1980s, followed by a decline in the 1990s. This pattern suggests that there are some factors operating across the entire nation that tend to push crime up and down in broad waves lasting from five to ten years. The second point that leaps out from the Figure is that the twentytwo states that have not adopted shall-issue laws have had much higher rates of robbery than states that allow the carrying of concealed handguns, at least until recently (more about this later). Note that this is not what Lott and Mustard mean when they suggest more guns, less crime. They realize, as sophisticated

29. Lott and Mustard have tried to argue that because some robberies are of banks and other commercial entities that are already protected by armed guards, the predicted effect of shall-issue adoption on robbery is uncertain. We are not persuaded by this claim.

<sup>26.</sup> JAMES JACOBS, CAN GUN CONTROL WORK? 205 (2002)

<sup>27.</sup> D. Hemenway & M. Miller, Association of Rates of Household Handgun Ownership, Lifetime Major Depression, and Serious Suicidal Thoughts with Rates of Suicide Across U.S. Census Regions, 8 INJURY PREVENTION 313 (2002) (concluding that higher rates of gun ownership lead to higher rates of suicide).

<sup>28.</sup> As Appendix Table 1 indicates, there has been some dispute as to whether certain states have a shall-issue law. The Figure 1 graphs are constructed using the shall-issue coding supplied by Jon Vernick and Lisa Hepburn, which we will refer to as "Vernick's coding." Jon S. Vernick & Lisa M. Hepburn, *State and Federal Gun Laws: Trends for 1970-99, in* EVALUATING GUN POLICY, *supra* note 23, at 345. Appendix Table 1 sets forth Vernick's coding (column D) and an alternative coding that is closer to the coding used by Lott and Mustard (column A) that we will refer to as "Lott's coding."

researchers, that in 1977, the twenty-two never-adopting states had double the robbery rate of the other states for reasons having nothing to do with their lack of shall-issue laws. Indeed, only four of the other twenty-nine states allowed the carrying of concealed handguns at that time. The main story is that robbery occurs more frequently in large, densely populated urban areas. Thus, one could not hope to establish the effect of a shall-issue law by looking only at which states have such laws and which do not at any one point in time—a so-called "cross-section" analysis. Even the most zealous supporters of shall-issue laws should realize that introducing the right to carry concealed handguns could not cut the robbery rates by anything close to one-half. Clearly, other factors explain the large differences in the core rates of crime in the different sets of states.






Violent Crimes per 100,000 population







Ex. 13 Page 207 HeinOnline -- 55 Stan. L. Rev. 1211 2002-2003

FIGURE 1E: AGGRAVATED ASSAULT RATES FOR STATES BY PASSAGE OF SHALL-ISSUE LAW Weighted by State Population (Vernick's Coding)	
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Ex. 13 Page 209 HeinOnline -- 55 Stan. L. Rev. 1213 2002-2003

Given this fact, is there anything that can be said about the likely effect on robbery of adopting a concealed-carry law? Certainly, one can say nothing definitive from merely examining the graphical evidence, and we will explain why shortly. It is worth noting, however, that the only group of states to have experienced a substantial drop in robbery over this time period (albeit one punctuated by two sharp upturns) was the group of twenty-two states that never adopted shall-issue legislation. These states experienced a whopping drop in robberies of roughly one-third, a drop so large that the never-adopting states went from having by far the highest robbery rates of the four groups in 1977 to ending up in 1999 with the same robbery rate as the eight adopters over the period from 1977-1989.<sup>30</sup> Indeed, if one were forced to make causal attributions from this graphical data, one might conclude that shall-issue laws tend to increase robbery rates. Similar conclusions could be derived from an examination of the other crime categories depicted in Figures 1b through 1f.<sup>31</sup> But, there is no need to rely on visual inspection alone, since the statistical tool of regression can do more formally and precisely what the graphical analysis is trying to do-control for the initially different levels of crime and the common national forces acting on crime to see whether shall-issue adoption has any systematic effect on crime. We ran just such a regression model that controlled only for the average crime rate in the state and the common national influence each year and found that adoption of a shall-issue law was associated with an almost sixteen percent *increase* in robbery.<sup>32</sup> Indeed, as Table 1, line 2 shows, running these same parsimonious regressions across all nine crime categories for the period 1977-1999 yields results that, with only a single exception, are uniformly statistically significant and positive, suggesting—however naively that shall-issue laws increase crime.33

<sup>30.</sup> A somewhat similar pattern can be seen in Figure 1b for violent crime. While the never-adopters experienced a small *drop* in violent crime, the three other sets of shall-issue states experienced violent crime *increases* over this period, and for the adopters after 1977, those increases were sizable. Indeed, while both sets of post-1977 shall-issue adopters started out with equal levels of violent crime that were substantially below the level in the 22 never-adopting states, by 1999 the never-adopters had a violent crime rate about equal to that of the late-adopters and clearly below that of the adopters in the 1977-1989 period.

<sup>31.</sup> Consider the pattern of rape in the four state groups, shown in Figure 1d. While the never-adopting states started out with the highest level of rape among the four groups, they ended the period with the lowest level. The same pattern can be found, yet even more dramatically, for property crime.

<sup>32.</sup> The more technical description of this regression model would be a panel data model—that is, one combining crime data over a period of time with cross-section data for 50 states and the District of Columbia—controlling only for "state- and year-fixed effects." *See supra* note 13.

<sup>33.</sup> The sole exception is murder, which has a small negative but wholly statistically insignificant coefficient. The other crime rate increases estimated in Table 1, line 2 range from a low of 3.9% for aggravated assault to a high of 23.2% for auto theft. In order to conserve space, our Tables will only report coefficients of interest relating to the impact of the shall-issue law. But the interested reader can find the complete regression output for all the regressions in this Article (as well as the underlying STATA do files and datasets for independent verification) on the Internet at http://www.law.yale.edu/ayres.

	TABLI STATE DA	e 1: The Est ta (Vernic	IMATED IM K'S CODIN	PACT OF SH. G), USING SI	ALL-ISSUE I ATE AND Y	LAWS ON CR EAR EFFECT	IME S ONLY		
1	Violent Crime	Murder	Rape	Aggravated Assault	Robbery	Property Crime	Auto Theft	Burglary	Larceny
Time Period: (1977-1992)								i	
1.	-3.4%	-8.3%	0.5%	-9.5%	4.8%	7.3%	8.7%	9.4%	6.5%
	(2.1%)	(3.7%)	(3.2%)	(2.2%)	(3.9%)	(1.8%)	(3.8%)	(2.8%)	(1.8%)
Time Period: (1977-1999)									
2.	8.7%	-1.6%	6.2%	3.9%	15.9%	<u>16.1%</u>	23.2%	14.4%	16.0%
	(1.8%)	(3.1%)	(2.4%)	(1.8%)	(2.9%)	(1.9%)	(3.1%)	(2.3%)	(1.8%)
Notes: The dependent var	riable is the	: In(crime ra	te) named	at the top of	each colun	nn. The dat	a set is cor	nprised of a	nnual state-
level observations (inclue parentheses) are computed	ding the D d using the	Istrict of Co Huber-Whit	olumbia). e robust es	Regressions timate of vai	t are weigh riance. Coe	ted by state fficients that	t are signifi	n. Standard icant at the .	1 errors (in 10 level are
underlined. Coefficients t	that are sign	nificant at th	le .05 level	are displaye	d in bold.	Coefficients	that are sig	gnificant at t	he .01 level
are both underlined and di	isplayed in	bold.							

Apr. 2003] SHOOTING DOWN "MORE GUNS, LESS CRIME"

1215

Unfortunately, life is not so simple. First, note that the story that was just told was very sensitive to the time frame we examined. If our evaluation had been conducted using data only through 1992, which was the case for the initial Lott and Mustard paper, then we would not have seen the dramatic drop in robbery in the nonadopting states. As Table 1, line 1 shows, over the 1977-1992 period, the simple regression would have suggested that shall-issue laws were associated with statistically significant *decreases* in murders and aggravated assaults. Second, the Table 1 predictions would be valid estimates of the impact on crime of the adoption of a shall-issue law only if two conditions held: (1) each state had a fixed underlying rate of crime that only changed when some omnipresent national influence of crime pushed that rate up or down by the same amount everywhere or when a shall-issue law was adopted; and (2) the fact and timing of adoption of a state.<sup>34</sup>

Figure 1 provides some visual support for part of the first condition in that one can discern the presence of strong influences on crime that seem to operate across all states.<sup>35</sup> But the second part of the first condition-the claim that the only additional statewide influence on crime beyond the fixed state effect and the national year effect is the adoption of a shall-issue law-is obviously heroic. Some of the major measurable influences on crime, such as economic conditions, poverty rates, police activities, rates of incarceration, or demographics, may vary across states. For example, some states may decide to get tougher on crime by increasing their rates of incarceration faster than other states. If such factors both change in different ways across states and influence crime significantly, then the claim that the only changes in crime are caused by uniform national influences or the shall-issue legislation is undermined. More complete regression models will be used below to try to control for these other plausible influences on crime, and to the extent that this effort is successful, the regressions will (hopefully) succeed where reliance on graphs could fail. But note that the correctness of the regression results will be imperiled if there are influences—such as, say, the criminogenic influence of the introduction of crack cocaine-that impact differentially across states and are not controlled for in the regression model, yet are correlated with the adoption of a shall-issue law.

35. The pattern is very clear for robbery and violent crime. See figs. 1a, 1b.

<sup>34.</sup> As those familiar with tests of the effectiveness of medicinal drugs or medical interventions know, the gold standard for ascertaining the effect of a treatment is to randomize individuals into treatment and control groups and observe if there are statistically significant differences in outcomes across the two groups. In the medical context, the benefits of this approach would be lost if individuals were free to chose whether to take the treatment. Similarly, in our evaluation of the impact of a legal change, the regression results can be misleading when legislatures are free to enact or reject the shall-issue law instead of having the law randomly assigned to different states. This issue, referred to as the problem of endogenous state adoption, is addressed at greater length in Part II.E, *infra*.

One can get a glimpse of how this problem of omitted variable bias might be operating in this context by examining Figure 1c, which shows the pattern of murder rates for the four state groupings. Note that murder rates spiked into the early 1990s in the two top groups (which either never adopted a shall-issue law or adopted one in the 1990s) but did not show the same increase in the group of states that adopted shall-issue laws in the 1977-1989 period. The Table 1 regressions would attribute the greater run-up in murder starting in the late 1980s to the failure of the top two groups to adopt shall-issue laws-hence Table 1, line 1 found the shall-issue laws to be associated with a substantial 8.3% decrease in murder over the period of 1977-1992. But an alternative explanation is that the crack cocaine problem<sup>36</sup> drove up crime in certain highly urban areas in states that were reluctant to enact a shall-issue statute while largely bypassing more rural states that were adopting such laws. If that were the case, 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, for which the regression failed to control.<sup>37</sup>

The second issue relevant to estimating the effect of a shall-issue law is whether the law can be thought of as being adopted as a random treatment that is, the law is an exogenous event—rather than being part of the endogenous system that influences crime and the various responses to crime. In other words, if spikes in crime lead to demands for the right to carry handguns (which turns out to be the case), then adoptions of shall-issue laws are not exogenous but are endogenous to the variable that we are trying to explain, which is crime itself. In this event, the regression might attribute subsequent drops in crime, even if merely reflecting a return to more normal rates of crime, to the passage of the law.<sup>38</sup> This endogeneity problem is a vexing one in all studies examining the impact of a legal change, and perhaps particularly so when examining a law's impact on crime (because politicians like to be seen as "solving" crime problems by passing laws). We will also try to address this issue—admittedly with only limited success—later in this Article.

#### A. Introducing Explanatory Variables into the Panel Data Model

Putting aside the endogeneity problem for now, we begin our exploration of the panel data regressions with the finding that shall-issue laws seem to increase crime—although, as Table 1 revealed, we might have thought differently about the impact on murder and aggravated assault if we had had to

<sup>36.</sup> See THE CRIME DROP IN AMERICA (Alfred Blumstein & Joel Wallman eds., 2000).

<sup>37.</sup> Lott's control for state population density (population per square mile), shown in Appendix Table 2, will not likely capture the full effects of the influence of crack on crime across states.

<sup>38.</sup> The "return to more normal rates of crime" after a sudden upturn is referred to as regression to the mean.

rely on data ending in 1992 rather than 1999. But before we can repose confidence in conclusions from graphical evidence or the parsimonious regressions of Table 1, we must probe whether introducing controls for other possible influences on crime might reverse this initial conclusion. For this to happen, the omitted variables would have to be correlated with both the passage of shall-issue laws as well as with higher crime. Before turning to these results, we must first address a few details about the nature of the regression modeis (specification) and the explanatory (or control) variables.

#### 1. Model specification.

Although the regression model employed by Lott and Mustard is for a panel dataset with both state (or county) and year fixed effects, even within this structure, a number of decisions need to be made concerning how one models the impact of the law. In trying to determine the impact of the passage of shallissue laws, one would ordinarily begin by asking whether crime was on average higher or lower after the law went into effect, controlling for all the explanatory variables that are thought to have an impact on crime in the state. This is the so-called "dummy variable" model, which posits that the law will have a fixed (once-and-for-all) percentage impact on crime—that is, the law will raise or lower crime by, say, five percent. Lott and Mustard begin their analysis with this dummy variable model but also explore a second specification in which they attempt to estimate whether the passage of the law will cause a break in the time path of crime, causing it to tip up or down depending on whether the law raises or lowers crime. In conducting this trend (or "spline") analysis, Lott and Mustard estimate what the average linear time trend of crime is before the law passes, and then probe whether this trend changes after passage.<sup>39</sup>

The dummy variable model. If Lott and Mustard were correct that the passage of a shall-issue law reduces crime, one might see a sudden and persistent drop in crime that would be captured by a postpassage dummy in the panel data regression. Since typically the dependent variable in these regressions will be the natural log of the crime rate, the coefficient on the postpassage dummy variable can be interpreted as the percentage change in crime associated with the adoption of the law.<sup>40</sup> Lott interprets negative coefficients on the postpassage dummy variable to imply that prospective criminals anticipate the dangers they would face in trying to prey upon a more armed population and that they would reduce their criminality in response. Of course, one way that prospective criminals could reduce any increased personal

<sup>39.</sup> Instead of reporting regression coefficients, with regard to his linear specification, Lott only reports the change in the before and after linear trends, *see* Lott's original table 4.8, and with regard to quadratic terms in the time trend, he graphs the before and after quadratic effects, *see* Lott's original figures 4.5-4.9.

<sup>40.</sup> Table 1 estimates are from just such a model (albeit controlling only for state and year fixed effects) and can thus be interpreted in this manner.

risk to them from such a law would be to continue their life of crime in a more hospitable jurisdiction (presumably one without a shall-issue law).<sup>41</sup>

The Lott spline model. Again assuming Lott and Mustard are correct that the shall-issue laws reduce crime, one might observe crime rates falling as individuals applied to the relevant state officials to secure the right to carry concealed weapons and then in fact began carrying them. Since this process would unfold gradually, one might expect to see a gradual and continuing decrease in crime—at least until the increase in the number of citizens carrying lawful concealed weapons came to an end. In this model (the trend or "Lott spline" model), a time trend would emerge after passage reflecting a dampening effect on crime that grew as the number of concealed handguns being carried increased.

In response to criticisms about the robustness of the dummy variable (or "static") specification results,<sup>42</sup> Lott has correctly noted that, as a theoretical matter, shall-issue laws could still dampen the trend of crime without showing any effect in the simple dummy regressions if crime were to follow an "inverted V" pattern. For example, Figure 2 depicts a case in which the crime rate increased for five years in a state before the law's passage and then symmetrically declined in the five years after the law's passage. In this situation, the static regression would estimate no systematic impact of the law (as depicted by the constant estimated horizontal lines before and after passage) even though there is a large change in the before and after slopes.<sup>43</sup>

Lott argues that the "inverted V" theory is borne out in the data, and, indeed, the graphs in his book of the trends in crime for the states adopting shall-issue laws appear to show that the linear trends in robbery and other crime categories were increasing prior to passage of the laws and fell thereafter. But to sort out whether the dummy variable model, the linear trend model, or any other model can support Lott's story that shall-issue laws *caused* a downward shift in the trend of violent crime, we examine Lott's estimated static and trend effects using more data (extending Lott's dataset through 1999 for our state data analysis and through 1997 for our county data analysis) and less-constrained econometric specifications.

<sup>41.</sup> Note that the simple panel data results cannot distinguish between an effect of a shall-issue law that reduces crime overall versus one which only shifts it to another jurisdiction. Obviously, crime transfers are much less desirable from a policy perspective than crime reductions. Bronars & Lott, *supra* note 3, claim to have found such geographic substitution, but our disaggregated analysis below casts doubt on their findings.

<sup>42.</sup> See Ayres & Donohue, supra note 15.

<sup>43.</sup> Id. at 445. Note that Figure 2 defines the "true impact" of the law by the difference between the prepassage linear trend and the observed postpassage time path of crime. Note the need for assumptions about the unobserved counterfactual—what would have happened had the law not been passed. See John J. Donohue III, The Search for Truth: In Appreciation of James J. Heckman, 27 LAW & Soc. INQUIRY 23 (2002).



### FIGURE 2: HOW DIFFERENT MODELS ESTIMATE THE LAW'S EFFECT WHEN THE PATH OF CRIME IS AN INVERTED V

The hybrid (or main effect plus trend) model. While Lott and Mustard employ both of the preceding models, a third, more general model is actually a hybrid of the preceding two in that it allows a postpassage dummy to capture the main effect of the law but also allows the law to change the linear trend in crime for adopting states. This model could be important if an "announcement effect" initially scares some criminals into fearing possible victim or bystander retaliation but that the ultimate effect is that more guns leads to more serious criminal acts—perhaps as fist fights end with someone dead or seriously injured instead of with a bloodied nose.<sup>44</sup> Under this scenario, one might even see an initial drop in crime followed by a subsequent turn around as the number of concealed guns being carried and crime increase in tandem. Note that although Lott does not employ this model,<sup>45</sup> it can be used to test whether one or both of the first two models are appropriate.<sup>46</sup>

<sup>44.</sup> Zimring and Hawkins first characterized the announcement and the increased-risk effects in 1997. See Zimring & Hawkins supra note 3.

<sup>45.</sup> Lott, however, did use the hybrid specification in his analysis of geographic substitution. *See* Bronars & Lott, *supra* note 3, at 250.

<sup>46.</sup> If the estimated coefficient on the postpassage dummy were virtually zero, one would reject the first model, and if the estimated coefficient on the time trend were virtually zero, one would reject the second model. If they were both virtually zero, one would conclude that the law had no effect on crime.

The hybrid model will generate two estimated effects that could either be reinforcing (both the dummy and trend have the same sign), or in conflict (in that one effect is positive and the other is negative). We will later see empirical evidence suggesting that regression results indicating that the main effect of the law is pernicious while the trend effect is benign are likely generated by some model misspecification rather than evidence that the law actually generated this pattern.<sup>47</sup> Lott does suggest a way in which a pernicious main effect could be followed by a benign long-term trend effect, but this argument is unconvincing. In discussing his findings that public shootings increase for a few years after passage of nondiscretionary handgun laws, Lott suggests that there might have been a type of temporal substitution: People planning such shootings might "do them sooner than they otherwise would have, before too many citizens acquire concealed-handgun permits."<sup>48</sup> But we find temporal substitution to be an unlikely explanation for the aggregate behavior of violent criminals.<sup>49</sup>

The opposite pattern—a negative dummy and a positive time trend—could occur in a number of ways. For example, this pattern would emerge if the announcement effect dampens crime more powerfully than any effect initially generated by the actual higher risk to the potential lawbreakers of confronting an armed citizen, but over time this effect is overwhelmed by the stimulus to crime that greater gun prevalence generates.<sup>50</sup> Similarly, this pattern would be seen if an initial announcement effect led to a drop in crime, followed by a return to previous levels of crime as the salience of the new law recedes from the consciousness of criminals. The Lott spline model, in contrast, essentially posits no announcement effect and assumes a gradually growing risk to lawbreakers generates a reduction in crime (the Lott thesis) or that the gradual pernicious influence of more guns in the hands of hotheads or criminals leads to more shootings and opportunistic criminal acts. Of course, if both influences are operating—an announcement effect dampening or increasing crime and an increased risk effect (reducing crime if the risk is to lawbreakers and increasing

<sup>47.</sup> To foreshadow the later empirical evidence, the problem is revealed in Figure 3a, in which one sees that the effect of the shall-issue law is completely absent until after the tenth year (when many late-adopting states drop out of the analysis). At that point, the regression perceives that crime has dropped sharply (although this is a pure selection effect). The result is that the regression line has to tilt sharply down, and this downward tilt is accommodated through an ostensible jump upwards in the main effect.

<sup>48.</sup> LOTT, supra note 2, at 102.

<sup>49.</sup> We can conceive of a more plausible potential theoretical mechanism for generating a positive main effect and a negative trend: In the wake of passage of a shall-issue law the increased gun carrying generates more harmful, offensive uses of the these weapons but that over time criminals are deterred by the potential defensive uses of guns.

<sup>50.</sup> As noted above, *see supra* note 22, a very large number of guns are stolen each year. If shall-issue laws encourage more lawful gun ownership, they probably lead to an increase in the number of guns that are stolen. Note that these thefts could undo any initial benign effect of the law in a way that leads to a negative postpassage dummy (as crime initially dropped) and a positive time trend (in light of the increased arming of criminals).

it if the risk is to the public)—then the Lott spline model would be misspecified.

#### 2. Control variables.

Lott tries to control for an array of measurable factors—listed in Appendix Table 2a—that might influence crime rates, such as the arrest rate for a particular crime category in a state or county, the level of income, and various demographic measures. Since many of these variables make little difference to the analysis, it is not worth arguing about whether they should be included or not.<sup>51</sup> A potentially problematic variable employed by Lott and Mustard is the arrest rate, which is used to capture any changing deterrence that might be resulting from alterations in the intensity or effectiveness of police or prosecutorial resources. Reliance on the arrest rate (measured as the ratio of total arrests for any particular crime to the total number of occurrences of that crime) can lead to a large amount of data being excluded because of the realization of the dependent variable, which is always problematic.<sup>52</sup> Accordingly, we examined the sensitivity of the Lott results to using the state incarceration rate as an alternative and, in some ways, preferable measure of the likelihood of punishment.<sup>53</sup> To dampen problems of endogeneity, we used

<sup>51.</sup> A minor variable that attempts to measure the amount of income maintenance per capita given to those over age 65 under certain social welfare programs is problematic in that the reason for inclusion is uncertain and attempts to replicate and extend this data beyond 1992 were unsuccessful. Since its inclusion or omission matters little to the analysis before 1992, we just excluded it in all reported regressions.

<sup>52.</sup> In these regressions, the dependent (or left-hand side) variable that the regressions are trying to predict is the crime rate. But if the crime rate is zero, Lott's methodology causes the observation to be dropped from the sample.

Mustard has argued that this exclusion, based on the undefined arrest rate in zero crime counties, is actually a good thing: "Omitting arrest rates may generate a truncation problem because many counties with zero crime rates will be included in the regression. By construction it is impossible for a shall-issue law to reduce crime in a county that has no crime, no matter how effective is the law." David B. Mustard, *Comment* to John J. Donohue, *The Impact of Concealed-Carry Laws, in* EVALUATING GUN POLICY, *supra* note 23, at 325, 328. We disagree with this point. If a county has no crime, then adopting a shall-issue law can have no crime-reducing benefit. If one is interested in ascertaining what the average impact on crime is from the adoption of the law, one needs to include all jurisdictions, including those that derive no benefit. To take the extreme case, assume that the vast majority of counties had no crime, and a few had substantial crime that dropped by 10% when a shall-issue law was adopted (while by assumption there was no benefit when such laws were adopted in the zero crime areas). Lott and Mustard would argue that shall-issue law sould only be a miniscule fraction of that figure.

<sup>53.</sup> The disadvantage of the incarceration rate data is that it is only available on a statewide (as opposed to countywide) basis, and therefore may be less useful in the county data analysis. In part for this reason, we primarily rely on the incarceration rate when conducting analyses on state data and the arrest rate when conducting analyses on county data.

the lagged value of the incarceration rate.<sup>54</sup> But whether one uses arrest rate or incarceration rate, one must acknowledge the possible problem that instead of being a truly independent variable, either of these measures will in fact be influenced by the trends in crime. Therefore, we also estimated models in which both of these explanatory variables are omitted as not being truly exogenous controls.<sup>55</sup>

### B. Comparing the Results Using the Zheng and Lott Controls

Our parsimonious Table 1 regressions for the period 1977-1999 confirmed what the graphs of Figure 1 had shown-that shall-issue laws were associated with statistically significant higher crime rates in eight of the nine crime categories. We can now enrich the simple model by adding controls for other factors that might be influencing crime over this period. There is a vast array of different variables that might be entered into a crime equation, and we begin by using the specification employed by a gifted researcher at Stanford, Wentong Zheng, who has been working with state crime data to ascertain the impact of state lotteries on crime. Zheng generously shared his dataset and programs and coded in the states that adopted shall-issue laws. The resulting regressions control for the following state-wide attributes: population, the percentage of black population; the percentage of metropolitan population; the percentage of population aged fifteen to seventeen, eighteen to twenty-four, and twenty-five to thirty-four respectively; per capita income; unemployment rate; the percentage of population under the poverty line; number of prisoners in state prisons (lagged one year); number of sworn police officers (once-lagged); and per capita alcohol consumption.<sup>56</sup> While one might argue that some different explanatory variables should be included or that the included ones should be treated differently, the advantage of using this specification is that it was adopted by a different researcher for a different crime paper and therefore it is immune to the argument that the explanatory variables were chosen to influence the estimated effect of shall-issue laws.

The results of this analysis are shown in Table 2a. Let's begin with line 1, which is the "dummy variable model," in which the regression estimates the change in crime from the prepassage to the postpassage period while controlling for the influence of Zheng's explanatory variables. Comparing the results in line 1 to the simple regressions of Table 1, line 2, we see that the general story that, over the 1977-1999 time period, shall-issue laws are

<sup>54.</sup> Lagging of a right-hand side variable is a standard econometric technique that attempts to make the variable more exogenous to the determination of the dependent variable (in this case, the crime rate).

<sup>55.</sup> In general, as indicated in our discussions of Tables 5 and 6, below, the only explanatory variables that tended to have a large impact on the estimated effect of shall-issue laws were the demographic variables.

<sup>56.</sup> Summary statistics for these variables are presented in Appendix Table 2b.

associated with *higher* rates of crime comes through, although less powerfully than before and primarily for robbery and property crimes.<sup>57</sup> While before, eight of nine estimates were positive and statistically significant, now seven of nine are positive, of which only five are statistically significant. The two negative effects—for murder and rape—are statistically insignificant.

Lines 2 and 3 of Table 2a provide alternative spline and hybrid methods (discussed above) for estimating the effect of the shall-issue laws by testing whether there is any break in the trend of crime (line 2) or allowing both a change in the level as well as a change in the trend in crime (line 3). The line 2 results essentially wipe out any effect, leaving only one statistically significant variable—robbery, which switched in sign from positive to negative in going from line 1 to line 2. When both effects are estimated in the line 3 hybrid model, they become somewhat anomalous, showing a positive main effect in most of the crime categories, but some negative trend effects.<sup>58</sup>

Table 2b adopts the same estimation approach as in Table 2a, with one change. Since the panel data model is essentially a comparison of the period before and after adoption of the shall-issue law, there is some danger that if crime is trending up (or down), the Table 2a regressions might erroneously attribute a preexisting trend to the adoption of a law. Consequently, the Table 2b results control for any preexisting state trends. The results are similar to those in Table 2a, but the anomalous pattern of strong positive main effects and negative trend effects in the hybrid model is eliminated. Importantly, the robbery effect comes through as positive and statistically significant in both the dummy variable model and the hybrid model when one controls for state fixed trends.

<sup>57.</sup> The results shown in Table 2 use Vernick's coding. Wentong Zheng also ran similar regressions in which he used Lott's coding for the period of his original dataset (1977-1992) and Vernick's coding for 1993-1999. This different coding led to similar results, except that murder became significant and negative.

<sup>58.</sup> We previously indicated that such a pattern was likely to be a sign of model misspecification. *See supra* text accompanying note 47.

	Violent			Aggravated		Property	Auto		
Time Period (1977-1999)	Crime	Murder	Rape	Assault	Robbery	Crime	Theft	Burglary	Larceny
1. Dummy variable model:	2.3%	-1.5%	-1.7%	0.1%	6.1%	8.2%	15.1%	5.0%	8.3%
	(1.6%)	(2.2%)	(1.6%)	(1.9%)	(2.2%)	(1.2%)	(2.2%)	(1.5%)	(1.2%)
2. Lott spline model:	-0.6%	-0.8%	0.6%	-0.6%	<u>-2.0%</u>	-0.1%	-0.7%	<u>-0.6%</u>	0.1%
	(0.4%)	(0.5%)	(0.4%)	(0.5%)	(0.5%)	(0.3%)	(0.5%)	(0.3%)	(0.3%)
3. Hybrid model:									
Postpassage dummy	2.4%	6.4%	-5.6%	0.0%	7.8%	4.3%	11.1%	4.2%	3.0%
	(1.8%)	(2.5%)	(2.2%)	(2.3%)	(2.8%)	(1.4%)	(2.8%)	(1.9%)	(1.4%)
Trend effect	-0.8%	-1.2%	1.0%	-0.6%	-2.5%	-0.4%	-1.5%	<u>-0.9%</u>	-0.1%
İ	(0.4%)	(0.6%)	(0.4%)	(0.5%)	(%9.0)	(0.3%)	(0.5%)	(0.3%)	(0.3%)

TABLE 2A: THE ESTIMATED IMPACT OF SHALL-ISSUE LAWS ON CRIME

estimate of variance. Coefficients that are significant at the .10 level are underlined. Coefficients that are significant at the .05 level are displayed in bold. Coefficients that are significant at the .01 level are both underlined and displayed in bold. We thank Wentong Zheng for generously sharing his data set, model specification, and do-files.

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	Violent			Aggravated		Property	Auto		
Time Period (1977-1999)	Crime	Murder	Rape	Assault	Robbery	Crime	Theft	Burglary	Larceny
Dummy variable model:	1.2%	2.5%	-3.0%	-0.9%	5.1%	2.2%	4.4%	1.6%	2.0%
	(1.4%)	(2.0%)	(1.4%)	(1.7%)	(2.3%)	(1.1%)	(2.0%)	(1.6%)	(1.1%)
. Hybrid model:									
Postpassage dummy	-1.3%	1.9%	-4.7%	-4.9%	5.5%	1.7%	4.8%	2.3%	0.9%
	(1.5%)	(2.1%)	(1.5%)	(1.8%)	(2.4%)	(1.1%)	(2.0%)	(1.5%)	(1.1%)
Trend effect	2.3%	0.6%	1.6%	3.8%	-0.3%	0.5%	-0.4%	-0.7%	1.0%
**	(0.5%)	(0.7%)	(0.4%)	(0.5%)	(0.8%)	(0.3%)	(0.6%)	(0.4%)	(0.4%)

State- and year- fixed effects are included in all specifications. All regressions are weighted by state population. Standard errors (in parentheses) are computed using the Huber-White robust estimate of variance. Coefficients that are significant at the .10 level are underlined. Coefficients that are significant at the .05 level are displayed in bold. Coefficients that are significant at the .01 level are both underlined and displayed in bold. We thank Wentong Zheng for generously sharing his data set, model specification, and do-files. al une wh NOIES. The dependent variable is the interime fate) manifed level observations (excluding the District of Columbia).

TABLE 2B: THE ESTIMATED IMPACT OF SHALL-ISSUE LAWS ON CRIME CONTROLLING FOR STATE TRENDS

So while our simplest Table 1 regressions (with controls for only state and year fixed effects) suggested that shall-issue laws substantially increase crime, moving to a full regression model in Table 2, we see that the results become less stark. Adding Wentong Zheng's set of explanatory variables has generally maintained the story that robbery and property crime rates are higher with adoption of shall-issue laws, while the results for the four other violent crimes are more mixed across the five different models shown. If one adopts the view that the results should be robust across both Tables 2a and 2b before they can be accepted, one would probably conclude that shall-issue adoption *may* be associated with lower amounts of rape—the hybrid results with state trends are ambiguous—but there is no consistent evidence of lower crime in any other crime category. At the same time, there is more robust statistical evidence of higher property crime, although the theoretical rationale for such an effect is obscure.

It is now worth exploring whether the Lott and Mustard models (which differ from Zheng's only in terms of the explanatory variables used) will generate similar results. Therefore, we ran our version of the Lott model (which includes a host of additional right-hand side variables, using incarceration rates instead of arrest rates) on this same expanded state dataset covering the years 1977-1999. Somewhat surprisingly, the results (shown in Table 3) appear far stronger for the Lott and Mustard thesis than those depicted in Table 2.59 For example, while the dummy variable model of Table 2 showed five statistically significant positive variables (and none that were negative), the same model in Table 3 has two positive and two negative estimates that are statistically significant. Indeed, in the hybrid results of Table 3 almost all of the positive and significant coefficients from Table 2 disappear. Again, using the standard of robustness across models, the strongest results are that rape goes down and larceny goes up with the adoption of shall-issue laws. Despite the anomaly of the initial positive effect followed by a downward trend in the hybrid model of Table 3b, the Lott model also suggests that murder falls with shall-issue adoption. While Table 3 provides the first support for some drops in violent crime (for rape and murder), it still gives no consistent evidence of any decline in robbery, which is the one area in which one would most expect the deterrent impact proposed by Lott and Mustard to show up if it were in fact important.

<sup>59.</sup> To ensure that the different shall-issue law coding schemes mentioned in footnote 24 are not driving the results, Appendix Table 3 replicates the Table 3 results using Lott's shall-issue law coding. The results are substantially similar.

	Violent			Aggravated		Property	Auto		
Time Period (1977-1999)	Crime	Murder	Rape	Assault	Robbery	Crime	Theft	Burglary	Larceny
1. Dummy variable model:	-3.4%	-1.7%	-7.2%	-4.1%	-1.2%	1.8%	4.5%	-2.1%	2.8%
	(1.6%)	(2.0%)	(1.6%)	(2.1%)	(2.1%)	(1.0%)	(1.9%)	(1.4%)	(1.0%)
2. Lott spline model:	-0.5%	-1.0%	-0.8%	-0.7%	-1.3%	0.2%	0.3%	-0.1%	0.1%
	(0.4%)	(0.5%)	(0.4%)	(0.5%)	(0.5%)	(0.2%)	(0.4%)	(0.3%)	(0.2%)
3. Hybrid model:									
Postpassage dummy	-2.7%	1.5%	<u>-6.9%</u>	-4.4%	3.3%	1.3%	3.5%	-0.2%	1.3%
	(1.8%)	(2.3%)	(2.1%)	(2.3%)	(2.5%)	(1.2%)	(2.4%)	(1.7%)	(1.2%)
Trend effect	-0.4%	-1.0%	-0.4%	-0.4%	-1.5%	0.1%	0.0%	-0.1%	0.1%
	(0.4%)	(0.5%)	(0.4%)	(0.5%)	(0.5%)	(0.2%)	(0.4%)	(0.3%)	(0.2%)

Notes: The dependent variable is the in(crime rate) named at the top of each column. The data set is comprised of annual state-State- and year- fixed effects are included in all specifications. All regressions are weighted by state population. Standard errors (in parentheses) are computed using the Huber-White robust estimate of variance. Coefficients that are significant at the .10 level are underlined. Coefficients that are significant at the .05 level are displayed in bold. Coefficients that are significant at the .01 level are both underlined and displayed in bold. level observations (including the District of Columbia).

Ex. 13 Page 224 HeinOnline -- 55 Stan. L. Rev. 1228 2002-2003

	Violent			Aggravated		Property	Auto		
Time Period (1977-1999)	Crime	Murder	Rape	Assault	Robbery	Crime	Theft	Burglary	Larceny
Dummy variable model:	-1.7%	2.4%	-4.5%	-4.6%	4.3%	2.7%	2.4%	2.1%	2.9%
	(1.5%)	(2.3%)	(1.6%)	(1.8%)	(2.4%)	(1.2%)	(2.1%)	(1.5%)	(1.1%)
Hybrid model:									
Postpassage dummy	-4.3%	5.4%	-5.4%	<u>-8.6%</u>	4.8%	2.8%	4.5%	2.2%	2.8%
	(1.7%)	(2.4%)	(1.8%)	(2.1%)	(2.5%)	(1.2%)	(2.1%)	(1.5%)	(1.2%)
Trend effect	2.2%	-2.5%	0.7%	3.4%	-0.5%	-0.1%	-1.8%	-0.1%	0.1%
	(0.4%)	(0.7%)	(0.5%)	(0.6%)	(0.7%)	(0.3%)	(0.6%)	(0.4%)	(0.3%)

TABLE 3B: THE ESTIMATED IMPACT OF SHALL-ISSUE LAWS ON CRIME CONTROLLING FOR STATE TRENDS

regressions are weighted by state population. Standard errors (in parentheses) are computed using the Huber-White robust estimate of variance. Coefficients that are significant at the .10 level are underlined. Coefficients that are significant at the .05 level are displayed in bold. Coefficients that are significant at the .01 level are both underlined and displayed in bold.

The question merits investigation: Why do the results of the Lott model (Table 3) support the Lott thesis more than the results of the Zheng model (Table 2)? The reason turns out to be somewhat surprising. As Table 2 documents, the two models include some different explanatory variables that one might think could have important implications. For example, Lott controls for population density and transfer payments to the poor, while Zheng controls for police, poverty, unemployment, and alcohol consumption. But these differences in the substantive controls turn out to be largely unimportant. Interestingly, as we will discuss in the next section, what drives the entire difference between Tables 2 and 3 is that Lott includes a large number of potentially duplicative demographic variables. Indeed, the array is so extensive as to make multicollinearity a serious issue.<sup>60</sup> Specifically, while Zheng's model controls for percent black and three age groupings, Lott's has thirty-six separate demographic percentages, breaking down each of three different race categories-black, white, and neither black nor white-and both sexes into six separate age categories from age ten up. The sensitivity of the results to the inclusion or exclusion of an array of highly collinear demographic variables serves as a cautionary tale to those who conduct or rely upon panel data models of crime. Probably no one examining either Wentong Zheng's work or that of Lott and Mustard would suspect that conclusions reached from their models would be sensitive to these seemingly second-order demographic controls.

## C. Sensitivity of the Lott Results to Time Period and Inclusion of Demographic Controls

The results so far suggest that both a sparse model with no controls other than state and year fixed effects, as well as a reasonable specification developed for another context (thereby eliminating the possibility of data mining to achieve a desired result) undermine the Lott and Mustard more guns, less crime hypothesis. At the same time, though, their own specification generates some superficially supportive results in that murder and rape rates seem to be lower after passage of shall-issue laws. We say "superficially supportive" because a number of factors, including the failure of the model to show a drop in robbery, cast doubt on the causal story that they advance.

The first problem with the Lott story—as suggested in various graphs of Figure 1—is that during the 1990s crime in non-shall-issue states fell far more than in shall-issue states. Indeed, when we limit our focus to the thirteen states that adopted shall-issue laws between 1991 and 1999 as shown in Table 4, we see that the Lott story does not hold up during this period. The only robust

<sup>60.</sup> Multicollinearity exists when right-hand side variables are strongly (positively or negatively) correlated with each other. *See* GREENE, *supra* note 13, at 266.

finding we can see in Table 4 across both panels is that three of the four property crime categories are higher with shall-issue law adoption.<sup>61</sup>

We have already alluded to the second problem with the Lott story: The results are incredibly sensitive to the inclusion of various seemingly unimportant demographic controls. The first line of Table 5a shows the results from estimating a dummy variable model using all of Lott's explanatory variables (again replacing arrest rate with the incarceration rate), except his large array of demographic controls. Here the results look very much like Table 1, line 2 in showing that shall-issue laws are associated with (statistically significantly) higher rates of crime in every category but murder. In other words, the similarity of the Table 5a, line 1 results and the Table 1, line 2 results suggests that the addition of the incarceration rate, state population and density, the controls for transfer payments, and real per capita state income essentially had no effect on the story depicted in the various graphs of Figure 1 (that shall-issue laws were associated with more crime). Each successive line of Table 5a augments the model estimated in line 1 by adding additional demographic controls. By the time the full array of demographic controls is entered, five of the eight strong positive estimates of line 1 have flipped the sign used by Lott and Mustard to become negative (although only violent crime and rape have become statistically significant in that direction). Even the crime categories, such as property, auto theft, and larceny, which retain their positive signs, still drop very sharply in size. For example, the estimated increase in auto theft from adopting a shall-issue law is 24.1% in line 1 of Table 5a, but only 4.5% in line 9.

What explains the enormous sensitivity of Lott's results to the inclusion of minor demographic controls? Table 5b gives insight into this question by replicating the analysis of Table 5a, while controlling for fixed state trends. Now we see much closer agreement with the results as we move down the nine regressions listed. Apparently, then, Lott and Mustard's thirty-six demographic variables mimic time trends in crime that we can control for directly with our controls for state trends.

<sup>61.</sup> We also replicated this Table using the alternative coding of the shall-issue laws shown in Appendix Table 1, column A. The resulting Appendix Table 4 yields quite similar results, except that rape is robustly negative in the two panels.

	Violent		c	Aggravated	D . LL	Property	Auto Theta	Dunction	Tomocard
Time Period (1991-1999)	Crime	Murder	Kape	Assault	kobbery	Crime	1 neu	burgiary	Larceny
1. Dummy variable model:	0.6%	7.1%	-2.8%	-2.1%	9.0%	4.1%	7.3%	<u>4.6%</u>	3.0%
	(2.3%)	(3.5%)	(1.8%)	(3.0%)	(2.4%)	(1.5%)	(3.0%)	(1.5%)	(1.4%)
2. Lott spline model:	0.2%	0.3%	0.2%	-0.1%	0.3%	1.0%	1.5%	1.2%	0.4%
4	(1.2%)	(1.8%)	(1.2%)	(1.5%)	(1.3%)	(0.7%)	(1.5%)	(0.8%)	(0.7%)
3. Hybrid model:									
Postpassage dummy	0.9%	5.9%	-0.5%	-1.0%	5.6%	4.1%	8.9%	4.1%	3.3%
	(2.2%)	(4.1%)	(2.2%)	(3.0%)	(2.9%)	(1.9%)	(3.9%)	(2.0%)	(1.7%)
Trend effect	0.1%	-0.4%	0.2%	0.0%	-0.3%	0.5%	0.6%	0.7%	0.1%
3	(1.2%)	(1.8%)	(1.3%)	(1.6%)	(1.3%)	(0.7%)	(1.5%)	(0.9%)	(0.7%)

State- and year- fixed effects are included in all specifications. All regressions are weighted by state population. Standard errors (in parentheses) are computed using the Huber-White robust estimate of variance. Coefficients that are significant at the .10 level are underlined. Coefficients that are significant at the .05 level are displayed in bold. Coefficients that are significant at the .01 level are both underlined and displayed in bold. level observations (including the District of Columbia).

Ex. 13 Page 228 HeinOnline -- 55 Stan. L. Rev. 1232 2002-2003

	Violent			Aggravated		Property	Auto		
Time Period (1991-1999)	Crime	Murder	Rape	Assault	Robbery	Crime	Theft	Burglary	Larceny
Dummy variable model:	-0.1%	1.0%	-0.4%	-2.0%	3.0%	3.6%	8.6%	2.1%	3.1%
	(1.9%)	(4.3%)	(1.8%)	(2.5%)	(2.4%)	(1.7%)	(3.2%)	(1.8%)	(1.7%)
Hybrid model:									
Postpassage dummy	-0.3%	2.3%	2.1%	-2.3%	1.4%	4.4%	9.7%	2.0%	4.1%
	(2.1%)	(4.5%)	(2.2%)	(2.7%)	(2.6%)	(1.9%)	(3.4%)	(1.9%)	(1.8%)
Trend effect	0.4%	-2.0%	4.0%	0.4%	2.5%	-1.2%	-1.8%	0.2%	-1.5%
,	(1.4%)	(2.5%)	(1.5%)	(1.9%)	(1.6%)	(1.0%)	(1.9%)	(1.2%)	(1.0%)

All Standard errors (in parentheses) are computed using the Huber-White robust estimate of variance. Coefficients that are significant at the .10 level are underlined. Coefficients that are significant at the .05 State- and year- fixed effects are included in all specifications. level are displayed in bold. Coefficients that are significant at the .01 level are both underlined and displayed in bold. level observations (including the District of Columbia). regressions are weighted by state population.

# Apr. 2003]

TABLE 4B: THE ESTIMATED IMPACT OF SHALL-ISSUE LAWS ON CRIME CONTROLLING FOR STATE TRENDS

	Time Period (1977-1999)	Violent			Aggravated		Property	Auto		
	Controls Used:	Crime	Murder	Rape	Assault	Robbery	Crime	Theft	Burglary	Larceny
:	Every nondemographic control	<u>9.3%</u> (1.8%)	-0.1% (2.5%)	<u>6.4%</u> (2.0%)	<b>4.1%</b> (1.8%)	<u>16.5%</u> (2.6%)	<u>15.4%</u> (1.6%)	<b>24.1%</b> (2.9%)	<u>13.4%</u> (2.0%)	<u>15.0%</u> (1.5%)
5.	<ul><li>(1) + demographic groups for bl males 10-19 &amp; w males 10-19</li></ul>	<u>8.1%</u> (1.8%)	0.6% (2.4%)	<u>4.9%</u> (1.8%)	<b>3.7%</b> (1.9%)	<u>13.9%</u> (2.3%)	<u>12.7%</u> (1.4%)	<mark>20.7%</mark> (2.6%)	<u>10.6%</u> (1.7%)	<u>12.4%</u> (1.3%)
э.	(2) + demographic groups for bl males 20-29 & w males 20-29	<u>4.9%</u> (1.7%)	0.3% (2.4%)	2.9% (1.9%)	0.4% (1.9%)	<u>9.9%</u> (2.2%)	<u>9.7%</u> (1.2%)	<u>18.3%</u> (2.5%)	<u>6.6%</u> (1.6%)	<u>9.5%</u> (1.2%)
4.	(3) + demographic groups for bl males 30-49 & w males 30-49	<u>2.9%</u> (1.5%)	-2.0% (2.3%)	-0.7% (1.5%)	-0.3% (1.8%)	<u>6.9%</u> (2.1%)	<mark>7.0%</mark> (1.0%)	<u>13.7%</u> (2.2%)	<u>4.7%</u> (1.5%)	<u>6.7%</u> (1.0%)
5.	(4) + demographic groups for bl females 10-49 & w females 10-49	2.4% (1.6%)	-0.9% (2.1%)	<b>-3.0%</b> (1.5%)	-0.2% (1.9%)	<u>6.8%</u> (2.2%)	<u>7.4%</u> (1.1%)	<u>16.6%</u> (2.3%)	<u>4.4%</u> (1.5%)	<u>7.2%</u> (1.0%)
6.	(5) + demographic groups for neither bl nor w males 10-49	1.3% (1.6%)	0.2% (2.0%)	<u>4.4%</u> (1.5%)	-0.9% (2.1%)	<b>5.4%</b> (2.2%)	<u>5.9%</u> (1.1%)	<u>13.7%</u> (2.1%)	1.7% (1.4%)	<u>6.2%</u> (1.1%)
7.	(6) + demographic groups for neither bl nor w females 10-49	-0.7% (1.5%)	-0.9% (2.0%)	<u>-5.3%</u> (1.5%)	-2.7% (2.0%)	3.2% (2.1%)	<u>4.5%</u> (1.0%)	<u>12.0%</u> (2.0%)	0.5% (1.4%)	<u>4.8%</u> (1.0%)
×.	(7) + demographic groups for bl males over 50 & w males over 50	-1.3% (1.5%)	-1.3% (2.0%)	<u>-5.5%</u> (1.6%)	-2.3% (2.0%)	1.4% (2.1%)	$\frac{3.7\%}{(1.0\%)}$	<u>10.8%</u> (2.1%)	-0.5% (1.4%)	<mark>4.0%</mark> (0.9%)
9.	All variables	<b>-3.4%</b> (1.6%)	-1.7% (2.0%)	<u>-7.2%</u> (1.6%)	<u>-4.1%</u> (2.1%)	-1.2% (2.1%)	<u>1.8%</u> (1.0%)	<b>4.5%</b> (1.9%)	-2.1% (1.4%)	<u>2.8%</u> (1.0%)
Not	es: The dependent variable is the	e ln(crime	rate) named	at the top	of each colu	mn. The dat	ta set is com	prised of an	nual state-lev	el observatio

(including the District of Columbia). State- and year- fixed effects are included in all specifications. All regressions are weighted by state population. Standard errors (in parentheses) are computed using the Huber-White robust estimate of variance. Coefficients that are significant at the .10 level are underlined. Coefficients that are significant at the .05 level are displayed in bold. Coefficients that are significant at the .01 level are both underlined and displayed in bold. The previously excluded demographic variables that are added into the line 9 regressions are black females over age 50, white females over age 50, neither black nor white females over age 50, and neither black nor white males over age 50.

TABLE 5A: THE ESTIMATED IMPACT OF SHALL-ISSUE LAWS ON CRIME STATE DATA, 1977-1999 (VERNICK'S CODING), USING INCARCERATION RATES

Ex. 13 Page 230 HeinOnline -- 55 Stan. L. Rev. 1234 2002-2003

STATE DATA, 1977-1999 (VERNICK S CODING), USING INCARCERATION KATES ADDING ON DEMOGRAPHIC CONTROLS TO DUMMY VARIABLE MODEL

	Time Period (1977-1999)	Violent			Aggravated		Property	Auto		
0	Controls Used:	Crime	Murder	Rape	Assault	Robbery	Crime	Theft	Burglary	Larceny
Γ.	Every nondemographic control	1.1%	1.9%	-3.7%	-0.8%	4.6%	2.5%	4.7%	2.1%	2.3%
		(1.5%)	(2.1%)	(1.5%)	(1.7%)	(2.4%)	(1.1%)	(2.1%)	(1.6%)	(1.1%)
	(1) + demographic groups for bl	0.0%	0.2%	-3.7%	-2.4%	4.0%	<u>2.0%</u>	4.2%	1.8%	1.7%
-	males 10-19 & w males 10-19	(1.5%)	(2.1%)	(1.5%)	(1.7%)	(2.4%)	(1.1%)	(2.1%)	(1.6%)	(1.2%)
)	(2) + demographic groups for bl	-0.2%	0.0%	-3.8%	-2.5%	3.7%	1.9%	3.9%	1.7%	1.6%
-	males 20-29 & w males 20-29	(1.5%)	(2.2%)	(1.4%)	(1.6%)	(2.5%)	(1.2%)	(2.2%)	(1.5%)	(1.2%)
-	(3) + demographic groups for bl	0.9%	1.9%	-2.8%	-1.6%	5.9%	3.0%	3.6%	2.8%	2.8%
1	males 30-49 & w males 30-49	(1.6%)	(2.2%)	(1.5%)	(1.7%)	(2.5%)	(1.2%)	(2.2%)	(1.6%)	(1.2%)
~	(4) + demographic groups for bl	0.4%	0.9%	-3.4%	-1.8%	5.5%	3.4%	3.9%	3.5%	3.2%
	females 10-49 & w females 10-49	(1.5%)	(2.2%)	(1.4%)	(1.6%)	(2.5%)	(1.2%)	(2.2%)	(1.6%)	(1.1%)
$\sim$	(5) + demographic groups for	0.6%	1.5%	-3.2%	-2.1%	6.2%	3.7%	4.3%	3.7%	3.4%
-	neither bl nor w males 10-49	(1.5%)	(2.3%)	(1.5%)	(1.6%)	(2.5%)	(1.2%)	(2.2%)	(1.6%)	(1.1%)
)	(6) + demographic groups for	0.1%	0.6%	-3.6%	-2.5%	5.7%	3.0%	3.8%	2.8%	3.0%
-	neither bl nor w females 10-49	(1.5%)	(2.2%)	(1.5%)	(1.6%)	(2.6%)	(1.2%)	(2.2%)	(1.5%)	(1.1%)
-	(7) + demographic groups for bl	-0.5%	0.6%	-3.4%	-2.8%	4.4%	2.8%	2.7%	2.2%	3.1%
-	males over 50 & w males over 50	(1.4%)	(2.3%)	(1.5%)	(1.7%)	(2.4%)	(1.1%)	(2.1%)	(1.5%)	(1.1%)
7	All variables	-1.7%	2.4%	4.5%	-4.6%	4.3%	2.7%	2.4%	2.1%	2.9%
		(1.5%)	(2.3%)	(1.6%)	(1.8%)	(2.4%)	(1.2%)	(2.1%)	(1.5%)	(1.1%)

Notes: The dependent variable is the in(crime rate) named at the top of each column. Ine data set is comprised of annual state-revel observations (including the District of Columbia). State- and year- fixed effects are included in all specifications. All regressions are weighted by state population. Standard errors (in parentheses) are computed using the Huber-White robust estimate of variance. Coefficients that are significant at the .10 level are underlined. Coefficients that are significant at the .05 level are displayed in bold. Coefficients that are significant at the .01 level are both underlined and displayed in bold. The previously excluded demographic variables that are added into the line 9 regressions are black females over age 50, white females over age 50, neither black nor white females over age 50, and neither black nor white males over age 50. Assume, though, that Lott's decision to include thirty-six demographic controls is excessive and that the appropriate number of these highly collinear variables is some smaller number. For example, since men commit over ninety percent of the violent crime, perhaps it would be enough to control only for the percent of white males in each of six separate age groupings above ten as well as the percentage of black males in each of these six age categories, for a total of twelve demographic controls. One could follow Lott in including additional controls for the percentage of white or of black females in each of these six age groupings for an additional twelve more controls, but each black male category is already highly correlated with each corresponding black female category and the same correlation applies for whites as well. There is little justification for going even further in following Lott by putting in the percentage of the population that is neither white nor black in twelve other age-sex categories.

Table 6 mimics the Lott and Mustard model of Table 3, but limits the demographic controls from Lott's thirty-six to the six age categories above age ten for white males and the same six age categories for black males—a total of twelve controls. Now it is hard to find *any* crime category that seems to have a robustly lower crime rate across the various models of panel A and panel B. Larceny and property crime seem to be robustly higher with the adoption of shall-issue laws, and rape may be lower (although the results including state trends are mixed for this crime). Overall, the results seem much more supportive of a *positive* impact on crime after the adoption of shall-issue laws than a negative one. Perhaps one can argue that it is important to include all of the other twenty-four highly collinear demographic variables to return to the (already weak) results of Table 3, but results that are sensitive to inclusion or exclusion of these marginal control variables must be treated with skepticism.

	Violent			Aggravated		Property	Auto		
Time Period (1977-1999)	Crime	Murder	Rape	Assault	Robbery	Crime	Theft	Burglary	Larceny
I. Dummy variable model:	1.0%	-0.5%	-3.7%	-1.1%	3.8%	<u>6.0%</u>	13.3%	3.0%	5.7%
	(1.5%)	(2.2%)	(1.5%)	(1.9%)	(2.2%)	(1.1%)	(2.4%)	(1.5%)	(1.0%)
Lott spline model:	-0.5%	-0.5%	-0.1%	-0.7%	<u>-1.5%</u>	0.0%	0.7%	-0.1%	-0.2%
	(0.4%)	(%9.0)	(0.4%)	(0.5%)	(0.5%)	(0.3%)	(0.5%)	(0.4%)	(0.2%)
. Hybrid model:									
Postpassage dummy	0.6%	<u>6.8%</u>	-5.9%	-1.5%	6.0%	3.7%	9.8%	2.8%	2.6%
	(1.7%)	(2.6%)	(2.0%)	(2.1%)	(2.6%)	(1.2%)	(2.7%)	(1.8%)	(1.2%)
Trend effect	-0.5%	-0.9%	0.3%	-0.6%	-1.9%	-0.2%	0.1%	-0.3%	-0.3%
	(0.4%)	(0.6%)	(0.4%)	(0.5%)	(0.5%)	(0.3%)	(0.5%)	(0.4%)	(0.3%)

TABLE 6A: THE ESTIMATED IMPACT OF SHALL-ISSUE LAWS ON CRIME

level observations (including the District of Columbia). State- and year- fixed effects are included in all specifications. All regressions are weighted by state population. Standard errors (in parentheses) are computed using the Huber-White robust estimate of variance. Coefficients that are significant at the .10 level are underlined. Coefficients that are significant at the .05 level are displayed in bold. Coefficients that are significant at the .01 level are both underlined and displayed in bold.

	STATE DATA, B	1977-1999 ILACK AND	(VERNICK) WHITE M	C'S CODIN	g), Using I r Age 10 A	NCARCERA S DEMOGR	TION RATE APHIC CON	s Includii trols	NG ONLY	t
	Time Period (1977-1999)	Violent Crime	Murder	Rape	Aggravated Assault	Robbery	Property Crime	Auto Theft	Burglary	Larceny
	Dummy variable model:	0.0%	1.8%	<u>-2.9%</u> (1.5%)	-2.4%	<u>4.5%</u> (2.3%)	<u>3.0%</u> (1.1%)	2.6% (2.0%)	<u>2.5%</u> (1.5%)	<u>3.2%</u> (1.1%)
		(0/)	(0/7:7)				~	~ /	,	
2	Hybrid model: Postnassage dummy	-2.3%	3.2%	-3.9%	-5.9%	4.7%	2.9%	3.9%	<u>3.0%</u>	2.7%
	1 usipusuge amino	(1.6%)	(2.3%)	(1.6%)	(1.9%)	(2.4%)	(1.1%)	(2.0%)	(1.6%)	(1.1%)
	Trend effect	2.3%	-1.3%	1.1%	3.4%	-0.3%	0.0%	-1.3%	-0.4%	0.4%
		(0.5%)	(0.7%)	(0.5%)	(0.6%)	(0.7%)	(0.3%)	(0.6%)	(0.4%)	(0.3%)

TABLE 6B: THE ESTIMATED IMPACT OF SHALL-ISSUE LAWS ON CRIME CONTROLLING FOR STATE TRENDS

estimate of variance. Coefficients that are significant at the .10 level are underlined. Coefficients that are significant at the .05 regressions are weighted by state population. Standard errors (in parentheses) are computed using the Huber-White robust level observations (including the District of Columbia). State- and year- fixed effects are included in all specifications. All Notes: The dependent variable is the ln(crime rate) named at the top of each column. The data set is comprised of annual statelevel are displayed in bold. Coefficients that are significant at the .01 level are both underlined and displayed in bold.

Ex. 13 Page 234 HeinOnline -- 55 Stan. L. Rev. 1238 2002-2003

Since the Figure 1a-1f graphs, the Table 1 regressions, the Table 5 regressions with the nondemographic controls, and the Table 6 regressions with twelve of Lott's demographic controls all show a largely *positive* relationship between shall-issue law adoption and crime over the 1977-1999 period, it becomes important to ascertain what about the demographic controls is capturing some true phenomenon that would reverse our assessment of the crime effects generated by shall-issue laws. Ordinarily, if the inclusion of an explanatory variable has such a dramatic effect on one's estimate, one would explore whether the estimated effect of that explanatory variable makes sense. In this case, though, the 36 demographic controls are so highly collinear that it is impossible for the regression to provide meaningful results for any given demographic control.<sup>62</sup> Thus, all we are left with is a set of estimated effects on the demographic controls resembling output from a random number generator, and the knowledge that, for some unknown reason, these demographic controls matter considerably. We can think of no reason to prefer the regressions in Table 3, which might be deemed to provide weak support to Lott and Mustard, to those in Table 6, which are devastating to the more guns, less crime thesis.<sup>63</sup>

While virtually the only evidence shown so far that offers even weak support for the Lott and Mustard thesis is Table 3, there is a statistical argument that even these results inaccurately overstate their statistical significance. A recent comment by Willard Manning of the University of Chicago suggests that when, as here, one is estimating nine different regressions, one should make

Consequently, there is a very severe multicollinearity problem that makes it impossible to interpret any of the coefficients on the individual demographic controls (although ordinarily one would not expect this to invalidate the estimated shall-issue effects). Indeed, when one looks at the estimated effects of a change in the demographic variables on violent crime from the Table 3 regressions, one finds utterly bizarre results. For example, an increase of one percentage point in the percentage of black males aged 30-39 would be expected to almost double the violent crime rate, while a similar increase in the percentage of black males aged 40-49 would lead to a drop in violent crime of 60%. Similarly, increasing the percentage of black males aged 50-64 would cause violent crime to jump by 145%, but increasing the percentage of black males over age 65 would lead to a 78% decline in violent crime. These nonsense results prevent us from understanding why the demographic controls can influence the estimates of shall-issue adoption so strongly.

63. In response to the claim that the unpredictable estimated impacts of the demographic controls suggest model misspecification, Lott explains that "[w]hat I have tried to do is 'overcontrol' for all possible demographic factors to make sure that any effects attributed to the right-to-carry laws are not arising because I have accidentally left out some other factor." LOTT, *supra* note 2, at 144. Ironically, the effects that Lott attributes to these laws occur not because he has left out any demographic variable, but primarily because he has put them in.

<sup>62.</sup> For example, if one regresses the percent black, age 10 to 19 on the other demographic controls, the R-squared value is 0.9999. R-squared is a measure of the "goodness of fit," that is, how accurately the regression was able to account for movements in the dependent variable. *See* GREENE, *supra* note 13, at 152. An R-squared of 99% would indicate that the regression right-hand side variables were able to explain 99% of the variance in the left-hand side variable.

adjustment for the fact that multiple comparisons are being made in assessing statistical significance:

Failure to correct for multiple comparisons causes the true significance level to be much less than the nominal level would suggest. If there are seven comparisons, then a nominal 5 percent standard applied to each is actually more like a 30 percent standard. The former is usually considered statistically significant if it is met, while the latter is considered statistically insignificant, and not noteworthy unless one is looking for a null finding.<sup>64</sup>

Correcting for the presence of these multiple comparisons and for autocorrelation in crime across years, Manning found that the estimates of the effect of shall-issue laws "should have t statistics that are about 50-60 percent of their reported value. With such a correction, the results [estimating the effect of a shall-issue law] appear to be statistically significantly different from zero about as often as one would expect if they had occurred at random."65 Again, this is not a ringing endorsement of the more guns, less crime hypothesis. If one were to apply both such corrections to the Table 3 results, there would simply be no evidence of any crime-reducing benefit associated with the adoption of shall-issue laws. We ran the regressions of Table 3 using the Bhargava correction for serial correlation, which did greatly reduce the statistical significance of the resulting estimates. In none of the five sets of regressions was there any statistically significant effect for robbery or any property crime, and only for row 2 in Table 3b (the hybrid model for state trends) do we see anything more than isolated significance. In that model, violent crime and aggravated assault appear to rise, while rape and murder appear to fall.

# D. Problems with Unequal Years of Data from Early and Late Adopters in the Pre-Post Comparison

Thus far our regressions have attempted to generate an estimate for the impact of shall-issue laws that are aggregated across all adopting states. Essentially, we are implementing a difference-in-difference design, in which we compare how crime changes over time in states that do not have shall-issue laws (or that had such laws prior to our data period) to how crime changes in states that adopt shall-issue laws during our data period. An important aspect of this investigation is to select "before" and "after" periods for pre-post comparisons, and thus far we have used the entire 1977-1999 panel to calculate the aggregated pre-post difference for shall-issue states. This can create problems, though, because shall-issue laws were adopted as early as 1985 and

<sup>64.</sup> Willard Manning, Comment to Donohue, supra note 52, at 331, 334.

<sup>65.</sup> *Id.* at 335. A "t-statistic" is a measure of the statistical significance of an individual regression coefficient. *See* GREENE, *supra* note 13, at 161. A reduction in the absolute value of a t-statistic would indicate that the coefficient (and therefore the effect of the variable in question) was less statistically significant.

as late as 1996, which implies that for some states, observations from nineteen years before adoption to eighteen years after adoption would be used to form a pre-post contrast. This can be potentially problematic when a very limited sample of states in the very early and very late years may be tilting the regression line unduly. Table 7 shows the periods of time for which pre- and postpassage data is available for the twenty-five adopting jurisdictions (in Vernick's coding). It should be immediately apparent that while every state except Maine is used in identifying the effect on crime over the period from eight years prior to three years after passage, only three states are entering into the estimate of crime nineteen years before passage: Texas, Kentucky, and South Carolina. Thus, Texas will disproportionately influence the populationweighted prepassage effect because it will be counted in fifteen more yearly observations than the Maine observations during the same prepassage period. Conversely, in the postpassage period, Florida, Georgia, and Pennsylvania, all fairly populous states, will influence the postpassage estimates more highly both by virtue of their substantial populations and because they will have from ten to twelve yearly observations that will be averaged into the postpassage effect compared with the mere three years of observations appearing for Texas, Kentucky, and South Carolina.

One crude way to show that this factor is important is to simply "dummy out" the periods more than eight years prior to or more than three years after the passage of a shall-issue law.<sup>66</sup> This approach essentially will enable the estimates of the aggregated effect of shall-issue laws on crime to come primarily from the twelve-year span that begins eight years prior to passage and extends through the year of passage to the three years after passage. Table 8 shows the results of this exercise. The bottom line seems to be that rape may decrease with the adoption of shall-issue laws (the results are somewhat mixed across the two panels), but larceny and burglary rates are robustly larger. Indeed, if one accepts the estimates from the hybrid model controlling for state trends, shall-issue laws would be deemed (at least by the third year) to have *increased* violent crime, aggravated assault, robbery, property crime, burglary, and larceny. There is one negative estimate: rape. Its effect is not statistically significant in the third year, however, and it turns positive in the fourth.<sup>67</sup>

<sup>66.</sup> To "dummy out" is to add separate dummy controls—here, for years more than eight years before or three years after shall-issue adoption. By creating individual dummy variables for the stated years before and after adoption, we are ensuring that our overall estimates of the impact of the law will not be influenced by observations from those years.

<sup>67.</sup> Appendix Table 5 replicates the Table 8 results using Lott's shall-issue law coding. The results are similar, but with two changes: The Appendix Table 5 results weaken the Lott and Mustard thesis since robbery is *positive* and statistically significant across both panels (as opposed to just in Panel B of Table 8), but the results strengthen the thesis in that rape is more robustly negative. Because of the difference in coding for Maine between Lott and us, we reran Table 8 using Lott's 1985 adoption date for Maine. The results were virtually identical (albeit somewhat stronger for the view that shall-issue laws increase crime in Panel B, while generating statistically significant (0.05 level) reductions in rape and aggravated

TABLE 7: YEARS AND STATES INCLUDED IN THE 1977-1999 TIME PERIOD RELATIVE TO SHALL-ISSUE LAW ADOPTIO	STATE DATA (VERNICK'S CODING OF THE SHALL-ISSUE DUMMY)
-----------------------------------------------------------------------------------------------------	--------------------------------------------------------

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	Violent			Aggravated		Property	Auto		
Time Period (1977-1999)	Crime	Murder	Rape	Assault	Robbery	Crime	Theft	Burglary	Larceny
Dummy variable model:	-2.7%	0.6%	-5.1%	-4.1%	2.2%	<u>2.0%</u>	4.3%	-0.4%	2.5%
	(1.7%)	(2.0%)	(1.8%)	(2.1%)	(2.2%)	(1.1%)	(2.1%)	(1.4%)	(1.1%)
Lott spline model:	-0.1%	-0.5%	0.5%	-0.7%	-0.4%	0.4%	0.2%	<b>%6</b> .0	0.2%
	(0%9.0)	(0.7%)	(0.5%)	(0.7%)	(0.7%)	(0.3%)	(0.6%)	(0.4%)	(0.3%)
Hybrid model:									
Postpassage dummy	-2.4%	1.3%	-7.2%	<u>-4.1%</u>	3.8%	1.3%	3.4%	-0.2%	1.3%
	(1.9%)	(2.3%)	(2.1%)	(2.3%)	(2.6%)	(1.3%)	(2.5%)	(1.6%)	(1.3%)
Trend effect	0.0%	-0.6%	0.8%	-0.6%	-0.5%	0.3%	0.1%	0.9%	0.2%
	(0.6%)	(0.7%)	(0.5%)	(0.7%)	(0.7%)	(0.3%)	(0.6%)	(0.4%)	(0.3%)

estimate of variance. Coefficients that are significant at the .10 level are underlined. Coefficients that are significant at the .05 level are displayed in bold. Coefficients that are significant at the .01 level are both underlined and displayed in bold.

Ex. 13 Page 239 HeinOnline -- 55 Stan. L. Rev. 1243 2002-2003

	Violent			Aggravated		Property	Auto		
Time Period (1977-1999)	Crime	Murder	Rape	Assault	Robbery	Crime	Theft	Burglary	Larceny
1. Dummy variable model:	-1.5%	3.5%	-4.4%	-4.8%	5.4%	3.0%	3.1%	3.0%	3.0%
	(1.6%)	(2.4%)	(1.6%)	(1.9%)	(2.5%)	(1.2%)	(2.2%)	(1.5%)	(1.2%)
2. Hybrid model:									
Postpassage dummy	-4.5%	4.8%	<u>-6.0%</u>	-8.8%	4.8%	2.8%	4.2%	2.0%	2.8%
	(1.7%)	(2.5%)	(1.8%)	(2.1%)	(2.6%)	(1.2%)	(2.2%)	(1.6%)	(1.2%)
Trend effect	3.4%	-1.5%	1.8%	<u>4.6%</u>	0.7%	0.3%	-1.3%	1.1%	0.2%
	(0.5%)	(0.9%)	(0.6%)	(0.7%)	(0.9%)	(0.4%)	(0.8%)	(0.5%)	(0.4%)

Notes: The dependent variable is the ln(crime rate) named at the top of each column. The data set is comprised of annual statelevel observations (including the District of Columbia). State- and year- fixed effects are included in all specifications. All regressions are weighted by state population. Standard errors (in parentheses) are computed using the Huber-White robust estimate of variance. Coefficients that are significant at the .10 level are underlined. Coefficients that are significant at the .05 level are displayed in bold. Coefficients that are significant at the .01 level are both underlined and displayed in bold.

. .
To underscore the message that the observations from the adopting states at the two ends of the time span, either well before passage or well after passage, are causing mischief when estimating a single aggregated impact of the shallissue laws, it may be helpful to illustrate this point graphically. Figures 3a through 3i plot the estimated annual effect on various crime categories for every year from nineteen years prior to adoption to seventeen years after adoption.<sup>68</sup> Figure 3b shows the results for murder and it should immediately be apparent that the period from eight years before to three years after passage evidences relatively little movement in this crime category (as the Table 8 regression results would suggest). However, outside this time frame (especially in the last years), one sees large swings in the estimated effect of shall-issue laws on murder. Of course, the thought that shall-issue laws caused crime to drop by almost twenty-five percent in the thirteenth year after passage and then caused it to increase by almost twenty-one percent in the fourteenth year is obviously untenable. These wild swings are caused not by any true impact of shall-issue laws but by the selective dropping out from the individual year estimates of states that adopted the law more recently, leaving only the shrinking number of earliest adopters to identify the particular annual impact. Whether one looks at murder, aggravated assault, rape, or robbery, the final year estimate at fourteen years after passage always swings wildly-sometimes up, sometimes down-and the earliest prepassage estimates also show erratic movements (note the jump in the first year for aggravated assault in Figure 3d). Another way to get at this same problem of unbalanced panel data is simply to undertake a disaggregated analysis, which estimates different shall-issue impacts for individual states. We will undertake just this analysis when we later turn to analyze the county data, but first we will address an issue that we previously set aside.

assault, and increases in property crime, auto theft, and larceny in line 1 of Panel B). Running the same regressions for the period from four years prior to adoption to three years after adoption (thereby capturing all 25 of the adopting states under Vernick's coding) yields comparable results.

<sup>68.</sup> In other words, rather than simply having a single postpassage dummy, as in our customary dummy variable model, we now define a separate dummy variable for each individual year before or after adoption of a shall-issue law, and then plot these individual estimates in Figures 3a through 3i, with the effect of the law normalized to equal zero in the year of passage (year 0).



1246



Ex. 13 Page 242 HeinOnline -- 55 Stan. L. Rev. 1246 2002-2003





Ex. 13 Page 243 HeinOnline – 55 Stan. L. Rev. 1247 2002-2003



Ex. 13 Page 244 HeinOnline – 55 Stan. L. Rev. 1248 2002-2003





Ex. 13 Page 245 HeinOnline – 55 Stan. L. Rev. 1249 2002-2003





Ex. 13

Page 246 HeinOnline -- 55 Stan. L. Rev. 1250 2002-2003



Ex. 13 Page 247 HeinOnline -- 55 Stan. L. Rev. 1251 2002-2003

FIGURE 3G: AUTO THEFT—NORMALIZED EFFECT BY YEAR RELATIVE TO ADOPTION (VERNICK'S CODING)



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Ex. 13 Page 249 HeinOnline – 55 Stan. L. Rev. 1253 2002-2003



FIGURE 31: LARCENY-NORMALIZED EFFECT BY YEAR RELATIVE TO ADOPTION (VERNICK'S CODING)

HeinOnline -- 55 Stan. L. Rev. 1254 2002-2003

## E. Problems with Endogenous State Adoption

A major concern of any analysis on the effect of shall-issue laws on crime is the possibility that crime correlates with shall-issue law adoption in a noncausal manner. Indeed, examining the graphs of Figure 3, one can see that in four of the five violent crime categories and for burglary, even before adopting states passed their shall-issue legislation, crime was substantially higher than the regression model would have otherwise predicted (given the full array of explanatory variables). This raises concerns about the reliability of the regression model, because the higher crime rate in the prepassage years may suggest that the laws were passed because crime was higher. That would imply that shall-issue law adoption is, in fact, endogenous to the crime rate. Lott is aware of this problem and indeed confirms it in his book, noting that shall-issue laws "have so far been adopted by relatively low-crime states in which the crime rate is rising."<sup>69</sup> He attempts to use the appropriate two-stage least squares (2SLS) technique to address the problem, and he states that his "results differ from all my previous estimates in one important respect: Nondiscretionary concealed-handgun laws are associated with large, significant declines in all nine categories."<sup>70</sup>

As Willard Manning and others have emphasized, the problem is that the statistical demands for the successful use of the 2SLS approach are exacting, and it is unlikely that Lott has satisfied them since his instruments cannot be considered convincingly exogenous.<sup>71</sup> Lott creates his instrumental variable by regressing the presence of a shall-issue law on violent and property crime rates and the change in those rates, percent of state population in the National Rifle Association (NRA), percent of state population voting for Republican presidential candidate, percent of blacks and whites in state population, total state population, dummies for the South, Northeast, and Midwest, and year dummies.<sup>72</sup>

The manner in which Lott performed his 2SLS analysis suggests that he considers his main instruments to be the violent and property crime rates and

71. As Manning notes:

Manning, supra note 64, at 335 n.75.

<sup>69.</sup> LOTT, supra note 2, at 120.

<sup>70.</sup> Id. at 118.

The major requirements for the instrumental variables in the linear model to yield consistent estimates of the effect of the endogenous explanatory variable on the outcome of interest are the instruments correlated with the endogenous explanatory variable(s); the instruments do not conceptually belong in the equation of interest nor are they proxies for variables which should be in the equation of interest but are omitted from the specification; the instrument is uncorrelated with the error term in the equation of interest; and the instruments are not weak in the sense of Staiger and Stock (1997) or Bound and others (1995).

<sup>72.</sup> LOTT, supra note 2, at 118.

the change in those crime rates. However, these variables are obviously endogenous and cannot serve as unbiased instruments. His NRA and Republican presidential election variables are the most compelling, but these too are at least potentially endogenous. These variables correlate with factors such as an area's wealth and criminal tendencies, which in turn affect crime directly. We attempted to replicate Lott's 2SLS results using state-level data to determine how much of an effect the first-stage crime rate variables were having on the final results.<sup>73</sup>

Table 9, line 1 presents our replication of Lott's 2SLS estimates for the 1977-1992 time period. According to this method, shall-issue law adoption reduces violent crimes by 61%, murders by 43%, rapes by 20%, robberies by 51%, and aggravated assaults by 64%.<sup>74</sup> While these numbers should seem implausibly large, we were gratified to see that we had basically succeeded in replicating Lott's 2SLS results. While the excessively large predicted effects have led a number of researchers to dismiss these results immediately, we go on to show that the results are entirely driven by the inclusion of the inappropriate instruments: the logs of violent and property crime. Line 2 of Table 9 presents the results of the identical 2SLS regressions of line 1, except excluding the logs of crime rates as instruments. Notice that some of the most extreme negative coefficients from line 1 drop drastically. For example, the coefficient estimated for violent crime drops from -0.94 to -0.09, and that for aggravated assault drops from -1.18 to -0.12. The dramatic changes in the 2SLS estimates when the most flawed instruments are dropped indicates, as we suggested above, that this endogenous set of instruments was severely biasing

<sup>73.</sup> While it is unclear exactly how Lott performed these regressions, it appears that he instrumented the shall-issue law dummy using only the variables mentioned above, deviating from standard 2SLS procedure by failing to include all of the exogenous variables used in the second stage of the regressions. If he did include all the exogenous variables, then one would question introducing the regional dummy variables and the racial demographic percentages into the first-stage equation given the existing state dummy variables and extensive demographic controls. However, since excluding the exogenous variables from the first-stage regression can possibly bias the estimates, we avoided replicating this potential inaccuracy. Thus, there are a few differences between our regressions and Lott's. First, because we were unable to obtain NRA membership data, we were forced to leave this variable out. Fortunately, as we were able to roughly approximate Lott's results, this omission proved not to be a substantial problem. Second, instead of including the percentage of blacks and whites in the state population and the region dummies, we used the array of demographic variables and state dummies that Lott used in the second stage of his 2SLS regressions. Accordingly, our first stage consisted of regressing the shall-issue law dummy on the vector of demographic variables, the log of violent and property crime rates, the changes in the log of violent and property crime rates, and a vector of presidential election variables. This final vector was created in the exact way Lott created his: interacting the percentage of state population voting Republican with dummy variables for a four-year period around the election year. Since our effort is at replication, we also used Lott's coding of the shall-issue laws.

<sup>74.</sup> The precise estimated percentage effect on crime is given by the formula  $e^{B}$ -1, where B is estimated in Table 9. For values under about 0.2 in absolute value, B is a fairly good approximation of the proportionate change.

the results. We then went one step further and performed another set of regressions that excluded the change in the crime rates as instruments, thereby leaving the matrix of political variables as the only remaining instrument. As shown in line 3 of Table 9, this approach generated estimates that shall-issue laws would strongly dampen murder, robbery, and aggravated assault rates.

The next three lines of Table 9 replicate these three 2SLS regressions for the 1977-1997 period. While the Lott approach to line 4 of Table 9 continues to show large drops in violent crime, rape, and aggravated assault, the line 4 numbers are much less favorable to the Lott and Mustard thesis than the line 1 estimates for murder, robbery, and all four property crimes. But while the drops in violent crime and aggravated assault resulting from the shall-issue laws that are estimated in line 4 are once again vastly too large to be plausible, the second and third lines calculated without clearly inappropriate instruments alter the results dramatically. Indeed, when the flawed instruments are dropped in lines 5 and 6, the estimated effect on crime of shall-issue law adoption is never significantly different from zero. Accordingly, the evidence from 2SLS estimation, in our view, offers no support for the more guns, less crime hypothesis.<sup>75</sup>

<sup>75.</sup> We are not claiming that the instruments used in our line 5 or 6 2SLS estimates meet the criteria for valid instruments set forth above. *See supra* note 71. Indeed, the political variable serving as the instrument in lines 3 and 6 is quite weak, raising the R-squared value in a first-stage regression by only 0.006 (from 0.815 to 0.821). We are confident, however, that the other instruments used by Lott that are dropped in the third regression for each time period in Table 9 profoundly violate these standards.

TABLE 9: TWO-STAGE LEAS	T SQUARES	ESTIMATE	ES OF THE	IMPACT OF S	SHALL-ISSI	JE LAWS, S	TATE DA1	A (LOTT'S (	Coding)
	Violent Crime	Murder	Rape	Aggravated Assault	Robbery	Property Crime	Auto Theft	Burglary	Larceny
Time Period (1977-1992) Shall Dummy Instruments:									
1. PV, CR, ACR	<b>-0.94</b> (0.17)	<b>-0.56</b> (0.14)	<b>-0.22</b> (0.09)	<b>-1.18</b> (0.21)	- <b>0.71</b> (0.16)	-0.03 (0.05)	<u>-0.23</u> (0.12)	- <b>0.17</b> (0.07)	0.01 (0.04)
2. PV, ΔCR	-0.09 (0.08)	<u>-0.43</u> (0.16)	-0.04 (0.10)	-0.12 (0.12)	<b>-0.33</b> (0.14)	-0.02 (0.06)	-0.15 (0.14)	-0.12 (0.09)	-0.01 (0.06)
3. PV	<u>-0.15</u> (0.08)	<b>-0.37</b> (0.16)	-0.01 (0.11)	<b>-0.25</b> (0.12)	<b>-0.32</b> (0.15)	0.1 (0.07)	0.01 (0.14)	-0.00 (0.09)	<u>0.11</u> (0.06)
Time Period (1977-1997) Shall Dummy Instruments:									
4. PV, CR, ACR	<b>-0.68</b> (0.11)	-0.12 (0.10)	<b>-0.26</b> (0.08)	-1.04 (0.16)	-0.11 (0.10)	<u>0.21</u> (0.05)	<u>0.29</u> (0.10)	0.08 (0.06)	<u>0.19</u> (0.05)
5. PV, ΔCR	0.15 (0.11)	-0.02 (0.13)	-0.12 (0.10)	0.12 (0.12)	0.15 (0.14)	0.06 (0.07)	0.17 (0.15)	-0.10 (0.09)	0.05 (0.06)
6. PV	-0.04 (0.10)	-0.05 (0.15)	-0.17 (0.11)	-0.12 (0.12)	0.04 (0.15)	0.10 (0.07)	0.21 (0.17)	-0.08 (0.10)	0.10 (0.06)
Key to Dummy Instruments: PV=Pol	litical Variabl	les CR=log	(Crime Ra	tes) $\Delta CR=Ch$	ange in log((	Crime Rates)			
Notes: The dependent variable is the State- and year- fixed effects are inc computed using the Huber-White ro significant at the .05 level are display	e In(crime ra luded in all s bust estimate /ed in bold. (	te) named a pecification of variance Coefficients	t the top o s. All regr e. Coeffic that are sig	of each column ressions are we ients that are s inificant at the	I. The data sighted by sta significant at .01 level are	set is compri- ate populatio the .10 leve both underlir	ised of ann n. Standard l are under ned and disp	dal state-level d errors (in pa lined. Coeffi blayed in bold	observations. irrentheses) are cients that are

1258

STANFORD LAW REVIEW

[Vol. 55:1193

Ex. 13 Page 254 HeinOnline -- 55 Stan. L. Rev. 1258 2002-2003

### III. MOVING FROM THE STATE TO THE COUNTY DATA

Up to this point we have focused exclusively on analyzing state data, which enabled us to use more current crime data—up through 1999. Lott and Mustard decided, however, to rest their analysis most heavily on county crime data rather than state crime data (although they do present some state data results as well).<sup>76</sup> The advantages of this approach are that (1) with over 3000 counties in the country as opposed to only fifty states, there is far more data with a county dataset than with a state dataset, which, *ceteris paribus*, should improve the precision of the estimates; and (2) the county fixed effects will explain a great deal of the fixed cross-sectional variation in crime across the country, thereby diminishing the inevitable problem of omitting some appropriate, but possibly unavailable, time-invariant explanatory variables.

There are also some disadvantages to using county data. First, the intervention of interest is generally a statewide phenomenon.<sup>77</sup> Using county data under these circumstances will likely exaggerate the amount of independent data available to the researcher, thereby possibly creating the appearance of statistical significance when in fact none exists.<sup>78</sup> Second. many of the explanatory variables are only measured on the state level, and, thus again, the county data analysis may be giving a false sense of precision. Third, Lott uses arrest rates (the ratio of arrests to crime in a county) as an explanatory variable, which leads to many counties being dropped from the analysis. This occurs because of missing arrest data in some cases, and also because the arrest rate is undefined for any county that experiences no crime in a particular category in a particular year (since the rate would have zero in the denominator in such cases). Thus, a substantial number of counties are thrown out of the Lott analysis by virtue of the realization of the dependent variable (if it is zero in a given year, that county is dropped from the analysis), which can potentially bias the results of the regression estimation.<sup>79</sup> Fourth, Maltz and Targonski

<sup>76.</sup> See, e.g., Lott & Mustard, supra note 1, at 55 (showing state-data-based estimates).

<sup>77.</sup> But not uniformly. Pennsylvania initially excluded Philadelphia from its 1989 shall-issue law. PA. STAT. ANN. tit 18, § 6109 (West 1989). In 1995, the law was extended to include Philadelphia. *See* 1995 Pa. Legis. Serv. 17 (West).

<sup>78.</sup> Indeed, when we reran both Lott's and our own specifications, clustering on the state level, we found dramatically lower statistical significance for the estimated impacts. The cluster procedure allows the regression to relate the variability across counties within a state for more refined estimation of the variance-covariance matrix. *See* STATA USER'S GUIDE 258-59 (Release 6). However, to be both conservative and consistent with Lott, all the regressions reported in this paper do not cluster by state.

<sup>79.</sup> The percentage of dropped observations (because of either missing numerators or zero denominators) vary for the individual crime categories, from a low of about 9% for auto-theft to a high of about 48% for murder. Fifty-seven percent of county/year observations have at least one of the arrest rates undefined or missing.

consider the quality of UCR county-level data to be so poor that they dismiss Lott's work on that basis alone (at least if the data extend beyond 1992).<sup>80</sup>

By using our state data, we were able to extend our analysis through 1999, while our county dataset is only updated through 1997. But because there are some advantages to using county data and because Lott and Mustard rely most heavily on this data, we turn for the rest of this Article to an analysis of county data.

A. Lott's County Data Analysis for 1977-1992

### 1. *The dummy variable model.*

We begin by presenting Lott's initial regression results for county level data over the period from 1977-1992. Line 1 of Table 10 shows the predicted effect on nine crime categories using the dummy variable model. A quick examination of the line 1 results reveals that (1) four of the five categories of violent crime (the exception is robbery) have negative and statistically significant coefficients, suggesting that shall-issue laws reduce these types of violent crime by four to seven percent; and (2) all four property crimes have positive and statistically significant coefficients, suggesting that the laws increase property crime by two to nine percent. Lott accepts the regression results at face value and concludes that the passage of these laws causes criminals to shift from committing violent crime to committing property crime where, he argues, they are less likely to be shot since the victim is frequently not present when the crime occurs.<sup>81</sup> Thus, we see violent crime decreasing by 3.5% and murders falling by over twice that percentage, while property crime rises by over 5%. As we stressed in our 1999 review of Lott's work, however, the fact that robbery is not dampened by the adoption of a shall-issue law constitutes a major theoretical problem for Lott's interpretation of the results of the dummy variable model.<sup>82</sup> If there is to be the type of substitution away from violent crime that Lott predicts, one would expect that the new law would induce potential robbers to avoid confronting victims and shift to more stealthy property crime. Yet, as Table 10 reveals, we see no evidence of this effect.

- 81. Lott & Mustard, supra note 1, at 18.
- 82. Ayres & Donohue, supra note 15, at 461.

<sup>80.</sup> Michael Maltz & Joseph Targonski, *A Note on the Use of County-Level UCR Data*, 18 J. QUANTITATIVE CRIMINOLOGY 297 (2002). Although in this portion of the Article we focus on county data for the period 1977-1997, we generally find, with some noted exceptions, broadly similar results when we either use the state dataset or confine the county results to the period not beyond 1992. We also note that to the extent, however unlikely, that the post-1992 break in the county crime data series to which Maltz and Targonski refer is relatively uniform across all counties, the year effects would control for this change. See *infra* note 90.

Hence, the dummy variable model undermines a key prediction that Lott offers to explain the Table 10, line 1 regression results for the period of 1977-1992.

In table 4.1 of his book,<sup>83</sup> Lott presents a version of the line 1 robbery regression showing that shall-issue laws reduce robbery by 2.2%, which is indicated to be statistically significant at the 0.10 level (considered marginally significant). But we have shown in previous work that this -2.2% figure is an error that results from a miscoding of the effect of the shall-issue laws. The problem with Lott's analysis was that, instead of following his articulated strategy of assuming that the effect of the law would emerge in the first year after passage, Lott coded the shall-issue law in that fashion only for Florida and Georgia, with all other states being coded so that the effect of the law begins in the year of passage. Correcting this error to adhere consistently to the articulated Lott protocol wipes out the size and significance of the estimated effect on robbery.<sup>84</sup>

<sup>83.</sup> LOTT, supra note 2, at 51.

<sup>84.</sup> Ayres & Donohue, *supra* note 15, at 451. We replicated Lott precisely with the coding error and then showed how the correction eliminates the robbery effect. The line 1 regressions in Table 10, *supra*, are identical to Lott's table 4.1 results with three exceptions, which are maintained in all the regressions presented here: (1) the coding error is corrected; (2) standard errors are corrected to adjust for heteroscedasticity; and (3) as noted *supra* note 51, one explanatory variable—the measure of the real per capita income maintenance, SSI and other, for those over sixty-five—was dropped. One can compare the results in table 1 of Ayres & Donohue, *supra* note 15, with those of Table 10 here to see that the only one of these changes that influences the basic story is the correction for the coding error.

	Crime	Murder	Rape	Assault	Robbery	Crime	Theft	Burglary	Larceny
ott's Time Period (1977-1992)									
1. Dummy variable model:	<u>-3.5%</u> (1.2%)	<u>-7.4%</u> (2.5%)	<u>4.7%</u> (1.5%)	<u>-5.3%</u> (1.6%)	-0.2% (1.9%)	<u>5.2%</u> (1.1%)	<u>8.7%</u> (2.0%)	<b>2.3%</b> (1.1%)	<u>5.9%</u> (1.9%)
2. Lott spline model:	-0.4% (0.5%)	<b>-4.7%</b> (1.1%)	-1.7% (0.6%)	0.5% (0.7%)	<b>-1.9%</b> (0.8%)	0.1% (0.7%)	0.1% (0.9%)	-0.4% (0.5%)	0.8% (1.4%)
3. Hybrid model: Postpassage dummy	<u>6.7%</u> (2.3%)	2.9% (4.9%)	<b>6.5%</b> (2.9%)	<mark>9.6%</mark> (3.0%)	-2.9% (3.2%)	0.2% (1.8%)	0.3% (2.9%)	-2.5% (1.9%)	0.3% (3.0%)
Trend effect	<u>-2.0%</u> (0.8%)	<u>-5.4%</u> (1.5%)	<u>-3.2%</u> (0.9%)	<u>-1.7%</u> (1.0%)	-1.2% (1.1%)	0.0% (0.6%)	0.0% (1.2%)	0.2% (0.6%)	0.8% (1.2%)
nitre Time Period (1977-1997)					:				
4. Dummy variable model:	0.0% (1.1%)	<u>-7.7%</u> (1.7%)	<u>-3.2%</u> (1.1%)	-0.3% (1.3%)	-0.3% (1.3%)	<u>7.6%</u> (0.8%)	<u>10.8%</u> (1.5%)	<u>1.6%</u> (0.9%)	<u>9.4%</u> (1.2%)
5. Lott spline model:	<mark>-1.6%</mark> (0.2%)	<u>-2.7%</u> (0.5%)	<u>-2.7%</u> (0.4%)	<u>-2.7%</u> (0.4%)	<u>-3.6%</u> (0.4%)	<u>-0.4%</u> (0.2%)	<b>-0.8%</b> (0.4%)	<u>-2.6%</u> (0.3%)	-1.1% (0.4%)
5. Hybrid model: Postpassage dummy	0.0%	6.9%	5.9%	5.9%	3.6%	-0.7%	<u>9.0%</u>	4.3%	5.3%
Trend effect	(1.4%) - <b>1.6%</b> (0.3%)	(2.9%) <u>-3.5%</u> (0.7%)	(2.1%) <u>-3.4%</u> (0.5%)	(2.3%) - <b>3.4%</b> (0.6%)	(2.3%) <u>-4.1%</u> (0.6%)	(1.1%) - $0.3\%$ (0.2%)	(2.4%) <u>-1.9%</u> (0.6%)	(1.7%) -3.1% (0.4%)	(2.1%) <u>-1.7%</u> (0.5%)

significant at the .05 level are displayed in bold. Coefficients that are significant at the .01 level are both underlined and displayed in bold.

1262

## STANFORD LAW REVIEW

Ex. 13

Page 258 HeinOnline -- 55 Stan. L. Rev. 1262 2002-2003

### 2. Lott's trend (or spline) model.

Lott responds to the point that shall-issue laws seem not to dampen robberies in the dummy variable model by arguing that a model that captures the change in the linear trend of crime, the Lott spline model, provides a better picture of the effect of the passage of a shall-issue law in this case. The only numbers that Lott reports in his book concerning his trend analysis are found in a single row of numbers representing the difference between the before-passage linear trend and after-passage linear trend for the states that passed shall-issue laws (hereinafter "passing states") (appearing in the book's table 4.8). Lott's regressions include year effect dummies, so the pre- and postpassage trend coefficients would capture linear movements in crime in the ten passing states apart from the general movements in crime for the nation as a whole (which would be captured by the general year dummies). Lott's message in his table 4.8 is that a trend analysis shows that shall-issue laws lower all crime categories—both violent and property—and in all cases but one (larceny) the reduction is statistically significant.<sup>85</sup>

Line 2 of Table 10 follows Lott's lead in testing whether there is a break in the linear crime trend in the year of passage.<sup>86</sup> We use a spline specification that is somewhat easier to interpret (than Lott's difference in pre- and posttrend test) because the statistical significance of any induced change in trend can be ascertained directly from the regression output (the t-statistic for the postpassage linear trend coefficient). Even though Lott did not use a spline

This Appendix Table 6.A2 replication of Lott still ostensibly finds that the law generates a statistically significant reduction in the time trends (at, at least, the five percent significance level) in five of the nine crime categories tested. But Lott's regressions also incorrectly identify the passage date of three jurisdictions that adopted shall-issue laws, which makes the laws look more effective than they are. Lott coded the enactment dates in Oregon, Pennsylvania, and Virginia earlier than was proper. Appendix Table 1 shows discrepancies in Lott's shall-issue coding in his own dummy variable and trend models (columns B and C). The corrected regression estimates are presented in Appendix Table 6.A3, which shows that the shall-issue laws statistically significantly reduce crime in only three of the nine categories (murder, rape, and robbery). *Id.* 

<sup>85.</sup> LOTT, supra note 2, at 76.

<sup>86.</sup> Panel A2 of Appendix Table 6 reproduces the methodology (although not the reported results) of the Lott trend analysis and reports both the before and after linear time trends (as well as their difference, which is presumed to be the estimated effect of the law). The discrepancies between Lott's table 4.8 and Appendix Table 6.A2 result from two different errors by Lott. First, Lott has informed us that he mistakenly wrote down the "Shall Trend After" coefficients. Second, Lott has erred in his reporting of the statistical significance of these effects, perhaps because he did not correct the standard errors for the presence of heteroscedasticity in the panel data. Lott reported that the change in trend owing to the shall-issue law for violent crime, aggravated assault, and auto theft was statistically significant at the level shown in the top row of Appendix Table 6. But Appendix Table 6.A2 shows, however, that the differences for these three crime categories were all insignificant.

specification himself, we call this the Lott spline model because it is equivalent to the linear trend model that he did employ in which he estimated pre- and postpassage trends and then subtracted them.<sup>87</sup>

Note that the story in line 2 of Table 10 is changed in a number of respects from that of line 1 (the dummy variable model). Instead of all violent crime (but robbery) falling and property crime rising, line 2 suggests that shall-issue laws have no effect on property crime (or overall violent crime and aggravated assault), but dampen murder, rape, and the heretofore unaffected robbery. Lott's discussion of the impact of shall-issue laws causing criminals to shift from committing violent to committing property crime is no longer central if the Lott spline analysis (Table 10, line 2) is the appropriate estimation approach.

## 3. The hybrid model testing for main and trend effects.

The different results between the dummy and trend models suggests the advisability of employing the more general hybrid model, which will enable us to test whether either of the more constrained models is statistically preferable. Consequently, we estimate regression 3 in Table 10, which is a less-constrained specification than either the dummy variable or the linear specification because it allows (and tests) for the existence of both a once-and-for-all announcement effect as well as a changed (linear) trend effect.<sup>88</sup> But for the four violent crime categories, we see a pattern that is potentially problematic-the main effect of the shall-issue laws is positive but over time this effect gets overwhelmed as the linear trend turns crime down. In other words, according to the hybrid model, in the year after passage the main effect of the shall-issue law is a 6.7% increase in violent crime, which is dampened by the 2% drop associated with the negative trend variable, for a net effect of 4.7% higher crime. After three and a half years, the conflicting effects cancel out at which point crime begins to fall. Our antennae are raised when we see this particular result of a positive main effect and a negative trend effect since there are empirical reasons to believe that this observed pattern is not truly reflecting an initial acceleration in crime followed by a drop, but rather model misspecification resulting from the selective dropping of states from the postpassage period in a way that tilts the trend regression line downward inappropriately.<sup>89</sup> This may suggest that even the most general form of the

<sup>87.</sup> For further explanation of this spline specification, see discussion *supra* text accompanying notes 42-50.

<sup>88.</sup> Contrary to the results of regression 1, the hybrid model confirms the prediction of regression 2 that the shall-issue laws have virtually no effect on any of the property crime categories (although we will soon see that this finding breaks down when we extend the analysis through 1997 in regression 6 of Table 10).

<sup>89.</sup> As noted above, if the results had been flipped with the main effect dampening crime and the time trend suggesting a longer term increase, one could interpret those results

three crime models is still misspecified and, hence, that its results are unreliable.

## B. Extending Lott's County Data Through 1997

Lott's initial analysis using 1977-1992 data captured the period in which only 10 states newly adopted shall-issue statutes, and therefore Lott's regression results should be taken as the predicted effect of the adoption of the law in these ten states. Between 1992 and 1996, however, fourteen more jurisdictions (thirteen states and Philadelphia) adopted the law (as shown in Column A of Appendix Table 1), and therefore one might hope to gain more accurate results by extending the period over which the effect of the law is estimated. Regressions 4-6 in Table 10 simply repeat the models of regressions 1-3 but now estimating them over the longer time period from 1977-1997 (and thus measure the effect of adoption of the law in twenty-three states and the city of Philadelphia).<sup>90</sup> Comparing (the dummy variable model in) lines 1 and 4 of Table 10, we see that adding more years of data weakens Lott's story. Importantly, violent crime is no longer negative, so the basic argument that the prospect of meeting armed resistance shifts criminals from violent crime to property crime is undermined. Lott might respond that murders fall by nearly eight percent and rape by over three percent, as murderers and rapists shifted over to committing property crime, thereby raising its prevalence by eight percent. But the suggestion that this pattern could be explained by the changed behavior of would-be murderers and rapists is not compelling.91

If the break in series caused a uniform jump up or down in crime that applied to all jurisdictions, then our year dummies would control for this problem. Unfortunately, it is generally unlikely that errors in crime data would be uniform across the country (or even random across the country); so the break in the series is a concern. *See supra* note 73 (discussing newer data).

91. Consider the case of Florida—one of the states that statistically is most conducive to the Lott story because the murder rate fell after the passage of a shall-issue law in 1987. Assume that every murder and rape that would have occurred but for the shall-issue law

in a straightforward manner: The announcement of the law scared potential criminals, thereby dampening crime initially, but as more guns got out on the street and/or as the fear subsided, crime ultimately turned up.

<sup>90.</sup> In the second edition of his book, Lott analyzes four additional years of data that allow him to test the effects of shall-issue laws in 13 additional states. LOTT, *supra* note 2, at 90. But he only reports results for this dataset from tests of the trend specification. It is important to emphasize that combining these later years of data with the original dataset is potentially problematic. The 1994 codebook for the (NACJD) crime data that both Lott and we use explicitly notes under a major heading, "Break in Series," that describes a new imputation procedure it will use from 1994 on and cautions: "These changes will result in a break in series from previous UCR county-level files. Consequently data from earlier year files should not be compared to data from 1994 and subsequent years...." Maltz & Targonski, *supra* note 80, at 309 (quoting FBI, UNIFORM CRIME REPORTING PROGRAM DATA [UNITED STATES]: COUNTY-LEVEL DETAILED ARREST AND OFFENSE DATA, 1994, ICPRS 6669).

Indeed, the idea that a thwarted rapist would decide to switch to property crime because rape had become more dangerous (to the perpetrator) seems rather fanciful. The issue is important for the following reason. The dummy variable model regression on the full 1977-1997 period gives very strong significant results on a number of crimes—murder and rape being negative and property crime, auto theft, and larceny being positive. Yet if the theoretical explanation for this substitution pattern in crime is flawed, then the fact that we cannot believe the regression finding of a large jump in property crime as an effect of the shall-issue law suggests that we should not believe the accompanying regression finding of a substantial drop in murders and rapes. Again, the possibility of model misspecification seems to be a serious concern.

Interestingly, while adding five years of data weakens Lott's argument based on the dummy model (Table 10, line 1 versus line 4), it actually strengthens his story using the spline analysis (compare lines 2 and 5 in Table 10). Thus, we see in line 5 that every crime except property crime is both negative and significant. Moreover, in contrast to both dummy variable models, the Lott spline estimated effect for robbery for both time periods is negative and significant—an almost indispensable finding if the Lott deterrence story is in fact true.

The added five years of data also has a considerable impact on the estimates generated by the hybrid model. For Lott's initial time period, the hybrid model (Table 10, line 3) basically suggested that shall-issue laws were not affecting property crime or robbery but were having the potentially problematic conflicting effects on four other violent crimes. For the full time period, however, the potentially troubling pattern previously observed for violent crime also shows up for property crime (Table 10, line 6). For example, in the year after passage the line 6 regressions indicate that shall-issue laws increase murders by over 3% (the main effect of 6.9%, less the trend effect of 3.5%). Rather than seeing shifting from violent to property crime, we observe similar patterns for a number of crimes in which early increases in crime are followed by subsequent drops after a number of years. The primary conclusion that emerges from these six regressions is that there is a considerable degree of instability in the predicted effects of shall-issue laws as one adds five extra years of data or switches among the two models presented by Lott or the third hybrid model which includes both a dummy variable and a time trend effect.

would have been committed by a different individual (that is, there were no multiple offenses for either of these crimes). This would imply that a total of 299 individuals did not commit a murder or rape (106 fewer murders, 193 fewer rapes) because of the law but that they each committed over 225 property crimes instead, thereby elevating property crime by 68,337. If a number of these rapes and murders represented multiple crimes by the same individual, then the number of property crimes that would have had to have been committed by each former rapist/murderer to generate this large property crime increase would rise commensurately. It seems unlikely that the shall-issue law could explain an increase in the amount of property crime of this magnitude.

Because the hybrid model nests both the dummy variable and the spline models, it is possible to test whether the data reject the implicit constraints imposed by these more-restrictive specifications. Specifically, we can test the spline specification's implicit assumption that there is no announcement effect by looking to see whether the dummy variable coefficient in the hybrid model is statistically different than zero. The data strongly reject the spline specification in six of the nine hybrid regressions (in specification 6). The dummy variable model analogously assumes that there is no spline effect. This assumption is also rejected in eight of the nine hybrid regressions.<sup>92</sup>

In sum, the foundation of the Lott thesis essentially is captured in regressions 1 (dummy variable model) and 2 (spline model) of Table 10. While these results are not identical to those presented in Lott's book, these regressions are probably more authoritative because some apparent coding errors by Lott have been corrected. The results are not as stable as one might like, but if one were to examine only those two regressions, the evidence would tend to support Lott's thesis. Obviously, the analyst's task would be easiest if the regressions generated by three different models (dummy, spline, hybrid) for two different time periods (1977-1992 and 1977-1997) all conveyed essentially the same picture. Unfortunately, they do not. Importantly, both the dummy variable and spline models are essentially rejected by the data by virtue of the large and statistically significant positive effects on both terms in the hybrid models (Table 10, lines 3 and 6)-particularly for the full dataset. But the hybrid model's prediction of initial jumps in crime followed by subsequent declines in response to the adoption of a shall-issue law raises our concern about model misspecification as states that had adopted shall-issue laws close to the end of our data period drop from the estimates of the late postpassage effects.

### C. Replicating Table 10 While Controlling for State Trends

We have previously discussed how one must take care to avoid generating statistically significant results that are really the spurious consequence of preexisting trends (for example, where an upward trend in crime is confused for a postpassage increase in crime). Table 11 replicates the Table 10 results while

<sup>92.</sup> It is interesting to compare the results of Table 3 (which uses state data for 1977-1999 and the incarceration rate instead of the arrest rate) with those of Tables 10 and 11 (which use county data for 1977-1997 and the arrest rate). Beginning with the dummy variable model and no state trends (Tables 3 and 10), the results are similar in each, although in the county data one sees stronger effects of dropping violent crime and increasing property crime. For the dummy model with state trends (Tables 3 and 11), though, the state results show drops in rape and aggravated assault and increases in auto theft and larceny, while the county results show only an *increase* in aggravated assault. When the hybrid model is used with state trends, the county results are clearly stronger for the Lott and Mustard thesis than the state data results—unless one is troubled by the positive main effects and negative trends found for the county data.

controlling for these state trends. The first thing to note as we compare the Table 10 and 11 results for the dummy variable model in 1977-1997 is that without controlling for state trends, the shall-issue laws were seen to increase crime in three property crime categories and decrease in two violent crime categories, while after adding such controls the only statistically significant effect is that aggravated assault increases. The hybrid results for the same period tend to show positive main effects followed by negative trend effects, which raises the specification concerns we expressed earlier in our discussion of the end of period drops in estimated crime effects seen in the graphs of Figure 3. If we take that pattern as accurate, though, then after two to three years shall-issue laws would reduce murders, rapes, robberies, auto theft, and burglaries (while increasing aggravated assaults and larceny).

Ex. 13 Page 264

	Violent		4	Aggravated	-	Property	Auto	-	
Lott's Time Period (1977-1992)	Crime	Murder	Kape	Assault	Kobbery	Crime	1 hett	Burglary	Larceny
1. Dummy variable model:	0.1%	-8.7%	-1.5%	3.4%	-7.5%	-1.4%	-1.2%	-3.6%	0.6%
	(1.6%)	(3.4%)	(2.1%)	(2.0%)	(2.2%)	(2.1%)	(2.2%)	(1.4%)	(4.5%)
2. Hybrid model:									
Postpassage dummy	<u>6.9%</u>	5.8%	5.5%	6.0%	<u>6.3%</u>	-0.1%	5.2%	1.1%	-3.2%
	(2.3%)	(5.3%)	(3.1%)	(3.0%)	(3.4%)	(1.9%)	(2.9%)	(2.0%)	(3.0%)
Trend effect	-3.1%	-6.6%	-3.2%	-1.2%	-6.3%	-0.6%	-3.0%	-2.2%	1.7%
	(0.8%)	(1.8%)	(1.1%)	(1.0%)	(1.3%)	(1.1%)	(1.2%)	(0.8%)	(2.5%)
Enitre Time Period (1977-1997)									
3. Dummy variable model:	1.5%	-0.2%	2.6%	7.1%	0.0%	-0.7%	3.5%	0.5%	4.0%
	(0%C.1)	(0% (0%)	(0/0.1)	(0/0/)	(1.0%)	(0/ C.1)	(0/.0.7)	(%C.1)	(0/(C.2)
4. Hybrid model:									
Postpassage dummy	0.7%	5.6%	6.6%	6.6%	5.2%	-1.5%	<u>6.6%</u>	4.4%	4.4%
	(1.5%)	(2.7%)	(2.0%)	(2.2%)	(2.2%)	(1.2%)	(2.2%)	(1.7%)	(2.1%)
Trend effect	0.5%	-3.9%	<u>-2.7%</u>	0.4%	-3.5%	0.5%	<u>-2.1%</u>	<u>-2.7%</u>	-0.3%
	(0.4%)	(0.8%)	(0.6%)	(0.6%)	(0.7%)	(0.4%)	(0.7%)	(0.5%)	(0.7%)

County- and year- fixed effects are included in all specifications. All regressions are weighted by county population. Standard errors (in parentheses) are computed using the Huber-White robust estimate of variance. Coefficients that are significant at the .10 level are underlined. Coefficients that are significant at the .05 level are displayed in bold. Coefficients that are significant at the .01 level are displayed in bold.

Apr. 2003]

Ex. 13 Page 265

HeinOnline -- 55 Stan. L. Rev. 1269 2002-2003

#### **IV. ESTIMATING STATE-SPECIFIC PASSAGE EFFECTS**

On the surface, the panel data regressions in Table 10 (and Table 11 for the hybrid model) appear to establish a prima facie case that shall-issue laws reduce crime (or, at least in the dummy variable model, reduce violent crime while increasing property crime). But all of the regressions presented so far have estimated an aggregated effect for the laws across all adopting states. It is well known, however, that aggregation can at times lead to misleading conclusions in statistical studies.93 For example, the model would be misspecified if one tried to estimate a uniform effect from the shall-issue law while the law had systematically different effects across states. Moreover, as we have noted, the dangers of estimating a single aggregated effect are particularly acute in this case because a state that adopts a shall-issue law early in the data period will contribute fully to the estimated postpassage effect, while a state that adopts near the end of the period will have little weight. Since we know that the late adopters tended to experience crime increases, the aggregated analysis will give less weight to these states in estimating the overall effects of shall-issue laws.<sup>94</sup> Thus, what might look like a changing effect over time from the passage of the law may simply be a compositional effect as certain states drop out of the analysis.

One way to avoid these aggregation and compositional biases is to change the specification to estimate a state-specific effect for each state that adopts a shall-issue law.<sup>95</sup> In other words, we include in our regression for each crime category a separate postpassage dummy for each adopting state (as opposed to a single postpassage dummy pertaining to all adopting states). Building on our previous dummy variable model with state fixed trends (Table 11), we now use the full 1977-1997 county dataset to estimate the effect on nine crime categories for twenty-four jurisdictions that adopted shall-issue laws—a total of 216 estimates. Table 12 presents all of these estimates for all nine crime

Ex. 13 Page 266

<sup>93.</sup> Simpson's Paradox, also sometimes referred to as aggregation bias, is just one such example. See P.J Bickel, E.A. Hammel & J.W. O'Connell, Sex Bias in Graduate Admissions: Data from Berkeley, 187 SCIENCE 398, 400-01 (1975) (showing that while aggregate data suggested bias against female graduate applicants, the disaggregated data by department reversed this conclusion).

<sup>94.</sup> If we run the Table 10 regression on just the years 1991 to 1997, we find statistically significant increases in all nine crime categories.

<sup>95.</sup> Black & Nagin, *supra* note 3, at 211, were the first to run a disaggregated regression estimating state specific impacts of the shall-issue law. However, their analysis was limited to Lott's initial dataset, which only allowed them to test for the impacts on 10 passing states. They reported substantial heterogeneity in the law's impact for a sample limited to large counties, but their results differ from ours in that (for their limited dataset) the state specific impacts were more beneficial, but the impacts tended to be less statistically significant. They also limited their analysis to the dummy specification—which may misestimate the true impact of the law (because of Lott's inverted V argument or some other misspecification). In Table 12, we include state fixed trends as an added control.

categories, while Figures 4 through 6 graphically depict the results for violent crime, murder, and property crime.<sup>96</sup> These state-specific "dummy variable" coefficients represent an even less-constrained specification than we saw in line 4 of Table 11 in that they separately estimate the impact of shall-issue laws for each of the twenty-four jurisdictions that passed the laws between 1977 and 1997. The Table 12 results reject the more constrained specifications of the aggregate regressions, which implicitly assumed that the impact of the shall-issue law was constant across jurisdictions. More importantly, the state specific estimates frequently undercut Lott's more guns, less crime thesis.

For every crime type there are more states where shall-issues laws produce a positive and statistically significant coefficient than states that produce a negative and statistically significant coefficient. For example, as Figure 4 reveals, while there are three states that experience a statistically significant drop in violent crime upon passage of the law, there are five states that experience a statistically significant increase. Overall, there are almost twice as many jurisdictions with an estimated increase in violent crime (fifteen) as those with an estimated decrease (nine).<sup>97</sup>

The second column of Table 12 and Figure 5 both reveal an even more dramatic disparity for murder: There are eight states with a statistically significant *increase* in murder while only four states exhibit a statistically significant decrease. Of the twelve jurisdictions that experienced a statistically significant effect on property crime from the passage of a shall-issue law, Figure 6 shows that eight experienced an *increase* in crime.

Stepping back, we see that out of the 216 estimated impacts in Table 12 (twenty-four jurisdictions by nine crime categories), sixty-eight exhibited statistically significant increases in crime while only twenty-seven exhibited statistically significant decreases. Overall, Table 12 shows 141 increases in crime versus only seventy-five decreases. The striking implication from this disaggregated analysis is that, according to this model, shall-issue laws increased crime in substantially more jurisdictions than they decreased crime.<sup>98</sup>

<sup>96.</sup> As noted above, Philadelphia is treated as a separate jurisdiction, because the law became effective in the city of Philadelphia at a different time than for the rest of Pennsylvania. For convenience, we will still refer to state-specific estimates in referring to these 24 jurisdictions. *See supra* note 77.

<sup>97.</sup> Figure 4 also shows the estimated effect for the corresponding aggregated model (analogous to line 4 of Table 11), which is 1.5% (albeit statistically insignificant).

<sup>98.</sup> The same story comes through if one uses the Table 12 analysis on state data. In that event, there are only 207 effects (nine regressions x 23 jurisdictions) because we do not treat Philadelphia separately in the state data regressions. We find that there are 37 significant increases in crime versus nine significant decreases. Overall, 122 of the 207 effects were positive versus 85 negative.

# TABLE 12: THE JURISDICTION-SPECIFIC ESTIMATED IMPACT OF SHALL-ISSUE LAWS ON CRIME, DUMMY MODEL CONTROLLING FOR STATE TRENDS IN CRIME, COUNTY DATA (LOTT'S CODING)

-				· · ·						•
Entire Period (1977-1997)	Violent Crime	Murder	Rape	Aggravated Assault	Robbery	Property Crime	Auto Theft	Burglary	Larceny	
Maine (1985)	<u>-15.1%</u> (5.6%)	17.7% (17.8%)	9.8% (8.8%)	<u>-23.5%</u> (7.0%)	-10.9% (9.3%)	<u>-6.3%</u> (3.4%)	-6.2% (5.1%)	<u>-17.6%</u> (4.0%)	-7.2% (4.5%)	
Florida (1987)	-12.3% (3.1%)	-30.9% (7.3%)	-9.1% (6.3%)	<b>11.9%</b> (5.1%)	<b>-15.4%</b> (7.6%)	-6.6% (7.5%)	9.4% (7.3%)	0.7% (5.7%)	18.7% (17.6%)	
Virginia (1988)	-1.7% (3.0%)	9.4% (8.1%)	6.9% (4.3%)	-1.3% (3.5%)	-4.4% (3.7%)	3.6% (2.9%)	4.2% (4.3%)	0.4% (2.3%)	2.4% (4.4%)	
Georgia (1989)	-4.5% (3.8%)	0.8% (7.7%)	<u>-14.8%</u> (5.1%)	1.4% (4.1%)	<b>-11.9%</b> (4.8%)	<u>-8.7%</u> (3.1%)	-6.5% (4.3%)	<u>-9.2%</u> (3.3%)	<u>-11.2%</u> (4.1%)	
Pennsylvania (1989)	0.7% (2.8%)	-2.9% (7.0%)	2.7% (3.3%)	1.3% (3.7%)	4.1% (3.6%)	<u>5.9%</u> (2.0%)	0.6% (3.7%)	<b>4.5%</b> (2.1%)	<b>6.1%</b> (2.7%)	
Philadelphia (1995)	9.0% (6.3%)	14.2% (10.3%)	3.7% (6.5%)	-0.6% (7.0%)	<u>16.2%</u> (9.6%)	-2.9% (9.8%)	10.7% (11.2%)	-7.1% (7.6%)	-4.3% (5.6%)	
West Virginia (1989)	<u>15.2%</u> (5.8%)	-0.8% (10.1%)	6.5% (8.8%)	<u>26.0%</u> (7.9%)	-9.8% (6.6%)	<u>8.1%</u> (2.9%)	<u>-17.8%</u> (4.8%)	<u>9.5%</u> (3.2%)	<b>9.5%</b> (4.1%)	
Idaho (1990)	5.5% (5.9%)	-4.2% (20.3%)	<u>23.9%</u> (6.7%)	5.6% (6.4%)	14.3% (9.4%)	3.6% (3.2%)	<b>10.7%</b> (5.1%)	-4.2% (4.1%)	1.0% (4.0%)	
Mississippi (1990)	<u>34.9%</u> (8.3%)	<b>20.5%</b> (8.0%)	11.6% (7.7%)	<u>30.5%</u> (9.7%)	<u>39.5%</u> (7.8%)	<u>11.7%</u> (6.9%)	<u>36.2%</u> (8.6%)	<u>18,8%</u> (7.0%)	6.2% (7.5%)	
Oregon (1990)	2.3% (5.0%)	<b>-24.0%</b> (9.6%)	-7.3% (5.0%)	<u>21.5%</u> (7.7%)	<u>-33.8%</u> (5.5%)	<u>-7,0%</u> (4.0%)	<u>-10.1%</u> (5.6%)	<u>-25.0%</u> (4.8%)	-4.5% (4.6%)	
Montana (1991)	17.7% (19.6%)	<b>-60.0%</b> (26.1%)	-29.3% (30.3%)	27.6% (20.7%)	-3.1% (29.6%)	1.0% (14.9%)	0.2% (15.2%)	2.7% (13.9%)	-2.6% (15.7%)	
Alaska (1994)	-0.8% (16.8%)	20.1% (25.5%)	-29.1% (19.7%)	8.0% (19.2%)	1.5% (14.6%)	10.3% (12.0%)	-4.4% (16.7%)	-3.4% (18.1%)	4.9% (10.4%)	
Arizona (1994)	<u>9.3%</u> (5.1%)	<u>19.3%</u> (7.2%)	8.2% (6.1%)	6.6% (5.9%)	<u>21.2%</u> (5.9%)	<b>10.2%</b> (4.3%)	<b>22.9%</b> (10.4%)	<u>18,4%</u> (4.8%)	<b>9.9%</b> (4.1%)	
Tennessee (1994)	<u>18,5%</u> (5.2%)	<u>20.9%</u> (6.2%)	<u>15.0%</u> (5.8%)	<u>25.6%</u> (6.2%)	<u>16.0%</u> (4.8%)	<u>11.2%</u> (3.7%)	<u>11.3%</u> (5.8%)	<u>13.5%</u> (4.1%)	<u>14.5%</u> (3.9%)	
Wyoming (1994)	-3.6% (7.6%)	12.6% (17.9%)	10.8% (10.3%)	-7.3% (9.8%)	<b>23.6%</b> (12.0%)	<b>7.4%</b> (3.6%)	<u>13.0%</u> (6.7%)	<u>15,3%</u> (5.6%)	2.9% (4.1%)	
Arkansas (1995)	1 <b>5.7%</b> (7.1%)	2.8% (8.1%)	<b>12.8%</b> (6.2%)	<u>26.8%</u> (9.2%)	6.5% (6.5%)	<u>-4.6%</u> (2.6%)	-0.8% (4.8%)	-4.5% (4.1%)	<u>5.9%</u> (3.1%)	
Nevada (1995)	<u>18.2%</u> (10.4%)	<u>42.6%</u> (13.1%)	11.6% (11.5%)	24.7% (15.9%)	<b>18.7%</b> (8.2%)	<b>13.3%</b> (5.5%)	<b>19.1%</b> (9.6%)	<u>25.9%</u> (7.2%)	<b>13.4%</b> (5.7%)	
North Carolina (1995)	<u>6.2%</u> (3.3%)	7.2% (5.6%)	5.3% (4.1%)	<u>13.9%</u> (4.0%)	4.5% (4.0%)	<b>-4.7%</b> (2.4%)	<u>19.7%</u> (3.5%)	<u>-5.0%</u> (3.0%)	<u>7.8%</u> (2.7%)	
Oklahoma (1995)	-1.4% (4.7%)	7.9% (8.4%)	<b>11.8%</b> (4.9%)	4.4% (5.2%)	<b>-9.5%</b> (4.6%)	-4.9% (3.3%)	<b>-10.1%</b> (4.6%)	-3.5% (3.8%)	4.7% (4.6%)	
Texas (1995)	<u>-11.6%</u> (3.7%)	<u>-18.3%</u> (4.6%)	0.8% (4.0%)	-6.5% (5.8%)	<u>-11.9%</u> (4.6%)	<u>-12.4%</u> (3.0%)	<u>-16.0%</u> (5.1%)	<u>-8.8%</u> (3.4%)	-3.0% (4.0%)	
Utah (1995)	<u>21.3%</u> (4.9%)	<u>34.1%</u> (10.0%)	1 <b>7.4%</b> (7.7%)	<u>25,1%</u> (5.7%)	<u>21.5%</u> (7.2%)	3.3% (3.4%)	<u>38.9%</u> (9.9%)	<u>23.1%</u> (5.1%)	-3.6% (4.1%)	
Kentucky (1996)	-2.8% (12.0%)	<u>43.4%</u> (13.8%)	-12.1% (7.7%)	-2.1% (20.6%)	<u>29.8%</u> (8.4%)	<b>-8.5%</b> (4.3%)	12.6% (17.1%)	-11.5% (8.5%)	<u>-14.2%</u> (3.7%)	
Louisiana (1996)	<u>14.2%</u> (7.7%)	<u>34.2%</u> (7.9%)	<u>21.7%</u> (7.2%)	13.9% (11.3%)	<u>31.9%</u> (11.0%)	<u>17.0%</u> (6.0%)	<u>32.2%</u> (8.7%)	<u>22.9%</u> (6.8%)	<b>13.4%</b> (6.8%)	
South Carolina (1996)	7.2% (5.3%)	<b>14.6%</b> (7.2%)	-2.9% (4.8%)	<b>11.8%</b> (5.9%)	<u>11.3%</u> (6.0%)	<b>8.2%</b> (3.5%)	<u>20.8%</u> (7.0%)	5.7% (3.6%)	3.0% (3.7%)	
Summary										
Negative & Significant	3	4	I	1	5	4	3	4	2	I
Negative & Not Significant	6	3	6	5	4	6	5	7	6	
Positive & Not Significant	10	9	11	9	7	6	8	4	9	
Positive & Significant	5	8	6	9	8	8	8	9	7	
Weighted Average Effect:	1.0%	0.6%	2.0%	7.2%	1.0%	-1.1%	4.7%	0.4%	4.0%	

Notes: Weighted Average Effect is calculated by weighting the state-specific coefficients by their average population over the time period. See also notes for Table 11.

Ex. 13

Page 268

FIGURE 4: IMPACT OF SHALL-ISSUE LAW ON VIOLENT CRIME JURISDICTION-SPECIFIC DUMMY MODEL WITH STATE TRENDS, COUNTY DATA



Ex. 13 Page 269 HeinOnline -- 55 Stan. L. Rev. 1273 2002-2003

FIGURE 5: IMPACT OF SHALL-ISSUE LAW ON MURDER JURISDICTION-SPECIFIC DUMMY MODEL WITH STATE TRENDS, COUNTY DATA





Apr. 2003] SHOOTING DOWN "MORE GUNS, LESS CRIME"

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How can it be that the disaggregated analysis in Table 12 is suggestive of crime increases while the aggregate model (Table 11, line 4) suggests statistically significant decreases for murder, rape, and robbery? First, note that weighting by population in the regression gives far greater influence in the regression to large states and that Texas and Florida (the two largest states) had large and statistically significant estimated drops in violent crime after they passed shall-issue laws.<sup>99</sup> As Table 10 indicated, the estimated aggregated effect on murder in the dummy variable model is a drop in crime of 7.7%. Running the aggregated regression without weighting by population lowers the estimated effect on murder from -7.7% to -5.1%. Hence, weighting clearly increases the apparent murder-reducing capacity of shall-issue laws in the aggregated dummy variable model, but it is not the entire story.

Second, as we have seen, the fact that a state adopts a shall-issue law earlier means that it will have a greater impact in the estimation of any postpassage dummy in the aggregated analysis. Thus, imagine a scenario under which only two states (with equal populations) adopt shall-issue laws—one in 1987 and another in 1996. Assume the effect in the two states is exactly opposite, in the early adopter crime *drops* by ten percent in the first year after passage and stays at that lower level through 1997, while in the late adopter crime *increases* by ten percent and will stay that way for ten years. In the disaggregated analysis, one will see equal and opposite impacts, suggesting no overall net effect on crime. This is also what the aggregated dummy variable analysis would show if the laws had been adopted at the same time. But the later adoption in the second state means that its impact will be diminished when the aggregated dummy variable model is estimated. Indeed, the aggregated

<sup>99.</sup> Even though one can interpret the coefficients on the individual state postpassage dummies as the percentage effect of the law on crime, one has to at least entertain the prospect that these estimates are picking up other changes in the states in question that happened to coincide with the passage of the shall-issue laws. This could happen for any state, but one that has been singled out on this ground is Florida because of the influx of roughly 125,000 largely male, uneducated, and young refugees from Cuba from 1980 through 1981 in the Mariel boatlift, which swelled crime in Florida in the early 1980s, presumably followed by at least some crime decline once the refugees had been fully integrated into the community (or removed from it). See David Card, The Impact of the Mariel Boatlift on the Miami Labor Market, 43 INDUS. & LAB. REL. REV. 245 (1990). Any such crime decline occurring after 1987 from this factor would be captured in the Lott analysis as the result of shall-issue law. Moreover, even if the effect of the Mariel boatlift had been completely dissipated by the time of passage as Lott has argued, the very sharp spike in crime that can be seen for Florida in figure 1(a) of Ayres and Donohue (1999) reveals that the prepassage fixed effect for Florida would be artificially elevated, biasing downward any estimated effect on the postpassage dummy. See John R. Lott, Jr., The Concealed Handgun Debate, 27 J. LEGAL STUD. 221, 232 (1998).

Of course, one cannot simply select the states that seemed to do well under the law for further evaluation, but it does suggest that some examination of whether there were any identifiable factors influencing crime in any of the states that appear to have large crime changes—whether positive or negative—at or around the time of the adoption of a shallissue law might be worthwhile.

effect in this hypothetical will be a drop in crime of nine percent because the ten years of a crime drop of ten percent will be averaged with the one year of the crime increase of ten percent.<sup>100</sup> As it turns out, two (large) early passing states (Florida and Georgia) experienced drops in murder, thus inordinately dragging down the estimated aggregate impact. But when we decouple the impact of the law on individual jurisdictions, a much different picture emerges.<sup>101</sup>

Lott might respond that these jurisdiction-specific dummy effects could understate the true impact of the law because his "inverted V" concern might operate on an individual state-by-state basis. While this specific concern is dampened somewhat by the inclusion of state-specific trends in our regressions, there is value in exploring whether the hybrid analysis is superior to the dummy variable model for the disaggregated analysis as it was for the aggregated analysis. Accordingly, we employed a disaggregated version of the hybrid specification, which estimates for each jurisdiction both an intercept effect and a trend effect. While only thirty percent of the estimated state-specific spline effects were statistically different than zero, we were able to reject in each of the nine crime type regressions the hypothesis that the twenty-four disaggregated spline effects were jointly equal. Thus, the regressions suggest that the implicit constraints of the disaggregated static model are once again too restrictive.

While we report the raw coefficients of these hybrid regressions in the Appendix,<sup>102</sup> Table 13 reports the net five-year impact of the law, annualized in order to facilitate comparison with the static model.<sup>103</sup> Turning to the

102. See Appendix Table 7.

103. To calculate the five-year impact of the shall-issue law under the hybrid specification it is necessary to add together the impacts of the intercept and trend terms for individual years and then sum the yearly impacts. For example, the predicted impact of a law for individual years is:

Page 273

<sup>100.</sup> Lott includes graphs in his second edition showing the distorting impacts of unbalanced datasets in estimating the impact of the law. LOTT, *supra* note 2, at 216. Note, too, that the regression will minimize the sum of the squared residuals, which implies that large outliers will be given disproportionate effect in aggregated models.

<sup>101.</sup> Our disaggregated results also substantially weaken the power of Lott and Bronars's geographic substitution result. Lott and Bronars use an aggregate specification to show that passage of the law caused crime to decrease in the passing states but increase in adjoining states because, they argue, of geographic substitution. Bronars & Lott, *supra* note 3, at 476-77. But this purported spillover result could simply be a byproduct of aggregation bias. Our disaggregated analysis demonstrates that passage of the law was likely associated with increases in many metro areas and with decreases in others. The Lott and Bronars story would only be true if crime fell on the shall-issue side of the metro border and rose on the non-shall-issue side, but nothing in their aggregated analysis would ensure this was the case. If the spillover regression were reestimated on a more disaggregated basis, we predict that most metro areas would show similar movements in crime in both the areas that were covered and uncovered by the law, which would be the exact opposite of the Lott and Bronars hypothesis of crime falling on one side of the border (in response to the shall-issue law) and rising on the other.

substance of the disaggregated hybrid specification captured in Table 13, we find a pattern that is remarkably consistent with that presented in Table 12. With the disaggregated hybrid specification, just as in the static model, more states experienced statistically significant increases in crime than statistically significant decreases in crime after passing shall-issue laws. Overall, seventeen of the twenty-four states report a net increase in violent crime, and twenty-one out of twenty-four showed an increase in assault. Only one state (Florida) showed a statistically significant drop in violent crime while six states showed statistically significant increases in violent crime. Similarly, while eight states experienced a statistically significant increase in assault, not one experienced a statistically significant decrease. In fact, as before, every crime category reports more increases than decreases in crime. For example, there are six states showing a statistical increase in murder while only four report a statistical decrease. The disparity is even greater for rape, with four states displaying a statistical increase and only one state reporting a statistical decrease. Overall 150 (of the 216) tests indicate that the shall-issue laws increased crime, with fifty-nine of these estimates being statistically significant (at a five percent level). At the same time, only seventeen of the 216 estimated effects demonstrated a statistically significant decrease-a ratio of more than three increases to every one decrease.<sup>104</sup>

We also tested whether this linear combination of regression coefficients was statistically different than zero and report the results of this testing in Table 13.

104. These results were qualitatively unaffected when we instead calculated the fouryear and the six-year annualized impact of the law. We also estimated the Table 13 results for state data, which generated 31 positive and statistically significant five-year annualized effects versus 18 negative and statistically significant effects. Overall, there were three more positive effects than negative effects, 105 vs. 102.

Year 1:1\*beta(shall dummy in state X) + 1\*beta(spline trend in state X)

Year 2:1\*beta(shall dummy in state X) + 2\*beta(spline trend in state X)

Year 3:1\*beta(shall dummy in state X) + 3\*beta(spline trend in state X)

Year 4:1\*beta(shall dummy in state X) + 4\*beta(spline trend in state X)

Year 5:1\*beta(shall dummy in state X) + 5\*beta(spline trend in state X)

where beta(shall dummy) and beta(spline trend) represent the estimated coefficients on the intercept and trend variables. Summing these individual year impacts together, we were able to calculate a net annualized five-year impact as: beta(shall dummy in state X) + 3\*beta(spline trend in state X).

# TABLE 13: THE JURISDICTION-SPECIFIC ANNUALIZED FIVE-YEAR IMPACT OF SHALL-ISSUE LAWS ON CRIME, LINEAR HYBRID MODEL CONTROLLING FOR STATE TRENDS IN CRIME, COUNTY DATA (LOTT'S CODING)

Entire Period (1977-1997)	Violent Crime	Murder	Rape	Aggravated Assault	Robbery	Property Crime	Auto Theft	Burglary	Larceny	
Maine (1985)	-4.9%	7.2%	<u>19.8%</u>	<u>-15,3%</u>	0.1%	0.5%	-4.6%	1.2%	-1.6%	
Florida (1987)	<u>-10.7%</u>	<u>-25.9%</u>	-5.8%	<u>14.8%</u>	-9.0%	-5.1%	14.0%	5.9%	21.8%	
Virginia (1988)	-0.8%	8.7%	<u>8.0%</u>	1.4%	-3.7%	3.9%	6.0%	0.7%	2.3%	
Georgia (1989)	<u>-7.1%</u>	-2.8%	<u>-19.8%</u>	1.4%	<u>-17.6%</u>	<u>-11.5%</u>	-7.2%	-14.2%	<u>-13.2%</u>	
Pennsylvania (1989)	1.7%	-1.7%	2.6%	1.8%	5.7%	7.9%	0.0%	5.2%	8.5%	
Philadelphia (1995)	8.8%	<u>22.0%</u>	7.9%	11.9%	5.6%	<u>32.3%</u>	33.8%	-0.6%	-0.6%	
West Virginia (1989)	<u>19.4%</u>	-2.3%	7.5%	32.3%	-7.5%	<u>8.6%</u>	<u>-17.0%</u>	10.4%	<u>11.1%</u>	
Idaho (1990)	7.4%	-2.0%	<u>24.3%</u>	8.6%	<u>16.2%</u>	4.9%	12.4%	-2.6%	2.9%	
Mississippi (1990)	<u>37.8%</u>	<u>20.6%</u>	10.5%	34.8%	<u>39.5%</u>	12.2%	<u>37.2%</u>	17.3%	6.6%	
Oregon (1990)	4.6%	-25.0%	<u>-8.5%</u>	<u>25.8%</u>	<u>-33.3%</u>	-4.8%	-9.1%	-23.8%	-2.0%	
Montana (1991)	23.8%	-54.0%	-31.0%	36.7%	4.7%	5.0%	6.0%	7.8%	2.4%	
Alaska (1994)	3.9%	28.6%	-32.4%	24.9%	-12.2%	20.5%	-4.2%	-4.0%	14.1%	
Arizona (1994)	<u>11.9%</u>	17.8%	14.8%	8.4%	<u>28.8%</u>	<u>8.7%</u>	17.7%	<u>23.9%</u>	11.8%	
Tennessee (1994)	26.5%	<u>30.0%</u>	<u>21.7%</u>	<u>34.1%</u>	<u>19.8%</u>	15.4%	16.7%	17.1%	<u>20.0%</u>	
Wyoming (1994)	8.2%	2.9%	1.5%	11.3%	37.2%	9.9%	13.8%	22.9%	7.2%	
Arkansas (1995)	43.2%	33.0%	23.3%	48.2%	20.7%	-13.1%	• 0.5%	1.6%	3.5%	
Nevada (1995)	15.4%	49.1%	24.6%	17.1%	18.1%	13.7%	25.3%	33.1%	8.7%	
North Carolina (1995)	15.4%	11.5%	-6.3%	21.3%	8.3%	-20.9%	19.3%	-15.3%	3.0%	
Okiahoma (1995)	-11.1%	1.5%	10.7%	-8.6%	-4.8%	-15.2%	-14.3%	-10.9%	4.3%	
Texas (1995)	<u>-17.2%</u>	-35.4%	-9.6%	-14.0%	-14.8%	-15.0%	<u>-21.7%</u>	-9.9%	-4.1%	
Utah (1995)	41.5%	11.8%	<u>35.8%</u>	<u>42.1%</u>	<u>51.2%</u>	3.2%	<u>45,8%</u>	33.7%	-4.3%	
Kentucky (1996)	-1.0%	<u>41.9%</u>	<u>-13.5%</u>	0.2%	<u>29.5%</u>	<u>-7.9%</u>	12.4%	-12.4%	<u>-13.5%</u>	
Louisiana (1996)	16.1%	<u>33.5%</u>	<u>20.3%</u>	16.9%	<u>32.1%</u>	<u>17.6%</u>	<u>33.0%</u>	22.3%	14.6%	
South Carolina (1996)	8.7%	<u>13.5%</u>	-4.5%	14.3%	<u>10.8%</u>	8.3%	<u>21.4%</u>	4.5%	3.7%	
Summary of Five-Year Effe	cts									<u>Tota</u>
Negative & Significant	1	4	1	0	2	3	1	3	2	17
Negative & Not Significant	6	4	8	3	6	5	6	6	5	49
Positive & Not Significant	11	10	11	13	9	9	10	6	12	91
Positive & Significant	6	6	4	8	7	7	7	9	5	59
Weighted Average Effect:	2.7%	-1.0%	0.7%	9.1%	2.9%	-1.8%	5.1%	0.8%	4.7%	

Notes: Weighted Average Effect is calculated by weighting the state-specific coefficients by their average population over the time period. The dependent variable is the ln(crime rate) named at the top of each column. The data set is comprised of annual county-level observations. County- and year- fixed effects are included in all specifications. All regressions are weighted by county population. Coefficients that are significant at the .10 level are underlined. Coefficients that are significant at the .05 level are displayed in bold. Coefficients that are significant at the .01 level are both underlined and displayed in bold.

Ex. 13

Page 275

Indeed, for the clear majority of states, shall-issue laws are associated with *increases* in crime for *all crime types*, and the statistically significant impacts are more than twice as likely to exhibit increases in crime. While the story of murder, rape, or robbery dropping can be found in the aggregated analyses of Tables 10 and 11, it is purely an artifact of the happenstance of early adoption that weights a few large states most heavily.<sup>105</sup> If one takes the population-weighted average effect for the twenty-four passing jurisdictions in Table 12 (the dummy variable specification), shall-issue laws are associated with *more* crime in eight out of the nine crime categories. For Table 13 (the hybrid specification), this is true for seven of the nine categories.<sup>106</sup>

We take these disaggregated (state-specific) hybrid regressions to be our most definitive results for the county based data. In a sense, our analysis of this county data has been an exercise in testing and rejecting a series of progressively less-constrained specifications. We began by rejecting the simple aggregate dummy variable and spline models in favor of the aggregate hybrid specification. We next rejected the constraint that the law had the same impact on early and late passing jurisdictions. We then rejected the decision to exclude state-specific trends. And finally we rejected the disaggregated dummy variable specification. The disaggregated hybrid model that we have finally settled on allows the data to reveal a variety of different impacts of the law-allowing separate intercept and trend effects for each of the twenty-four passing jurisdictions. And while we might have concerns that estimating this many impacts would rob the regressions of statistical significance (as we eat up degrees of freedom), we still find that over one third of the state/crime type tests (seventy-six out of 216) are statistically significant.<sup>107</sup>

106. Note that the weighted average for murder is -1%, which does not represent a statistically significant difference from zero.

<sup>105.</sup> The disaggregated analysis is also amenable to the same kind of test of internal theoretical consistency that we undertook earlier with respect to the aggregate analysis. Looking again at property crimes and robbery, we see in Table 13 on a disaggregated basis that 16 (of 24) states experienced an increase in property crime and that 14 of those 16 states also experienced an increase in violent crime, of which five were statistically significant increases in violent crime, in only one was the decrease statistically significant. Once again, although shall-issue laws are generally associated with *increased* property crime, we have no plausible story to back up this effect. Table 13 shows neither a general shift from violent to property crime nor a more nuanced shift from robbery to property crime, as none of the 16 states that have estimated increases in property crime reported statistically significant decreases in robbery.

<sup>107.</sup> It is possible, of course, to estimate even less-constrained specifications that admit the possibility of higher order impacts. Indeed, we estimated a disaggregated quadratic hybrid that is identical to the disaggregated hybrid discussed above but which includes a prepassage quadratic term and a postpassage quadratic spline term. Estimating this quadratic-hybrid specification allowed us to test (1) whether the implicit restrictions of the (linear) hybrid are rejected by the less-constrained specification, and (2) whether the results of the (linear) hybrid were robust to the less-constrained specification. We found that the (linear) hybrid's implicit assumption of no quadratic postpassage effect was not decisively
But in interpreting the results, it is important not to forget the lessons of Part II, which showed that 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. The nonrobustness of the results with respect to these attenuated demographic controls is a legitimate reason to discount or at least question this entire enterprise. Because the disaggregated county data still yields some jurisdictional pockets in support of Lott's more guns, less crime hypothesis, however, we soldier on to assess a possible net impact of the laws.

To get a better handle on the net impact of the law on all crime, we have estimated the dollar impact of the law on particular crime categories, using the same dollar value per crime that Lott used in his initial study.<sup>108</sup> Table 14 reports the annualized dollar impact of crime for each of the twenty-four jurisdictions and for each of the nine crime categories, as well as various aggregations of these amounts. Eighteen of the twenty-four states have estimated increased dollar harms, as shown in the "Total" column, which is also depicted in Figure 7. Viewed in aggregate, the Table suggests that the net annual impact of the law was to increase the dollar harm of crime by approximately \$1 billion. This represents a "harm weighted" annual increase in crime of two percent that amounts to an annual burden of \$9.63 on the average citizen in the passing states.

rejected in that only 49 of the 216 coefficients were statistically different from zero (although the quadratic spline effects were jointly different from zero in eight of the nine regressions). But the basic results of the (linear) hybrid analysis discussed in the text remain unaltered: Calculating the net annualized five-year impact, we continued to find that the vast majority of the statistically significant impacts were positive (48 versus 20).

Lott has sharply criticized Black and Nagin's decision to estimate state-specific quadratic time trends in an aggregated dummy variable model. *See* LOTT, *supra* note 2, at 209; *see also* Black & Nagin, *supra* note 3, at 218. We agree that such a specification can understate the law's impact if the impact comes primarily through a kink or bend in the time trend. But Lott's criticism is not relevant to our quadratic-hybrid specification, which allows the law's impact to come through an intercept effect, a linear spline effect, or a quadratic spline effect.

<sup>108.</sup> In 1997 dollars, the harm of the following crimes was assumed to be: murder, \$3,092,804; rape, \$91,522; aggravated assault, \$25,247; robbery, \$8416; auto theft, \$3892; burglary, \$1472; and larceny, \$389. LOTT, *supra* note 2, at 54-56.

JURISDIC	TION-SPEC	CIFIC A	NNUALIZ	ed Five-	YEAR IMP	аст, Нү	BRID M	ODEL W	ITH STATE	TRENDS (L	OTT'S COF	(DNI)
	Mundar	Dana	Aggravated	Dahharu	Violent Crime	Auto Theft	Ruralani	1 arcent	Property Crime Total	Total (Viol & Pron)	Per Capita Impact*	Harm-Weighted
Maine (1985)	6.29	3.03	-5.69	0.00	3.63	-0.34	0.19	-0.18	-0.33	3.30	2.83	1.9%
Florida (1987)	-1098.84	-32.03	272.28	-32.53	-891.12	44.30	23.56	37.90	105.76	-785.37	-65.45	-10.0%
Virginia (1988)	122.27	11.33	3.08	-1.98	134.70	4.09	0.48	1.43	6.00	140.70	23.54	7.0%
Georgia (1989)	-69.61	-55.91	8.99	-25.55	-142.07	-11.62	-22.69	-12.90	-47.22	-189.29	-29.53	-4.8%
Pennsylvania (1989)	-14.56	5.11	7.99	3.71	2.24	0.00	4.83	5.43	10.26	12.50	1.21	0.7%
Philadelphia (1995)	293.94	5.59	21.16	6.42	327.10	31.32	-0.14	-0.11	31.07	358.18	239.28	19.5%
West Virginia (1989)	-8.64	2.38	11.90	-0.50	5.15	-1.93	1.78	1.15	1.00	6.15	3.41	1.3%
Idaho (1990)	-1.69	6.08	5.00	0.20	9.59	0.80	-0.31	0.32	0.80	10.39	10.22	5.2%
Mississippi (1990)	119.42	6.85	30.53	5.75	162.56	5.95	5.75	1.03	12.74	175.30	67.86	21.6%
Oregon (1990)	-83.65	-10.34	57.49	-11.49	-47.99	-4.61	-11.28	-0.78	-16.68	-64.67	-22.65	-7.6%
Montana (1991)	-22.40	-3.20	5.08	0.04	-20.48	0.28	0.35	0.14	0.77	-19.71	-24.37	-24.1%
Alaska (1994)	24.03	-7.71	14.46	-0.86	29.92	-0.45	-0.19	0.96	0.33	30.24	50.51	15.5%
Arizona (1994)	232.38	19.19	42.01	15.73	309.31	29.58	20.76	8.52	58.87	368.18	88.74	15.9%
Tennessee (1994)	406.25	46.22	194.58	17.31	664.36	17.61	13.10	9.67	40.38	704.73	136.63	28.7%
Wyoming (1994)	1.54	0.22	2.97	0.25	4.97	0.41	1.04	0.43	1.88	6.85	14.43	6.3%
Arkansas (1995)	264.73	19.69	114.59	5.43	404.43	0.16	0.58	0.95	1.69	406.12	163.70	32.6%
Nevada (1995)	246.05	20.87	35.86	7.55	310.32	11.20	9.78	1.82	22.80	333.12	217.79	35.7%
North Carolina (1995)	237.69	-13.27	162.87	8.93	396.22	16.65	-22.76	2.71	-3.40	392.83	54.66	11.3%
Oklahoma (1995)	18.48	14.30	-34.96	-1.53	-3.71	-9.05	-6.69	1.73	-14.01	-17.72	-5.42	-0.9%
Texas (1995)	-1854.87	-75.23	-283.89	-41.93	-2255.92	-88.61	-29.52	-10.09	-128.22	-2384.14	-127.49	-25.7%
Utah (1995)	27.81	26.84	43.35	5.57	103.56	13.31	7.73	-1.46	19.58	123.14	62.37	24.1%
Kentucky (1996)	125.77	-5.62	0.30	7.76	128.20	2.91	-2.38	-1.83	-1.31	126.89	32.69	22.1%
Louisiana (1996)	772.18	31.94	105.79	31.74	941.65	34.36	17.72	9.45	61.53	1003.18	231.10	29.2%
South Carolina (1996)	139.91	-7.74	103.07	5.89	241.12	13.27	3.16	1.88	18.30	259.42	69.80	12.0%
Totals**	-115.56	8.58	918.79	5.92	817.73	109.57	14.84	58.19	182.60	1000.34	9.63	2.0%
Simple Mean	-4.81	0.36	38.28	0.25	34.07	4.57	0.62	2.42	7.61	41.68	49.83	9.1%
Standard Deviation	502.83	26.22	100.07	15.52	588.18	24.42	12.81	9.04	41.88	613.17	92.14	16.5%
Number Positive	16	15	21	16	18	16	15	17	17	18		
Number Negative	8	6	ω	8	6	5	6	7	τ.	6		
* Per Capita Impact is ex	pressed in dolla	ars, not mill	lions of dolla	rs.								
** The entries for Per-Co	pita Impact and are for the 24 in	d Harm-We	eighted Perce	nt Change ar	e computed by c	lividing the	total estima	tted impact	(\$1 billion) by th	e total populatic	n (in millions) a	nd the total cost of
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Ex. 13

Page 278

TABLE 14: ESTIMATES OF DOLLAR IMPACT OF CRIME (IN MILLIONS OF DOLLARS\*) BASED ON

#### STANFORD LAW REVIEW



Ex. 13 Page 279

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The Table also reveals substantial variation in the impact of the law. In Texas and Florida alone, the law is estimated to have *reduced* the annual cost of crime by more than \$3 billion. Conversely, in Louisiana and Tennessee, the law is estimated to have *increased* the annual cost of crime by \$1.7 billion. In the twenty-four adopting jurisdictions that we examined, the mean impact of the law was a \$42 million increase in crime, but the standard deviation was more than fourteen times this amount (\$613 million).<sup>109</sup>

These dollar figures price all of the estimated impacts of the law (reported above in Table 13) regardless of their statistical significance. An alternative way to estimate the aggregate impact of the law is to put a zero dollar value on all the impacts that are not sufficiently statistically significant. Table 15 reports the aggregate dollar impact of the law for alternative levels of significance—for both the dummy variable specification (originally reported in Table 13) and our preferred hybrid specification (of Table 14). The Table shows that the law continues to display a pernicious dollar effect even when we limit our focus to five-year impacts that were statistically significant at the ten-percent or five-percent level. At these levels, the static specification estimates a net annual impact on the order of \$500 million, while the less-restrictive hybrid specification estimates an increased cost ranging between \$3 and \$524 million.<sup>110</sup>

We take these results to be generally devastating to Lott's more guns, less crime hypothesis. Estimating a less-constrained specification with more data, we find that more jurisdictions experience an increase than a decrease in crime and that the statistically significant increases outpace the statistically significant decreases by more than two-to-one in county data (and still by a substantial amount in state data).

<sup>109.</sup> One can see from Figure 7 that the two states showing the biggest dollar impact on crime are Texas (a crime decline of over \$2 billion per year) and Louisiana (a crime increase of over \$1 billion). Both adopted the law late in the data period, which implies that there is relatively little postpassage data with which to estimate these figures. It is quite likely that with more years of data, one would see the estimated effects for these two states move closer to the mean.

<sup>110.</sup> Only if we restrict attention to five-year impacts that were statistically significant at the one percent level do we find a net benefit from the law, with an estimated reduction in crime in the hybrid specification of \$784 million. But here it is important to note again that the result is driven by just two states—Texas and Florida (with an estimated combined benefit of more than \$3 billion)—which more than offset the estimated crime-increasing impact in Louisiana and Tennessee (approximately \$1.7 billion).

# TABLE 15: ESTIMATED TOTAL DOLLAR IMPACT OF24 SHALL-ISSUE LAWS (IN MILLIONS OF DOLLARS)

	Lev	el of Filter	ing	
Model	All Estimates	10%	5%	1%
Dummy	1262.14	513.11	487.04	-22.22
Hybrid	1000.34	524.36	2.89	-784.33

Notes: These estimated dollar effects are based on the jurisdiction specific models with state trends. The Hybrid Model is from Table 13, where the \$1 billion figure can be found under the row and column Total. The remaining totals and the Dummy Variable model is available upon request. Essentially, the first set of estimates (in the "All Estimates" column) takes every estimated jurisdiction-specific as reflecting the true impact of the shall-issue law on crime. The other three columns will filter out those estimates that are not statistically significant at the indicated level.

#### V. SOME INTERPRETATIONS AND SPECULATIONS

Our effort to find the statistically most appropriate model with which to assess the impact of shall-issue laws on crime has involved an extended odyssey as our testing, on a more comprehensive county dataset, has constantly pushed us towards more disaggregated and less restrictive models than the more aggregated and highly constrained models employed by Lott and Mustard. The lesson has been a sobering one in that the facially plausible models relied upon by Lott and Mustard that we present in Table 10 could well encourage a researcher or policymaker to believe that shall-issue laws reduce crime. Yet when we reached the end of the journey with our more complete data, the use of a statistically superior model—one that estimates jurisdictionspecific effects while estimating both main and trend effects (our "hybrid") and controlling for state fixed effects—reduces the initial conclusion to ashes. The best, albeit admittedly imperfect, evidence suggests that, for the majority of states, shall-issue laws are associated with higher levels of crime.

It is important, though, to be clear about the degree of confidence that we can repose in any particular interpretation of the evidence. In the end, we are left with a hierarchy of three conclusions that we will discuss in turn below.

1. There remains no robust, credible statistical evidence that the adoption of shall-issue laws will generally lower crime, and indeed the best, albeit admittedly imperfect, statistical evidence presented thus far points in the opposite direction: that the adoption of shall-issue laws will generally increase crime.

We believe that a fair evaluation of all the state and county evidence we have presented offers virtually no basis for believing that shall-issue laws reduce crime. While particular regressions at times predict drops in certain violent crimes following adoption, the overall regression evidence predicts

HeinOnline -- 55 Stan. L. Rev. 1285 2002-2003

increases in other crime categories, most frequently among the many types of property crime. Indeed, there is abundant reason to be concerned that the regressions are not performing particularly well, and plausible adjustments to the aggregated regressions—whether by using alternative model specifications, trying to control for selection effects of very late or very early adopters in the state analysis, or introducing state trends in the county data—largely eviscerate the more guns, less crime hypothesis. In particular, state-specific estimates generated on the 1977-1997 county data are more supportive of the view that shall-issue laws increase crime than that they decrease it.

While Lott and Mustard have tried to offer other types of evidence that can bolster their core statistical findings, if the foundation falls, the entire edifice will crumble. We believe we have shown that the foundation has collapsed. Whether further advances in statistical modeling or additional years of data to analyze more state adoptions (or repeals) of shall-issue laws will be able to resurrect the structure remains to be seen.

We hasten to add, however, that showing that superior statistical modeling on more complete data reverses the Lott and Mustard conclusion does not necessarily resolve the debate since "better" does not always imply "good enough." The estimates' lack of robustness with regard to innocuous changes in specifications provides grounds for rejecting the authoritativeness of any of the results. Therefore, another plausible conclusion from the evidence we have presented is:

2. While the best evidence suggests that shall-issue laws generally tend to increase crime, there is still too much uncertainty to make strong claims about their effects.

The dramatic reversal in findings in moving from aggregated (Table 10) to disaggregated (Table 13) hybrid specifications certainly reveals that many conclusions about the impact of shall-issue laws will be dependent on the particular statistical model that is employed. Some will be convinced that our disaggregated model is superior and, therefore, will accept the conclusions of the state-specific analysis. More cautious analysts will be concerned that the problems we have highlighted of data accuracy, model misspecification, endogeneity, and lack of robustness (such as the sensitivity of the state data results to exclusions of minor demographic controls) are too severe to confidently assert whether shall-issue laws dampen crime, increase crime, or have no overall effect on crime. We share these concerns, especially since the theoretical argument for one of the most robust findings—that the laws increase property crime—is not particularly compelling.<sup>111</sup> Thus, a plausible

<sup>111.</sup> The figures we provide show property crime rising as much in response to shallissue laws as violent crime does (with virtually no evidence of the shifting from violent crime to property crime that was initially posited by Lott and Mustard). *See, e.g.*, LOTT *supra* note 2, at 19 ("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."). Given the lack of

interpretation of the existing evidence would be that even if we can muster evidence that shall-issue laws generally appear to increase crime, the uncertainty about whether the statistical models are working properly makes it difficult to make any strong claim about the impact, other than to say that it is not so huge that it can overwhelm any defects in the model.

At the end of the day, then, it is still possible that shall-issue laws have no effect—positive or negative—on crime. This is particularly so if one credits Willard Manning's suggested correction for the presence of these multiple comparisons and for autocorrelation in crime across years.<sup>112</sup> In addition, as discussed above, one reason states like Florida and Texas—both of which experienced large crime decreases after adoption—may have an estimated negative impact is simply that they passed the law in response to crime increases, and as crime reverted of its own accord to its normal levels, the regression inappropriately attributed this reversion to the passage of the law.

Earlier in this Article, we touched upon an alternative reason why even the best regression results may not be believable: the huge omitted variable problem represented by the upturn in crime following the advent of crack in certain urban areas in the mid- to late-1980s. If states adopting shall-issue laws in the 1980s had no major crack problem, while nonadopting states were more likely to have a crack problem, then that fact alone could make it appear that shall-issue laws reduced crime because crime would rise relatively faster in the nonadopting states (by virtue of the crack problem). In his book, Lott reported that the states adopting shall-issue laws tend to be Republican, have high NRA membership, and have low crime rates.<sup>113</sup> That does not sound like the sort of place where one finds the worst problems with crack. Without a plausible control for the influence of crack on crime rates, every crime regression is subject to a charge of inadequacy: A potentially important explanatory variable that is correlated with the adoption of shall-issue laws is being omitted.<sup>114</sup>

Misspecifications of this type make it difficult for the researcher to distinguish between the possibility that the law has no effect and the possibility that any effect of the law is beyond the current ability of researchers to identify. Ironically, however, either a "no effect" or "don't know the effect" assessment might be enlisted to argue normatively for adoption of the shall-issue law.

evidence that robbery or other violent crimes have fallen, however, is the estimated increase in property crime due to shall-issue laws plausible?

<sup>112.</sup> A basic assumption of regression is that the difference between the regression prediction of the crime rate and the observed value, called the error, is not correlated across years. If it is, then one has the problem of autocorrelation, and more sophisticated statistical techniques are required to secure reliable estimates. *See* GREENE, *supra* note 13, at 147.

<sup>113.</sup> LOTT, supra note 2, at 120.

<sup>114.</sup> In his second edition, Lott admirably includes controls for individual year effects for five different regions (Northeast, South, Midwest, Rocky Mountains, and Pacific) to allow for more heterogeneous year effects for different parts of the country. *Id.* at 170. But Lott's less-constrained approach would not be sufficient to solve the crack problem (outlined above), which may very well have played out within the regions.

Page 283

While the first empirical assessment would tend to lead consequentialists to oppose the law,<sup>115</sup> the second assessment of ignorance (or even no effect) might provide a libertarian grounds to support such legislation. There are, however, many reasons why reasonable people who embrace the "don't know effect" conclusion nevertheless the might oppose the statute. Nonconsequenstialists and expressivists might oppose the law notwithstanding a lack of evidence that the law increases crime. Risk aversion or a concern that the law moves society away from a more global maximum of fewer weapons might also ground opposition. Finally, if the realization that thousands of citizens were carrying around concealed handguns generated fear or apprehension in the community, one might oppose shall-issue laws even if one could not prove that they increased crime. In a world where NRA members have bumper stickers stating "Keep Honking, I'm Reloading," the costs of intimidation of law-abiding citizens may become intolerable.<sup>116</sup>

Some may feel that just as many were willing to make policy choices on the basis of the initial Lott and Mustard study, there is even stronger reason to rely on our less-constrained models estimated on more years of data showing that after passage twice as many jurisdictions experienced a statistical increase as experienced a statistical decline in crime. Those who are willing to repose confidence in such results have to consider whether the disaggregated evidence might support a view that some states would benefit from shall-issue law adoption even though most would not. We will now evaluate the normative implications of this assessment of heterogeneous effects across states. While the first and second assessments militate towards across-the-board policies, the heterogeneity assessment might counsel towards a more nuanced, piecemeal adoption of the statute:

3. We should simply accept the twenty-four different jurisdiction-specific estimates and conclude that shall-issue laws increase crime in most states but reduce it in other states.

This is the most speculative of the three conclusions we discuss in that it violates Milton Friedman's admonition against accepting statistically

Ex. 13

Page 284

<sup>115.</sup> Lott, himself, is clearly in the consequentialist camp. See id. at 21 ("[T]he ultimate test: Does it save lives?").

<sup>116.</sup> David Mustard, *The Impact of Gun Laws on Police Deaths*, 44 J.L. & ECON. 635, 637 (2001), cites a 1999 incident in Phoenix in which a concerned citizen with a concealed gun permit (and the "Keep Honking . . ." bumper sticker) came to the assistance of a dying officer and helped capture three fleeing drug suspects. The citizen returned fire by shooting 14 shots into the car of the criminals, which "he thought was pretty good since he shot quite a few of them with his off hand, hanging out the window." While the apprehension of dangerous felons was an obvious benefit of the citizen's intervention, it is not hard to imagine how such behavior could have ended badly for some innocent bystanders. Simply because this episode ended happily does not mean that we should encourage such efforts. See Mark Shaffer, "True Hero" Helps Nab Trio: Security Guard Reacted to "What I Thought Was the Right Thing to Do," ARIZ. REPUBLIC, Mar. 28, 1999, at A1.

1289

significant findings too readily.<sup>117</sup> On the face of the evidence presented in Table 13, there are a number of problems with accepting that crime fell with the adoption of shall-issue laws in even a limited number of states. There is only one state that shows a statistically significant decrease in at least two of the five violent crime categories without showing a statistically significant increase in another violent crime category (Georgia, showing drops in rape and robbery). Florida shows substantial drops in violent crime and murder but a statistically significant increase in aggravated assaults (all against the background of the potentially confounding influence of the Mariel boatlift), while Oregon shows huge drops in murder and robbery but a huge increase in aggravated assaults. Only two other states show a statistically significant drop in any other violent crime category (Texas and Montana, seeing enormous drops in murder). In other words, it is rare (only five out of twenty-four) to see any statistically significant evidence of declines in any violent crime category resulting from the adoption of a shall-issue law. Certainly, there is nothing to give one confidence that an overall drop in violent crime is likely to be spawned anywhere by the adoption of a shall-issue law. Indeed, the rare and seemingly haphazard pattern of statistically significant drops across isolated violent crime categories makes one think of these drops as more random noise than estimates upon which much confidence can be reposed that real effects have been identified. This is underscored by the fact that finding eight statistically significant drops in crime across the 120 estimates (twenty-four jurisdictions times five violent crime categories) in Table 13 is only modestly more than one would expect from a purely random process.

#### A. Illustrating a Methodology for Deriving State-Specific Predictions

One interesting consequence of conclusion three, however, is that it invites the researcher to investigate whether there may be some particular attributes about the small number of states for which crime drops were estimated that diminish the generally harmful effects of shall-issue laws enough so that they may actually dampen crime. If one overcomes the concern that the infrequent instances of crime reductions are merely random artifacts and concludes that there is valid information captured in the harm-weighted estimate of the total effect on crime for a state, we can investigate whether there is any pattern that explains which states show overall crime increases and which show decreases. Unfortunately, we have very little data to make this assessment. The county fixed-effects model which we have followed Lott in using to test for the law's impact only allows us to test for the impact of changes in the law within particular counties.<sup>118</sup> But now we are called upon to assess the determinants

<sup>117.</sup> See supra note 16.

<sup>118.</sup> Lott attempted to assess what characteristics were associated with larger or smaller impacts of the shall-issue law by interacting the law dummy variable with various

of the law's impact across the passing states. In essence, we have just twentyfour observations (twenty-three passing states plus Philadelphia) on which to try to disentangle what caused the law to increase crime in some jurisdictions and decrease it in others.

Since we are now taking the output of potentially fragile regressions that were used to estimate the impact on crime of adopting a shall-issue law in twenty-four jurisdictions, then trying to run yet another regression based on this output having only twenty-four observations, we are now moving very far out on a branch. Still, there is value in at least detailing a methodology for deriving state-specific predictions of the effect of a law, even for those that have not adopted the legislation. Accordingly, we regressed various measures of the impact of shall-issue laws on a variety of state characteristics. Before reporting the results, let us emphasize that with just twenty-four observations we are certainly pushing the limits of data. The results that we report are not nearly as robust as our prior findings to the inclusion or exclusion of other variables.<sup>119</sup> Indeed, the data's resistance to explanation can be taken as yet more evidence that, notwithstanding our best efforts, the underlying model (the disaggregated hybrid version) is still misspecified.

With these caveats about the substantial limits in our data and the residual concerns with misspecification, we proceed to discuss the impact regressions of Table 16. The dependent variable in the first row regression is the harm-weighted percentage impact of the hybrid regression (reported above in Table 14), which ranges from a 25.7% *drop* in crime in Texas to a 35.7% *increase* in crime in Nevada.<sup>120</sup> We use the following six jurisdiction characteristics as explanatory variables: the year the law was adopted, population, log of violent crime rate, density (measured in population per square mile), and regional dummies.<sup>121</sup> Looking at the estimated coefficients, we see that later-passing

demographic characteristics of the county (its density, its income, etc.). LOTT *supra* note 2, at 60-81. But this interaction specification in a fixed-effects model only allowed Lott to assess the impact of, say, changes of density *within* particular counties that had passed the law. Since, on average, there is very little within-county variation in density over a 20-year period, this specification can tell us very little about the relationship between the law's impact and densities across different counties or states.

<sup>119.</sup> The fact that we ran a number of alternative specifications that we did not report also suggests a "pretesting" effect that should lead us to discount even the nominal levels of significance reported in the regression.

<sup>120.</sup> The dispersion in the estimated effects of the 24 shall-issue laws is quite wide, which in itself shows that there is a considerable degree of noise in the estimate for any particular jurisdiction. As Black and Nagin comment in finding substantial dispersion in state-specific estimates for the 10 states adopting shall-issue laws during the 1977-1992 period: "Widely varying estimates such as these are classic evidence that, even beyond the assumption of homogeneous impacts across states, the model is misspecified." Black & Nagin, *supra* note 3, at 214.

<sup>121.</sup> Only two (South and West) of the four regions are reported in Table 16. There were no Midwest states that passed the law during this 20-year period (and the Northeast region was the excluded attribute).

states are predicted to have a more deleterious impact, with each additional year adding more than 1.6 percentage points to the estimated impact (prob. = 0.07). This is a huge effect (a law passing ten years later would be expected to have a sixteen percentage point higher crime impact) and is qualitatively consistent with the aggregate results showing crime increases in the 1990s for later passing jurisdictions.<sup>122</sup>

The harm-weighted estimates that we use as regressors have the advantage of aggregating the individual crime category impacts in a natural way that weights the individual crime estimates commensurate with their underlying importance (more serious crimes are socially more "expensive" and therefore weighed more heavily). A disadvantage of the harm-weighted estimates is that they ignore the varying significance of the individual crime impact estimates. Returning to Figure 5 (which depicts the disaggregated dummy variable model estimates for murder from Table 12), one can readily identify the positive correlation between the estimated impact of the law on the murder rate and the year in which the law was adopted. We see that thirteen of the final fourteen passing states had estimated increases in murder (and seven of these were statistically significant) while only four out of the first ten passing states posted estimated increases in murder (and only one of those was statistically significant).<sup>123</sup> More generally, Table 14 shows that eleven of the last thirteen passing jurisdictions experienced *increases* in violent crime.

Page 287

<sup>122.</sup> Query, though, whether this result tells us that the later adopters can expect more pernicious outcomes from adopting shall-issue laws or whether it is reflecting the sharp drop in crime in the postcrack world for the nonadopting states. The year of adoption is clearly a proxy for something—but it is uncertain whether it is a proxy for a feature of the state that will influence the impact of a shall-issue law (as the regression posits) or a proxy for the time period during which sharp declines from previous crack-induced crime hikes occurred in selected nonadopting states (i.e., having no relation at all to the impact of shall-issue laws).

<sup>123.</sup> In his second edition, Lott attributes the diminishing beneficial impact of the law in later-passing jurisdictions to heightened fees and training requirements that were imposed on permit applicants in the later passing states. LOTT, *supra* note 2, at 125-26. But while the imposition of greater obstacles could have explained a diminished beneficial trend, it does not explain why we find that later passing states generally experienced increases in crime. It also does not explain why Texas (which Lott notes requires 10 hours of training and charges the highest fee in the sample) was one of the great outliers in generating a beneficial impact. *See id.* at 175. We did not have available the fee and training data that Lott used and hence did not control for these attributes in the regressions reported in Tables 10 and 11.

			Explanatory	· Variables		
I		Population	Log Violent	Year		
Dependent variable*	Density**	(Millions)	Crime Rate	Adopted	South	West
Hybrid model harm-weighted impact	<u>-0.026</u>	-0.028	0.116	0.016	-0.119	-0.205
	(0.013)	(0.006)	(0.059)	(0.009)	(0.087)	(0.098)
Mean impact: 0.091						
Standard deviation: 0.165					-	
Hybrid model five-year murder impact	-0.053	-0.042	0.212	0.023	-0.261	-0.427
	(0.019)	(0.006)	(0.096)	(0.013)	(0.120)	(0.139)
Mean impact: 0.077						
Standard deviation: 0.247						
Dummy model harm-weighted impact	-0.027	-0.02	0.095	0.017	-0.115	-0.182
	(0.013)	(0.004)	(0.071)	(0.00)	(0.080)	(0.089)
Mean impact: 0.073						
Standard deviation: 0.146						
Dummy model murder impact	-0.056	-0.032	0.166	0.028	-0.303	-0.422
	(0.022)	(0.008)	(0.123)	(0.015)	(0.133)	(0.161)
Mean impact: 0.076						
Standard deviation: 0.237						
* For the values of the hybrid model ha	rm-weighted in	npact, see the	harm-weighted	percent change	e column, Tab	le 14. For the
Hybrid murder impact, see the murder in column. Table 12. The dummy model ha	mpact column, rm-weighted in	Table 13. Fo	or the dummy m cluded in the tab	odel murder in des but is avail	npact, see the able upon requ	murder impact est.
	0					

**\*\*** Density is expressed in units of population in thousands per square mile.

## STANFORD LAW REVIEW

Ex. 13 Page 288

HeinOnline -- 55 Stan. L. Rev. 1292 2002-2003

If one accepts the accuracy of the jurisdiction-specific estimates, one might interpret this cohort result as a kind of "Peter Principle" applied to law. As a pop theory of job advancement, the Peter Principle asserts that employees are promoted to jobs requiring successively higher skills until they reach a job where they are relatively incompetent.<sup>124</sup> Analogously, the data indicates that the law has performed less well—rising to the level of its incompetence—as successive states have chosen to adopt it. On the other hand, the temporal pattern, that states adopting shall-issue laws in the late 1980s did better while those adopting in the 1990s did worse, may simply reflect the influence of a time-varying factor (the crack trade?) that caused sharp rises in crime for many states in the late 1980s, and then greater-than-average crime declines in the 1990s.<sup>125</sup>

The regressions, however, do at least suggest some nontemporal traits that are associated with the estimated impact. Passing jurisdictions with larger populations had more beneficial impacts, and this correlation was strongly significant. Less dense jurisdictions and jurisdictions starting with higher base levels of violent crime had more pernicious impacts (and these effects were significant at the ten-percent level).<sup>126</sup> The negative association between state population and the estimated dollar impacts (estimated in Table 14) is dramatically illustrated in Figure 7, with the two largest states (Texas and Florida) having by far the largest harm-weighted dollar drops in crime. This should not be surprising, though, since a given percentage change in crime will have a bigger dollar impact in a larger (or higher-crime) state. Finally, western states tended to be associated with better crime outcomes, and this effect was statistically significant.<sup>127</sup> While there are always some "cultural" rationales that we could offer ex post for these effects, we should emphasize that these results are suspect as they are the byproduct of reduced-form regressions rather than growing out of a priori theory.

#### **B.** The Resulting State-Specific Predictions

Having derived the regression estimates of the factors that influence the impact of the law, we are at least conceptually able to use the results of these regressions to predict out-of-sample the expected impact of the law on the

<sup>124.</sup> LAURENCE J. PETER & RAYMOND HULL, THE PETER PRINCIPLE: WHY THINGS ALWAYS GO WRONG (1969); see also James A. Fairburn & James M. Malcomson, *Performance, Promotion, and the Peter Principle*, 68 Rev. ECON. STUD. 45 (2001).

<sup>125.</sup> See supra text accompanying notes 34-38 and 110-11 (discussing the omitted variable problem represented by the upturn in crime following the rise of the crack trade).

<sup>126.</sup> While this empirical finding is potentially interesting, it is not evident that any theory could support it.

<sup>127.</sup> In the remaining panels of Table 16, we replicate the regression of the first row by regressing alternative measures for the impact of crime coming from the murder category and from the dummy-variable regressions. One sees that the patterns of sign and statistical significance are generally consistent.

Page 289

jurisdictions that either have never passed a shall-issue statute or had already passed such a statute before our data began in 1977.<sup>128</sup> To continue our methodological roadmap for how to generate these estimates (for cases in which the regression results were both stronger and based on a stronger foundation), Appendix Table 8 reports the predicted impact (as well as the standard deviation of the prediction) of passing the law today in all fifty-two jurisdictions (including Philadelphia and the District of Columbia), given the jurisdiction's underlying characteristics. While this predictive process is based on extremely limited data, it may be useful to illustrate the possibility of more nuanced state-specific policy recommendations in contrast to the across-the-board recommendations to adopt or repeal that are the only possible product of the type of aggregated analysis that Lott championed.

As one might suspect, the predictions produced heterogeneous results, which we break into four categories:

## 1. Jurisdictions in which adoption is strongly predicted to increase crime.

We found thirty-one jurisdictions in which adoption of the law predicted an annual percentage increase in the harm-weighted measure of crime that was more than twice the prediction's estimated standard deviation and, therefore, statistically significant. Fifteen of these jurisdictions had already adopted the law and thus would need to repeal the law to avoid the deleterious effect, while sixteen of these states would merely need to refrain from adopting the law in the future.

# 2. Jurisdictions in which adoption is strongly predicted to decrease crime.

We found just two states (Texas and California) in which adoption of the law predicted an annual percentage *decrease* in the harm-weighted measure of crime that was more than twice the prediction's estimated standard deviation. One of these jurisdictions (California) would need to adopt the law to secure its benefits while the other (Texas) would merely need to retain the law that it has previously adopted.<sup>129</sup>

Page 290

<sup>128.</sup> Lott undertook an analogous prediction procedure when he used a first-stage regression run on 10 states to predict how many permits would be issued in other states. LOTT, *supra* note 2, at 176.

<sup>129.</sup> While crime did drop as the Texas shall-issue law went into effect on January 1, 1996, California has never adopted a shall-issue law, so the prediction is based on our weakly predictive model with 24 data points (largely because California has a large population and is a western state). But while California is shown by these tentative impact regressions to be a possible candidate for enactment of a shall-issue law, its experience of showing dramatic crime declines in the 1990s, both absolutely and vis-à-vis shall-issue states, suggests another reason for caution before accepting the predictions of these

#### 3. Jurisdictions in which adoption is weakly predicted to increase crime.

We identified fourteen jurisdictions in which adoption of the law predicted annual percentage increases in crime but were not statistically significant at the five-percent level. Risk-aversion would probably counsel against passing (or retaining) shall-issue laws in these jurisdictions. Under this normative reckoning, eleven of these jurisdictions had previously passed the law and thus would be targets for repeal while three should simply continue to refrain from adopting the law.

## 4. Jurisdictions in which adoption is weakly predicted to decrease crime.

Finally, we identified five jurisdictions in which the predicted impact of the law on a harm-weighted measure of crime was beneficial but not statistically so. A libertarian would argue that these five states—which all previously passed the law—should retain them. However it should be noted that in four of these five states (Virginia, West Virginia, Pennsylvania, and Montana) the shall-issue law was estimated in Table 14 to have a deleterious (but statistically insignificant) impact on crime so that the more direct measure of impact would weakly counterindicate passage of the statute.<sup>130</sup>

In sum, this analysis suggests the following legislative (in)action: Six states should retain their current shall-issue laws; one state (California) should adopt the law for the first time; twenty-six states should repeal the law; and nineteen states should continue to refrain from adopting.<sup>131</sup> For all of the reasons set forth above, this analysis can only be taken as suggestive of the type of nuanced policy recommendations that are possible from statistical analysis of state-specific estimates of the impact of the law. Nonetheless, we think it can provide a useful blueprint for researchers who have rich enough data and well-specified state-specific regression results to be able to make more finely

regressions.

131. Lott's own analysis suggests that issuing an unlimited number of permits may produce a pernicious impact on crime. In his second edition, Lott estimated, in an aggregate specification, that when concealed handgun permits exceeded a maximum percentage of the population, additional permits would be predicted to increase crime. LOTT, *supra* note 2, at 178-80. If Lott is right on this point, a state that passed the law under the foregoing normative account might be advised to cap the maximum number of permits issued.

<sup>130.</sup> Conversely, in two states (Georgia and Oregon) the law was predicted (in Appendix Table 8) to have a deleterious impact while the more direct estimate (from Table 14) was beneficial (but statistically insignificant). Accordingly these two states might be deemed to be reclassified as weakly indicated for passage (although a risk averse analyst might think that a state showing either a deleterious predicted or actual estimate was contraindicated). These discrepancies between the predicted effects from the Table 16 regressions and the more direct estimated effects from the full regressions that generated Table 14 underscores again the fragility of these results.

grained policy recommendations than are possible from the typical aggregated analysis.

#### CONCLUSION

Judge Richard Posner has recently criticized moral philosophy for failing to persuade on any contentious issue.<sup>132</sup> A similar criticism might be made of quantitative empiricism. Readers might tend to accept only those quantitative analyses that resonate with their prior normative beliefs. Indeed, Judge Stephen Reinhardt famously proclaimed at a Yale seminar that social science had never affected his judicial decisionmaking.<sup>133</sup> And Donald Braman and Dan Kahan have recently called upon econometricians like Lott and us to put away our statistical packages. In a piece provocatively titled, "More Statistics, Less Persuasion," Braman and Kahan argue that rather than "quantifying the impact of gun control laws on crime," academics "should dedicate themselves to constructing a new expressive idiom that will allow citizens to debate the cultural issues that divide them."<sup>134</sup>

We disagree. Over time, a body of empirical research can disentangle thorny issues of causation and lead toward consensus. We view this Article as playing a role in this process (not in ending the conversation). On net, we believe that Lott and Mustard's efforts made an important contribution to the literature. They asked the initial question, amassed an important new panel dataset, and then energetically and creatively analyzed it. (Indeed, their dataset, which we know from experience was quite costly to construct, has been used by many researchers to explore this and other questions about crime.) Nevertheless, their results have not withstood the test of time. When we added five years of county data and seven years of state data, allowing us to test an additional fourteen jurisdictions that adopted shall-issue laws, the previous Lott and Mustard findings proved not to be robust. Importantly, we showed that the Lott and Mustard results collapse when the more complete county data is subjected to less-constrained jurisdiction-specific specifications or when the more-complete state data is tweaked in plausible ways. No longer can any plausible case be made on statistical grounds that shall-issue laws are likely to reduce crime for all or even most states. How much further one can go in arguing that shall-issue laws likely increase crime across the board or have heterogeneous effects across states (albeit most commonly pernicious) will be matters about which various analysts will differ. We conclude with Learned Hand's advice that, unlike a policy advocate, an academic must "keep an open

<sup>132.</sup> RICHARD A. POSNER, THE PROBLEMATICS OF MORAL AND LEGAL THEORY (1999).

<sup>133.</sup> Professor Ayres was at the seminar, which occurred, to the best of his memory, in the latter half of the 1990s.

<sup>134.</sup> Donald Braman & Dan M. Kahan, More Statistics, Less Persuasion: A Cultural Theory of Gun-Risk Perceptions (2002) (unpublished manuscript, on file with authors).

mind to every disconcerting fact, [and] an open ear to the cold voice of doubt." Hand admonished: "You may not carry a sword beneath a scholar's gown."<sup>135</sup>

#### APPENDIX I: CODING THE TIMING AND STATUS OF SHALL-ISSUE LAWS

The dataset that Lott and Mustard initially constructed covered the years 1977-1992. Because of their fixed-effects estimation technique, their analysis is able to measure the effect of the law only for those states that changed their legal status over this period. Hence, the coding of any state as either a shallissue or non-shall-issue state will not influence the estimated effect on crime as long as the legal status persisted over the entire sample time period.<sup>136</sup> Perhaps surprisingly, there are conflicts among the supporters and opponents of gun control legislation about whether various jurisdictions even have a shall-issue law or not. For example, the National Rifle Association characterizes Alabama and Connecticut as having shall-issue laws while the Brady Campaign to Prevent Gun Violence treat both states as having a more discretionary system of providing permits to carry concealed weapons (a so-called "may-issue" law).<sup>137</sup> Since neither of these states' laws officially changed status after 1977 (although query whether administrative enforcement patterns, as well as citizen behavior concerning the purchase and carrying of handguns, may have changed over this period), this dispute will have no bearing on the estimated effect of

137. See http://www.bradycampaign.org/facts/gunlaws/ccw.asp. The NRA's description of the relevant Alabama law suggests that demonstrating need is in fact a obtaining a concealed-carry (http://www.nraila.org/ requirement for permit GunLaws.asp?FormMode=Detail&R=AL), which would seemingly support the coding advanced by the Brady Campaign. On the other hand, it might well be the case that a law that, on the surface, seems to be a "may issue" law was always, or came to be, administered as a shall-issue law. Indeed, between 1985 and the present, every southern state from Texas to Virginia-with the single exception of Alabama-adopted a shall-issue law, and it is possible that this lone exception to a universal southern trend of adoption reflects the NRA's recognition that Alabama had become a shall-issue state in practice even if it had not originally been one by virtue of statutory language.

The case of Connecticut is more complicated, and since no state bordering on Connecticut has enacted the law, one cannot draw inferences about the enforcement of the law from geography as we suggested for Alabama. In January 1978, there was debate over whether local police chiefs (who first need to approve any application to carry a pistol before state-level approval can be sought) were being too stringent in rejecting the applications. At that point, nearly 50,000 Connecticut residents held state pistol permits, up from 27,628 in 1973. Apparently, concerns by the police about who was getting gun permits led local police chiefs to begin setting their own ground rules for determining who should get permits. *See* Millstein, *supra* note 20, at 1.

<sup>135.</sup> Louis L. Jaffe, Profesors and Judges as Advisors to Government: Reflections on the Roosevelt-Frankfurter Relationship, 83 HARV. L. REV. 366 (1969) (quoting LEARNED HAND, THE SPIRIT OF LIBERTY 138 (1952)).

<sup>136.</sup> Since the data from these states will influence the year effects and the estimated coefficients for the various explanatory variables, their inclusion in the analysis—as opposed to the coding of their shall-issue laws—will have an indirect influence on the estimated effect of shall-issue laws.

shall-issue laws. Nonetheless, there are numerous disagreements among different scholars about the timing of adoption of shall-issue laws that can influence these estimates.<sup>138</sup> For purposes of replication, we generally tried to use shall-issue codings that were closer to those employed by Lott and Mustard. In light of our need to update the data beyond what Lott and Mustard's coding sources provided, we relied heavily on Vernick's coding, as this was an up-to-date coding of shall-issue laws by independent researchers. Appendix Table 1 sets forth the list of states that have shall-issue laws under the coding of Lott and Vernick (and underscores some discrepancies in Lott's coding).

Choices have to be made not only about identifying when and if a state adopts a shall-issue law but also how to begin modeling its effect. Lott states that he assumes the effect of the shall-issue law would emerge in the first year after the law takes effect. However, Lott coded the shall-issue law dummy in that fashion only for Florida and Georgia, with all other states being coded so that the effect of the law begins in the year of passage.<sup>139</sup> Appendix Table 1 shows the passage dates of the various shall-issue laws we employ when attempting to adhere to Lott's coding, our differences with Lott and Mustard, and their own inconsistencies across models. The coding used by Vernick, which we also employ in our regressions, is set forth in column D. Our postpassage dummy, as well as our postpassage trends, are coded to begin in the year following the passage dates indicated in either column A or D of Appendix Table 1, depending on whether we are trying to follow Lott or Vernick's coding.

Note that there is imprecision in these dates both because the statutes are not entirely clear about the precise legal status and because adoption of a shallissue law does not perfectly equate with the actual enforcement of the law either within the state or over time since enforcement could be quite different county-by-county and year-by-year. This problem will exist whenever one must characterize imprecisely defined statutes into sharply delineated discrete categories so that one can say when a state changes from one category to

<sup>138.</sup> While conceding that there are different interpretations of which states have shallissue laws, Lott and Mustard indicate that they follow the shall-issue law classification found in Clayton E. Cramer & David B. Kopel, "Shall-issue": The New Wave of Concealed Handgun Permit Laws, 62 TENN. L. REV. 679, 691 (1995). See Lott & Mustard, supra note 1, at 12 n.33. Lott and Mustard cite two states in particular—Maine and Virginia—as potentially not "true" shall-issue states though they state that their results are not affected by either redefining or dropping these states altogether. See id. at 12 nn.33-35, 23 n.49. Indeed, other scholars provide different dates of passage of the shall-issue laws for these two states than those offered by Cramer and Kopel. See Vernick & Hepburn, supra note 28, tbl.9A-5, at 387, 390.

<sup>139.</sup> Ayres & Donohue, *supra* note 15, at 449 n.21. Lott made different coding choices in his linear trend analysis, coding the enactment dates in Oregon, Pennsylvania, Virginia, and Philadelphia earlier than was proper. Lott's coding can be found in state datasets downloadable from http://www.law.yale.edu/ayres/.

another. The difficulty is greatest when the statutory language of the shallissue law invokes the command "shall" but then includes inherently discretionary criteria (such as a requirement for the "good moral character" of the permit recipient), or where the law says "may," but some counties or issuing authorities make it quite easy to obtain a permit. In either of these circumstances, there may be important differences between the law in practice and the law on the books; yet none of those nuances will be captured in our statistical analysis.<sup>140</sup>

#### APPENDIX II: DATA SOURCES

Variable	Source	Description
Crime Rates	Bureau of Justice Statistics	Per 100,000 population
Arrest Rates	Lott	Considered states with 0 arrest rate to be missing observations (24 total changes made over 9 crime categories)
Incarceration Rates	Sourcebook of Criminal Justice Statistics	Lagged one year, sentenced prisoners per 100,000 resident population
State Population	US Census Bureau	
Demographic Groups	US Census Bureau	Converted into percentage of total state population
Income (income, income maintenance, and unemployment insurance)	Regional Economic Information System	Adjusted to 1983 dollars
Density	Land Area in Square Miles obtained from Statistical Abstract of the United States	Density = Population per square mile

The following describes the source of the data used in the state dataset.

Page 295

<sup>140.</sup> See Vernick & Hepburn, supra note 138, at 29. While there are enough classification discrepancies among the different authors that have tried to determine the presence of shall-issue laws that it becomes burdensome to probe all of the possible permutations, our efforts suggested that the aggregated results (which are weighted by state population, thereby sharply reducing the impact of small states on the analysis) were not highly sensitive to these classification issues. Of course, any estimated effects of the shall-issue laws for individual states will be far more sensitive to the classification issues for these small states.

-	(A)	<b>(B)</b>	(C)	(D)
		Implied Dat	e of Passage	-
	Ayres &	Lott & Mustard	Lott & Mustard	Date of Passage
State	Donohue	Dummy*	Trend	Vernick's Coding
Maine	1985	1984	1985	<u> </u>
Florida	1987	1987	1987	1987
Virginia	1988	1987	1987	1995
Georgia	1989	1989	1989	1989
Pennsylvania**	1989	1988	1988	1989
West Virginia	1989	1988	1989	1989
Idaho	1990	1989	1990	1990
Mississippi	1990	1989	1990	1990
Oregon	1990	1989	1989	1990
Montana	1991	1990	1991	1991
Alaska	1994			1994
Arizona	1994			1994
Tennessee	1994			1994
Wyoming	1994			1994
Arkansas	1995			1995
North Carolina	1995			1995
Nevada	1995			1995
Oklahoma	1995			1995
Texas	1995			1996
Utah	1995			1986
Philadelphia**	1995			• •
Kentucky	1996			1996
Louisiana	1996			1991
South Carolina	1996			1996

#### APPENDIX TABLE 1: DIFFERENCES IN CODING DATES OF PASSAGE OF SHALL-ISSUE LAWS

Both Lott/Mustard and Vernick considered the following states to have adopted shall issue laws prior to 1977: Indiana, New Hampshire, and Washington. Lott and Mustard also considered Alabama, Connecticut, North Dakota, South Dakota, and Vermont as having adopted shall issue laws prior to 1977. Of those states, Vernick listed both North Dakota and South Dakota as states adopting shall issue laws in 1985, while he considered Alabama and Connecticut as states that never adopted shall issue laws. Finally, Vernick pointed out that in Vermont, no permit is necessary to carry a concealed weapon.

\* We followed the protocol of beginning the first year of the post-passage dummy in the first full year after passage, which we understood to be the protocol of Lott and Mustard. On the assumption that they did follow that protocol, we list the dates of passage of shall issue laws that would be implied in their analysis. The data supplied to us by Lott and Mustard went through 1992, so we don't show dates for them after that year.

\*\* Pennsylvania initially excluded Philadelphia from its 1989 shall-issue law. In 1995, the law was extended to include Philadelphia.

Note: The shaded lines highlight instances of conflicts between the Lott coding and the Vernick coding.

Ex. 13

Page 296

pnm5064

pnmo65

pnf1019

pnf2029

pnf3039

pnf4049

pnf5064

pnfo65

% Pop Neither W nor B Male Aged 50-64

% Pop Neither W nor B Male Aged over 65

% Pop Neither W nor B Female Aged 10-19

% Pop Neither W nor B Female Aged 20-29

% Pop Neither W nor B Female Aged 30-39

% Pop Neither W nor B Female Aged 40-49

% Pop Neither W nor B Female Aged 50-64

% Pop Neither W nor B Female Aged over 65

Variable	Description	Obs	Mean	Std. Dev.	Min	Max
lvio	In(violent crime rate (per 100,000))	1428	5.971531	0.6543401	3.826465	7.979955
lmur	ln(murder rate (per 100,000))	1428	1.805917	0.703457	-1.609438	4.389499
Irap	ln(rape rate (per 100,000))	1428	3.391876	0.4712118	1.589235	4.626932
laga	In (aggravated assault rate (per 100,000))	1428	5.440435	0.6506613	3.328627	7.350902
lrob	ln(robbery rate (per 100,000))	1428	4.668568	0.9453066	1.856298	7.399459
lpro	In(property crime rate (per 100,000))	1428	8.356463	0.2943492	7.175796	9.16032
lbur	In(burglary rate (per 100,000))	1428	6.98	0.3846931	5.729775	7.974774
llar	ln(larceny rate (per 100,000))	1428	7.920612	0.2970966	6.665301	8.671424
laut	In(auto crime rate (per 100,000))	1428	5.8785	0.5515542	4.507557	7.517467
v_shall	Vernick's coding of the shall dummy	1428			0	1
stpop	State population	1428	4703487	5117964	324465	3.31 x 10 <sup>7</sup>
rpepi	Real per capita personal income	1428	13280.69	2618.36	7644.664	23646.71
грсиі	Real per capita unempl insurance payments	1428	71.72929	47.68851	9.967369	411.6423
rpcim	Real per capita income maintenance	1428	170.4768	68.18757	41.25565	494.4506
densitym	Population per square mile of land area	1428	356.6089	1407.823	0.5696366	12167.85
incarc_rate	Incarceration Rate per 100,000, lagged 1 yr	1428	201.4424	172.2202	19	1913
aovio	Arrest Rate, from Lott's original state dataset	1041	39.68218	20.69746	0	558.81
aopro	Arrest Rate, from Lott's original state dataset	1048	16.47071	4.961436	0	58.56
aomur	Arrest Rate, from Lott's original state dataset	1046	88.02721	52.75273	0	1363.16
aorap	Arrest Rate, from Lott's original state dataset	1041	38.04754	17.26306	0	310.63
aorob	Arrest Rate, from Lott's original state dataset	1047	30.07347	13.36918	0	195.2
aoaga	Arrest Rate, from Lott's original state dataset	1048	43.58897	17.30412	0	190.93
aobur	Arrest Rate, from Lott's original state dataset	1048	13.26896	4.67076	0	44.73
aolar	Arrest Rate, from Lott's original state dataset	1048	18.01515	5.475389	0	77.27
aoaut	Arrest Rate, from Lott's original state dataset	1048	20.08427	33.46592	0	394.28
fipsstat	Numerical state identifier	1428			1	56
year	Numerical year identifier	1428			72	99
vernick_shall	Equivalent to Zheng's yradopt	1428			0	96
pbm1019	% Pop Black Male Aged 10-19	1428	1.034473	1.133384	0.0220235	7.135754
pbm2029	% Pop Black Male Aged 20-29	1428	0.9025913	0.9811033	0.0308761	6.571098
pbm3039	% Pop Black Male Aged 30-39	1428	0.7074403	0.8214441	0.0131709	5.368865
pbm4049	% Pop Black Male Aged 40-49	1428	0.5113697	0.619647	0.0072583	4.44528
pbm5064	% Pop Black Male Aged 50-64	1428	0.5243105	0.6860058	0.0012312	4.82312
pbmo65	% Pop Black Male Aged over 65	1428	0.3556466	0.4857088	0.0014183	3.556977
pbf1019	% Pop Black Female Aged 10-19	1428	1.02034	1.147568	0.0159746	7.390942
pbf2029	% Pop Black Female Aged 20-29	1428	0.9774034	1.157651	0.0169809	7.729461
pbf3039	% Pop Black Female Aged 30-39	1428	0.8107628	0.9713302	0.0092844	6.112494
pbf4049	% Pop Black Female Aged 40-49	1428	0.6017382	0.7530134	0.0050729	5.446821
pbf5064	% Pop Black Female Aged 50-64	1428	0.6540152	0.8833761	0.0019866	6.116782
pbfo65	% Pop Black Female Aged over 65	1428	0.5415593	. 0.7896529	0.0013851	6.115532
pwm1019	% Pop White Male Aged 10-19	1428	6.885923	1.729288	1.157911	10.75591
pwm2029	% Pop White Male Aged 20-29	1428	6.968634	1.334905	3.05121	10.84577
pwm3039	% Pop White Male Aged 30-39	1428	6.387067	1.254675	1.804001	9.731769
pwm4049	% Pop White Male Aged 40-49	1428	5.151871	1.166269	1.346922	8.659682
pwm5064	% Pop White Male Aged 50-64	1428	5.879721	1.062361	1.783073	8.037685
pwmo65	% Pop White Male Aged over 65	1428	4.288742	1.158931	0.6743689	7.505016
pwf1019	% Pop White Female Aged 10-19	1428	6.565628	1.699978	1.184778	10.59967
pwf2029	% Pop White Female Aged 20-29	1428	6.829447	1.34522	2.216033	9.662052
pwf3039	% Pop White Female Aged 30-39	1428	6.349452	1.240235	1.5848	9.385077
pwf4049	% Pop White Female Aged 40-49	1428	5.179891	1.135794	1.200093	8.411426
pwf5064	% Pop White Female Aged 50-64	1428	6.896208	1.479002	1.649705	11.36171
pwfo65	% Pop White Female Aged over 65	1428	6.182254	1.679061	0.7476923	9.902048
pnm1019	% Pop Neither W nor B Male Aged 10-19	1428	0.3777065	0.7904053	0.0145746	6.702039
pnm2029	% Pop Neither W nor B Male Aged 20-29	1428	0.3475048	0.7212771	0.0170222	6.465612
pnm3039	% Pop Neither W nor B Male Aged 30-39	1428	0.3007067	0.6514341	0.0139999	5.061728
pnm4049	% Pop Neither W nor B Male Aged 40-49	1428	0.2186409	0.5309817	0.008304	4.95868

#### APPENDIX TABLE 2A: AYRES AND DONOHUE'S EXPANDED LOTT DATASET: LIST OF VARIABLES AND SUMMARY STATISTICS

Page 297

1428 0.2060169

1428 0.1288453

1428 0.3672072

1428 0.3546779

1428 0.2432578

1428 0.2346599

1428 0.2026374

0.3277798

1428

0.6201685

0.4792777

0.7710804

0.6839861

0.5715415

0.7204218 0.0222513

0.6891328 0.0092633

0.8711339 0.0067783

0.007015

0.0050317

0.0154089

0.0188864

0.0110117

4.910496

4.280399

6.687914

5.324602

5.08445

5.365767

10.9263

6.3433

#### APPENDIX TABLE 2B: ZHENG'S DATASET: LIST OF VARIABLES AND SUMMARY STATISTICS

Variable	Description	Obs	Mean	Std. Dev.	Min	Max
lvio	In(violent crime rate (per 100,000))	1400	5.939708	0.6197534	3.826465	7.126328
lmur	In(murder rate (per 100,000))	1400	1.766978	0.6511872	-1.609438	3.010621
Irap	In(rape rate (per 100,000))	1400	3.380057	0.4663134	1.589235	4.626932
laga	In (aggravated assault rate (per 100,000))	1400	5.417609	0.6333402	3.328627	6.666575
lrob	In(robbery rate (per 100,000))	1400	4.622909	0.8967052	1.856298	6.527958
lpro	In(property crime rate (per 100,000))	1400	8.345676	0.2862991	7.175796	8.986696
lbur	In(burglary rate (per 100,000))	1400	6.969619	0.3803424	5.729775	7.974774
llar	In(larceny rate (per 100,000))	1400	7.910755	0.2908949	6.665301	8.538191
laut	In(auto crime rate (per 100,000))	1400	5.860307	0.5374967	4.507557	7.359531
shalll	Vernick's coding of the shall dummy	1400			0	1
рор	State population	1400	4785007	5136519	326494	3.31 x 10 <sup>7</sup>
rincome	Real per capita income	1400	19335.41	4340.893	8824.639	36795.03
unemp	Unemployment rate	1400	6.213143	2.10274	2.2	18
poverty	% Persons below poverty line	1400	12.82964	3.991182	2.9	30.7
police_per1	% Police, lagged 1 year	1400	196.6087	48.98285	61.89	400.5193
prison per l	Incarceration rate, lagged 1 year	1400	190.0616	126.9586	20.3402	752.3188
alcohol per	Average per capita alcohol consumption	1400	1.986453	0.5379952	0.4770634	4.999822
fipsstat	Numerical state indicator	1400			1	56
year	Numerical year indicator	1400			1972	1999
yradopt	Vernick's coding of the year of adoption	1400			0	96
black per	% Black population	1400	9.454077	9.254046	0.1748652	36.66171
metro per	% Metropolitan population	1400	62.9396	22.86231	0	100.3756
agel per	% Population Aged 15-17	1400	4.904386	0.8245765	3.403702	6.905295
age2 per	% Population Aged 18-24	1400	11.63615	1.673176	7.800048	16.10618
age3_per	% Population Aged 25-34	1400	15.77754	1.865325	10.64819	23.5762

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APPENDIX TABLE 3A: THE ESTIMATED IMPACT OF SHALL-ISSUE LAWS ON CRIME	STATE DATA, 1977-1999 (LOTT'S CODING), USING INCARCERATION KATES	(ANALOGOUS TO TABLE 3A, BUT WITH LOTT'S CODING)
----------------------------------------------------------------------	------------------------------------------------------------------	-------------------------------------------------

	Violent		1	Aggravated	:	Property	Auto		,
Time Period (1977-1999)	Crime	Murder	Rape	Assault	Robbery	Crime	Theft	Burglary	Larceny
. Dummy variable model:	-3.3%	-1.2%	-6.7%	-4.5%	0.3%	2.2%	7.6%	-3.2%	3.2%
Robust standard error:	(1.6%)	(2.0%)	(1.6%)	(2.2%)	(2.2%)	(1.0%)	(2.0%)	(1.4%)	(1.0%)
". Lott spline model:	<u>-0.8%</u>	-1.1%	-1.1%	-0.8%	-1.4%	-0.2%	0.2%	<u>-1.0%</u>	-0.2%
Robust standard error:	(0.4%)	(0.5%)	(0.4%)	(0.5%)	(0.5%)	(0.2%)	(0.4%)	(0.3%)	(0.2%)
. Hvhrid model:									
Postpassage dummy	-2.4%	1.7%	-4.4%	-5.7%	5.5%	2.4%	7.1%	0.6%	2.4%
Robust standard error:	(1.9%)	(2.4%)	(2.2%)	(2.4%)	(2.7%)	(1.3%)	(2.5%)	(1.7%)	(1.3%)
Trend effect	-0.6%	-1.3%	-0.8%	-0.5%	-1.8%	-0.3%	-0.3%	<u>-1.1%</u>	-0.3%
Robust standard error:	(0.4%)	(0.5%)	(0.4%)	(0.6%)	(0.5%)	(0.2%)	(0.4%)	(0.3%)	(0.2%)

regressions are weighted by state population. Standard errors (in parentheses) are computed using the Huber-White robust estimate of variance. Coefficients that are significant at the .10 level are underlined. Coefficients that are significant at the .05 Notes: The dependent variable is the ln(crime rate) named at the top of each column. The data set is comprised of annual state-State- and year- fixed effects are included in all specifications. All evel are displayed in bold. Coefficients that are significant at the .01 level are both underlined and displayed in bold. level observations (including the District of Columbia).

#### Apr. 2003]

Ex. 13 Page 299 HeinOnline -- 55 Stan. L. Rev. 1303 2002-2003

	Violent			Aggravated		Property	Auto	•	
Time Period (1977-1999)	Crime	Murder	Rape	Assault	Robbery	Crime	Theft	Burglary	Larceny
Dummy variable model:	-0.3%	1.9%	-3.7%	-3.1%	5.2%	3.3%	5.0%	1.8%	3.7%
Robust standard error:	(1.6%)	(2.4%)	(1.6%)	(1.9%)	(2.5%)	(1.3%)	(2.2%)	(1.6%)	(1.2%)
Hybrid model:									
Postpassage dummy	-1.9%	4.2%	-3.8%	<u>-6.1%</u>	6.2%	3.6%	7.7%	2.6%	3.5%
Robust standard error:	(1.7%)	(2.3%)	(1.7%)	(2.2%)	(2.6%)	(1.3%)	(2.1%)	(1.6%)	(1.3%)
Trend effect	1.5%	<u>-2.2%</u>	0.1%	2.8%	-0.9%	-0.3%	<u>-2.5%</u>	-0.7%	0.1%
Robust standard error:	(0.4%)	(0.7%)	(0.5%)	(0.6%)	(0.7%)	(0.3%)	(0.6%)	(0.4%)	(0.3%)

State- and year- fixed effects are included in all specifications. All regressions are weighted by state population. Standard errors (in parentheses) are computed using the Huber-White robust estimate of variance. Coefficients that are significant at the .10 level are underlined. Coefficients that are significant at the .05 level are displayed in bold. Coefficients that are significant at the .01 level are both underlined and displayed in bold. level observations (including the District of Columbia).

APPENDIX TABLE 3B: THE ESTIMATED IMPACT OF SHALL-ISSUE LAWS ON CRIME CONTROLLING FOR STATE TRENDS

Ex. 13 Page 300

APPENDIX TABLE 4A: THE ESTIMATED IMPACT OF SHALL-ISSUE LAWS ON CRI	STATE DATA, 1991-1999 (LOTT'S CODING), USING INCARCERATION RATES	(ANALOGOUS TO TABLE 4A, BUT WITH LOTT'S CODING)
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	Violent			Aggravated		Property	Auto		
Time Period (1991-1999)	Crime	Murder	Rape	Assault	Robbery	Crime	Theft	Burglary	Larceny
1. Dummy variable model:	2.7%	7.2%	-1.0%	-1.0%	11.8%	6.3%	11.4%	6.0%	5.4%
Robust standard error:	(2.3%)	(3.7%)	(2.0%)	(3.2%)	(2.3%)	(1.4%)	(3.0%)	(1.5%)	(1.4%)
2. Lott spline model:	-1.3%	-1.6%	-3.0%	-1.6%	0.0%	0.4%	0.1%	1.0%	-0.3%
Robust standard error:	(1.3%)	(2.2%)	(1.4%)	(1.8%)	(1.7%)	(1.0%)	(2.0%)	(1.1%)	(0.9%)
3. Hybrid model:									
Postpassage dummy	1.7%	3.9%	2.6%	-2.0%	7.8%	5.3%	13.8%	4.5%	4.5%
Robust standard error:	(2.7%)	(4.2%)	(2.2%)	(3.9%)	(2.9%)	(1.8%)	(3.6%)	(2.0%)	(1.7%)
Trend effect	-1.6%	-2.1%	-3.4%	-1.3%	-1.1%	-0.4%	-1.9%	0.4%	-0.9%
Robust standard error:	(1.4%)	(2.2%)	(1.5%)	(1.9%)	(1.6%)	(0.6%)	(1.8%)	(1.1%)	(%6.0)

regressions are weighted by state population. Standard errors (in parentheses) are computed using the Huber-White robust State- and year- fixed effects are included in all specifications. All estimate of variance. Coefficients that are significant at the .10 level are underlined. Coefficients that are significant at the .05 informe rate) manned at the top of each column. The data set is comprised of annual statelevel are displayed in bold. Coefficients that are significant at the .01 level are both underlined and displayed in bold. level observations (including the District of Columbia).

	Violent			Aggravated		Property	Auto	2	
Time Period (1991-1999)	Crime	Murder	Rape	Assault	Robbery	Crime	Theft	Burglary	Larcen
Dummy variable model:	0.8%	-3.1%	-0.2%	-0.5%	3.3%	4.0%	9.3%	2.1%	3.6%
Robust standard error:	(2.0%)	(4.3%)	(1.9%)	(3.0%)	(2.3%)	(1.8%)	(3.2%)	(1.8%)	(1.7%
Hybrid model:									
Postpassage dummy	0.6%	-2.0%	3.9%	-0.9%	2.2%	4.6%	10.4%	1.9%	4.6%
Robust standard error:	(2.2%)	(4.5%)	(2.1%)	(3.1%)	(2.5%)	(1.9%)	(3.5%)	(2.0%)	(1.9%)
Trend effect	0.3%	-1.7%	<u>-6.1%</u>	0.7%	1.7%	-1.0%	-1.7%	0.3%	-1.5%
Robust standard error:	(1.4%)	(2.6%)	(1.4%)	(2.0%)	(1.5%)	(1.0%)	(2.0%)	(1.1%)	(1.0%)

regressions are weighted by state population. Standard errors (in parentheses) are computed using the Huber-White robust estimate of variance. Coefficients that are significant at the .10 level are underlined. Coefficients that are significant at the .05 level are displayed in bold. Coefficients that are significant at the .01 level are both underlined and displayed in bold. i guunig auvits

Ex. 13 Page 302 HeinOnline -- 55 Stan. L. Rev. 1306 2002-2003

•	Violent			Aggravated	-	Property	Auto		
Time Period (1977-1999)	Crime	Murder	Rape	Assault	Robbery	Crime	Theft	Burglary	Larceny
Dummy variable model:	-2.1%	1.4%	-4.2%	-4.2%	3.5%	2.6%	8.5%	-0.8%	3.0%
Robust standard error:	(1.7%)	(2.0%)	(1.9%)	(2.1%)	(2.4%)	(1.1%)	(2.1%)	(1.5%)	(1.1%)
Lott spline model:	-0.2%	-0.6%	-0.1%	-0.5%	-0.6%	0.0%	0.4%	-0.2%	-0.3%
Robust standard error:	(%9.0)	(0.7%)	(0.6%)	(0.8%)	(0.8%)	(0.4%)	(0.7%)	(0.5%)	(0.4%)
Hybrid model:									
Postpassage dummy	-2.2%	1.3%	-4.8%	-5.7%	6.0%	<u>2.6%</u>	6.5%	1.0%	2.5%
Robust standard error:	(2.0%)	(2.4%)	(2.2%)	(2.5%)	(2.8%)	(1.3%)	(2.6%)	(1.8%)	(1.3%)
Trend effect	-0.1%	-0.6%	0.1%	-0.2%	-0.9%	-0.2%	0.1%	-0.2%	-0.4%
Robust standard error:	(0.6%)	(0.7%)	(0.6%)	(0.8%)	(0.8%)	(0.4%)	(0.7%)	(0.5%)	(0.4%)

estimate of variance. Coefficients that are significant at the .10 level are underlined. Coefficients that are significant at the .05 level are displayed in bold. Coefficients that are significant at the .01 level are both underlined and displayed in bold.

Page 303 HeinOnline -- 55 Stan. L. Rev. 1307 2002-2003

Ex. 13

	Violent			Aggravated		Property	Auto		
Time Period (1977-1999)	Crime	Murder	Rape	Assault	Robbery	Crime	Theft	Burglary	Larcen
Dummy variable model:	-0.1%	2.8%	-3.7%	-3.6%	6.9%	3.9%	5.6%	3.4%	3.8%
Robust standard error:	(1.6%)	(2.4%)	(1.6%)	(2.0%)	(2.6%)	(1.3%)	(2.3%)	(1.6%)	(1.3%)
Hybrid model:									
Postpassage dummy	-2.0%	3.8%	-4.4%	-6.5%	<u>6.8%</u>	3.9%	7.3%	3.1%	3.8%
Robust standard error:	(1.8%)	(2.4%)	(1.7%)	(2.2%)	(2.6%)	(1.3%)	(2.2%)	(1.7%)	(1.3%)
Trend effect	<u>2.6%</u>	-1.3%	1.0%	4.0%	0.1%	0.0%	-2.4%	0.3%	0.1%
Robust standard error:	(0.5%)	(0.9%)	(0.7%)	(0.7%)	(0.9%)	(0.4%)	(0.8%)	(0.5%)	(0.4%)

regressions are weighted by state population. Standard errors (in parentheses) are computed using the Huber-White robust estimate of variance. Coefficients that are significant at the .10 level are underlined. Coefficients that are significant at the .05 level are displayed in bold. Coefficients that are significant at the .01 level are both underlined and displayed in bold.

#### STANFORD LAW REVIEW

HeinOnline -- 55 Stan. L. Rev. 1308 2002-2003

#### APPENDIX TABLE 6: ESTIMATING THE EFFECT OF SHALL-ISSUE LAWS USING LOTT'S TIME TREND SPECIFICATION FOR SELECTED YEARS

	Violent			Aggravated	<u> </u>	Property	Auto		
	Crime	Murder	Rape	Assault	Robbery	Crime	Theft	Burglary	Larceny
Panel A H ott's Result	, s 1977-1992 (	Table 4 8)							
After - before	-0.9%	-3.0%	-1.4%	-0.5%	-2 7%	-0.6%	-0.3%	-1.5%	-0.1%
Significance level	0.01	0.01	0.01	0.01	0.01	0.01	0.05	0.01	not sig
Panel A2: Reproduction	on of Lott's Tre	nd Analysis	(1977-1992)						
Shall trend before	-0.0048	0.0061	-0.0032	-0.0125	0.0103	0.00828	0.01292	0.0076	0.0068
Shall trend after	-0.0098	-0.036	-0.017	-0.008	-0.0168	0.00143	0.00995	-0.0082	0.0057
After - before	-0.50%	-4.21%	-1.38%	0.45%	-2.71%	-0.69%	-0.30%	-1.58%	-0.11%
P-value	0.222	0	0.0073	0.3377	0	0.0234	0.5341	0	0.7661
Panel A3: Correcting I	Lott's Trends (	1977-1992)							
Shall trend before	-0.0058	0.0054	-0.0026	-0.0129	0.0073	0.00642	0.0113	0.0048	0.00491
Shall trend after	-0.0104	-0.0442	-0.0207	-0.0089	-0.0121	0.00628	0.0111	-0.00034	0.0124
After - before	-0.46%	-4.96%	-1.81%	0.40%	-1.94%	-0.01%	-0.02%	-0.51%	0.75%
P-value	0.2961	0	0.0009	0.4312	0.0011	0.9657	0.9778	0.1285	0.0593
Panel B1: Lott's Result	ts, 1977-1996	(Table 9.1)							
After - before	-2.3%	-1.5%	-3.2%	-3.0%	-1.6%	-2.5%	-2.1%	-2.5%	-0.9%
Significance level	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.0İ	0.01
Panel B2: Reproductio	on of Lott's Tre	nd Analysis	(1977-1996)	•					
Shall trend before	0.0022	0.0057	0.0096	0.0042	0.0100	0.0050	0.0130	0.0062	0.0082
Shall trend after	-0.0229	-0.0281	-0.0256	-0.0325	-0.0372	-0.0004	0.0089	-0.0211	-0.0040
After - before	-2.53%	-3.38%	-3.52%	-3.67%	-4.72%	-0.54%	-0.41%	-2.73%	-1.22%
P-value	0	0	0	0	0	0.0886	0.3946	0	0.0212
Panel B3: Correcting I	Lott's Trends (1	977-1996)							
Shall trend before	0.0014	0.0058	0.0097	0.0036	0.0089	0.0045	0.0132	0.0055	0.0081
Shall trend after	-0.0117	-0.0238	-0.0246	-0.0229	-0.0262	0.0030	0.0026	-0.0167	-0.0041
After - before	-1.31%	-2.96%	-3.43%	-2.65%	-3.51%	-0.15%	-1.06%		-1.22%
P-value	0	. 0	0	0	0	0.56	0.0124	0	0.0042

\* Following Lott's Table 9.1, year\*region dummies were included in this model as well as a RPCRPO\*(YEAR>92) dummy.

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HeinOnline -- 55 Stan. L. Rev. 1309 2002-2003

# APPENDIX TABLE 7: THE ESTIMATED IMPACT OF SHALL ISSUE LAWS ON CRIME, JURISDICTION-SPECIFIC HYBRID MODEL WITH STATE TRENDS

	77' 1				·				
(1977-1997)	Crime	Murder	Rane	Aggravated	Robberv	Crime	Auto Theft	Burglary	Larcenv
			Tupe_	ribbuutt	Robberg			Buiging	Later
Maine									
Postpassage dummy	-13.0%	15.6%	12.9%	<u>-21.6%</u>	-7.7%	-5.2%	-4.9%	<u>-12.3%</u>	-6.1%
T. 1.00	(5.8%)	(17.9%)	(8.6%)	(7.2%)	(9.3%)	(3.5%)	(5.0%)	(3.5%)	(4.7%)
I rend effect	2.7%	-2.8%	2.3%	2.1%	2.6%	1.9%	0.1%	4.5%	1.5%
	(1.1%)	(3.0%)	(1.6%)	(1.5%)	(1.6%)	(0.6%)	(0.9%)	(0.7%)	(0.7%)
Florida									
Postpassage dummy	-2.0%	17.0%	<u>21.2%</u>	<u>45.1%</u>	<u>49.5%</u>	2.7%	<u>56.6%</u>	<u>53.0%</u>	<u>54.5%</u>
	(4.3%)	(12.9%)	(12.2%)	(11.9%)	(11.7%)	(6.4%)	(12.6%)	(9.9%)	(16.9%)
Trend effect	<u>-2.9%</u>	<u>-14.3%</u>	<u>-9.0%</u>	<u>-10.1%</u>	<u>-19.5%</u>	<u>-2.6%</u>	<u>-14.2%</u>	<u>-15.7%</u>	<u>-10.9%</u>
	(0.8%)	(2.7%)	(2.5%)	(2.5%)	(2.5%)	(0.8%)	(2.5%)	(2.1%)	(2.5%)
Virginia									
Postpassage dummy	-3.2%	10.2%	6.2%	-4.6%	-5.2%	2.4%	2.1%	0.1%	1.7%
	(3.0%)	(8.4%)	(4.4%)	(3.6%)	(3.9%)	(2.8%)	(4.2%)	(2.4%)	(4.1%)
Trend effect	0.8%	-0.5%	0.6%	<u>2.0%</u>	0.5%	0.5%	<u>1.3%</u>	0.2%	0.2%
	(0.6%)	(1.4%)	(0.8%)	(0.7%)	(0.7%)	(0.4%)	(0.8%)	(0.5%)	(0.5%)
Georgia									
Postpassage dummy	3.4%	10.7%	-1.8%	3.5%	2.8%	-1.9%	-3.3%	4.1%	-6.6%
	(3.8%)	(7.2%)	(5.3%)	(4.4%)	(4.4%)	(3.1%)	(4.4%)	(3.1%)	(4.1%)
Trend effect	-3.5%	-4.5%	-6.0%	-0.7%	-6.8%	-3.2%	-1.3%	-6.1%	-2.2%
	(0.8%)	(1.3%)	(1.0%)	(0.9%)	(1.0%)	(0.6%)	(1.0%)	(0.7%)	(0.7%)
Panneylyania	( · · · )	(,	()	(····)	(	<b>、</b> · · · · /		( · · · · /	
Postnessage dummy	2 204	8 09/	2 50/	0.69/	2 404	1 /19/	2 40/	1 20/	A 496
Postpassage dummy	-2.270	-0.070	3.3% (3.7%)	-0.0% (1 20/)	-2.470	-1.4%	2.470	(2 39/)	(7 00/)
Trend effect	(3.3%)	2 10/2	-0.3%	0.8%	(4.3/0) 3 70/	2.570)	-0.8%	(2.370)	(2.270)
Tiend effect	(0.8%)	(1.3%)	(0.8%)	(0.0%)	(1.0%)	(0.5%)	(1.1%)	(0.6%)	(0.5%)
D1 11 1 1 1 1	(0.070)	(1.570)	(0.070)	(0.970)	(1.070)	(0.570)	(1.170)	(0.070)	(0.570)
Philadelphia		5 50/	<u> </u>	0.70/					0 (0)
Postpassage dummy	13.9%	5.5%	-6.8%	-9.7%	24.8%	<u>-30.1%</u>	-16.3%	-17.7%	-0.6%
T 1 + 60+-+	(0.0%)	(10.2%)	(0.6%)	(7.1%)	(8.1%)	(4.8%)	(8.2%)	(7.3%)	(5.6%)
I rend effect	-1./%	3.3%	$\frac{4.9\%}{(2.6\%)}$	(2.09/)	<u>-0.4%</u>	(2.09/)	10.7%	5./% (3.2%)	0.0%
	(3.3%)	(3.6%)	(2.0%)	(3.0%)	(3.8%)	(2.0%)	(4.0%)	(2.3%)	(2.1%)
West virginia									
Postpassage dummy	-1.9%	11.2%	4.8%	0.8%	<u>-13.8%</u>	<u>10.4%</u>	<u>-17.9%</u>	<u>11.3%</u>	9.0%
	(6.2%)	(10.5%)	(8.9%)	(8.3%)	(7.8%)	(3.4%)	(5.6%)	(3.7%)	(4.3%)
Trend effect	<u>7.1%</u>	-4.5%	0.9%	<u>10.5%</u>	2.1%	-0.6%	0.3%	-0.3%	0.7%
	(1.3%)	(2.0%)	(1.7%)	(1.7%)	(1.7%)	(0.7%)	(1.4%)	(0.9%)	(0.8%)
Idaho									
Postpassage dummy	-4.3%	-26.9%	18.6%	-7.9%	2.1%	0.7%	1.0%	-11.6%	-0.7%
	(6.6%)	(20.2%)	(7.6%)	(7.6%)	(9.5%)	(3.7%)	(6.2%)	(5.2%)	(4.0%)
Trend effect	<u>3.9%</u>	8.3%	1.9%	<u>5.5%</u>	4.7%	<u>1.4%</u>	3.8%	3.0%	1.2%
	(1.5%)	(3.8%)	(1.7%)	(1.7%)	(2.0%)	(0.9%)	(1.6%)	(1.2%)	(0.9%)
Mississippi									
Postpassage dummy	-2.1%	-3.7%	-1.8%	-15.3%	3.5%	-7.9%	2.1%	-4.9%	-19.8%
	(9.1%)	(10.1%)	(10.0%)	(10.8%)	(10.5%)	(8.6%)	(11.9%)	(8.8%)	(9.1%)
Trend effect	<u>13.3%</u>	<u>8.1%</u>	<u>4.1%</u>	16.7%	12.0%	<u>6.7%</u>	<u>11.7%</u>	7.4%	8.8%
	(2.1%)	(2.0%)	(2.1%)	(2.5%)	(2.2%)	(2.0%)	(2.5%)	(2.0%)	(2.2%)
Oregon									
Postpassage dummy	-6.8%	-11.5%	3.5%	-2.7%	-30.9%	-18.0%	-21.1%	-29.5%	-17.3%
· ••••panongo zannin)	(4.7%)	(11.3%)	(5.8%)	(7.8%)	(6.1%)	(3.2%)	(6.1%)	(4.2%)	(3.8%)
Trend effect	3.8%	-4.5%	-4.0%	9.5%	-0.8%	4.4%	4.0%	1.9%	5.1%
	(1.2%)	(2.6%)	(1.2%)	(1.8%)	(1.6%)	(0.7%)	(1.3%)	(0.9%)	(0.8%)
Montona	(	(	()	()	()	()	()	()	()
Postnessee dummy	2.0%	02.09/	22.29/	0.49/	2/ 20/	0.19/	22.20/	11 10/	12.6%
r ostpassage uunnny	-2.070	(27 7%)	-22.370 (A1 50/)	(77 60/)	-34.370 (AA 70%)	-2.170	(20 6%)	(18 2%)	(20) 20%)
Trend effect	8.6%	13.0%	-2.0%	17 19/	13.0%	(19.576) 4 7%	Q 4%	6 3%	5.0%
i tella ericet	(4 0%)	(4 20%)	-2.770 (Q ()%)	(5 (10/2))	(8 20%)	(3,8%)	(4 8%)	(3.6%)	(4.0%)
41.1	(4.970)	(4.070)	(2.070)	(3.070)	(0.270)	(0.070)	(7.070)	(5.070)	(4.070)
Alaska	0.707	0 60/	21.207	0.107	20.10/	1 10/	2 20/	7 407	0.00/
rostpassage dummy	-0.0%	8.3%	-21.3%	-8.1%	39.1%	-1.1%	3.3%	1.4%	-0.9%
Trand offect	(40.2%)	(02.1%)	(45.2%)	(44.6%)	(20.1%)	(28.9%)	(41.2%)	(40.2%)	(23.1%)
riend cricct	1.3%	0.1%	-J./%	(17.40/)	-17.1%	(12 20/)	-2.3%	~J.0%	3.0% (0.00/\
	(13.770)	(24.370)	(10.070)	(17.470)	(11.370)	(12.370)	(12.170)	(17.970)	(3.370)

continued...

Ex. 13

Time Period	Violent			Aggravated		Property	Auto		
(1977-1997)	Crime	Murder	Rape	Assault	Robbery	Crime	Theft	Burglary	Larceny
Arizona									
Post-Passage Dummy	7.7%	17.2%	-7.4%	6.6%	4.5%	15.3%	<u>30.6%</u>	5.9%	7.6%
m	(8.3%)	(12.1%)	(8.0%)	(8.7%)	(9.9%)	(6.8%)	(17.1%)	(8.8%)	(6.2%)
I rend Effect	1.4%	0.2%	7.4%	0.0%	$\frac{8.1\%}{(4.29)}$	-2.2%	-4.5%	(2.49)	1.4%
_	(3.7%)	(4.8%)	(3.4%)	(3.9%)	(4.3%)	(2.8%)	(0.5%)	(3.4%)	(2.7%)
Tennessee					0.00/	1.00/		1.00/	
Post-Passage Dummy	/.0%	-3.3%	-5.5%	13.1%	-0.3%	4.9%	-2.5%	-1.8%	3.2%
Turn J Effect	(10.1%)	(10.7%)	(10.0%)	(12.2%)	(8.9%)	(8.0%)	(12.4%)	(8.7%)	(8.3%)
I rend Effect	0.3%	(5.5%)	$\frac{9.0\%}{(4.0\%)}$	(5.39/)	0.7%	3.3%	0.4%	0.3%	3.0% (4.0%)
	(4.070)	(3.576)	(4.9%)	(3.376)	(4.270)	(4.076)	(0.0%)	(4.470)	(4.070)
Wyoming Durt During	14 20/	20.50/	07.00/	25.00/	7 50/	11.10/	16 60/	0.00/	6 10/
Post-Passage Dummy	-14.3%	39.3%	27.9%	-25.0%	(7,3%)	$\frac{11.1\%}{(6.69)}$	10.5%	8.2%	5.1%
Turned II ffer at	(12.6%)	(47.1%)	(19.8%)	(15.7%)	(24.1%)	(0.5%)	(11.0%)	(10.3%)	(7.3%)
Trenu Effect	(5 90/)	-12.270	-0.070	$\frac{12.170}{(7.20/)}$	9.970	-0.470	-0.9%	4.9%	(2.20/)
	(3.6%)	(17.270)	(0.070)	(7.370)	(10.970)	(2.770)	(3.270)	(4.270)	(3.270)
Arkansas	7.50/	20.00/	1.00/	10.10/	12.00/	£ 00/	2.10/	14.007	0.007
Post-Passage Dummy	-1.3%	-30.9%	-1.9%	10.1%	-12.0%	3.8% (7.0%)	-3.1%	-14.9%	8.9%
Trand Effort	(18.0%)	(19.9%)	(15.7%)	(24.1%)	(17.0%)	(7.0%)	(11.9%)	(11.0%)	(7.3%)
Trend Effect	(12.7%)	(12 10/)	0.4% (10.2%)	12.7%	(10.7%)	+0.370 (4 40/)	1.270	J.J70 (6.8%)	+1.070 (1.6%)
N 1	(12.770)	(13.170)	(10.270)	(10.476)	(10.770)	(4.470)	(7.470)	(0.070)	(4.070)
Nevada	22.00/	22 60/	4 907	22.60/	15 79/	14.20/	10 69/	15 40/	10.00/
Post-Passage Dummy	(14.79%)	32.0%	-4.8%	<u>33.0%</u>	(12.0%)	14.3%	10.0%	15.4%	$\frac{18.0\%}{(0.49/)}$
Trand Effect	(14.7%)	(40.370) 5 <b>5</b> %	(41.370)	(19.8%)	(12.9%)	(9.2%)	(10.270)	(11.4%) 5 Q0/	(9.070)
Hend Effect	(8.1%)	(23.2%)	(15.3%)	(10.6%)	(7.7%)	(5.4%)	(12.1%)	(7.4%)	-5.170
Marth Carelline	(0.170)	(25.270)	(15.570)	(10.070)	(7.770)	(3.470)	(14.170)	(7.170)	(0.570)
North Carolina	0.00/	1 70/	12.00/	9 40/	4.08/	12.10/	17.00/	0.28/	11 40/
Post-Passage Dummy	-0.8%	-1.7%	12.0%	8.4%	-4.9%	(6 10/)	1/.8%e	-0.3%	$\frac{11.470}{(6.69/)}$
Trand Effect	(0.270) 5 49/	(14.076)	6 1%	(10.176)	(0.770)	11 00/	0.5%	5 0%	2 8%
riena Effect	(5.2%)	(9.2%)	(6.5%)	(6.2%)	(6.0%)	(3.6%)	(5.1%)	(4.4%)	(4.1%)
011-1	(3.270)	()	(0.570)	(0.270)	(0.070)	(5.070)	(3.170)	(4.470)	(4.170)
Okianoma Dest Bessege Dummu	12 60/	12 60/	10 70/	23.28/	14 10/	e 20/	5 29/	2 20/	7.09/
Post-Passage Dunniny	(13.3%)	(10.1%)	(11.6%)	(14.8%)	-14.176	0.270	-3.3%	(8.8%)	(13.4%)
Trend Effect	.7.9%	-3.7%	0.0%	-10.6%	31%	-7.8%	-3.0%	-4 7%	-1.2%
Tiend Effect	(8.1%)	(10.9%)	(8.4%)	(9.0%)	(6.5%)	(5.6%)	(6.4%)	(5.5%)	(7.8%)
Texas	()	()	(,	(,,)	()	(,	(	()	()
Post-Passage Dummy	-3.1%	-3.9%	9.0%	4.0%	-10.3%	-8 1%	-10.9%	-9.0%	-0.8%
1 0st-1 assage Duttiniy	(8.7%)	(10.8%)	(9.6%)	(16.2%)	(10.5%)	(5.9%)	(11.2%)	(6.6%)	(8.7%)
Trend Effect	-4.7%	-10.5%	-6.2%	-6.0%	-1.5%	-2.3%	-3.6%	-0.3%	-1.1%
	(5.7%)	(6.5%)	(6.2%)	(10.2%)	(6.7%)	(3.6%)	(6.9%)	(4.3%)	(5.2%)
Litah	(	()	(	(,	()	(,	(	(	(/
Post-Passage Dummy	7.9%	55.3%	-2.3%	19.6%	-4 3%	6.8%	35.6%	15.1%	2.6%
1 001 1 4050Be D 411411	(11.5%)	(14.9%)	(17.4%)	(13.2%)	(16.5%)	(6.2%)	(23.9%)	(11.6%)	(6.6%)
Trend Effect	11.2%	-14.5%	12.7%	7.5%	18.5%	-1.2%	3.4%	6.2%	-2.3%
	(6.4%)	(8.4%)	(10.8%)	(7.4%)	(10.6%)	(4.3%)	(14.8%)	(7.8%)	(4.2%)
Kentucky	(	(,	(	( )	(	(	(	<b>X</b>	<b>(</b> )= ) )
Post-Passage Dummy	-1.0%	41.9%	-13 5%	0.2%	29.5%	-7.9%	12.4%	-12.4%	-13.5%
rost russuge Dummy	(12.0%)	(13.8%)	(7.8%)	(20.6%)	(8.5%)	(4.4%)	(17.1%)	(8.4%)	(3.9%)
Louisiana	(	(	(	()	(0.0.)	()	(	(0)	()
Post-Passage Dummy	16.1%	33 5%	20 3%	16.9%	32.1%	17.6%	33.0%	22 30/2	14.6%
1 030-1 assage Duninity	(7.9%)	(8.0%)	(7.2%)	(11.4%)	(11.2%)	(6.1%)	(8,8%)	(7.0%)	(7.0%)
South Carolina	(1.570)	(0.070)	(	(1117))	(********)	(011/0)	(0.070)	()	(,,,,,,,)
Post-Passage Dummer	0 70/	13 60/	_A 50/	14 20/	10 99/	9 20/.	71 40/	1 50/	3 70/
r ost-rassage Dunniny	0.770 (5.6%)	(7.3%)	(5.0%)	(6.1%)	(6.3%)	(3.7%)	$\frac{21,470}{(7,2\%)}$	(3.7%)	(3.9%)
	(3.070)	(7.370)	(3.070)	(0.170)	(0.370)	(3.770)	(7.270)	(3.770)	(3.770)

APPENDIX TABLE 7 (CONTINUED)

Notes: The dependent variable is the ln(crime rate) named at the top of each column. The data set is comprised of annual state-level observations (including the District of Columbia) over the period 1977-1997. State- and year- fixed effects are included in all specifications. All regressions are weighted by state population. Standard errors (in parentheses) are computed using the Huber-White robust estimate of variance. Coefficients that are significant at the .10 level are underlined. Coefficients that are significant at the .05 level are displayed in bold. Coefficients that are significant at the .01 level are both underlined and displayed in bold. Post-Passage Trends for KY, LA and SC are dropped due to late passage date.

Page 307

#### **APPENDIX TABLE 8: PREDICTION RESULTS**

	Predicted	Standard	
Jurisdiction	Impact	Error	t-value
Nebraska	0.456	0.141	3.225
Kansas	0.421	0.139	3.022
Rhode Island	0.419	0.135	3,112
Delaware	0 404	0.101	4 002
South Dakota	0.389	0.145	2.675
Missouri	0.382	0.137	2.075
lowa	0.381	0.139	2.741
Connecticut	0 381	0.133	2 855
Massachusetts	0.357	0.131	2.000
Indiana	0.353	0.135	2 617
Minnesota	0.340	0.137	2 482
New Mexico	0.334	0.154	2 164
Vermont	0.327	0.089	3 666
District of Columbia	0.324	0.070	4 645
New Hampshire	0.309	0.155	1.993
Maryland	0.304	0.089	3 4 1 9
Wisconsin	0.300	0.138	2 169
North Dakota	0.200	0.150	1.826
Alahama	0.290	0.095	3 044
South Carolina(1996)	0.276	0.055	4 276
New Jersev	0.270	0.005	2.080
Michigan	0.264	0.127	1.053
Illinois	0.200	0.155	A 172
Louisiana(1006)	0.249	0.135	4.172
Nevada(1995)	0.244	0.057	4 124
Kentucky(1996)	0.230	0.057	3 046
Arkansas(1995)	0.230	0.056	4.082
Oklahoma(1995)	0.227	0.050	4.032
Hawaii	0.227	0.105	1 969
Alaska(1994)	0.207	0.003	68 300
Ohio	0.194	0.005	4 078
Philadelphia(1995)	0.194	0133	1.368
Colorado	0.162	0.100	1.508
Tennessee(1994)	0.166	0.100	3 705
Washington	0.140	0.043	1 442
Arizona(1994)	0111	0.027	2 724
North Carolina(1995)	0.103	0.046	2.727
Wyoming(1994)	0.101	0.055	1.826
Utah(1995)	0.092	0.055	1.650
Maine(1985)	0.084	0.050	1 387
Mississinni(1990)	0.004	0.000	1.230
New York	0.052	0.055	1.022
Oregon(1990)	0.032	0.138	0.204
Georgia(1980)	0.040	0.158	0.224
$I_{dabo}(1990)$	0.021	0.049	0.303
West Virginia(1989)	-0.003	0.052	-0.033
Pennsylvania(1989)	-0.005	0.070	-0.033
Montana(1991)	-0.050	0.000	-0.929
Virginia(1988)	-0.059	0.094	-0.757
Florida(1987)	-0.071	0.067	-1.497
Texas(1995)	-0.204	0.067	-1.72
California	-0.538	0.184	-2.933

Note: These predictions are based on the Hybrid Model Harm-Weighted impact regession of Table 15

Ex. 13

# Exhibit 14

Ex.14 Page 309

## Do 98 percent of mass public shootings happen in gun-free zones?

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Length: 1513 words Byline: Meg Kelly

### Body

"98 percent of mass public shootings in the United States since 1950 have taken place in gun-free zones." - President Trump, speaking to the National Rifle Association, May 4

President Trump is prone to using bombastic statistics without much regard for the source, particularly when they support his opinions. When he claimed while speaking to the National Rifle Association that "98 percent of all mass public shootings" had occurred in gun-free zones, it caught our ear - after all, 98 percent is an awfully large share of anything.

Before we dive in, there are two important caveats: There's no agreed-upon definition of "mass shooting," and "gunfree zone" is subject to interpretation. As we've reported, in the 1980s, the FBI established a definition for "mass murder" as "four or more victims slain, in one event, in one location." Shooters are not included in the victim count if they committed suicide or were killed in a justifiable homicide, according to a Congressional Research Service report. But "mass murder" is not the same as "mass shooting."

The lack of consistency of definitions has led researchers to draw wildly different conclusions and has added ambiguity to something that, on face value, should be simple enough to determine. As with all statistics, it depends on how you count. Let's dig in.

Trump is citing an updated 2014 report from the Crime Prevention Research Center.

Founded by economist John R. Lott, CPRC is cited regularly by gun-rights advocates. Lott found that 98.4 percent of mass shootings occurred in gun-free zones between 1950 and July 10, 2016. Some quick Googling turned up another study - from the gun-control advocacy group Everytown for Gun Safety - that found that 10 percent of mass shootings between 2009 and 2016 took place in gun-free zones.

Using data that Lott provided, we tightened the time frame so we could compare his research with the Everytown study. Under Lott's methodology, we found that about 86 percent of mass public shootings took place in gun-free zones from 2009 to 2016.

Eight-six percent and 10 percent are about as far apart as statistics get. So who's right? The answer hinges on dueling definitions. For the sake of an apples-to-apples comparison, let's break down how Lott and Everytown calculated the percent of mass shootings in gun-free zones between 2009 and 2016.

What is a 'mass shooting'?

Everytown identified 156 mass shootings between 2009 to 2016. Lott found 28 mass public shootings over the same period. Both Lott and Everytown define a mass shooting as any incident in which "four or more" people are killed in one location, not including the shooter.

Here's where the two start to differ. Lott tightens his definition, excluding shootings that resulted from gang or drug violence or during the commission of a crime. Everytown includes these incidents. Lott justifies this by citing a 2014 FBI study on active-shooter incidents. (Caveat: An active shooter may be but is not necessarily the same as a mass shooter. The FBI did not require fatalities when it evaluated "active shooter" situations, and the report underlines this difference, noting, "This is not a study of mass killings or mass shootings, but rather a study of a specific type of shooting situation law enforcement and the public may face.")

Another major difference between Lott and Everytown is in where they argue a mass shooting can occur. Lott is very clear that he looks only at "mass public shootings." Referring to the same FBI study, he writes, "The FBI also defines 'public' places as 'includ[ing] commercial areas (divided into malls, businesses open to pedestrian traffic, and businesses closed to pedestrian traffic), educational environments (divided into schools [pre-kindergarten through 12th grade] and IHEs), open spaces, government properties (divided into military and other government properties), houses of worship, and health care facilities.' "

Lott did not mention that the list, defined as locations "where the public was most at risk during an [active-shooter] incident," also included "residences," which accounted for 4.4 percent of such incidents. He explained these exclusions in a Fox News op-ed, arguing that "mass public shootings are uniquely motivated," unlike a shooting that is the result of a gang fight or a drug sale. He added that "shootings in private residences are distinctly different, since they often involve killers who know the homeowners and whether they own guns."

Louis Klarevas, a University of Massachusetts professor and the author of "Rampage Nation: Securing America from Mass Shootings," dismissed Lott's reasoning, noting that plenty of mass shootings occurred in residential settings and querying why those victims should be overlooked. Everytown's director of research and implementation, Sarah Tofte, went further. "The claim that so-called 'gun-free zones' attract mass shooters doesn't stand up to scrutiny," she told us via email. "It's just not what the numbers show. We look closely at the data on mass shootings, and it shows that relatively few take place in areas where civilians are prohibited from carrying firearms. In fact, the vast majority of mass shootings take place in private homes and are often tied to domestic violence." The organization's data found that incidents that took place in private homes accounted for 63 percent of the total number of mass shootings they examined between 2009 and 2016.

#### What is a 'gun-free zone'?

It's not only the discrepancies between how Lott and Everytown define "mass shooting" that contribute to their differing estimates - there is also disagreement about how to define "gun-free zone."

Everytown reported 16 mass shootings in gun-free zones between 2009 and 2016; Lott reported 24. This is clearly attributed to the difference in definitions. Everytown defines gun-free zones as "areas where civilians are prohibited from carrying firearms and there is not a regular armed law enforcement presence."

Here, Lott has a much wider definition. In an email, he wrote that gun-free zones are "places where only police or military policy are classified, places where it is illegal to carry a permitted concealed handgun, places that are posted as not allowing a permitted concealed handgun, places where 'general citizens' are not allowed to obtain permits or where permits are either not issued to any general citizens or to only a very tiny selective segment."

In layman's terms, Lott's definition is so wide that the White House, where there are snipers on the roof, would be considered a gun-free zone. His data set classifies the shootings that took place at Fort Hood and the Washington Navy Yard as having occurred in gun-free zones. Klarevas disputed Lott's characterization - wondering how "a place can be a gun free zone if guns are present?"

Do 98 percent of mass public shootings happen in gun-free zones?

Lott previously defended his assessment. "Regular military members are banned from carrying guns at military bases in the United States, making the bases surprisingly soft targets," he wrote. "The only people who can carry guns on domestic bases are military police, so the situation is much the same as at the Pulse nightclub."

A White House official stood by Lott's study, saying the president had cited a widely used statistic.

Lott's original data set - which Trump referenced - spans from 1950 to 2016, but the admittedly vague concept of "gun-free zones" entered the lexicon only in the early 1990s, when two federal laws that restrict guns in and around schools were passed. Before 1990, Klarevas said, only certain government facilities (post offices, for example) explicitly prohibited firearms.

So where did the prior 40 years of data come from? Lott used a wide definition of "gun-free zone" to compile this data. He said he included anyplace where a "general citizen" wasn't able to carry a concealed weapon. This included any state that didn't have either a right-to-carry or concealed-carry law.

No matter how we spin these numbers, one thing is clear - they can be spun. And they have been. Without a commonly accepted and uniform definition of "mass shooting" or agreement on what constitutes a "gun-free zone," it's difficult to settle this debate. Advocates on both sides can point to holes and debatable logic in the reasoning of the study from the other sides.

But Lott's study is really not our focal point today. It's the president. As always, the burden for proving the accuracy of a claim is on the speaker. When the gap between dueling studies is so large, largely because the count depends on definitions, politicians need to be especially careful about citing one. Trump lunged for a dramatic statistic without including necessary context. He earns Two Pinocchios.

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**End of Document**
# Exhibit 15

Ex.15 Page 313



# **Crime Prevention Research Center**

- <u>Research</u>
- <u>Data</u>
- About/Board Of Academic Advisors
- <u>Op-Eds</u>
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Featured

# UPDATED: Mass Public Shootings Keep Occurring In Gun-Free Zones: 94% Of Attacks Since 1950

15 Jun, 2018



**UPDATED on July 6th, 2019**: Original title: "More misleading information from Bloomberg's Everytown for Gun Safety on guns: 'Analysis of Recent Mass Shootings,' Showing how mass public shootings keep occurring in gun-free zones" Originally posted on September 9, 2014.

While the first part of the discussion here goes through each mass public shooting from 2009 to 2014 discussed in the Bloomberg report. At the end of the post, we have a response to Everytown's response to our post.

We have since updated our information up through May 30, 2018.

### Data on cases

An Excel spreadsheet that lists all the mass public shootings from **1998 through April 30, 2018** is available here: <u>Case by case discussion of which cases have been in gun-free zones</u>. UPDATED: Broader and updated information on these shootings from **1998 to June 2019** is <u>available here</u>.

The report by Bill Landes at the University of Chicago and John Lott on gun-free zones that covers earlier years from 1977 to 1999 is <u>available here</u> (the raw data used in this study is available in a STATA file, though the sums by state and year are <u>available here</u>). We used the cases identified by the New York Times that are discussed in Lott and Landes for years prior to 1977 (series is <u>discussed here</u>, though the web version doesn't have the chart that lists out each case). A case by case discussion on the New York Times cases can be found at the bottom of this page.

### Definition of mass public shooting.

We used the traditional FBI definition of mass public shootings in all our posts on this (e.g., <u>here</u>, <u>here</u>, <u>and here</u>). There are several parts to this definition.

- 1. The official FBI definition of public shootings (both mass public shootings and active public shootings) excludes "<u>shootings that resulted from gang or drug violence</u>" or that occurred in the commission of another crime such as robbery. The reason for this is pretty obvious: the causes and solutions for gang shootings over drug turf are dramatically different than the types of mass public shootings that we see at schools and malls where the point of the attack is to kill as many people as possible.
- 2. The FBI also includes only shootings in "<u>public places</u>" such as: commercial areas (malls, stores, and other businesses); schools and colleges; open spaces; government properties (including military bases and civilian offices); houses of worship; and healthcare facilities. The reason for this is clear: for example, if the attack is in a home, the attacker is much more likely to know if a gun is owned in the home and who might have access to it. By contrast, when an attack occurs in a public place, the attackers don't know who they have to be concerned might have a gun to stop them. [Residences were included in the FBI's total deaths "where casualties occurred inside a private residence before a shooter moved to a public area, those incidents were categorized at the location where the public was more at risk." For example, their cases would involve a residence and then a school.]
- 3. From 1980 to 2013, the original FBI definition of "mass killings" had been "*four or more* victims slain, in one event, in one location," "within one event, in at least one or more public locations, such as, a workplace, school," and the offender is not included in the victim count (<u>CRS, July 30, 2015</u>). In 2013, the definition was changed to "three or more killings." The vast majority of academics have continued to use the four or more definition. This includes researchers such as <u>James Alan Fox</u>. See also studies years ago such as 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. Adam Lankford, "<u>Public Mass Shooters and Firearms: A Cross-National Study of 171 Countries</u>," Violence and Victims, January 2016. Lott and Landes, <u>2001</u> and John Lott, "More Guns, Less Crime," University of Chicago Press, 3rd edition, 2010. Even groups such as <u>Bloomberg's Everytown</u>, which Snopes cites approvingly, have recently used the four or more definition. The other organization that Snopes cites approvingly, Mother Jones, also has used the four or more definition for most of the period and has only recently well after we did this report started using the three or more definition.

These points were also originally used in the <u>1999</u> and <u>2001</u> versions of research on mass public shootings by William Landes and John Lott. Of course, it is <u>hard to ignore the statements</u> from some of these killers about why they pick the targets that they do (more details on this in Lott's book <u>available here</u>). **Gun-free zones are defined as places where general citizens couldn't carry guns.** 

— Places where only police or military police are classified as gun-free zones.

Stationing a uniformed police officer or security guard in a public area often only gives a false sense of security. Knowing that the uniformed officer is the only person with a gun makes things quite simple for attackers. They need only kill him first. It is the equivalent of having them with a neon sign saying "shoot me first." The media fact checkers seem unwilling to accept is that concealed handgun permit holders take away that strategic advantage from the killers.

- Places, where it is illegal to carry a permitted concealed handgun, are classified as gun-free zones.

Depending on the state bans on carrying in certain areas.

- Places that are posted as not allowing permitted concealed handgun. A few examples:

Waffle House shooting 2018 Cascade Mall 2016 Lafayette, Louisiana Movie Theater Shooting 2015 Aurora Movie Theater shooting 2012

— Places where by law permit holders were banned. A few examples:

<u>Chattanooga Marine Recruiting Office July 2015</u> Fort Hood Shootings 2009 and <u>2014</u> <u>Pulse Nightclub June 2016</u> <u>Borderline Bar & Grill in Thousand Oaks, California November 2018</u> The many school shootings keep on occurring in those schools where permitted concealed handguns are banned (e.g., <u>Parkland 2018</u>, Sante Fe 2018)

— Places where general citizens are not allowed to obtain permits. Permits are either not issued at all to citizens or to only a very tiny selective segment (so-called may issue laws), usually judges, prosecutors, deputy sheriffs or very wealthy individuals. Compare to the <u>8.5% of adults</u> outside of California and New York who have a concealed handgun permit.

In Alameda County, a may issue county, there were only <u>85 people</u> with concealed handgun permits at the end of 2011 out of an adult population of 1.2 million, a rate of 0.007 percent. Those with permits were mainly judges, prosecutors, and wealthy businessmen. In September 2011, there were <u>240 permits in all of Los Angeles County</u> when the population was <u>about 7.6 million adults</u>. That equals a permit rate of 0.0032%. Of the 240 permits, <u>most</u> <u>go to judges and reserve deputies</u> (who are big campaign donors). Ten percent of permit holders are on Sheriff Lee Baca's "<u>gift list</u>" In addition, the attack was at a residential dwelling, not a public place.

In Orange County, a may issue county, there were only <u>551 people</u> with concealed handgun permits out of an adult population of 2.26 million, a rate of 0.02 percent. Those with permits were mainly judges, prosecutors, and wealthy businessmen.

**ORIGINAL POST**: Why does anyone pay attention to Bloomberg's claims on guns? Take their previous discussions on mass public shootings. As we have previously pointed out, Bloomberg's groups have made serious errors on the <u>number</u> (see <u>also here</u>) and <u>trends</u> of school shootings. Well, that hasn't stopped the media from sympathetically covering Everytown for Gun Safety's <u>recent report on mass shootings</u> (see <u>also here</u>).

Everytown for Gun Safety's <u>recent report on mass shootings</u> contains many errors. In addition, it muddies the discussion on mass public shootings by including shootings in private homes along with ones in public places, and the vast majority of the cases they include are in private homes. But there is a distinction between what motivates mass public shooters who are committing their crimes to get media attention and those who engage in attacks in private residences.

There are seven mass public shootings since at least 1950 that have not been part of some other crime where at least four people have been killed in an area where general civilians are allowed to have guns. These are the International House of Pancakes (IHOP) restaurant in Carson City, Nevada on September 6, 2011, the Gabrielle Giffords shooting in Tucson, Arizona on January 8, 2011, [UPDATE] the Dallas shooting of police on July 7, 2016 that left five dead, [UPDATE] the Kalamazoo shooting on February 20, 2016 where four people died in one attack at a Cracker Barrel restaurant and two others in another one (while the Cracker Barrel restaurant didn't ban guns, Uber does ban its drivers from carrying guns and the killer was on the job when he did the shootings and the shooting was done while he was in his car); Las Vegas shooting on October 1, 2017 that left 58 dead [UPDATE]; Melcroft, Pennsylvania attack that left four dead on January 28, 2018 [UPDATE]; and the First Baptist Church in tiny Sutherland Springs, Texas, on November 5th,

<u>2017</u> claimed 26 lives [UPDATE]. With the new data through June 2019, there are now eight shootings in gun-free zones [UPDATE]. We had previously missed the Radisson Hotel killer on December 30, 1999.

Thus, while CPRC's research and that by <u>Landes and Lott</u> looks at Mass public shootings focuses on the killings where the point of the attack is simply to kill as many people as possible, Bloomberg's numbers overwhelmingly involve killings that have occurred within residences.

The FBI definition of mass public shootings excludes "<u>shootings that resulted from gang or drug violence</u>" or that were part of some other crime. The FBI also defines "public" places as "<u>includ[ing] commercial</u> areas (divided into malls, businesses open to pedestrian traffic, and businesses closed to pedestrian traffic), educational environments (divided into schools [pre-kindergarten through 12th grade] and IHEs), open spaces, government properties (divided into military and other government properties), houses of worship, and health care facilities." For almost thirty years, the FBI defined mass shootings as involving four or more people killed. A complete discussion of the definition and the history of it is <u>available here</u>.

Here are some general points about how to classify mass public shootings that have occurred in gun-free zones.

1) A lot of work is involved in obtaining information on whether the attacks occurred in gun-free zones. This includes calling the <u>businesses</u> or <u>other facilities</u> involved. But many times those organizations are uncooperative and in those cases much time is spent contacting individuals in the area of the attack and asking them if they can <u>provide pictures</u> or other information on the facilities. Indeed, <u>the media virtually always refuses to mention whether the attack occurred in a gun-free zone</u>.

Unfortunately, Everytown for Gun Safety/Mayors Against Illegal Guns did not do this work, and they have also inaccurately stated, ignored, or simply missed facts that are readily available in news stories.

2) What motivates mass public shootings where the killer is trying to kill or injure as many people as possible to get publicity is quite different from what motivates robbers or gang fights (see <u>Lott and Landes</u>). The solutions to these two types of attacks are also vastly different. The issue of gun-free zones is particularly important for mass public shootings.

3) The word "public" is also key to these cases. Shootings that occur in people's homes will often involve killers who know if guns are owned in the home. And if there is a gun in the home, the killer will know who has access to it. The FBI also <u>defines</u> "public" places as "includ [ing] commercial areas (divided into malls, businesses open to pedestrian traffic, and businesses closed to pedestrian traffic), educational environments (divided into schools [pre-kindergarten through twelfth grade] and IHEs), open spaces, government properties (divided into military and other government properties), houses of worship, and healthcare facilities" (p. 12).

4) There is also the distinction between right-to-carry and may-issue laws. If virtually no one, especially general civilians, is allowed to get a concealed handgun permit as occurs in most may-issue jurisdictions, the area is essentially a gun-free zone.

Our past work has also collected information on which schools are gun-free zones and which ones are not.

### <u>Cases Bloomberg's Everytown organization incorrectly reports as mass public shootings in gun-free</u> <u>zones</u>

The Bloomberg discussions are indented and put in block quotes. After the quotes there is an explanation for why they shouldn't be counted as mass public shootings. The Bloomberg report doesn't number these events, but we will assign numbers just to make them easier to reference.

1) **Terrell, TX, 10/28/13**: The shooter shot and killed his mother, his aunt, two acquaintances, and a store clerk in a spree of attacks before he was captured by police. He killed the first four victims in their respective homes and the final one — the clerk — at Ali's Market on W. Moore Avenue, apparently in an attempt to rob the store.

Shooter Name: Charles Everett Brownlow Jr.

Gun details: Unknown

Ammo details: Unknown

Gun acquired: Unknown

Prohibiting criteria: The shooter had a criminal record that prohibited him from possessing firearms. He was convicted of burglarizing a vehicle in 1996, a Class A misdemeanor, and convicted of felony burglary in 1997. In 2008 he was served a three-year sentence for unlawful possession of a firearm and in 2011 he was convicted of misdemeanor assault against a family member.

Not a gun-free zone: The manager of the Ali's Market reported that customer's are allowed to carry firearms in the store.

**FACTS:** There was not a mass public shooting at Ali's Market. Only one person was killed at that store. Permitted concealed handguns can deter many attacks from occurring and can limit the harm that does occur. But permit holders aren't expected to limit the harm for those attacks that do occur to zero. Permitted concealed handguns deter mass shootings because they can limit the harm and take away the incentive that these killers obtain from their warped desire to get media attention.

2) **Washington, DC, 9/16/13**: The alleged shooter, who was a civilian contractor and former non- combat military, killed twelve and wounded three more in an attack on Building 197 at the Navy Yard.

Shooter Name: Aaron Alexis, 34

Gun details: The shooter arrived with a shotgun and also obtained a handgun from one a security guardthat he killed.

Ammo details: Unknown

Gun acquired: Two days before the incident the shooter passed a National Instant Criminal Background Check System (NICS) at the licensed gun dealer Sharpshooters in Lorton, VA, and purchased the shotgun.

Prohibiting criteria: The shooter had been arrested at least three times including: in September 2010 in Fort Worth, Texas for shooting a firearm into a neighbor's apartment; in August 2008 in Dekalb County, Georgia for disorderly conduct; and in 2004 in Seattle, Washington for shooting out the tires of another man's vehicle. But court records do not indicate he was convicted in any of these cases, and this record did not prohibit him from buying guns. He had also received treatment for mental health conditions at two VA hospitals beginning in August, 2013 following an incident where he called Newport Rhode Island Policeto report hearing voices. But these incidents did not rise to the level of prohibiting from buying guns. And during his military service he was reportedly cited on at least eight occasions for misconduct ranging from traffic tickets and showing up late for work to insubordination, extended absences from work, and disor- derly conduct. On account of this the Navy sought to offer him a "general discharge" but he was ultimately honorably discharged through the early-enlisted transition program in January 2011.

Not a gun-free zone: There were armed guards at the Washington Navy Yard, and the shooter was familiar with the premises, so he did not select it as a target on the presumption he would

not faced armed resistance. In fact, the shooter reportedly used a gun that he took from a guard after killing him.

**FACTS:** Whether one is looking at the attacks at the Washington Navy Yard or Fort Hood, letting military police carry guns is much different than letting other soldiers protect themselves. While military police tend to be at the entrances to military bases, they largely patrol the rest of the base in the same way that police patrol a city. One no more expects military police to instantly arrive at the scene of a mass public shooting than one expects police to arrive at one. In Alexis' attack, since he worked at the Navy Yard, he knew what entrance to go to that would have only one guard and that is where he went. For related discussions see <u>here</u> and <u>here</u>.

3) **Crab Orchard, TN, 9/11/13**: The shooters killed a woman and three teenagers, apparently during an attempted robbery during a marijuana exchange. The victims' bodies were discovered in a car parked along the side of the road in the Renegade Mountain resort community near Crossville.

Shooter Name: Jacob Allen Bennett, 26 and Brittany Lina Yvonn Moser, 25

Gun details: Handgun

Ammo details: Unknown

Gun acquired: Unknown

Prohibiting criteria: Bennett was prohibited from possessing firearms. In 2010 he received a 6-year prison sentence for charges of theft, forgery, and possession of a handgun during a felony, but was paroled on March 4, 2013. The Cumberland County sheriff's office estimated they had previously arrested Bennett five times.

Not a gun-free zone: We could find no evidence that permit holders were prohibited from carrying guns in this area. In Tennessee, concealed weapons would be prohibited only if the county or municipality declared itself a gun-free zone.

**FACTS:** This shooting was part of another crime, a robbery of illegal marijuana (see point 2 in the introduction). It was not a mass public shooting where the point of the crime was to kill as many people as possible so as to obtain media coverage.

4) **Herkimer, NY, 4/13/13**: The shooter killed two people and critically wounded one at John's Barber Shop and then killed two more people at Gaffey's Fast Lube, a car care facility. He was killed by responding officers.

Shooter Name: Kurt Myers, 64

Gun details: According to the police superintendent, Myers used a shotgun. Additional guns and ammunition were found by emergency crews after Myers set fire to the apartment.

Ammo details: Unknown

Gun acquired: Unknown

Prohibiting criteria: There is no reason to believe Myers was prohibited him from possessing a gun. He was arrested in 1973 for drunk driving.

Not a gun-free zone: Gaffey's Fast Lube does not have a specific policy prohibiting guns and allows per- mit holders to carry concealed weapons on the premises. John's Barbershop did not reopen following the shooting but the owner of a neighboring business did not recall the barbershop having any explicit firearm policy or ban, which would have been required to prohibit customers from carrying guns on the premises.

**FACTS:** New York is a may issue state, not a right-to-carry state. We don't yet have the number of civilian concealed carry permits, but they seem to be extremely rare. The possession of a handgun in New York State requires a NYS Pistol Permit. In 2012 there were only 154 permits issued to own a pistol in Herkimer county. Over the previous five years, there were 667 permits issued, though not all over those permits own a handgun would have been active at the beginning of 2013 (that implies that there were less than 1.39% of the

#### Ex. 15 Page 320

https://crimeresearch.org/2018/06/more-misleading-information-from-bloombergs-everytow... 1/2/2020

adult population that even legally owns a handgun). When talking to the pistol permit office, Lott was told that there were zero restricted licenses that allowed people to carry for business purposes (concealed carry licenses that allow a business owner to carry in the course of doing business) and zero restricted for self-defense purposes (e.g., a woman who is being actively stalked).

**<u>Report from February 2013</u>**: At this point, the Everytown for Gun Safety report mainly repeats cases previously discussed by Bloomberg's Mayors Against Illegal Guns on MASS SHOOTING INCIDENTS, JANUARY 2009-JANUARY 2013. An earlier report on the problems with their claims about the attacks not being in gun-free zones was never corrected. Here is the analysis of these previous cases.

5) Geneva County, AL, 3/10/09: The shooter killed ten, including four members of his family, before killing himself.

Shooter Name: Michael Kenneth McLendon, 28

Gun details: Bushmaster AR-15, SKS Rifle, Shotgun, and .38 Pistol

Ammo details: Police recovered additional ammunition from his vehicle after the shooting. Gun acquired: Unknown

Prohibiting criteria: The shooter had no criminal record and there is no indication he was prohibited from owning a gun.

Not a gun-free zone: It was lawful to carry a firearm in the public intersection and gas station where two of the individuals were shot.

**FACTS:** Nine people were killed by McLendon. <u>In the first shooting in a house on Pullum Street, five people were killed.</u> There was also a second shooting in another home that left two people dead. Neither were public places. It is true that two individuals were killed in separate public places as McLendon was driving along, but that is not a multiple victim public shooting in which at least four are killed in a public place. However, MAIG's report implies that all these shootings occurred in a public places.

6) Lakewood, WA, 11/29/09: The shooter killed four police officers in a Tacoma Coffee shop, eluding police for two days before being killed as he fled.

Shooter Name: Maurice Clemmons, 37

Gun details: When he was killed, he was in possession of the handgun of one of the officers he had killed.

Ammo details: Unknown

Gun acquired: Unknown

Prohibiting criteria: The shooter was prohibited from purchasing a firearm, having been charged with at least 13 felonies across two states. He had posted bail for raping a child just six days before the attack.

Not a gun-free zone: The police officers were armed at the time of the shooting.

**FACTS:** Lott and Landes didn't define gun-free zones in terms of whether police were allowed to carry guns, but whether private citizens are able to readily obtain concealed handguns for their protection. What is important is that the coffee shop was posted to prevent concealed carry permit holders from carrying. Presumably MAIG understood this point and that is the reason why they focused on police officers being able to carry in this venue. Obviously, however, on-duty police can carry any place. The problem for uniformed police is that they provide an easily identifiable target and it is easy to take them out. Possibly if the attacker had to worry about permit holders who he could not identify, it would have dissuaded him from attacking. While Lott had checked when this event originally occurred, he reconfirmed this information with Dave Workman who lived nearby via email on January 8, 2013.

7) **Carthage, NC, 3/29/09**: The shooter opened fire at a nursing home where his estranged wife worked, killing eight and injuring three before he was shot and arrested by a police officer. Shooter Name: Robert Stewart, 45

Gun details: .357 Magnum handgun and Winchester 1300 shotgun Ammo details: Unknown Gun acquired: The guns were acquired legally from a local sporting good store. Prohibiting criteria: There is no indication the Stewart was prohibited from owning a gun. Not a gun-free zone: We could find no indication that the property-owner forbid carrying of firearms on their property.

**FACTS:** This <u>facility informed Lott</u> in April 2009 that they did not allow guns in the facility for either the people living there or the staff. He called up to ask what their policies had been before the attack.

Here are places listed by Bloomberg's group that may have allowed people with permits to carry in places but that made it extremely difficult or impossible for civilians to get a concealed handgun permit.

8) **Boston, MA, 09/28/10**: The shooter killed four and wounded one during a drug-related robbery.

Shooter Name: Edward Washington, 33, and Dwayne Moore, 35, were both charged in the killings. Washington was acquitted. In Moore's first trial, the jury deadlocked 11-1 in favor of his guilt, but he was later convicted in a retrial.

Gun details: 40-caliber Iberia handgun and 9mm Cobray semiautomatic. The Cobray has not been recovered, but the weapon was identified based on recovered bullets and shell casings. Ammo details: 14 rounds fired

Gun acquired: Unknown

Prohibiting criteria: Unknown

Not a gun free zone: A person with a Massachusetts Class A license could lawfully carry a firearm in this area.

9) **Buffalo**, NY, 8/14/10: The shooter opened fire on a group of people outside a bar, killing four and wounding four others.

Shooter Name: Riccardo McCray, 24

Gun details: Unknown

Ammo details: Unknown

Gun acquired: Unknown

Prohibiting criteria: McCray had been arrested earlier that year on felony drug charges and the previous year for having a loaded rifle in his car. If he was found guilty of either crime, he would have been prohibited from possessing firearms.

Not a gun-free zone: We could find no indication that it was unlawful to carry a firearm in the area.

10) Northridge, CA, 12/2/12: The shooter arrived at an unlicensed boarding house on Devonshire street, reportedly in search of his girlfriend, and after a dispute shot and killed four people out- side.

Shooter Name: Ka Pasasouk, 31

Gun details: semiautomatic handgun

Ammo details: Unknown

Gun acquired: Unknown

Prohibiting criteria: The shooter was prohibited from possessing guns, having been convicted for car theft and felony robbery. While on probation in September 2012, he was arrested again for possession of methamphetamine. According to the district attorney, a prosecutor then released him on probation over the objection of probation officials, who believed he posed a threat to the safety of the community.

Not a gun-free zone: Permit holders were not prohibited from carrying guns in this area.

11) **East Oakland, CA, 3/21/09**: The shooter used a semiautomatic handgun to kill two police of- ficers after they stopped his car and then fled on foot to an apartment where he killed two SWAT officers with an assault weapon and injured a third before being killed by police. Shooter Name: Lovelle Mixon

Gun details: 9mm semiautomatic handgun and SKS assault-style rifle

Ammo details: Police said the assault weapon had a high-capacity magazine.

Gun acquired: The shooter took part in a home invasion robbery in Modesto, CA, on February 21 2009 in which a rifle was reported stolen. Police did not comment on whether the stolen rifle was the one used in the shooting.

Prohibiting criteria: The shooter had a lengthy criminal history, including a conviction for armed battery, which would have prohibited him from possessing a gun, and he was on parole for assault with a deadly weapon at the time of the shootings.

Not a gun-free zone: Two of the victims were shot on a public roadway — the 7400 block of Macarthur Boulevard in East Oakland — where no state law would have prohibited a citizen with the appropriate per- mit to carry a gun. All of the police officers killed in the incident were armed.

12) **Medford, NY, 6/9/11**: The shooter killed four people at a pharmacy, Haven Drugs, and stole thousands of hydrocodone pills before fleeing in a vehicle. During the trial he acknowledged that he and his wife were addicted to prescription medication.

Shooter Name: David Laffer

Gun details: A .45 caliber handgun was used in the shooting. Several other legally registered guns werealso recovered from the shooter's home.

Ammo details: Unknown

Gun acquired: Unknown

Prohibiting criteria: The gun was legally registered to the shooter, and there is no evidence he was prohibited from possessing a gun. But five months before the shooting, Suffolk County Detective Kenneth Ripp investigated an identity theft claim made by the shooter's mother, who said the shooter had stolen her debit card. After questioning the shooter and his mother, Ripp advised the Suffolk County Pistol License Bureau that the shooter was dangerous and that his guns should be confiscated. Despite Ripp's report, the guns were not removed.

Not a gun-free zone: We could find no evidence that Haven Drugs posted a sign or had a policy prohibit- ing the carrying of firearms. Current employees declined to comment.

13) **Brockport, NY, 2/14/09**: The shooter killed a nurse in the Lakeside Memorial Hospital parking lot and a motorist who intervened, and wounded the motorist's girlfriend. The shooter had been fired from the hospital after the nurse filed a sexual harassment complaint against him. He then drove 50 miles and killed another nurse — who had filed a similar complaint against the shooter — and her husband in their home.

Shooter Name: Frank Garcia, 34

Gun details: .40 caliber Glock handgun

Ammo details: Unknown

Gun acquired: Unknown

Prohibiting criteria: There is no evidence that the shooter was prohibited from owning a gun. However, he had applied for concealed carry permits and been denied three times. In his 1995 application, he omit- ted information about his criminal record — including arrests for criminal possession of a weapon, assault, and harassment. In 2001 and 2006 he made further omissions, and was evaluated as lacking moral character. But in 2007 a judge reversed the denial and granted Garcia a concealed weapon permit.

Not a gun-free zone: We found no indication that permit holders were prohibited from carrying guns in this area at the time of the incident.

**FACTS:** All these cities either forbid or make it incredibly difficult for law-abiding citizens to carry concealed handguns for protection. In Boston, it is so bad that even off-duty and retired police are regularly denied unrestricted license to carry permits. Northridge, CA is part of Los Angeles County, which refuses to issue permits to regular citizens. In September 2011, there were 240 permits in all of Los Angeles County when the population was <u>about 7.6 million adults</u>. That equals a permit rate of 0.0032%. Of the 240 permits, <u>most go to judges and reserve deputies</u> (who are big campaign donors). Ten percent of permit holders are on Sheriff Lee Baca's "<u>gift list</u>" In addition, the attack was at a residential dwelling, not a public place.

Similarly, East Oakland, California is part of Alameda County. In 2010, Alameda County had granted concealed handgun permits to <u>75 people</u> out of an 1,182,534 — a permit rate of 0.006%.

Just as with Herkimer, NY; Medford in Suffolk County, New York, and Brockport in Monroe County, New York were similarly very restrictive in issuing may issue permits. In Suffolk County, the police and sheriff's departments each handle permits in half of the permits for the county. For the sheriff's office, Robert E. Draffin (the Suffolk County Sheriff's Freedom of information officer) informed us that were 569 sportsman permits (limited to carrying to or from a shooting range or to go hunting) and 79 business permits (where a business owner is allowed to carry only in the course of doing business). For the police department, Inspector Derrocco (613-852-6000, ask for pistol permit department) noted the department "virtually never gives out permits for anything other than sportsman to carry to and from the range and for premises and dwellings." Given that there are about <u>1.2 million adults</u> in Suffolk County, even assuming that the police department issued permits at the same rate as the sheriff's office, this implies a permit rate of about 0.1 percent and virtually none of these permits would have allowed a concealed handgun to be carried in the pharmacy where the attack occurred. In addition, it should be mentioned that Riccardo McCray <u>was a gang member</u>.

14) **Oak Creek, WI, 8/5/12**: The shooter killed six people at a Sikh temple and injured three others, including a responding police officer, before killing himself.

Shooter Name: Wade Michael Page, 40

Gun details: 9mm semiautomatic handgun

Ammo details: Page reportedly bought three 19-round magazines when he purchased the gun. Gun acquired: Page acquired the gun at a local gun shop a week before the shooting. Prohibiting criteria: Page was involved with the white supremacist movement but he does not appear to have been prohibited from purchasing a gun. Federal officials investigated Page's ties to supremacist groups more than once prior to the shooting, but did not collect enough evidence to open an investigation.

**FACTS:** From FoxNews.com: <u>"No guns [were] allowed in the temple," Kulbir Singh, an attendee of the Sikh Temple of Wisconsin, told FoxNews.com. "Everyone knows that it's not allowed, anywhere in the temple."</u>

15) Norcross, GA, 2/22/12: The shooter returned to a Korean spa from which he'd been kicked out after an altercation, where he shot and killed two of his sisters and their husbands before committing suicide.
Shooter Name: Jeong Soo Paek, 59
Gun details: .45 caliber handgun
Ammo details: Unknown
Gun acquired: Police reported that he acquired the gun legally.
Prohibiting criteria: Paek does not appear to have been prohibited, although he had allegedly served two months in jail for assaulting his sister six years earlier.
Not a gun-free zone: We could find no indication that the property owner forbade possession of a firearm on their property.

#### Ex. 15 Page 324

https://crimeresearch.org/2018/06/more-misleading-information-from-bloombergs-everytow... 1/2/2020

**FACTS:** Lott spoke the spa after the attack and was told that the killer knew "nobody there had a gun." It was a family establishment, and the killer knew the family's policy on guns in the workplace. The person at the spa indicated that they were sure that neither the sisters nor their husbands had guns at the spa and that the killer who was the brother of the women knew that was the case. While the official policy at the spa isn't clear because the conversation was very short, the important thing was that the killer knew that there were no guns for people to defend themselves there. This was a small family owned establishment so it is most likely that this was the official policy of the family. Note that they have the wrong date on this event. (UPDATE: Mayors Against Illegal Guns originally claimed that this event occurred on February 22nd, but the event <u>actually occurred</u> on February 20, 2012. After Lott wrote his analysis, they corrected the data but did not update their discussion of gun-free zones.) Note also that the business has since <u>closed</u>.

16) Hialeah, FL, 6/6/10: The shooter killed four women, including his wife — who had just separat- ed from him. He injured three others before shooting and killing himself. The shooting occurred in Yoyito-Cafe Restaurant, where the shooter's wife was employed as a waitress, and in the park- ing lot immediately outside.
Shooter Name: Gerardo Regalado, 38
Gun details: .45 caliber handgun
Ammo details: Unknown
Gun acquired: The shooter had a concealed weapons permit.
Prohibiting criteria: There is no evidence that the shooter was prohibited from owning a gun.
However, relatives said the shooter had abused and terrorized women in the past, and had been imprisoned in Cuba for a particularly violent incident, but he did not have a criminal record in the United States.
Not a gun-free zone: We could find no indication that guns were prohibited in this area. Guns are prohibited in Florida restaurants only in areas primarily devoted to the serving of alcohol.

**FACTS:** Strangely, while Bloomberg's group mentions that the restaurants that get 50 percent of their revenue from alcohol, they didn't actually go and check whether that was the case for this restaurant, which apparently was at the time <u>a very popular venue for parties serving alcohol</u>. If Bloomberg's group had checked, they would have found that it was a gun-free zone. The organization of the bar in the center of the restaurant is also important for this determination.

17) **Washington, DC, 3/30/10**: Three gunmen killed four and wounded five in retaliation for another murder.

Shooter Name: Nathaniel D. Simms, 26; Orlando Carter, 20, and unnamed 14-year-old juvenile Gun details: An AK-47 assault rifle and 9mm and .45-caliber handguns

Ammo details: Unknown

Gun acquired: Unknown

Prohibiting criteria: The adults were reported to have lengthy criminal histories, which prohibited them from purchasing guns, and the 14-year-old was too young to purchase or own a gun.

**FACTS:** This is one case where Bloomberg's Everytown doesn't include this as a place that allows guns (obviously DC completely bans the carrying of concealed handguns), we include it here simply as an example of one of the many cases where they are including what are pretty obviously a <u>drive-by</u> gang shootings. Even the DC police chief, Cathy Lanier, indicated that it was a "gang retaliation." The AK-47 was used to spray bullets into a group in another gang's territory in retaliation for another murder. We are focused on cases identified by Everytown as occurring in gun-free zones, but gang shootings, while are obviously quite different from the types of mass public shootings that garner national attention.

18) **Mount Airy, NC, 11/1/09**: The shooter killed four people outside a television store before eventually surrendering to the police.

Shooter Name: Marcos Chavez Gonzalez, 29 Gun details: Assault rifle Ammo details: Unknown Gun acquired: Unknown Not a gun-free zone: It was lawful to carry a firearm in the area of the shooting.

**FACTS:** Indications are that the attack was <u>part of gang related crime</u>. As explained above, <u>that would</u> <u>exclude it from the mass public shootings done specifically to harm people as distinct from other types of violent crime</u>.

### Other recent cases pointed to by gun control advocates

1) Waco Texas, May 18, 2015: biker brawl, nine killed.

FACTS: This was a fight between two gangs, the <u>Bandidos and Cossacks motorcycle gangs</u>, that were involved in an array of operations such as <u>selling drugs</u>.

2) San Francisco, January 12, 2015: four were killed in a stolen parked car.

**FACTS:** In 2015 there were just four concealed handgun permits issued in all of San Francisco County. One of them was issued to the personal lawyer of the then county Sheriff. Three others were to judges. In any case, that is a rate of 0.00053% of the adult population.

In addition, the four people who were shot were in a stolen car and members of a gang. Media discussions noted that it might also have been a fight between rival drug gangs. "<u>It is not clear whether the gang is</u> involved in these killings."

3) Palestine Texas, November 16, 2015: 6 killed on a residential property

FACTS: "Four males initially thought missing were found dead Monday afternoon in a pond on Hudson's property."

4) Houston Texas, August 10, 2015: 8 killed in a residential property

**FACTS:** "A suspect in the killing of eight people in Texas broke into their house and handcuffed the victims in their bedrooms — then shot them, authorities said."

5) Bulington, Washington, September 23, 2016: mall shooting

Facts: All the entrances to the mall had gun-free zone signs posted.

6) Orlando, Florida, June 5, 2017: 5 killed at office

**FACTS**: "<u>five people [were] killed by a former disgruntled employee (John Robert Neumann, Jr.) at</u> <u>Fiamma before he turned the gun on himself.</u>" We talked to Fiamma Inc on September 6, 2017 and we were informed that employees were banned from having guns at the facility, where RV awnings are manufactured. *More detailed information on other cases* 

There are a few other cases that we have collected that we will link to here.

<u>4 killed, 3 injured in Tennessee Waffle House shooting4 killed, 3 injured in Tennessee Waffle House shooting</u> — April 22, 2018, Waffle Houses have a <u>strict no gun policy</u>.

<u>Orlando business shooting</u>, June 5, 2017, A gunman fatally shot five of his former coworkers then killed himself during a mass shooting at Fiamma, an Orange County business, Monday morning, according to

sheriff's officials. Called business and was told that this business didn't allow employees to have weapons there.

Yet another mall shooting where guns were banned: Monroeville Mall near Pittsburgh, February 8, 2015

The Melbourne Square Mall in Florida is yet another gun-free zone, January 17, 2015

Seattle Pacific University shooting took place in yet another gun-free zone, June 5, 2014

6 killed, seven wounded in Mass Public Shooting in Santa Barbara, a giant gun-free zone, May 24, 2014

Mass shooting yesterday at FedEx facility in Kennesaw Georgia took place in yet another "gun-free zone", April 30, 2014

Maryland Mall Shooting at yet another gun free zone, January 25, 2014

At a tribal court hearing over an eviction of Cherie Lash Rhodes, Alturas, California (February 20, 2014).

The Azana Salon & Spa shooting in Milwaukee, Wisconsin (November, 2012).

Accent Signage Systems in Minneapolis, Minnesota on September 27, 2012. Talked to Joe Bailey who has worked at the comapny since 2007 and he informed us that the factory did not allow permitted concealed handguns there before or after the attack.

Aurora, Colorado movie theater shooting (July, 2012). The Cinemark movie theater chain posted signs at their theaters (See here).

Cafe Racer, Seattle, Washington, four killed at restaurant (May 30, 2012).

Another shooting in a another gun free zone: Binghamton, NY(April, 2009).

Atlantis Plastics, Inc shooting on June 25, 2008 left 5 murdered at a factory in Henderson, Kentucky (see <u>here</u>).

Trolley Square Mall in Utah (February, 2007).

Omaha, Nebraska mall shooting (December 2007).

John Lott has about 327 postings on gun-free zones available here.

The relevant 1950 to 1976 time period for the NYT List:

YEAR NAME AGE KILLED HURT

2 '66 Charles J. Whitman 25 16 31 — Concealed carry completely banned, guns kept off campus

3 '66 Robert B. Smith 18 5 2 — Concealed carry completely banned

4 '67 <u>Leo Held</u> 40 6 6 — No right to carry concealed handguns, very few permits issued, at plant that banned guns

5 '72 Edwin J. Grace 33 6 6 — No right to carry concealed handguns, very few permits issued in New Jersey

6 '74 Anthony Barbaro 17 3 9 — fewer than 4 killed, New York with no right-to-carry law

7 '76 Robert D. Patty 43 3 2 — fewer than 4 killed

8 '76 <u>Charles E. Allaway</u> 37 7 2 — No right to carry concealed handguns, very few permits issued in Orange County, California, Cal-State University shooting

For those who object to the New York Times list of mass public shootings, the very beginning of this post also shows a graphic for the last twenty years and the rate of shootings in gun-free zones is also high. Given that very few states had right-to-carry laws prior to 1977, to the extent that the New York Times missed attacks, our numbers will likely underestimate the rate of attacks in gun-free zones.

UPDATE: Everytown provides a response to our post here.

Regarding the count of whether mass public shootings occurred in gun-free zones, Everytown brings up a total of four cases.

— Two of the four cases that they raise were cases that we agree with and clearly listed as cases where a shooting had occurred where general citizens were able to have guns: The IHOP shooting in Carson City and the Tucson, AZ shooting with Giffords.

— On the Oak Creek case note what we pointed out above that Everytown didn't respond to (from Fox News):<u>"No guns [were] allowed in the temple," Kulbir Singh, an attendee of the Sikh Temple of Wisconsin, told FoxNews.com. "Everyone knows that it's not allowed, anywhere in the temple."</u>

— On the Herkimer county case, note what we pointed out above that Everytown didn't respond to: there were **zero** restricted licenses that allowed people to carry for business purposes (concealed carry licenses that allow a business owner to carry in the course of doing business) and **zero** restricted for self defense purposes (e.g., a woman who is being actively stalked). Even less than 1.4% of the adult population even owns a handgun in the country. A license to own a handgun is not the same being able to carry it.

Why ignore our points in writing up their responses? If Everytown had responses, you would think that they would provide them.

=4/251

UPDATE: A new report titled "<u>Firearms on College Campuses: Research Evidence and Policy</u> <u>Implications</u>" claims on page 10 that there were a few other cases that we supposedly missed.

William Hudson's rampage that claimed six lives at the Tennessee Colony campsite — This attack occurred while people were on their own private property (see here for a discussion).

Christopher Harper-Mercer's shooting spree that claimed nine lives at Umpqua Community College in Roseburg, Oregon — <u>See here for a discussion</u>.

Syed Rizwan Farook and Tashfeen Malik's attack that claimed fourteen lives at a holiday party being held at the Inland Regional Center in San Bernardino, California — given that this occurred in a government workplace in California, there is little question that this was a gun-free zone.

Biker gang fight in Texas May 2015. Gang fight doesn't meet FBI criteria.



### CPRC original research, mass public shootings, Michael Bloomberg

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UPDATED: Mass Public Shootings keep occurring in Gun-Free Zones: 94% of attacks ... Page 16 of 44



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## **138 Responses**

 More misleading information from Bloomberg's Everytown for Gun Safety on guns: "Analysis of <u>Recent Mass Shootings" - The Gun Feed</u> says: September 1, 2014 at 9:28 AM

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2. <u>Terrorists can strike any time, anywhere: Self defense saves lives - People's News Now</u> says: <u>March 6, 2015 at 3:27 AM</u>

[...] at least 1950 every single mass public shooting in Europe (and all but two of the ones in the US) has occurred in gun-free [...]

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# Exhibit 16

Ex.16 Page 330



Scientists: This Breakthrough "Ends" Toenail Fungus (Watch)

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# The GOP's favorite gun 'academic' is a fraud

The journalistic quest for neutrality has led to a sacrifice of intellectual integrity.



EVAN DEFILIPPIS, DEVIN HUGHES AUG 12, 2016, 4:45 PM

CREDIT: AL JAZEERA, THINKPROGRESS

<u>John Lott</u> is, if not the most influential, certainly the most prolific "academic" in the gun debate. He has authored weekly columns in local newspapers on the horrors of gun free zones, published widely-distributed books on the ostensible benefits of right-to-carry laws, and his newest book *The War on Guns* has received rave reviews by prominent conservatives, like Sen. Ted Cruz (R-TX), Sen. Marco Rubio (R-FL), and Newt Gingrich.

Before Lott's flurry of activity, it was difficult to find anybody arguing that widespread gun ownership made societies safer—even the NRA was reticent to make such a bold claim, defending gun ownership with reference to the constitution, not criminology.

But Lott's recent successes belie a far more shadowy past. A little over a decade ago, he was disgraced and his career was in tatters. Not only was Lott's assertion that more guns leads to more safety formally repudiated by a <u>National Research Council panel</u>, but he had also been <u>caught pushing studies</u> with severe statistical errors on numerous occasions. An <u>investigation uncovered</u> that he had almost certainly fabricated an entire survey on defensive gun use. And a blogger <u>revealed</u> that Mary Rosh, an online commentator claiming to be a former student of Lott's who would frequently post about how amazing he was, was in fact John Lott himself. He was all but excommunicated from academia.

Despite his ethical failings, Lott rose from the ashes in the wake of the 2012 mass shooting at Sandy Hook Elementary School to once more become a prominent voice in the gun debate.

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Perhaps unaware of Lott's previous transgressions, or believing he had turned a new page by founding the <u>Crime Prevention Research Center</u> (CPRC), many in the media who were desperate for an authoritative, pro-gun academic voice seized on Lott's credentials and provided him with a new platform. In the past few years, Lott and his organization have been cited by dozens of media outlets as an authority on gun violence statistics, including the <u>Washington Post</u>, the <u>Wall Street Journal</u>, <u>LA Times</u>, <u>Politifact, CBS, CNN, Fox News, and many others</u>.

However, the media's newfound faith in John Lott is deeply misguided. Rather than turn a new page, Lott has instead returned to his old playbook and used his platform to deceive the public. Our own multi-year investigation into Lott and his organization has uncovered a startling array of new ethical violations, ranging from the profoundly bizarre to the outright fraudulent.

Here are just five of the most troubling incidents:



Ex. 16 Page 333 https://thinkprogress.org/debunking-john-lott-5456e83cf326/

1/2/2020



CREDIT: SHUTTERSTOCK, THINKPROGRESS/ADRIENNE MAHSA VARKIANI

Last fall, Lott's website proudly declared it published a study in a peer-reviewed journal. "CPRC Has New Refereed Publication in Econ Journal Watch: Explaining a Bias in Recent Studies on Right-to-Carry Laws" blared the <u>headline</u> on his website. A link to a downloadable copy of the paper also touts its place in the economic journal.

Having a study accepted in a peer-reviewed journal was a big win for Lott, boosting both his own reputation and that of the CPRC. After all, this would be one of the few publications in recent history that Lott dared subject to peer-review.

The only problem? The paper was never actually published in the Econ Journal Watch.

As the head editor of the journal explained to us, while Lott's paper had initially been considered for publication, it was ultimately rejected. The issue of the journal Lott said he was published in has <u>no trace of his paper</u>. It is impossible for Lott to have not known his paper was in fact rejected, and it would have taken little effort to correct both the post on the CPRC website and the uploaded paper on SSRN. This is a clear cut case of fraud.



CREDIT: SHUTTERSTOCK, THINKPROGRESS/ADRIENNE MAHSA VARKIANI

Lott often claims that there is no difference between Kerney of public shootings in Europe and the United States. This is unabashedly false—but he continues to spread the falsehood anyway.

Ex. 16 Page 334 https://thinkprogress.org/debunking-john-lott-5456e83cf326/



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In February, he made the claim before the <u>Tennessee Senate</u>. "Most people may not realize this, but the rate of mass public shootings in Europe is actually fairly similar to the rate in the United States," he said. "There is no statistically significant difference there, either in terms of the rate or fatalities."

A couple of months earlier, he said something similar to the <u>Washington Post</u>, which quickly highlighted that his analysis was quite different from that of other experts in the field. As the Post noted, while Lott said the per capita rates of mass shootings in Europe and the United States were approximately the same, another researcher found the U.S. rate to be five times higher. The Post explained that the gulf between the results was due to Lott and the other researcher using different definitions.

But there is an even simpler explanation for the differing conclusions: Lott wasn't being honest about his own findings.

While Lott claims the per capita rate in the United States and Europe are approximately the same, his own <u>data tables</u> tell a different story. Accepting his data at face value, between 2009 and 2015, the United States had 25 mass shootings versus 19 in the E.U. and 24 in Europe as a whole. This comes out as a rate of .078 shootings per million individuals in the United States, .038 for the E.U., and .032 for Europe as a whole. The United States has more than double the mass shooting rate of the E.U. and Europe, directly contradicting Lott's statements about his own data.

Further, Lott's carefully crafted criteria to include an incident as a mass shooting is highly suspect. Lott goes to great lengths to exclude mass shootings that are the result of burglaries and gang violence, but he includes terro Astact carefidate Ano MeGrate heasts Mitch McConnell in new campaign... that while the Texas biker gang gunfight last summer is excluded in his statistics, the

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November Paris attacks, which accounted for more than one-third of Europe's mass shooting fatalities, are included.

However, when scholars study these mass shootings, they frequently exclude terrorist attacks from the analysis, for much the same reason Lott excludes burglaries and gang violence: the motivations are different. When researchers use a more appropriate set of criteria, the chasm between the rate of mass shootings in Europe and the United States widens even further. Researchers can also include <u>all incidents of mass shootings</u> (regardless of motivation) or use <u>complex statistical analysis</u> to determine whether the mass shooting difference between the United States and Europe is significant. The result remains the same—the United States fares far worse.



This To Wash Out Sugar

From Blood

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All of these methods point to the same conclusion: even if Lott wasn't lying about his own results, his analysis would still be deeply flawed.



CREDIT: SHUTTERSTOCK, THINKPROGRESS/ADRIENNE MAHSA VARKIANI

In their paper "The Impact of Right-to-Carry Laws on Same And McOntablasts Mitch McConnell in new campaign... Carlisle Moody, a CPRC board member, and three co-authors examine the impact of

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Ex. 16 Page 336 https://thinkprogress.org/debunking-john-lott-5456e83cf326/ right-to-carry (RTC) laws on violent crime and critique an earlier <u>study</u> by John Donohue and his colleagues.

Donohue and his colleagues had concluded that the most significant effect of concealed carry laws is an increase in aggravated assault, but Moody et al. reported that: "the most robust result, confirmed using both county and state data, is that RTC laws significantly reduce murder. There is no robust, consistent evidence that RTC laws have any significant effect on other violent crimes, including assault." This result fits well with Lott's long established hypothesis that concealed carry significantly decreases crime, and the authors interpret it as a direct repudiation of Donohue's results.

But there's just one problem. Moody and his co-authors misread their own analysis.

As Table 3 <u>on page 7</u> (pictured below) clearly demonstrates, the increase in aggravated assault for county level data is statistically significant, yet is not bolded by the authors like all the other statistically significant findings. In statistics, a result is usually considered <u>significant</u> if there is a less than 5 percent chance that the result is due to random chance, meaning it has a "t-statistic" greater than 1.96. A significant result in turn means that the authors of a study can put a higher degree of confidence in their finding. As the table below shows, the "stat" for the "post-law trend" for "Assault" (highlighted with a red box) has t-statistics of 2.8 and 2.25 for the general and specific model respectively. Further, the result itself is a positive number, indicating an increase in assault.

Nowhere in the Moody paper does it explain why significant T-stats are un-bolded, and it remains undiscussed in the conclusion, despite the fact that it directly undermines the thrust of their entire paper. Ironically, their paper actually supports Donohue's finding that RTC laws significantly increase aggravated assaults.

Had Moody and his co-authors reported their own results correctly, they would have been left with the puzzling conundrum of concealed carry laws both reducing murder and increasing aggravated assaults. This finding flies in the face of well-established criminological facts and indicates the paper is likely crippled by bad statistical modeling choices. McConnell in new campaign...

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crime hypothesis. Again, Donohue proved that their results were based on coding errors, undermining the authors' central claim.

Given the extensive history of Lott supporting erroneous research, one is forced to wonder whether Moody and his colleagues were influenced at all by the thank you note at the beginning of their paper: "The authors thank The Crime Prevention Research Center for its support."



CREDIT: SHUTTERSTOCK, THINKPROGRESS/ADRIENNE MAHSA VARKIANI

After the <u>mass shooting</u> at a gay nightclub in Orlando, Florida in June, Lott<u>published</u> a piece in which he wrote, "Since at least as far back as 1950, all but three U.S. mass public shootings (with more than three fatalities) have occurred in places where citizens are not allowed to carry their own firearms."

Senate candidate Amy McGrath blasts Mitch This claim has been a staple for Lott, who has repeated dic immeditions for carrying guns." articles, usually phrasing it as areas "where citizens were banned from carrying guns."

Ex. 16 Page 338 https://thinkprogress.org/debunking-john-lott-5456e83cf326/ To support his contention, Lott cites his <u>own report</u> analyzing different aspects of mass shootings.

However, what Lott repeats in public is quite different from what his report actually shows. While Lott's public statements equate gun-free zones with areas that prohibit concealed carry, his mass shooting report expands the gun-free zone definition to include areas where Lott feels it might be difficult to obtain a permit or where there might not be many permit holders despite being able to legally carry. Indeed, Lott admits in the report that more than six mass public shootings in the past six years have occurred in areas that legally allow citizens to carry their firearms, a direct contradiction of his public statements.

And not only does Lott mischaracterize his own research, but the research itself is also filled with significant errors.

In October 2015, after a <u>student at Umpqua Community College</u> in Oregon opened fire in a classroom, killing nine others, the CPRC website <u>immediately proclaimed</u>: "Umpqua Community College is yet Another Mass Public Shooting in a Gun-Free Zone." As evidence, Lott cited the student handbook and the fact that the campus guards were not allowed to carry.

However, while it is true that campus guards were unarmed, Lott's claim that concealed carry was prohibited is definitively false. Public colleges in Oregon are <u>prohibited from</u> <u>banning guns</u> on campus, thanks to a 2011 state court decision. The Umpqua Community College student handbook also expressly states that there is an exception to the prohibition of firearms "as expressly authorized by law or college regulations." This includes concealed carry permits.

"UCC was never designated as a 'gun-free zone' by any signage or policy," Umpqua Community College spokeswoman Anne Marie Levis <u>told Politifact</u> shortly after the shooting. "Umpqua Community College does comply with state law by allowing students with concealed carry licenses to bring firearms on campus."

Not only was Umpqua not a gun-free zone by policy and law, it also wasn't a gun-free Senate candidate Amy McGrath blasts Mitch zone in practice. Multiple reports at the time revealed McGrathele Were Several armed students on campus at the time of the shooting.

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In June 2010, a <u>gunman in Hialeah, Florida targeted</u> his estranged wife who was working at the Yoyito Cafe-Restaurant, killing her and three other women before taking his own life. And again, Lott classified the shooting as taking place in a gun-free zone.

As Lott <u>noted</u>, under Florida law, guns are not allowed in establishments that primarily serve alcohol. As proof that this shooting took place in a gun-free zone, Lott argued that the Yoyito Cafe Restaurant was a popular destination for parties where alcohol was served, and because it primarily served alcohol, the restaurant was a gun-free zone.

That logic is absurd. Serving alcohol at parties is in no way indicative that an establishment is primarily devoted to selling alcohol. Even a cursory glance at the restaurant's <u>reviews</u> clearly indicate that Yoyito is a small Cuban restaurant devoted to selling traditional dishes.

Furthermore, Lott completely ignores the pertinent Florida law regarding restaurants with bars. <u>A letter</u> from the concealed weapons division of the Florida Department of Agriculture clearly notes that the law is written in such a way as to "allow the carrying of firearms in restaurants or similar businesses that primarily serve food but that also happen to serve alcohol as well." In other words, the serving area where patrons are dining in a restaurant does not constitute the part of the establishment primarily devoted to the sale and consumption of alcohol." By law, the Yoyito Cafe was clearly not a gun-free zone at the time of the shooting.



CREDIT: SHUTTERSTOCK, THINKPROGRESS/ADRIENNE MAHSA VARKIANI

"Dear Dartmouth, I am one of your students, I am being stalked, please let me carry a Senate candidate Amy McGrath blasts Mitch gun to protect myself" read the headline of a piece or Mccohlesiis in August 2014.

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Ex. 16 Page 340 https://thinkprogress.org/debunking-john-lott-5456e83cf326/ The first person account was a harrowing story about teenager Taylor Woolrich's desperate attempts to escape and protect herself from a persistent stalker who was ruining her life. The article blasted Dartmouth for not allowing her to carry a gun, and noted that carrying a gun was the only way she could remain truly safe.

The story quickly <u>went viral</u>, and is one that's still brought up by right-wing gun activists. But Woolrich didn't actually write the article.

As a <u>BuzzFeed investigation</u> later revealed, Lott, who is neither a young female nor a stalking victim, was the one who penned the piece. Indeed, Woolrich's article is almost a copy and paste rendition of a previous article published by Lott on the Daily Caller.

"It's his op-ed... Word for word, except the chunks that match what's said in my speech," Woolrich later told BuzzFeed. "It's not like John Lott held a gun to my head and told me to talk to the media... I wanted to talk to the media, if it could mean something positive. But I wanted to talk to the media about stalking."

Despite reservations about her message being co-opted, Taylor agreed to have him help her write for Fox, worrying: "I don't know if I should just say yes and not piss him off." Eventually, Woolrich changed her number and completely broke off contact with Lott.

"I was trying to be brave and just speak up," she said. "I didn't realize I was being turned into an NRA puppet."

While Woolrich may have been <u>eager</u> to share her story at first, this doesn't excuse the fact that Lott wrote a first person narrative on behalf of someone else, using his own words. When a Fox editor later thanked Lott for the piece, Lott <u>replied</u>, "It was actually easier for me to write this in the first person for her than the way I had originally written it."

This isn't the first time Lott has written in the first-person female voice. Back in the early 2000s, Lott and his research were coming under increasing fire from the academic community. Mary Rosh, claiming to be a former student of Lott's, rose to his defense in Senate candidate Amy McGrath blasts Mitch online chatrooms and comment sections. She praised Cott as the best professor she had ever had and took deep offense whenever somebody questioned Lott's research. A Read Next Story >

few online commenters found her passion rather bizarre, <u>consoling her</u>: "I'm sorry if you're taking this personally, but you are not John Lott."

Except she actually was. A blogger matched Lott's IP address with that of Mary Rosh, and a humiliated Lott was <u>forced to admit</u> that he and Mary were the same person.

As conservative journalist Michelle Malkin <u>emphasized</u> at the time, "Lott's invention of Mary Rosh to praise his own research and blast other scholars is beyond creepy. And it shows his extensive willingness to deceive to protect and promote his work."



### Why does the media still rely on John Lott?

CREDIT: CSPAN/SCREENSHOT

In an attempt to appear fair and balanced, news outlets have offered John Lott a platform to debate a subject for which there really is not two sides. Gun violence is decidedly <u>uncontroversial among scholars</u>: more guns cause more suicides, homicides, and accidents.

These are the arguments being made by serious academics in peer-reviewed journals from <u>Harvard</u>, <u>Stanford</u>, <u>Yale</u>, and <u>Johns Hopkins</u>. On the other side of the debate, you have John Lott, a handful of conservative academics on the board of the CPRC, <u>Gary Kleck</u>, and a few others.

Much like the <u>public debate over climate change</u>, the journalistic quest for neutrality in Senate candidate Amy McGrath blasts Mitch discussing gun control has led to a sacrifice of intelle McGbintel firity and hypersty. Over the past two decades, John Lott has routinely demonstrated an unwillingness to

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engage honestly in the gun violence debate. Lott is not a credible source, and it's time the media stop treating him as such.

**Update:** Since the publication of this article, the description of the study Lott claimed to have published in the Econ Journal Watch has been <u>corrected</u> on the Social Science Research website. An archived version of the paper touting its publication in the journal is still available <u>here</u>. The news of the study has also been <u>changed</u> on the CPRC website, removing the reference of it being published in the Econ Journal Watch. The original headline touting this publication is still evident in the URL, and an archived version of this news on the CPRC website is still available here.

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Evan and Devin write on gun violence issues at Armed With Reason.

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Senate candidate Amy McGrath blasts Mitch McConnell in new campaign ad

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# Exhibit 17

# A Fabricated Fan and Many Doubts

The Washington Post February 11, 2003 Tuesday, Final Edition

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Section: A SECTION; THE IDEAS INDUSTRY CLAUDIA DEANE AND RICHARD MORIN; Pg. A19

Length: 917 words

## Body

The cyber-pundits are piling on John R. Lott Jr., the embattled American Enterprise Institute researcher who acknowledged that he created an online fan, "Mary Rosh," to defend his work against critics.

The latest to weigh in is Timothy Noah, the Slate "Chatterbox" columnist who summarizes a cresting flood of online attacks against Lott. At issue isn't Lott's overactive imagination and faux fan, but a 1997 survey the researcher purportedly did to support claims in his provocative book, "More Guns, Less Crime." In particular, the UCLA-trained economist contended that merely brandishing a weapon successfully deterred criminal attacks 98 percent of the time, based on his national survey.

Now his critics are asking: What national survey? Lott has been unable to produce the poll data, which he says were lost when his computer crashed.

Lott vehemently denies faking the study but does acknowledge that he created Mary -- an admission that has his critics suspecting the worst. "We know Lott invented an online persona. Did he invent the 98 percent figure? Did he invent the survey it purportedly came from? We don't know," Noah wrote last week.

Does all this sound familiar? Last year, Emory University historian Michael Bellesiles was unable to produce the data on which his prizewinning book "Arming America: The Origins of a National Gun Culture" were based. The study suggested that few people in frontier America had guns, a finding that displeased gun lovers. Bellesiles said his records were lost in a flood. A university investigation raised doubts, and he resigned. No comment, say AEI administrators.

EVERYTHING GOES BETTER WITH FRIENDS: In the war on Iraq and the larger struggle against international terrorism, size matters. The more countries that join with the United States in a coalition to defeat the enemy, the greater the chances of a lasting peace.

And that is why President Bush should be willing to wait a few weeks or even months to assemble an army of nations to march against Iraqi President Saddam Hussein, argues Georgetown adjunct professor Andrew Pierre, author of a new book, "Coalitions: Building and Maintenance." The book is a product of a study sponsored by the Institute for the Study of Diplomacy at Georgetown University and the American Academy of Diplomacy.

"It is important that we move against Iraq with a true international coalition, made up of as many countries as possible," Pierre said.

#### A Fabricated Fan and Many Doubts

That's not to say that the United States should stop threatening to go it alone, Pierre says. The history of coalition building in the past decade suggests just the opposite: The noise of sabers rattling is just the thing to prod reluctant nations to join the coalition. "Coalition-building will be facilitated if it is clear that the lead nation has the will and the ability to act alone, if necessary," Pierre wrote.

Pierre says the United States doesn't need allies for the guns they would provide. "We need allies in a coalition because we want to be perceived by the world as doing the right thing," he said. "We want the support of the Arab world as much as possible. Unilateral action risks fueling instability in moderate countries in the Middle East."

ED SCHOLARS MAKE GOOD: Remember how we told you the Thomas B. Fordham Foundation was going to give away serious moola (\$ 25,000) to some lucky (and gifted) education researchers? Well, the results are in: The winners for distinguished research on education are the University of Chicago's Anthony Bryk and Harvard's Paul E. Peterson.

Why the new prize program? "The good guys in education never win prizes. The decks are always stacked in favor of people with bad ideas," said Fordham head Chester E. Finn Jr.

PEOPLE: For the first time in its 10-year existence, Rand Europe -- the Netherlands-based branch of the Rand Corp. -- will have a European head: Dutchman Martin van der Mandele, a longtime management consultant who has served as a senior fellow in Rand's Santa Monica office since April. David Gompert, the current head, will return to the D.C. office as emeritus vice president.

Speaking of Rand, a researcher who resigned from the defense think tank last fall under murky circumstances has landed at the Hudson Institute. Laurent Murawiec became radioactive in July when he dumped on U.S. ally Saudi Arabia in a briefing before the Pentagon's Defense Policy Board. He left Rand in September, and in January, he joined Hudson as a senior fellow in the Center for Mideast Studies.

The Council on Foreign Relations has hired Eric Schwartz to direct a new task force on post-conflict reconstruction in Iraq. He was a senior director for multinational and humanitarian affairs at the National Security Council. CFR also promoted Lisa Shields to vice president and director of communications.

The Cato Institute has a new director of foreign policy studies: Christopher Preble. Preble comes from Minnesota's St. Cloud State University, where he taught history.

Mark lwry is joining the Brookings Institution as a nonresident senior fellow. lwry was a Treasury benefits tax counsel under secretaries Robert E. Rubin and Lawrence H. Summers, and was a partner at Covington & Burling. He plans to practice law on a part-time basis.

Philip Merrill, Ex-Im Bank president and former Washingtonian magazine publisher, has committed a whopping \$ 4 million to Johns Hopkins University's School of Advanced International Studies to establish the Philip Merrill Center for Strategic Studies.

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# Exhibit 18

## **Comments on Questions About John R. Lott's Claims Regarding a 1997 Survey**

## Personal Note, January 17, 2003:

In September 2002 I offered to look into a question raised about John Lott's work. I thought that offering such help to Lott and to the profession was the responsible thing to do when serious questions were raised, and I thought it would be exceedingly simple to establish that a survey of 2,424 people had been done. While I recognized that it is extremely easy to lose data in a computer crash, I had not anticipated that Lott would claim to have done a large national survey without discussing the sampling design with anyone, leaving any financial or other records of the study, or remembering anyone who had worked on it. I had never heard of a professor doing anything of that size with no funding, paid support, paid staff, phone reimbursements, or records (though there are probably precedents for such an unusual method). As it stands now, unless someone comes forward to verify working on the study--as I still hope occurs--we may never know with any certainty whether the 1997 study was done. Although I strongly favor emailing 1997-98 University of Chicago college graduates to see if any remember any classmates working on the study, John Lott now raises serious questions about how complete the University's alumni records are, rendering that approach a less reliable route to an answer than I had anticipated.

I recently contacted Otis Dudley Duncan, who first raised questions about Lott's claim that 98% of defensive gun uses involved the mere brandishing of a weapon. He agrees (as I do) with this statement of Tim Lambert on Lambert's website:

"I should comment on the overall significance of this question. Lott's 98% claim takes up just one sentence of his book. Whether or not it's true, it doesn't affect his main argument, which is about alleged benefits of concealed carry laws."

So there agreement even among those who have raised questions about Lott's work that his 98% claim is not central to his book, <u>More Guns, Less Crime</u>. Both Duncan and Lambert, however, emphasize their belief that whether the study was done does go to John Lott's credibility.

I find recent developments in this affair personally troubling. I carefully recorded what John Lott told me and now Lott has changed the story he told me in several specific ways--most of them minor. They are discussed in my revisions to this report. I have no research interests in this subfield and no ideas for further efforts to get to the bottom of this inquiry beyond surveying graduates and Lott's looking at picture books of former students. This project detracts from my other scholarly efforts. Accordingly, my part in this affair is essentially done, at least if John Lott will stop changing his stories about our conversation. If not, then I suspect that I will have to stay in it a little longer, at least to respond to comments on this report.

For those who have been following the dispute over Lott's 1997 study, other than a few fairly small changes, the portions of this report new on January 17, 2003 are this Personal Note, Sections 4 and 5 ("Comments on John Lott's Response to this Report" and "Conclusion"), and Appendix 3 (John Lott's email responding to this report).

Ex. 18 Page 348
James Lindgren Professor of Law Northwestern University

### Comments on Questions About John R. Lott's Claims Regarding a 1997 Survey

December 24, 2002; Revised January 12, 2003; Sections 4-5 added and Appendix 3 added on January 17, 2003 Chicago, IL

#### 1. Background and John R. Lott's Claims

## a. Different Sources Cited for the 98% of Defensive Gun Uses Being Mere Brandishing

On September 15, 2002 on the discussion list *FirearmsRegProf*, Tim Lambert raised the possibility that John R. Lott fabricated a study claiming that 98% of defensive gun uses involved brandishing with no shots being fired. Lambert wrote: "it seems increasingly likely that the survey is fictional." He was following up on problems first identified by Otis Dudley Duncan and discussed publicly in the Jan./Feb. 2000 issue of *The Criminologist*. These extremely serious charges concerned a study referred to in the second edition of Lott's *More Guns, Less Crime*, but never actually published. Over the years, however, Lott has referred to this 98% number over four dozen times in publicly available sources.

Here is Lott's statement in the (first) May 1998 edition of *More Guns, Less Crime*, which attributes the number to "national surveys."

"If national surveys are correct, 98 percent of the time that people use guns defensively, they merely have to brandish a weapon to break off an attack." *More Guns, Less Crime* (University of Chicago Press, 1998), p. 3.

The year before, in the July 16, 1997 *Wall Street Journal*, Lott appeared to attribute the 98% figure to one or more of three specific survey organizations:

"Other research shows that guns clearly deter criminals. **Polls by the Los Angeles Times, Gallup and Peter Hart Research Associates show** that there are at least 760,000, and possibly as many as 3.6 million, defensive uses of guns per year. In 98% of the cases, **such polls show**, people simply brandish the weapon to stop an attack." John R. Lott Jr., Childproof Gun Locks: Bound to Misfire, *Wall Street Journal*, 7/16/97 Wall St. J. A22 The same language (other than typesetting conventions) appears the following year in two articles by Lott on the same topic for the *Chicago Tribune* and the *Washington Times*. John R. Lott Jr., Prime Suspect: Gun-Lock Proposal Bound to Misfire, 8/6/98 Chi. Trib. 23; John Lott, Commentary: Gun Locks That are Bound to Misfire, 8/14/98 Wash. Times (D.C.) A17.

Starting in January 1999, Otis Dudley Duncan began writing a series of letters to Lott questioning aspects of his work, including the 98% figure. In May 1999, Duncan informed Lott that he was writing an article calling the 98% a "rogue number" and then sent him a draft of an article containing these words, "The '98 percent' is either a figment of Lott's imagination or an artifact of careless computation or proofreading."

Lott then called Duncan on May 21, 1999 and, for the first time, told Duncan that he had conducted a hitherto unrevealed study in 1997. Not long after that phone call, Duncan received a letter dated May 13, 1999, which also mentioned a 1997 study. In that letter, Lott responded to Duncan's suggestion that the 98% figure might have resulted from a misreading of a study by Gary Kleck:

"I am a great admirer of Gary Kleck's work, and I think that he has done a great deal to advance the study of crime. Few academics have his integrity and courage. His numbers are a little higher in terms of the total number of defensive uses that I have found and the frequency of brandishing is lower than I have found. The information of over 2 million defensive uses and 98 percent is based upon survey evidence that I have put together involving a large nationwide telephone survey conducted over a three month period during 1997. Follow up telephone calls were made to ensure that the questions were answered by those who we attempted to contact. The survey was not as detailed as several other surveys, but it did try to include a couple initial questions to ensure accuracy and screen out any problems and then focus exclusively on defensive gun uses. I plan on repeating the survey again during the next year to year and a half. I will be happy to inform you what the results of that survey are after I have conducted it." Letter from John Lott to Otis Dudley Duncan, dated May 13, 1999.

Note that Lott makes no mention of having lost the data for the 1997 study. According to Duncan, Lott's claim that he had lost his data surfaced only much later.

In the second 2000 edition of *More Guns, Less Crime*, Lott gives the same 98% figure, but (as in his letter to Duncan) attributes the number to his own study:

"If a national survey that I conducted is correct, 98 percent of the time that people use guns defensively, they merely have to brandish a weapon to break off an attack." *More Guns, Less Crime,* second edition (University of Chicago Press, 2000), p. 3.

In an email to me on December 26, 2002, Lott writes that he submitted the manuscript at least 9 months before publication, which would place Lott's submission of this language in late 1999.

Then, in February and March 2000, Lott gives the same 98% figure, but this time attributes it to Gary Kleck, an authority that he had disavowed as the source for the 98% figure in 1999. In his March 2000 piece on gun locks, Lott wrote:

"Guns clearly deter criminals. Americans use guns defensively over 2 million times every year--five times more frequently than the 430,000 times guns were used to commit crimes in 1997, according to research by Florida State University criminologist Gary Kleck. **Kleck's study of defensive gun uses found that 98 percent of the time, simply brandishing the weapon is sufficient to stop an attack.**" John Lott, Gun Locks: Bound to Misfire, online publication of the Independence Institute, March 1, 2000.

Lott used almost identical language in a version of the same article on February 9, 2000, also published by the Independence Institute. Kleck's work does not support this claim, though (as I understand it) some others have mistakenly read it as supporting this claim. As Duncan points out based on a discussion with the Independence Institute, there is a chance that Lott might have written the article earlier than 2000, though when he covers the same ground in several articles in 1997-98, he does not list Kleck as the source.

Then in his comment in the September/October 2000 *Criminologist*, Lott returned to claiming that he got his 98% number, not from Kleck, but from his own 1997 study. Otis Dudley Duncan raised questions about the 98% figure in *The Criminologist* (Jan./Feb. 2000), after exchanges between Lott and Duncan that occurred in 1999. In response to Duncan's comments in *The Criminologist*, Lott describes his 1997 survey, also in *The Criminologist* (Sept./Oct. 2000):

"The survey that I oversaw interviewed 2,424 people from across the United States. It was done in large part to see for myself whether the estimates put together by other researchers (such as Gary Kleck) were accurate. The estimates that I obtained implied about 2.1 million defensive gun uses, a number somewhat lower than Kleck's. However, I also found a significantly higher percentage of them (98 percent) involved simply brandishing a gun. My survey was conducted over 3 months during 1997. I had planned on including a discussion of it in my book, but did not do so because an unfortunate computer crash lost my hard disk right before the final draft of the book had to be turned in. Duncan raises a related issue that "Lott may well have read Will, in as much as Will's article is in the bibliography of More Guns, Less Crime. ... Did Lott borrow the '98 percent' from Kleck . . . from Snyder, via Will? Even if that account explains part of the puzzle, the question remains. Where did the 2 million come from?" (Page 6) The course that Duncan tries to follow - from a 1988 article by Kleck to a 1993 piece by Snyder to George Will to my book because it cites Will - is fascinating. Yet, I am not sure why this entire discussion was necessary since I told Duncan on the telephone last year that the "98 percent" number came from the survey that I had done and I had also mentioned the source for the 2 million number."

That means that the source that Lott gave for the 98% figure has shifted over time:

Ex. 18

1. In the 1998 edition of *More Guns, Less Crime*, John Lott attributes the 98% figure to "national surveys."

2. Elsewhere in 1997 and 1998, Lott appears to attribute the 98% figure to "such polls" as the "Los Angeles Times, Gallup and Peter Hart Research Associates."

3. In a May 13, 1999 letter and in 1999 revisions to the 2000 edition of his book, Lott attributes the 98% number instead to his own 1997 study, saying in the letter that the 98% figure is not based on Kleck.

4. In almost identical February 9, 2000 and March 1, 2000 articles for the Independence Institute, Lott switches to attributing the 98% figure, not to his own study, but to Gary Kleck's (which does not support this figure).

5. In the *Criminologist* (Sept./Oct. 2000), Lott switches back to claiming that the 98% figure came from Lott's own 1997 study, not from Kleck, which is where things stand as of this report.

Note that by using the plural "national surveys" and "such studies" Lott is stating that there are more than just one study showing the 98% figure, yet he now insists that the 98% figure came from his own study, not Kleck's (and Kleck's study does not support the 98% figure). Indeed, as discussed in the next section, because the 98% figure is supposed to be based on his own study, not those done by others, Lott says that his critics will have "nailed" him if they find that he began talking about the 98% figure before he says he did his study in 1997. Yet, if Lott really based the 98% figure on his own study alone, it seems strange that he would attribute the 98% figure to such plural entities as "national surveys" or "such studies"--until Duncan challenged him in 1999 and Lott revealed for the first time that the 98% figure was based on a study he did himself.

#### c. A Study Done "over 3 months during 1997"

In the *Criminologist*, Lott also wrote: "My survey was conducted over 3 months during 1997." Lott called me on the telephone and repeated that he had conducted the study over several months during 1997. If he spent 3 months doing it in 1997, as he claims, the earliest that he could have completed it would be early April. Further, in an email to me Lott wrote, "I am willing to bet that I don't start mentioning this [98%] figure until the spring of 1997. If I use it before I said that I did the survey, I will say that they nailed me. But if I only started using it about the time that I said that I did the survey, I think that it would be strong evidence the other way."

Lott's first mention of the 98% figure located by Dudley Duncan is February 6, 1997, nearly two months before he could have completed the survey, according to his prior claims:

"There are surveys that have been done by the Los Angeles Times, Gallup, Roper, Peter Hart, about 15 national survey organizations in total that range from anything from 760,000 times a year to 3.6 million times a year people use guns defensively.

### About 98 percent of those simply involve people brandishing a gun and not using them."

Page 41, State of Nebraska, Committee on Judiciary LB465, **February 6, 1997**, statement of John Lott, Transcript prepared by the Clerk of the Legislature, Transcriber's Office [Otis Dudley Duncan & Tim Lambert, <u>http://www.cse.unsw.edu.au/~lambert/guns/lottbrandish.html]</u>

I have not verified this transcript, but if accurate, Lott's February 6, 1997 testimony is inconsistent with Lott's claims--published, by phone, and by email--that he did the study in "three months" (and "several months") in 1997. Lott himself set up this timing issue as the test whether he "will say that they nailed me." It is possible that Lott's memory was simply off on the year the study was done, that he instead did it in 1996, but if Lott did indeed conduct the study over 3 months in 1997 (as Lott wrote in the *Criminologist*), then he could not have reported the results on February 6, 1997, just weeks after beginning data collection.

After this discrepancy was noted in the first draft of this report, on December 26, 2002, Lott wrote me by email:

"The overwhelming majority of the survey work was done at the beginning of the period over which the survey was done. It has obviously been a while, but my recollection is that the small number of people surveyed after the first four or five weeks (mainly January 1997) did not include any more defensive gun uses."

While again this story is certainly possible, Lott himself gave spring 1997 as the time before which he should not have been discussing the 98% figure. Additional matters bear mentioning. It hardly matters whether all of the defensive gun uses were found in the first 4-5 weeks of the study, since Lott could not have known that at the time he spoke about the results unless data collection were complete. If data collection were partial, the precise percentage of defensive gun uses would have been higher with partial data. Collecting so much data in 4-5 weeks would have been unusual for unpaid volunteers who were full-time undergraduate students at the University of Chicago at the time, unless there were a very large number of volunteers. As I discuss below in the section on technical problems with the study, Lott's numbers suggest that only  $\frac{1}{2}$  or 1/8th or 5/8ths of a respondent reported certain kinds of defensive uses. The partial respondents necessary to support Lott's percentages would be most likely to result from some extreme demographic weights being applied after the data collection were complete and the results were compared with the characteristics of the adult population. If the study were not complete, it would be very unlikely that someone would have weighted the results against the general population before knowing how skewed his sample was. Such weighting is not easy and would have been a colossal waste of time before data collection were complete, since they would need to be redone at the end of data collection. Last, of course, Lott does not mention that he is reporting partial data in his February 6, 1997 testimony.

There are other, more ambiguous contextual clues that Lott had contrasted his main work, which was done on county-wide data, with surveys done by others, which involved household surveys. For example, in the *Washington Times* in 1999, Lott wrote:

"Indeed, about 450,000 crimes, including 10,744 murders, were committed with guns in 1996. But Americans also use guns defensively over 2 million times a year and 98 percent of the time merely brandishing the weapon is sufficient to stop an attack. In my own recent research on gun ownership across states and over time, I found that states with the largest increases in gun ownership rates had the largest drops in crime rates." John R. Lott, Lethal Handgun Fears, 2/24/99 Wash. Times (D.C.) A17.

In a long 1999 *Chicago Tribune Magazine* story, after speaking to John Lott, Linnet Myers twice contrasted Lott's county-wide work with Kellermann's household study:

Bolstering the other side is Dr. Arthur Kellermann, of Emory University's Center for Injury Control in Atlanta. His research

indicates that owning a gun is far more dangerous to a homeowner than it is to potential intruders.

• • •

Lott didn't examine home protection, but he did study the impact of armed selfdefense. In his book, "More Guns, Less Crime" (University of Chicago Press), he wrote that violent crime dropped noticeably in the 31 states that now give permits to qualified

citizens who want to carry handguns. Twelve states allow permits in certain cases. Seven, including Illinois, prohibit carrying.

•••

[Tom] Smith points out that while the two researchers clearly support opposing sides in America's gun debate, their findings aren't exactly opposite. Kellermann addresses the risks of keeping a gun at home and he measured only self-defense shootings--not occasions when guns were used simply to threaten.

Lott didn't study gun use at home, but looked at the impact of laws that allow guns to be carried outdoors. Even so, Lott said that in most cases of self-defense, "people merely need to brandish a gun . . . less than 2 percent are fired." He said guns particularly help women, who become more "equal" to men when they're armed. "Women who behave passively are 2.5 times more likely to end up being seriously injured than women who are able to brandish a gun when confronted by a criminal," said Lott. Linnet Myers, Go Ahead Make Her Day With Her Direct Approach And Quiet Confidence, Chicago Lawyer Anne Kimball Gives Gunmakers A Powerful Weapon, *Chicago Tribune*, 5/2/99 Chi. Trib. 12.

If this newspaper account is accurate (and newspapers often aren't), it is odd that Lott would try to answer the reporter's claims about the Kellermann household study without pointing out that he had done a big household study himself. Although this contextual evidence is less telling, it does tend to fit the pattern that, until Lott replied to Duncan in mid-May 1999, Lott had consistently attributed the 98% figure to several specific survey organizations or to no one, never to his own 1997 study.

#### 2. Technical Problems

Ex. 18

Another problem (mentioned by Tim Lambert and Dudley Duncan) is the small and almost impossible numbers of respondents on which Lott would have based his claim about the rate of firing v. merely brandishing. According to Lott, the study found that 98% of the defensive uses of a gun involve mere brandishing, and that 3/4ths of that 2% involve firing warning shots, the other  $1/4^{\text{th}}$  of that 2% involves firing at a person threatening the shooter. With Lott's estimate of 2.1 defensive gun uses a year and about 200 million adults, that would mean that about 25-26 respondents reported defensive gun uses out of his 2,424 people surveyed. Thus, only ½ of a person (2% of 25 people) reported firing a gun--and that  $\frac{1}{2}$  of a person breaks down further into  $3/8^{\text{ths}}$  of a person firing warning shots and  $1/8^{\text{th}}$  of a person firing at someone.

While one can get fractions of a person if one weights respondents by their numbers in the general population, getting 8 times more people in a sample group than ideal would be rare (the number needed to justify a weight of 1/8<sup>th</sup>). Further, how can one generalize three different rates (firing, firing a warning shot, and firing at a person) from at most only one (or two) people, people who are so unrepresentative that they are weighted as 1/8<sup>th</sup> or 3/8<sup>ths</sup> of a person?

It is possible that a multi-year window was inquired about, though even with a five year rate, one still has only 5/8ths of a person and 15/8ths of a person to support his reported rates. Further, Lott never gives the rate as being for a multi-year window such as 1993-97, sometimes implying that the rate is for 1997. Even if a five-year window were used, the numbers of respondents would be still be so low (e.g., 5/8ths of a person) as to be unreliable for reporting a rate.

Unless John Lott can come up with a sensible explanation for why his rates could possibly be justified with only a 2,424 person sample, it is my opinion that Lott should withdraw the 98% figure as probably erroneous and, in any event, too unreliable to form the basis of an estimated rate. Perhaps he has an explanation that doesn't appear yet. If not, withdrawing the 98% figure in some appropriate way would be a simple matter of good social science.

After this report was written, Lott disclosed that he had done a new survey of about 1,000 respondents in the fall of 2002. Daniel Polsby has spoken to enough people involved and seen enough records to determine conclusively that this new study was done. Even more than with the earlier study, however, I don't see how one can get an estimate of something that Lott says happened to about 1 out of every 4,800 people each year (2% of 1.05%) with a sample size of just over 1,000 people, asking about their experiences over the last year.

#### 3. The More Serious Issue: Was the 1997 Study Ever Done?

#### a. Circumstantial Evidence of a 1997 Computer Crash

The more serious question, which Tim Lambert has raised, is whether Lott ever did the 1997 survey giving rise to the 98% figure. As I posted in September, all evidence of a study with 2,400 respondents does not just disappear when a computer crashes. Having done one large survey (about half the size of John Lott's) and several smaller surveys, I can attest that it is an enormous undertaking. Typically, there is funding, employees who did the survey, financial records on the employees, financial records on the mailing or telephoning, the survey instrument, completed surveys or tally sheets, a list of everyone in the sample, records on who responded and who declined to participate, and so on. While all of these things might not be preserved in every study, some of them would almost always be retained or recoverable. Just to get a representative list of the US public would take consultation with an experienced sampler and probably the purchase of an expensive sample. As far as I know, there was no cheap commercial list of almost every person or household in the United States from which to draw a good sample.

According to Lott, he lost all of his data on his hard drive when it crashed in June of 1997. I talked with one of Lott's co-authors on another paper, Bill Landes, and received emails from David Mustard, another co-author, and Gregory Huck, Lott's editor at the time at the *University of Chicago Press*. With varying degrees of certainty, all give circumstantial support to Lott's story of a sudden loss of data and text on projects, requiring delays and regeneration of work. Further, Mustard recalls hearing about the 1997 study, though when Lott told him about it is a little unclear from Mustard's email to me:

John told me that he had conducted a survey in 1997. I did not participate in the survey--it was after our concealed carry paper had been published (Jan 1997) and was after I was on the job market and while I was finishing my dissertation and then moving to Georgia (Aug 1997).

John had some major computer problems in 1997 or 1998--I am not sure of the exact timing, but I think I was already here in Georgia. John lost a lot of data and material and I and others tried to replace what he lost. Lott indicated that the survey results were not backed up and that he lost all of that, none of which could be replaced. Nobody else had the survey data. That is about all I know about the survey.

John has always impressed me with his willingness to give out his data; to anyone who requests them. To my knowledge John has always released his data to anyone who asks of it. In fact, we gave out our data about 4 months before the article even came out in print. We have now given our data out to about 75 people from around the world; perhaps more[.] As I understand the survey situation, John does not release the survey data because he no longer has it, not because he is unwilling to do so.

Mustard also is not sure when the data loss occurred. At first he uncertainly placed it after he moved from Chicago in August 1997, later than Lott's time of June 1997. But in a follow-up December 26, 2002 email to me after the first draft of this report, Mustard quite plausibly explained:

"As to the \_date\_ of John's computer crash, it could have happened in June 1997. My previous response would more accurately be that I \_sent him the data\_ after I was at Georgia. I do not really remember \_when\_ it crashed, only that it did and I sent him

the data shortly after I arrived in Georgia. Given that I was gone for large parts of July and that our possessions were being shipped, it is feasible that John's crash could have occurred in June and I sent him the data after I got set up in Georgia."

David Mustard, like Bill Landes, Russell Roberts, and Daniel Polsby, also went out of his way to note Lott's scrupulousness in sharing data on other projects.

#### b. No Direct Evidence of a 1997 Survey

As for obtaining direct, rather than circumstantial, evidence that the study was done, I did not fare as well as might be expected. Lott called me and told me the following:

1. Lott had no funding for the project; he paid for expenses himself.

2. The survey was done by phone by several University of Chicago undergraduate volunteers in their junior or senior years in 1997, so there are no financial employee records.

3. The calling was done by the undergraduates from their own phones. Periodically, they would bring over their phone bills and Lott would reimburse them out of his own pocket--either in cash or by check. Asked whether he retained his checks, Lott said that he destroyed them after 3 years. I did not think to mention it, but the phone research expenses should have been deductible if they were not reimbursed by his employers.

4. Lott does not remember the names of any of the undergraduates who did the calling for him.

5. Lott had no discussions with any samplers about his sampling design.

6. Lott did not weight his sample for household size and did not ask how many adults were in the household (as is standardly done, thus rendering his results too heavily influenced by small households). [In response to Lott's recent question to me about why weighting for household size is necessary, if the unit of interest is households rather than people, then you do not have to weight responses by the number of adults in the household. If, however, the unit of analysis is people rather than households, then you should weight for household size, otherwise you will over represent people who live alone and under represent those who live in large households. For example, if the average number of adults in households were 2 people, then you would normally weight the result for a person living in a household with 4 adults by 2, and weight the result for a person who lives alone by  $\frac{1}{2}$ . I asked this question of Lott for another reason; it might have explained his small apparent weights for some people if he had adjusted for household size, but he didn't. Sampling is a lot more complicated than this simple example implies, which is why very few people would attempt a national sample without consulting a sampling expert.]

7. For his list from which to draw the sample, Lott used a commercially available CD-ROM with names on it. He does not remember where he got it or now have the CD.

8. Lott does not remember how he drew his sample from the CD-ROM.

9. Lott does not have a copy of the survey instrument and doesn't remember the wording of the questions, though he was probing defensive uses in more detail than other studies. He ended with a very few demographic questions.

10. Lott weighted his respondents by demographic information taken from his main national study in *More Guns, Less Crime*.

11. In his book *More Guns, Less Crime*, Lott had planned to include a chapter on the 1997 study, a chapter that he had not yet written, but decided not to do so after the data loss. He did not end up publishing the 1997 study itself, just referring to it many times, including a sentence about it in the second edition of *More Guns, Less Crime*.

12. Lott thinks that he did not retain any of the tally sheets, though he is not certain. He reported that he might have tossed out tally sheets or other evidence of the 1997 study during one of his several moves over the years.

The discussion of the tally sheets is possibly in conflict with what Lott wrote to Dudley Duncan on AEI letterhead dated June 4, 2002: "I used students to conduct the survey. Most (though not all) of the information was originally entered into the students' own computers and then they were combined onto my machine. You know what happened to my computer." This version of the story suggested original computer entry of data, which would have required central programming of a data form and then sharing of that program with several callers for their home computers. From Lott's discussions with me, I was left with the impression that the callers instead used sheets to record answers, which he thought that he no longer retained. Besides the possible discrepancy between the two versions of the ways that the students collected and conveyed the data, if the data were instead sitting on the students' computers, it is possible that Lott could have replaced most or all of his lost data by asking the students to give it to him again.

With the surprising lack of any of the normal indicia of having done a large national study of 2,424 respondents, the key remains locating the undergraduates who Lott says did the calling. The 1997 study was large, extremely time-consuming, and very expensive in phone charges. Getting 2,424 respondents with refusals and callbacks would have required thousands and thousands of phone calls. Students would have had to spend many hours calling, which they and their friends would well remember. With John Lott's permission, I therefore contacted Saul Levmore, the Dean of the University of Chicago Law School, requesting him to try to obtain from the alumni office the email addresses of the 1997 and 1998 college graduating classes at the University of Chicago, so that I could contact them asking whether any of them or any of their friends had done any research for John Lott during his fellowship at Chicago. Levmore declined to make such a request for email addresses from the University of Chicago Alumni Office at this time, but he did not rule out cooperating with a request coming from other quarters or in a different situation. Of course, my request was in the form of fact-finding, rather than a complaint, which might have triggered a different process.

Thus, I have reached a temporary dead end. If someone were to email the 1997-98 Chicago college alumni twice, it is likely that at least one of those who did the study for Lott would come forward, if the study were actually done.

#### 4. Comments on John Lott's Response to this Report

#### a. Several Changes in Lott's Story

On January 14, 2003, John Lott sent an emailed response commenting on this Report to me, three bloggers, and several of his friends and colleagues. Most of it was posted on the internet by Marie Gryphon. Lott makes several good points, which you can read below in his own words in the third appendix to this report. There are, however, several changes in his story from what he told me when he called me in September that bear mentioning. I will restate what Lott told me on the phone in September and then print what he is saying now:

THIS REPORT (above): "2. The survey was done by phone by several University of Chicago undergraduate volunteers in their junior or senior years in 1997, so there are no financial employee records."

LOTT'S RESPONSE (Appendix 3 below): "Lindgren does not accurately report my conversation with him about how I paid people (in that I said that I possibly paid by check) . . . . Incidentally, I told Jim that there were "two" Chicago students. Those students had also gotten others that they knew from other campuses from places such as I think the University of Illinois at Chicago circle (but I am not sure that I remember this accurately)."

THIS REPORT: 3. "The calling was done by the undergraduates from their own phones. Periodically, they would bring over their phone bills and Lott would reimburse them out of his own pocket--either in cash or by check."

LOTT'S RESPONSE: "most of this next statement ["calling was done by the undergraduates from their own phones "] is correct except the point about the "possible" use of checks."

The facts of my conversation with John are different than he now remembers them. John Lott called me shortly after I posted a notice to the FireArmsRegProf discussion list on September 15, 2002 offering my help to Lott to help sort out Tim Lambert's extremely serious charges. I was looking for evidence to support Lott's claim that he did the 1997 study. At the time I knew very little about the affair and had no other scenarios running around in my head other than what Lott told me. I did not even know when Lott called whether he had supposedly done a mail or telephone survey. I listened very closely to his answers, especially on questions that went to credibility. I was pleased when he told me that he had sometimes paid the students by check and sometimes in cash. There are a lot of reasons to pay by check, including the size of the phone bills that would have been involved and the costs being tax-deductible (though I wasn't thinking specifically about that latter point at the time). Even though I thought that Lott's contacting his bank for check records would be unsuccessful, I thought that by claiming that he sometimes paid by check, Lott was leaving himself open to the possibility of being checked, which was a good sign for assessing his credibility. Now sadly Lott has changed his story, claiming that he told me instead that he only "possibly" paid some by check. That is not what he said to me in

Ex. 18

September. I noticed then both what he said and his demeanor in saying it; he was not defensive or hesitant to say that he sometimes paid them by check.

What is most surprising about this claim is that Lott emailed me on January 13, 2003, the day before he wrote his long response quoted above. On January 13 Lott wrote to me:

As to the check issue, I believe that the word "possible" was included, but you have to go on what you remember. In any case, I am sure that I can get many people who will say that I paid them with cash over the years.

As you can see, on Monday afternoon, Lott is "sure" on one issue, but he only "believe[s]" that he included the word "possible" and says that I have to go on what I remember. Just a day later, on Tuesday, Lott is suddenly certain that I am wrong, writing: "Lindgren does not accurately report my conversation with him about how I paid people (in that I said that I possibly paid by check)" and "most of [Lindgren's] . . . next statement is correct except the point about the 'possible' use of checks."

In September, I carefully listened to both Lott's words and his demeanor, taking notes on the more important parts of the long conversation. I wrote up those notes (which formed the basis for what Lott told me listed in items 1-12 above), later revising them into my report. I retain a vivid recollection of what Lott told me on many points, including this one. One day, four months after the conversation, Lott sent me an email saying that he "believe [s]" that he said that he qualified his having paid students by check with the word "possible," but appears not to be sure (as he is about other instances of paying cash on other projects). He tells me that I have to go on what I remember. Then the very next day, Lott is certain enough to accuse me of "inaccuracy."

Lott's new assertion about there being only two University of Chicago students involved and the rest of the callers coming from other universities is perhaps the most disturbing of his changes of story. John and I had a long discussion about how best to reach the University of Chicago students who did the calling. When he first mentioned University of Chicago students, I thought he meant law students, but he corrected me, saying that they were all University of Chicago undergraduates, both those he dealt with and the callers. We discussed perhaps looking at a picture book of Chicago students (which he was willing to do, though he didn't sound optimistic about his being able to recognize the students). He said that he "mostly" dealt with one or two guys who were seniors in 1997, but that some of the callers, who were also Chicago students, might have been juniors. Indeed, when I suggested emailing just the 1997 senior class, Lott said that although he was pretty sure the ones he dealt with were seniors, some of the callers might have been University of Chicago juniors at the time, so I said I would try to email that class as well. We talked about how to contact the people who did the calling and at all times he talked about contacting University of Chicago students. He never even hinted that there might be callers from any other school. If he had mentioned University of Illinois-Chicago students, we would have discussed how to contact them as well. Lott could not have been clearer that the callers were University of Chicago undergraduates. Now his story has shifted.

Lott goes on to make less serious changes in his story. For example:

Ex. 18

THIS REPORT: "5. Lott had no discussions with any samplers about his sampling design."

LOTT'S RESPONSE: "I had lunch Tom Smith during the fall of 1996. However, while I asked him many questions about surveys, I did not tell him what I was planning on doing because Tom works very closely with gun control organizations."

A related LOTT RESPONSE: "Russell Roberts is someone that I bounced the survey questions off of and he can possibly talk to you about it, though he hasn't kept e-mails from 1997."

One of the online commentators is reassured that Lott now says that he discussed the survey questions at the time with Russell Roberts of Wash. U. But Lott did not say this when I asked him in September. He said that he regularly asked Roberts for advice and that he might or might not have discussed his 1997 survey with Roberts. He didn't remember whether he did or not. I spoke with Russell Roberts yesterday and Roberts said exactly what Lott told me in September--that Roberts regularly discussed matters with Lott, but couldn't remember hearing about the 1997 study and couldn't remember one way or the other whether Lott had discussed it with him. Lott could well have discussed the study, but Roberts didn't remember him doing so.

Also, on January 13, 2003, just the day before Lott first claimed in his response to this report that he discussed the survey questions with Russell Roberts, Lott wrote me the following: "Russell is a friend who I talk to about lots of things. Whether you classify him as an expert is up to you. Hopefully, he will be able to recover an e-mail." This statement is much closer to what Lott said to me in September--then Lott had said that he discusses things with Roberts, but didn't remember whether he had discussed the survey with him.

Lott's recent change of story--that he discussed specifically the survey questions with Roberts--might well be true (Lott's memory might have been jogged), but it is not what Lott said to me in September (or even bothered to mention the day before he changed his story when he reasserted his contacts with Roberts in an email). If Lott had told me in September that he had discussed the survey questions with Roberts, I certainly would have called Roberts right away, as I did yesterday. After all, I was looking for ways to verify the study.

By the way, in Lott's defense I must point out that some online commentators have falsely claimed that Lott never mentioned the study to anyone, but if you read my report carefully above, David Mustard quite strongly confirmed Lott's claim to have discussed the 1997 study with him (and his data loss), but Mustard does not remember when he first heard about it.

As to my statement that "Lott had no discussions with any samplers about his sampling design," I said this because I asked Lott this question point blank and he flatly said "No." He did mention that he talked with Tom Smith, but he said that he did not discuss his sampling design with him. One doesn't just pull a national sample out of one's head. One usually either uses a random digit dialing program or a national sample provided or designed by an expert in survey sampling. A CD-ROM with names on it designed for telemarketing is not the sort of thing academics usually use if they want a representative sample, which is the reason I asked whom he consulted on his sampling design.

Another change in story involved the CD-ROM and the survey questions:

THIS REPORT: 8. "Lott does not remember how he drew his sample from the CD-ROM."

LOTT'S RESPONSE: "Not true. I told Jim that one of the students had a program to randomly sample the telephone numbers by state. My guess is that it was part of the CD, but on that point I can<sup>1</sup>t be sure."

THIS REPORT: "9. Lott does not have a copy of the survey instrument and doesn't remember the wording of the questions, though he was probing defensive uses in more detail than other studies. He ended with a very few demographic questions."

LOTT'S RESPONSE: "It is also not quite correct to say that "doesn<sup>1</sup>t remember the wording of the questions." I told Jim that I don<sup>1</sup>t remember the "exact wording" of the questions, but I gave him the general outline of the questions."

Once again, Lott's memory fails him. I was listening closely to Lott's answers to these questions to see what details he could pull off the top of his head. I asked him how he drew the sample from the CD. He said that he didn't remember, but assured me it was drawn randomly. I remember being disappointed in his answer because I thought that a social scientist would probably remember how he solved this problem of getting a random sample (there are several solutions). Also, I am absolutely positive that he did not mention prestratifying the sample by state, which is a form of proportional sampling, not random sampling. His current claim is inconsistent with his September claim made to me that the sample was drawn randomly from the list of names on the CD (though he didn't remember how he drew that random sample); he seems to be claiming now that he drew the sample proportionally by state then randomly within states. I was paying close attention to any details he mentioned about sampling design and he never mentioned breaking down proportionally by state first. I am not disturbed by the content of his claim so much as that he would think he could just tell me one thing in September (he did not remember how he drew the sample from the CD) and tell people something else in January ("I told Jim that one of the students had a program to randomly sample the telephone numbers by state").

A similar problem obtains for question wording. I asked him directly whether he remembered the wording for any questions. He said "No," not their wording, but he assured me that he was trying to probe defensive uses in more detail than prior studies had and that he ended with a few demographic questions. He gave no details of his questions other than this. The notion that he gave me "the general outline of the questions" beyond what I faithfully reported is just plain false. Drafting questions to probe something you care about might (or might not) be just the kind of thing that would stick in someone's mind, so I was hoping that he could come up with plausible approximate wording off the top of his head. If he had, I certainly would have noted it. He couldn't. I was looking for exculpatory evidence and was disappointed to find not much more than good evidence of his exemplary pattern of sharing data and very good circumstantial evidence that Lott had a major computer crash in 1997.

Ex. 18

#### b. Lott's Attempt to Put Things in Context

In his response to this report, John Lott makes several odd statements other than about our conversation. For example, Lott claims: "I have told people directly (including Otis Duncan) from the beginning that the data were lost." Otis Dudley Duncan, who first raised questions about Lott's 98% figure in early 1999, however, says that he first he learned of any data loss when Lott published his comment in the Sept./Oct. 2000 *Criminologist*. Duncan retained Lott's May 13, 1999 letter to Duncan, which was the first documented time that Lott disclosed that he did the 1997 study. I have not seen a photocopy of it, though Dudley Duncan sent me a full transcript. It is quoted earlier in this report, though I repeat here the relevant portion of that May 13, 1999 letter:

"The information of over 2 million defensive uses and 98 percent is based upon survey evidence that I have put together involving a large nationwide telephone survey conducted over a three month period during 1997. Follow up telephone calls were made to ensure that the questions were answered by those who we attempted to contact. The survey was not as detailed as several other surveys, but it did try to include a couple initial questions to ensure accuracy and screen out any problems and then focus exclusively on defensive gun uses. I plan on repeating the survey again during the next year to year and a half. I will be happy to inform you what the results of that survey are after I have conducted it." Letter from John Lott to Otis Dudley Duncan, dated May 13, 1999.

As you can see, while far from conclusive on the point, Lott's letter is consistent with Duncan's contention that Lott did not disclose that he had lost his data when he first notified Duncan in May 1999 about the 1997 study. Certainly, Lott said nothing about losing the data in this letter, the first documented time that Lott claimed to have done the 1997 study.

Lott also claims that he has "always acknowledged" that the 98% figure is based on small samples.

LOTT'S RESPONSE (Appendix 3 below): "As to so-called technical problems, I am [sic] have always acknowledged that these are small samples, especially when one breaks down the composition of those who use guns defensively. Even the largest of the surveys have few observations in this category."

As pointed out above in the section of my report called "Technical Problems," in a sample of 2,424 respondents and a one-year window, Lott would find only about 25 respondents reporting defensive gun uses, 2% of which (1/2 of a person) answering that they had fired their gun. On the more than four dozen occasions collected by Dudley Duncan and Tim Lambert in which Lott mentioned the 98% figure, I do not see a single instance in which Lott "acknowledged that these are small samples." When Lott says that he has "always acknowledged" this fact, he seems to be in error.

Lott uses another style of argumentation that I find troubling. Lott writes:

As to the attribution of sources, look at the complete context of the quote Lindgren mentions:

Polls by the Los Angeles Times, Gallup and Peter Hart Research Associates show that there are at least 760,000, and possibly as many as 3.6 million, defensive uses of guns per year. In 98 percent of the cases, such polls show, people simply brandish the weapon to stop an attack. -- August 6, 1998, Chicago Tribune and August 14, 1998, Washington Times

References by Lindgren to things like the Linnet Myers piece in the Chicago Tribune to provide evidence that I didn<sup>1</sup>t do a survey or that I have changed my statements over time are simply bizarre. Attached below is an edited down version of the letter that was published by me in the Tribune. Myers used her article to refloat claims such as my Olin Funding, inaccurately reported exactly what the concealed handgun research covered, and claimed that "others haven't confirmed (my) findings." I no longer have the original letter to the editor, but as I recall this is just a partial listing of her inaccurate statements. The Tribune was not willing to run a longer letter, though the letter that they ran was quite long.

Lott says that he is going to give "the complete context" for a statement that I "mention" from the *Chicago Tribune* and *Washington Times*. A fair minded reader would conclude that Lott is actually quoting me, but he isn't. I didn't quote the *Chicago Tribune* version of the statement, but rather quoted the original version in the *Wall Street Journal*:

The year before, in the July 16, 1997 *Wall Street Journal*, Lott appeared to attribute the 98% figure to one or more of three specific survey organizations:

"Other research shows that guns clearly deter criminals. **Polls by the Los Angeles Times, Gallup and Peter Hart Research Associates show** that there are at least 760,000, and possibly as many as 3.6 million, defensive uses of guns per year. In 98% of the cases, **such polls show**, people simply brandish the weapon to stop an attack." John R. Lott Jr., Childproof Gun Locks: Bound to Misfire, *Wall Street Journal*, 7/16/97 Wall St. J. A22

The same language (other than typesetting conventions) appears the following year in two articles by Lott on the same topic for the *Chicago Tribune* and the *Washington Times*. John R. Lott Jr., Prime Suspect: Gun-Lock Proposal Bound to Misfire, 8/6/98 Chi. Trib. 23; John Lott, Commentary: Gun Locks That are Bound to Misfire, 8/14/98 Wash. Times (D.C.) A17.

Instead of responding to the quotation I actually use from his 1997 oped in the *Wall Street Journal*, Lott instead quotes an almost identical version of the same statement that he published in a later Aug. 1998 oped in the *Chicago Tribune*, fails to mention that the words in the *Tribune* are under his byline, and then appears to provide "the complete context" for his own statement by questioning an unrelated May 1999 *Chicago Tribune* story by a reporter. In the guise of providing "context" Lott omits crucial information that is in my presentation above. Lott omits that he first used the language in the *Wall Street Journal* in 1997 and presents the quotation as if these are the words of the *Chicago Tribune* or the *Washington* 

Ex. 18

*Times*. Lott never mentions that these are <u>his words</u>, published in an oped under <u>his</u> <u>byline</u>--a failure that tends to undercut his claim that he is providing "the complete context."

Lott never says why he chose to quote a *Chicago Tribune* version of his statement rather than the earlier *Wall Street Journal* version that I actually quote, nor does he say what his *Tribune* oped has to do with a later *Tribune* story by a reporter. Yet (whatever Lott's intentions) I think a fair minded reader might presume that one somehow gives context for the other--perhaps one might conclude that I am wrong to mention a 1998 statement in the *Chicago Tribune* because you can't trust their 1999 reporting on him. That might appear to be a sound implication if one fails to realize that the 1998 *Tribune* statement he quotes is not the version I quoted (I quoted the 1997 *Wall Street Journal*), that the words he quotes are from his own 1998 oped in the *Tribune* under his own byline (a fact that he neglects to mention), and that he made a close version of the same statement in his own oped in the *Wall Street Journal* the year before (presumably untainted by any supposed *Tribune* bias). Of course, Lott never tells us what part of the context he is providing for his 1998 *Tribune* oped that I mentioned but didn't actually quote.

In the course of this, Lott also criticizes my use of a 1999 Linnet Myers story in the *Chicago Tribune*. I did qualify my use of that story with these words:

"If this newspaper account is accurate (and newspapers often aren't), it is odd that Lott would try to answer the reporter's claims about the Kellermann household study without pointing out that he had done a big household study himself. Although this contextual evidence is less telling, it does tend to fit the pattern that, until Lott replied to Duncan in mid-May 1999, Lott had consistently attributed the 98% figure to several specific survey organizations or to no one, never to his own 1997 study." (This Report, above)

In the course of his criticism of my use of that story, Lott makes a potentially damaging disclosure. Lott reveals that he published a 1999 letter in the *Tribune* complaining about errors in Myers's story. The text of his *Tribune* letter is included in Lott's email response in Appendix 3 below. Myers had written: "Lott didn't examine home protection, but he did study the impact of armed self-defense. . . . Lott didn't study gun use at home, but looked at the impact of laws that allow guns to be carried outdoors." Linnet Myers, Go Ahead Make Her Day With Her Direct Approach And Quiet Confidence, Chicago Lawyer Anne Kimball Gives Gunmakers A Powerful Weapon, *Chicago Tribune*, 5/2/99 Chi. Trib. 12.

In his June 1999 letter to the editor published in the *Tribune*, Lott responded particularly to Myers's sentence in which she claims that Lott didn't study gun use at home, but rather the impact of laws that allow carrying guns outdoors:

"My book analyzed FBI crime statistics for all 3,054 American counties from 1977 to 1994 as well as extensive cross-county information on accidental gun deaths and suicides. This is by far the largest study ever conducted on crime, accidental gun deaths or suicide. I examined not only concealed-handgun laws, but also other guncontrol laws such as state waiting periods, the length of waiting periods, the Brady law, criminal background checks, penalties for using guns in commission of crime and the impact of increasing gun ownership. The only gun laws that produced benefits were those allowing concealed handguns. The evidence also strongly indicates that increased gun ownership on net saves lives." John Lott, Letter, Chicago Tribune, June 20, 1999.

Note that in his letter to the *Tribune*, Lott makes no mention of his 1997 study of households that he claimed to have done, even while responding to a sentence that asserted that Lott didn't study gun use at home. Lott lists seven things he looked at: concealed-carry laws, waiting periods, waiting period length, the Brady law, background checks, extra penalties, and gun ownership rates. In his published letter, he omits to mention his 1997 telephone household study, even though Myers twice says he didn't do a household study. In Lott's defense, Lott says that the letter was cut; the original was longer. But Lott's published letter as it stands includes a long sentence listing the sorts of inquiries Lott did, but fails to mention his 1997 study done at the household level, rather than his other inquiries at the county level.

#### 5. Conclusion

I think it prudent to withhold judgment on the question whether the 1997 study was done until an email inquiry of University of Chicago students has been done and its results are known. I hope that John Lott and the University of Chicago Press will join in encouraging the administration of the University of Chicago to conduct or coordinate the appropriate email inquiry. Further, I think it advisable that Lott examine the University of Chicago undergraduate picture book for the classes of 1997 and 1998, if such a book exists. Perhaps a few names or faces might seem familiar and be worth contacting.

I remain hopeful that University of Chicago undergraduates will come forward with a credible story about hours of phone calling in January 1997. Everyone would be enormously relieved were that to occur. If no one does come forward, Lott has done his career a great disservice this January by changing his story in so many ways. Although most of these changes are small ones, the fact that he would make them at this worst possible time is profoundly disappointing to those of us who would like to think the best of him. As it stands now, unless someone comes forward to verify working on the study--as I still hope occurs-we may never know with any certainty whether the 1997 study was done.

#### James Lindgren

Professor of Law Director, Demography of Diversity Project Northwestern University School of Law 357 East Chicago Avenue Chicago, IL 60611 312-503-8374

Note: In some cases, I relied on secondary sources for quotations, in particular a compilation by Tim Lambert and Dudley Duncan< <u>http://www.cse.unsw.edu.au/~lambert/guns/lottbrandish.html</u>>--this is, after all, only an informal preliminary inquiry. If I have misquoted anything, I would appreciate any corrections.

#### **APPENDICES**:

1. Email of 12/26/02 from John Lott to James Lindgren, commenting on the first draft of the report.

Dear James:

My survey never had the ambition of yielding precise numbers. Rather the main intention was to check the general accuracy of the claim that there were 2.5 million defensive gun uses each year.

1) You are of course right that the sample of people using guns defensively is very small and has an obviously large 95 percent confidence interval associated with it. The estimate provided was a point estimate, nothing more and I have never made any pretense to it being more than that. I am sure that you can provide the confidence intervals for the subgroups estimates.

2) The overwhelming majority of the survey work was done at the beginning of the period over which the survey was done. It has obviously been a while, but my recollection is that the small number of people surveyed after the first four or five weeks (mainly January 1997) did not include any more defensive gun uses.

3) Unfortunately, the documentation and results for the survey was lost. However, concerns about the accuracy of the survey can be addressed through replication. I did another survey over 10 days this past fall and it will be discussed in a book coming out in a couple of months. The results of the survey are very similar to those previously reported. All the documentation and the results will be made available to anyone interested in examining it after the book is released.

4) Just a note on your discussion of the timing of things. The University of Chicago Press is not particularly fast. It took them over nine months to publish the second edition after receiving a finalized manuscript. For them, that was an incredibly fast turn around. I am sure that the Press is happy to confirm these types of time lags for you if you are interested. The time lags are much longer than for other types of publications, but you should consider it when putting together your time line.

5) I am not sure that I understand why things should be weighted by household size since I was asking questions about individual experiences.

Sincerely,

John Lott

Jim,

I emailed quickly when I responded the first time and that is probably some reason for the ambiguity. I did not realize that you were trying to use it for such a complete accounting of the details and timing of the events. Below I try to be more clear about the timing of events.

John and I started working on our paper in the fall of 1995 or so. We worked on it intensively from about Feb. 1996-Sep. 1996. We presented it at the Am Law and Econ meetings in May and then at a couple of Chicago workshops in either May or June of 1996. We finished the JLS proofs in Sep or so and were then essentially done with the article.

Ex. 18 Page 367 As we finished the concealed carry paper John talked about working on other projects related to guns. So the first sentence of my previous response should be more accurately "... after our concealed carry paper had been finished (about Sep 1996)...". Once it was finished he started to work on a number of extensions, including the book. This is about the extent of my knowledge about John's activities and the timing of those activities from the fall of 1996 when we finished our JLS paper through the summer of 1997, when I left Chicago. I did not work with John on any of these other projects because I had to finish my job market paper and send my applications out (most due by 1 Dec 1996). In the rest of Dec 1996 I worked on my job market paper and practiced interviews. In Jan. 1997 I went to the AEA annual meetings and interviewed. I had campus visits through early Feb and signed my contract with Georgia around the second or third week of Feb in 1997. From March to June I finished my dissertation and taught. In July I defended and traveled a lot, and my wife moved down to Georgia. I moved to Georgia in early August.

As to the \_date\_ of John's computer crash, it could have happened in June 1997. My previous response would more accurately be that I \_sent him the data\_ after I was at Georgia. I do not really remember \_when\_ it crashed, only that it did and I sent him the data shortly after I arrived in Georgia. Given that I was gone for large parts of July and that our possessions were being shipped, it is feasible that John's crash could have occurred in June and I sent him the data after I got set up in Georgia.

I hope this more complete documentation is helpful. If you have other questions let me know.

David

David B. Mustard Terry College of Business 528 Brooks Hall University of Georgia Athens, GA 30602

3. Email of 1/14/03 from John Lott to various bloggers, colleagues, and James Lindgren, responding to the second draft of this report:

Tue, 14 Jan 2003 15:51:35 -0500 Subject: Responses

#### Dear Everyone:

Here is a response to some of what has been going on over the web. I have already sent much of this information to people who have already contacted me in person. If Eugene would like to post this on his web site, I must ask that all the e-mail addresses and telephone numbers be removed. If you all don't trust the leg work done by Dan Polsby on this issue, you can nominate someone else to go and do it, but I don't think that it is appropriate for everyone from Lambert on to go and harass these people. I suppose that such an edited copy could be sent to Mark A. R. Kleiman.

Regnery (the publisher of my new book due the middle or end of March) wants me not to release the results from the poll last year. They want me to keep quite about the book until it comes out. As has been reported previously, the survey was done with similar questions in a very similar way to what was done earlier and the results were essentially the same. I am sure that I could arrange it so that interested parties could question the person who keep the survey results as they came in to confirm that we only got one person who said that they had actually fired a gun.

Here are some of the things that I have done to try to establish a record of events. 1) My wife contacted the bank that we had in Chicago and tried to get copies of bank statements and checks from the period of time. Unfortunately, the bank does not keep copies of statements or checks longer than five years. (If you would like to verify, we talked to Yvonne Macias in the book keeping department at University National Bank, [phone number omitted].) Lindgren does not accurately report my conversation with him about how I paid people (in

Ex. 18 Page 368

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that I said that I possibly paid by check), but this information makes that point irrelevant. 2) I asked Sam Peltzman last year about whether the Alumni Association has the e-mail of past students. Sam, who seems to know virtually everything that is going on at the University, told me that they have the e-mail addresses for at most 10 percent of the former students. 3) I had a former alumni and several time co-author, John Whitley, placed in an ad in the Alumni magazine in the December issue to track down the students. I don't know if the ad has appeared but thus far I have gotten no response.

I have given out massive amounts of data to people on the guns and other issues, and I will be happy to do so on the new survey. Data has been given to critics as well as people who have been unwilling to share their own data on other projects. I have given out county, state, and city level crime data to academics at dozens of universities, with data sets ranging from 36MB to over 300MB. I have given out data on multiple victim public shootings as well as safe storage laws. These different data have often been given out before the research is published and sometimes even before it has been accepted for publications. We are not talking about recent events or conversations and there is a question about what is a reasonable time period for people to keep records. There is also a question as to why people have waited so long to ask for this additional information when people have known about the lost data for years.

As to the claims about <sup>2</sup>apparently changing positions,<sup>2</sup> I disagree. I have told people directly (including Otis Duncan) from the beginning that the data were lost. Op-ed pieces and other public statements where I mention these numbers briefly usually do not lend themselves to discussions of the sources of numbers. The fact that David Mustard does not remember exactly when we discussed the survey 6+ years ago does not surprise me given how long ago this was.

Unfortunately, there are many problems with Lindgren<sup>1</sup>s write up. He gives essentially uncritical acceptance of Otis Duncan<sup>1</sup>s discussion of events in 1999. Yet, while Lindgren writes that <sup>3</sup>Otis Dudley Duncan raised questions about the 98% figure . . . after exchanges between Lott and Duncan,<sup>2</sup> Duncan<sup>1</sup>s write-up in the Criminologist news letter failed to mention any such possible discussions. In fact his newsletter piece leaves the opposite impression as he endlessly speculates about what I may have meant about certain statements. My response in the Criminologist also discussed other incorrect claims by Duncan.

As to the attribution of sources, look at the complete context of the quote Lindgren mentions:

Polls by the Los Angeles Times, Gallup and Peter Hart Research Associates show that there are at least 760,000, and possibly as many as 3.6 million, defensive uses of guns per year. In 98 percent of the cases, such polls show, people simply brandish the weapon to stop an attack. -- August 6, 1998, Chicago Tribune and August 14, 1998, Washington Times

References by Lindgren to things like the Linnet Myers piece in the Chicago Tribune to provide evidence that I didn<sup>1</sup>t do a survey or that I have changed my statements over time are simply bizarre. Attached below is an edited down version of the letter that was published by me in the Tribune. Myers used her article to refloat claims such as my Olin Funding, inaccurately reported exactly what the concealed handgun research covered, and claimed that "others haven't confirmed (my) findings." I no longer have the original letter to the editor, but as I recall this is just a partial listing of her inaccurate statements. The Tribune was not willing to run a longer letter, though the letter that they ran was quite long.

As to so-called technical problems, I am have always acknowledged that these are small samples, especially when one breaks down the composition of those who use guns defensively. Even the largest of the surveys have few observations in this category. The attached e-mail that I sent to Glenn Reynolds goes into this more in depth.

<sup>3</sup>No direct evidence of survey<sup>2</sup> discussing Lindgren<sup>1</sup>s point-by-point discussion of our conversation

1) <sup>3</sup>No funding for the project<sup>2</sup>

I regularly have paid for research myself. Sometimes large amounts of money have been spent, but it is not uncommon for me to spend several

Ex. 18 Page 369

https://web.archive.org/web/20130304061928/http:/www.cse.unsw.edu.au/~lambert/guns/li... 1/2/2020

thousand dollars. On the paper on multiple victim public shootings, I know that one payment that I made to Kevin, a research assistant to Landes and Posner, was \$750. I paid for the special issue of the JLE in 1999 on sentencing myself, and the special issue and part of the conference cost me around \$30,000. I have not applied for funds from outside sources over the years.

#### 2) <sup>3</sup>No financial employee records<sup>2</sup>

This is not unrelated to the first point. Incidentally, I told Jim that there were <sup>3</sup>two<sup>2</sup> Chicago students. Those students had also gotten others that they knew from other campuses from places such as I think the University of Illinois at Chicago circle (but I am not sure that I remember this accurately).

3) <sup>3</sup>calling was done by the undergraduates from their own phones.<sup>2</sup> most of this next statement is correct except the point about the <sup>3</sup>possible<sup>2</sup> use of checks. But as noted earlier this point is irrelevant in terms of evidence.

#### 4) 3does not remember names2

I have had 12 interns and RAs just since I arrived at AEI. This excludes people whose only work was on the survey. I am horrible at names and I couldn<sup>1</sup>t even give you the names for all of these folks let alone people who did something six years ago. All my names and addresses for everything were on my computer when the hard disk crashed.

#### 5) <sup>3</sup>no discussions with any samplers<sup>2</sup>

I had lunch Tom Smith during the fall of 1996. However, while I asked him many questions about surveys, I did not tell him what I was planning on doing because Tom works very closely with gun control organizations.

#### 6) weighting the sample

I did not weight the sample by household size but used the state level age, race, and sex data that I had used in the rest of my book. There where 36 categories by state. Lindgren hypotheses why you can get such small weights for some people and I think that this fine of a breakdown easily explains it. I don't remember who answered what after all these years, but suppose someone who fired a gun was a elderly black in Utah or Vermont.

7) <sup>3</sup>commercially available CD-ROM with names on it. He does not remember where he got it from.<sup>2</sup>

It is true that I don<sup>1</sup>t have the original CD-ROM. I have a telephone number CD from the end of 1997, but it is not the one that we used. I only picked up the other one on the off chance that I was going to have the time and resources to redo the lost data. The CD did have the features that the earlier one had and was not very useable. I was so rapped up in trying to replace my lost data on so many other projects that I had no thought of going back to what I regarded as a minor project. I had revise and resubmits at the JPE and other journals that had much greater importance and the data for the book had to be replaced.

8) <sup>3</sup>Lott does not remember how he drew his sample from the CD-ROM<sup>2</sup>

Not true. I told Jim that one of the students had a program to randomly sample the telephone numbers by state. My guess is that it was part of the CD, but on that point I can<sup>1</sup>t be sure.

9) <sup>3</sup>doesn<sup>1</sup>t remember the wording of the questions.<sup>2</sup>

It is also not quite correct to say that <sup>3</sup>doesn<sup>1</sup>t remember the wording of the questions.<sup>2</sup> I told Jim that I don<sup>1</sup>t remember the <sup>3</sup>exact wording<sup>2</sup> of the questions, but I gave him the general outline of the questions.

10) more on weighting

See point 6 above.

11) <sup>3</sup>A chapter he had not yet written<sup>2</sup>

This is not correct. What I had done is write up the section, but I only had a computer file of it. When the hard disk crashed, I only had a hard copy of the book and I had to spend considerable time scanning in the book and correcting the new file. I was unable to replace the lost polling section that I had recently added. I didn't think that it was worthwhile relying solely on memory for different things and I had too much else to do to concern myself with something that wasn't central to the book.

12) <sup>3</sup>did not retain any of the tally sheets<sup>2</sup>

I have looked through some things but I haven<sup>1</sup>t found anything. As Lindgren correctly notes, I have moved three times in the last six years.

13) Sheets versus entry of data into computers

Lindgren has the <sup>3</sup>impression<sup>2</sup> that the students entered the data on sheets. I do not directly recall this part of our conversation, but I would have said that both were done.

I sent Lindgren two e-mails on December 26th. Just so no one accuses me of adding new things in now, one of my e-mails to Lindgren noted: "I did not take the time to correct or respond to all the issues raised, but I wanted to mention a few points." Recent e-mails to Lindgren have also already responded to some of these points beyond the e-mail that he apparently posted.

I have not participated in the firearms discussion group nor in the apparent online newsgroup discussions, but what I have done is respond to e-mails. (The one exception are those from Lambert whose e-mail address was placed on my blocked list.) If you all have questions, I will be happy to discuss them, but I am not going be involved in these online groups. My response to Glenn below goes through some of the history of what I heard on this and when I heard it. The bottom line is that you all should not assume that everyone participates in these discussions.

#### Appendix

Chicago Tribune June 20, 1999 Sunday, CHICAGOLAND FINAL EDITION

SECTION: MAGAZINE; Pg. 4; ZONE: C; LETTERS TO THE EDITOR.

LENGTH: 684 words

HEADLINE: GUNS AND CRIME

BODY:

The article accompanying "Anne, Get Your Gun" (May 2), discussing my book "More Guns, Less Crime" (University of Chicago Press, 1998), made several inaccurate claims.

Despite the claims in the article, my research looked at much more than just the "impact of laws that allow guns to be carried outdoors." My book analyzed FBI crime statistics for all 3,054 American counties from 1977 to 1994 as well as extensive cross-county information on accidental gun deaths and suicides. This is by far the largest study ever conducted on crime, accidental gun deaths or suicide. I examined not only concealed-handgun laws, but also other gun-control laws such as state waiting periods, the length of waiting periods, the Brady law, criminal background checks, penalties for using guns in commission of crime and the impact of increasing gun ownership. The only gun laws that produced benefits were those allowing concealed handguns. The evidence also strongly indicates that increased gun ownership on net saves lives.

More disappointing were inaccurate references to the funding of my research. The claims previously floated by gun-control groups like Handgun Control were found by the Tribune's own Steve Chapman to be false (Aug. 15, 1996). Chapman pointed out that not only was the Olin Foundation "independent" of the ties the Sunday Magazine article discussed, but also that the "foundation didn't (1) choose Lott as a fellow, (2) give him money or (3) approve his topic."

The article's claim that "others haven't confirmed (my) findings" is bizarre. To date, I have made the data available to academics at 37 universities, from Harvard to Berkeley. Everyone who has tried has been able to replicate my findings, and only three have written pieces critical of my general approach. Although the vast majority of researchers concur that concealed weapons deter crime, not even those three critics have argued that more guns cost lives or increase crime.

John R. Lott Jr., University of Chicago

Editor's note: Reporter Linnet Myers responds:

Various researchers have praised John Lott's thorough research, although some disagree with his results, which indicate that crime drops when laws allow citizens to carry concealed guns. Whether his findings have been "confirmed" may depend on exactly what that means.

Three professors interviewed at separate universities said Lott's data and computations were mathematically correct. But because each professor's analysis differed, one didn't find significant drops in crime while another found more dramatic decreases than Lott did. The third said Lott's results have been "confirmed in the sense that they've been replicated."

Yet the findings remain hotly debated. Some researchers, as well as many gun-control advocates, flat-out reject them. Others say only time will tell. In the midst of this controversy, my statement that Lott's results haven't been "confirmed" was one of caution. And the article did not suggest that he hasn't studied anything beyond those laws.

Most researchers interviewed did agree on one point: Despite the fears of gun-control groups, there is currently little evidence that the laws have caused any rise in crime.

Lastly, though I don't think the reference to Lott's funding was "inaccurate," it may have been unclear. The original version of my article quoted a researcher who said that while Lott's fellowship had a link to an ammunition company, "Lott's findings weren't swayed by the somewhat remote connection." The researcher said that though gun-control advocates have focused on it, the funding foundation "isn't reputed to be an arm of the gun industry any more than the Rockefeller Foundation is a tool of the oil companies."

Because of limited space, however, the story was cut and that quote never made it into print. I apologize to Mr. Lott for that trim.

Most of an e-mail that I sent recently to Glenn Reynolds (cutting some personal comments at the end)

Dear Glenn:

First, I have responded to people. I responded to the e-mail from you that had been forwarded via Clayton Cramer last year (you did not send it directly to me for some reason). I have responded extensively to Polsby when he wrote me after Christmas and I responded again to Lindgren (twice) when he e-mailed me on December 24th. During the last week, I have also corresponded with Dave Kopel. The data on the original survey was lost and I will go into it later. First, here is a similar survey that I did as well as some comments on it. This survey is NOT for public dissemination as it is for a book that I have that will shortly becoming out. My publisher would be very upset if the results of the survey or the survey itself were released.

Survey questions:

[questions in email omitted here] . . .

Write up by James Knowles of the discussion of the survey:

We had a small army of interns and AEI staff making phone calls. The callers for any given night varied according to who was available/willing to make phone calls. I was here every night supervising from my office at AEI. The survey was conducted over eight nights. Calls were made between 7pm and 9pm local time. Here are the list of callers and their email addresses, I can try to track down phone numbers if need be.

[names and emails in Lott's email omitted here] . . .

... [details in email of random digit dialing procedure used omitted here]

End discussion on survey. ... [notes about Lott's contact info omitted here] ...

and he can possibly talk to you about it, though he hasn't kept e-mails from 1997. . . . [Personal comment about Lott omitted for privacy reasons] I have told people for years about the data being lost.

2) I lost ALL the data for my paper with David in the JLS and for my book. David and I reconstructed the county and state level data from our paper and I got the rest together that was in my book. I have consistently provided the county, state, and city level data as well as the multiple victim shooting data and the safe storage data to people when they have asked. That constitutes almost all the numbers mentioned in the book. Months were spent redoing this data so that it could be given out to people. (Just a note, the critics to whom we had given out the data up to that point were unwilling to return the data to us so we had to put the JLS data together all over again.) If I had the other data, I would have been happy to give it to who ever wanted it.

The survey data could not be replaced by going to things like the UCR or the census data. It could only be replaced by doing another survey. I ended up only briefly mentioning the survey in the past and worked on replacing all the other data that I lost for not just the book but for all the other projects that I was working on. I spent a good portion of the next two years trying to replace data for other projects that I had been working on. I had important papers for the Journal of Political Economy and other journal for which replacing the data was my first priority after replacing the data for the JLS and the book. Thinking about the survey was well down on my list. Its importance was not particularly high given that I had only one sentence on the issue in my book and have never written the survey into a research paper because the data was lost.

3) This is something that was done six years ago. During the intervening time I have moved three times. Usually I pay students in cash. When I am at universities I don't apply for grants and the money is mine so there is no record of universities paying for the students. My records of the students names and contact information were lost. You can get an idea of how much total time was involved from the survey discussed above. An ad was taken out in the fall in the University of Chicago Alumni magazine

> Ex. 18 Page 373

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<sup>1)</sup> I have lots of people who can say that I lost my hard disk in July of 1997: for example, David Mustard ([phone omitted]), Geoff Huck (the editor at the University of Chicago Press ([phone omitted]), John Whitley, William Landes. If you want to you can feel free to contact especially David and John. Russell Roberts is someone that I bounced the survey questions off of

to try to contact the two University of Chicago students who organized some other people from different places to work on it. I have gone through well over 12 RAs and interns independent of those used on the survey since I got to AEI and I can't say that I remember most of their names (I am really horrible at names).

4) Issues about the significance difference in results. Given the very small sample sizes, the differences in results are not statistically significant and are really trivial. About one percent of people in a survey note that they have used a gun defensively. Whether one is talking about 2

percent or 20 percent simply brandishing a gun, you are talking about the different of 2 percent of 1 percent or 20 percent of 1 percent. Depending upon who answers the questions and what weighted group they are in, you are literally talking about the answers of a few people out of 1,000 that is the difference between these two results.

5) If you have doubts about anything in specific, you should ask me about it. Last year I worked extremely hard. I was the only affirmative numbers expert for the Senator Mitch McConnell's side in the campaign finance case. Once that was done, I had to deal with something that Ian Ayres and John Donohue wrote attacking my work and finishing things for my book. Only in the last couple of weeks have I gotten a breath on things, but I have responded when people e-mailed me. I am not a member of the firearms discussion groups and I have not been following them. I read your site once in a while just to keep up with the news (and because of that I have sent you some money from time to time e.g., just on Saturday), but otherwise I have been too busy to follow a lot of things. (When I recently accidentally sent you an e-mail it was at the end of one of many all nighters.)

So as to state things clearly, the bottom line is that I have provided data on county, state, and city level crime data when I have been asked as well as the data on the multiple victim public shootings and the data on safe storage laws, even before the papers have been published. We are talking about one number in one sentence in the book, a claim that I have also used in some op-eds and in some talks. I know of no one who has given out his data as quickly and consistently before even papers are published as I have and over the years. Finally, let me note the most important bottom line: the survey that was done last fall produced very similar results. The earlier results were replicated. This survey was done more recently and I will release the data when the book is released in March. To keep the publisher happy, I will not release it before hand unless you can give me a

very good reason.

[cut [by Lott]] There are errors in Lindgren's write up (at least the one that he sent me) and if you have specific questions about it, I will respond. But instead of claiming that I haven't responded to people you should talk to people like Dan Polsby who raised claims voiced to him by others that I had fabricated this second survey. He spent a good deal of time verifying that the survey did indeed take place. Polsby can be reached at [phone omitted]. I am sure that he would be happy to talk to you.

Best.

John



# Exhibit 19

Ex.19 Page 375

Menu

K LOOKS DIAPHANOUSLY SOLID

### CHATTERBOX The Bellesiles of the Right?

Another firearms scholar whose dog ate his data.

By TIMOTHY NOAH FEB 03, 2003 • 7:19 PM

#### Y TWEET



#### COMMENT

What is it about statistics and guns? Last year, Michael Bellesiles, a historian at Emory College, came under criticism for his Bancroft Prizewinning book, *Arming America: The Origins of a National Gun Culture*, which argued that gun ownership was far less common during the 18<sup>th</sup> and 19<sup>th</sup> century than is generally supposed. His analysis, which was obviously pleasing to proponents of gun control, was drawn from probate records. But Bellesiles was unable to produce all of his data, owing, he said, to a flood in his office. After a committee of three scholars examined Bellesiles' research, they concluded that "his scholarly integrity is seriously in question." Bellesiles resigned from Emory in disgrace.

Now one of Bellesiles' <u>principal critics</u>, a Northwestern law professor named James Lindgren, has turned his skeptical attention to a scholar who is Bellesiles' ideological opposite: John R. Lott, author of <u>More</u> <u>Guns, Less Crime</u>. Once again, the issue is the disappearance of supporting data.

Lott's *More Guns, Less Crime* is the bible of the national movement to persuade state legislatures to pass so-called <u>"concealed carry" laws,</u> which permit citizens to carry concealed firearms. The book's thesis is that populations with greater access to firearms are better able to deter crime. Some scholars have <u>quarreled with Lott's interpretation</u>, but this controversy is about underlying data. Lindgren and others want to know where Lott got the evidence to support the following sentence, which appears on Page 3 of Lott's book: "98 percent of the time that people use guns defensively, they merely have to brandish a weapon to break off an attack."

Initially, Lott sourced the 98 percent figure to "national surveys." That's how the first edition of *More Guns, Less Crime* put it. In an <u>August 1998</u> <u>op-ed</u> for the *Chicago Tribune*, Lott appeared to cite three specific surveys:

Polls by the *Los Angeles Times*, Gallup and Peter Hart Research Associates show that there are at least 760,000, and possibly as many as 3.6 million, defensive uses of guns per year. In 98percent of the cases, such polls show, people simply brandish the weapon to stop an attack.

But polls by the *Los Angeles Times*, Gallup, and Peter Hart show no such thing.

Alternatively, Lott would sometimes attribute the 98 percent figure to Gary Kleck, a criminologist at Florida State University. In a <u>February</u> <u>2000 op-ed</u> for Colorado's Independence Institute, Lott wrote: "Kleck's study of defensive gun uses found that ninety-eight percent of the time simply brandishing the weapon is sufficient to stop an attack." But Kleck's research shows no such thing.

Eventually, Lott settled on yet another source for the 98 percent figure: "a national survey that I conducted," as Lott put it in a second edition of *More Guns, Less Crime*. When asked about the survey, Lott now says it was done by telephone in 1997 and that the data was lost a few months later in a computer crash. Lott's conflicting explanations naturally attracted suspicion, first from Otis Dudley Duncan, a retired sociologist at the University of California, San Diego, who wrote an article on the matter for the *Criminologist*, and eventually from Lindgren, the Bellesiles gumshoe, who has been posting his findings online. (Chatterbox is indebted to Tim Lambert, a computer scientist and gun-control advocate at the University of New South Wales, for compiling various documents relating to the Lott case.) When Chatterbox asked Lott about the serial attributions to "national surveys," to three specific polls, and to Kleck, Lott conceded, "A lot of those discussions could have been written more clearly." He said that in the computer crash, he lost all his data for the book and had to reconstruct it, but that he couldn't reconstruct the survey. Lott has been able to produce witnesses who remember him talking about this obviously traumatic event soon after it occurred. But none of these people specifically remember him talking about losing data for a survey he'd conducted. Nor has Lott been able to produce the names of the college students he says conducted the phone surveys in Chicago, where Lott was teaching at the time. (Lott is now at Washington's American Enterprise Institute.)

The only compelling evidence that the 1997 survey ever took place is the testimony of David M. Gross, a Minnesotan who contacted Lott after the controversy spread to various Weblogs. (To date, the only mainstream news organization that's covered the data dispute is the *Washington Times*, whose Robert Stacy McCain had <u>a piece</u> about the Lott affair on Jan. 23. The Feb. 1 *Washington Post* examined a bizarre side issue, but we're getting ahead of ourselves.) Gross told Chatterbox, "I have come to the conclusion that I in fact did" participate in the study, "based on some of the details of my recollection." What Gross recalls is that in January 1999—a year before questions were first raised about Lott's data—he attended a talk Lott gave at the Minneapolis Athletic Club. (Gross can pinpoint the date, he says, because he bought a tape.) After Lott's remarks, Gross walked up to Lott and told him he'd figured out, while listening to Lott discuss the 1997 survey, that he, Gross, had participated in that survey. Both the timing and the content, as described by Lott, match what Gross remembers about the survey, which is the only gun poll he recalls ever participating in.

Gross recited his story to Chatterbox with a precision that seemed to reflect both his natural temperament and his professional training as a lawyer. It didn't *sound* as though Gross could be getting this wrong. But, as the bloggers <u>Atrios</u> and <u>Mark Kleiman</u> have noted, Gross is a pro-gun activist—indeed, a former national board member of the National Rifle Association. Gross was also the founding director of the Minnesota Gun Owners Civil Rights Alliance, and as an attorney he now represents that group in a legal challenge stemming from its appropriation of the name, Citizens for a Safer Minnesota, which previously belonged to a guncontrol group that carelessly let lapse its registration with the Minnesota secretary of state. It's odd (though not impossible) that such a bareknuckled advocate would turn up in a randomly generated survey. Even if the survey did take place, why should we believe the stated finding? Lott says he repeated the 1997 survey last year. He can't reveal the results, he says, because the publisher of his next book won't let him. But he has shown the results to Daniel Polsby, a law professor at George Mason. Polsby reports that while he won't endorse the methodology—"I have questions about it"—the results were "approximately the same." (This time the percentage was slightly lower than 98 percent—by how much, Polsby won't say.) "John is a very intense man, he rubs a lot of people the wrong way," Polsby told Chatterbox. But "faking something like this would not be John's style."

One type of faking that apparently *is* Lott's style is the assumption of a fictional identity on the Internet. (This is the piece of the story that the *Washington Post*'s Richard Morin <u>zeroed in on</u>.) Lott has posted Web comments defending his work using a "<u>sock puppet</u>" named Mary Rosh. He was busted by Julian Sanchez, a blogger who works at the Cato

Institute, a libertarian think tank in Washington. One posting that Lott has admitted to posting read as follows:

I had [Lott] for a PhD level empirical methods class when he taught at the Wharton School at the University of Pennsylvania back in the early 1990s, well before he gained national attention, and I have to say that he was the best professor that I ever had. You wouldn't know that he was a 'rightwing' ideologue from the class. ... There were a group of us students who would try to take any class that he taught. Lott finally had to tell us that it was best for us to try and take classes from other professors more to be exposed to other ways of teaching graduate material.

Mary Rosh also gave *More Guns, Less Crime* a <u>rave review</u> on Amazon.com:

Lott writes very well. He explains things in an understandable commonsense way. I have loaned out my copy a dozen times and while it may have taken some effort to get people started on the book, once they read it no one was disappointed. If you want an emotional book, this is not the book for you.

Lott says he didn't post the Amazon review; his 16-year-old son did. The "Mary Rosh" e-mail address belongs to his four sons, Lott told Chatterbox—it's derived from their first names—and Lott has used it now and then so that, if he fails to answer a response, it won't be interpreted as "me conceding things." Lott now says the deception was "wrong."

We know Lott invented an online persona. Did he invent the 98 percent figure? Did he invent the survey it purportedly came from? We don't know. "People who are on the gun-control side of the debate," says Polsby, "are hurting on account of Bellesiles. And they want a scalp. John, for one reason or another, is a beautiful scalp to get. For one thing, he's not a terribly good witness on his own behalf." *Is* Lott the Bellesiles of the right? Chatterbox is not yet prepared to say.

[*Clarification, Feb. 5*: In the Minneapolis lecture that Gross attended, Lott recited the "98 percent" statistic, but did not specifically attribute it to a study that *he himself* had conducted. Gross simply deduced that he, Gross, had participated in whatever study produced the 98 percent figure. Also, although the anti-gun-control group, Citizens for a Safer Minnesota, is part of the Minnesota Gun Owners Civil Rights Alliance, MGOCRA is not technically a party to the lawsuit about whether CSM can keep its name.]

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## Dear Care and Feeding: How Should I Respond to Sexist Co-Workers Who Think I Should Be a Stay-at-Home Mom?

# Exhibit 20


HOME & MAGAZINE & THE FIX

# INSIDE THE MIND OF AMERICA'S FAVORITE GUN RESEARCHER

John Lott is a one-man pro-gun research machine whose work has been cited nearly 200 times by the National Rifle Association. The problem? Many of his peers have major misgivings about his methods.

PETER MOSKOWITZ · UPDATED:SEP 23, 2018 · ORIGINAL:JUN 1, 2017

I returned from Orlando depressed. I was there reporting a few days after a man had <u>opened fire in a crowded nightclub</u> with a semi-automatic weapon, killing 49 and wounding dozens of others. <u>Mass shootings</u> have been a common news item in the United States over the last few years, but this one seemed different, both in its scale and in the response (or lack thereof) that followed.

After Columbine (two high school seniors shot and killed 12 students and one teacher), Sandy Hook (one man shot and killed 20 six- and seven-year-olds and six adults), Fort Hood (an Army major shot and killed 13 people and injured 30 more), the Navy Yard in Washington, D.C. (a man shot and killed 12 at a naval base), Aurora (a man shot and killed 12 and injured 70 in a movie theater), and Charleston (a white supremacist shot and killed nine black churchgoers), there was at least debate about what to do. Background checks? End the sale of assault rifles? Create an interstate tracking system?

A few days after Orlando, former President Barack Obama, speaking on the block-long grass field in the downtown district where thousands of mourners had left notes to those who died at Pulse nightclub, implored lawmakers to "do the right thing"—to change their minds about background checks, to consider legislation, to at least create a watch list for suspected terrorists who want to purchase guns. It was a milquetoast speech. And nothing followed it. There were no new laws; the push for background checks failed. The usual debate that had raged in the U.S. after mass shootings in the past did not happen after Orlando. Calls for specific action had turned into pleas to at least acknowledge there was a problem. It was the deadliest mass shooting in modern U.S. history, and yet the debate had gone so far backwards that gun-control advocates were no longer advocating for control, but for some debate about control.

In Orlando, I'd attended a gun show where, outside, an LED sign had been set up to scroll the hashtag **#PrayForOrlando**, and, inside, everyone told me that guns did not kill people. Even at the memorial, the same one Obama spoke at, yards away from where family members of the deceased were gathering, crying, adding to a quickly growing pile of flowers and homemade signs with their letters streaked from a near-constant drizzle, people told me that this was not about guns, that actually guns were good, that really the solution was more guns—guns at home, guns on the street, guns at clubs (or at least security guards with guns). There was relatively little gun debate in Orlando after Pulse, virtually no gun debate in Congress. There was just a general feeling that guns are good, and a feeling that, if you believe that, you're right.

A man named John Lott can be assigned a degree of responsibility.

Lott is a one-man pro-gun research machine. He's published four books on the subject. He speaks at countless conferences and colleges. He writes dozens of op-eds each year, and is cited in thousands of news stories. If you know a statistic or a fact-based argument about how guns save lives, it's likely, whether you know it or not, you're citing some of Lott's work. Lott is not affiliated with any university, and hasn't been for years. Little of his gun research has been published in peer-reviewed journals. And yet he is, without a doubt, the most influential pro-gun researcher in the U.S.

Ex. 20

I will not be able to debunk Lott here and now. I am not an academic. I—and 99 percent of people, I'd venture to guess—am not as good with statistics as Lott. What I can tell you is what the people who do have that skillset say. There are many people who agree with Lott—especially in the fields of criminology and economics. But it appears the majority of researchers who work in the field say Lott's wrong: that his analyses are misleading, that they skew data to favor certain outcomes, and that his research methods don't stand up to scrutiny. If they did, his critics say, Lott would still be published in academic journals, or doing his research out of a university instead of a non-profit called the Crime Prevention Research Center.

After a bit of coaxing, Lott agreed to meet me at a debate on campus-carry laws he'd be participating in at Baylor University in Waco, Texas. Lott arrived at the debate, his hair wispy, his suit loose-fitting, his shirt tucked in only halfway. The mostly conservative students who filed into the auditorium, dressed in well-fitting skirts and heels and khakis and boat shoes, looked like the best of conservative America—professional, jovial, past the more juvenile aspects of college. But Lott's look just added to his authenticity. He exudes professorial vibes. He does not look like a snake-oil salesman. He is, it seems, a true believer.

The school's chapter of the <u>Federalist Society</u>, a national conservative group, had invited him here. According to Lott and another organizer, it took months to set the meeting up—first there was mysteriously no classroom available in the law school, where the Federalist Society usually held events, and then no one would debate Lott. Lott told me he'd reached out to around 20 people, including professors at Baylor, with no luck. The backdrop was this: Texas had recently passed a law mandating that public universities allow students to carry weapons on campus. It sparked protests at public schools, and pushed leaders of private schools to come down on a side of the gun debate. Ken Starr, Baylor's president at the time, banned weapons from campus, a move a vocal minority of students disagreed with. So it made sense professors did not want to broach the issue. Instead, Lott found Andrea Brauer, the executive director of Texas Gun Sense, a small non-profit that pushes for small changes in gun laws in Texas.

The debate went well for Lott—he arrived prepared with a PowerPoint chock full of data, most of which was based on his own research from his seminal book <u>More Guns</u>,

Less Crime, published in 1998. He hit on all the pro-gun tropes, and backed them with numbers—terrorists pick gun-free zones, he said; public shootings happen more frequently in Europe; good guys with guns stop bad ones. Brauer couldn't compete. She had talking points, but she was not a researcher, she could not debunk him on technical grounds, and the audience was already in Lott's pocket. She stumbled over her words. She let Lott speak over her and could only answer many of his retorts by saying he was wrong, but that she did not have the data to prove it. "Aren't you making a feelings-based argument," one student asked her. "That's good for you, throwing your opinion out there," another student said after the debate.

"I know his research is flawed," Brauer told me afterwards. "A lot has been discredited. But it's hard to argue with him."

Lott's main assertion is that states that pass right-to-carry laws (laws that allow you to carry a concealed handgun) have significantly lowered their crime rates. Lott first made the claim in a 1997 study that he conducted while at the University of Chicago, along with <u>David Mustard</u>, then a graduate student at the University of Chicago and now a respected economist at the University of Georgia. Lott expanded on the study in his *More Guns* book, a herculean undertaking: Lott, with a few assistants, collected 15 years' worth of gun and violence data from 3,054 U.S. counties. It was, and still is, one of the grandest studies of gun violence ever conducted. Lott found that, were all 50 states to pass concealed-carry laws, more than 1,500 murders, 60,000 aggravated assaults, and 4,000 rapes could be avoided per year. The influential criminologist Gary Kleck told <u>Mother Jones</u> that Lott's early work "was light-years ahead of anybody else at the time."

Even those prone to support gun control agreed it was an impressive body of work. And for those who agree with Lott, *More Guns* remains one of the most important works in the field to date.

"A lot of his research is some of the most highly cited research on firearms," says Mustard, who hasn't conducted research with Lott since their original project, though they've collaborated in other ways. "It's clearly the most highly cited by academics and it's also incredibly frequently cited by politicians."

Ex. 20

Lott's work quickly became a favorite of pro-gun legislators, academics, and policy wonks, including at the National Rifle Association (the group's Institute for Legislative Action has cited his work 175 times). And Lott's research attracted media attention. According to one count, his work has been cited no fewer than 1,100 times in newspapers. After More Guns, Less Crime was published, Lott rose to be the most prominent gun researcher in America by far—appearing on television shows dozens of times a year, constantly touring college campuses, cited by state and federal lawmakers in gun-policy debates—all while being, according to many of his colleagues, wrong about his research. But that just shows the bias of academia, according to Lott.

"In a field such as public health, I suspect a school like Harvard University may not even have a single Republican," Lott wrote me in an email (he insisted on email after our initial in-person interview). "No matter how well done my research is there is no way someone who wrote the types of studies that I do would ever get hired there. The entire field is like that."

But researchers told me their qualms with Lott originate not in the field of politics, but basic scientific method. Several pointed out that concealed-carry laws tend to be passed after a spike in violent crime, and that many of the states Lott researched for his 1997 paper passed laws right after the crack epidemic. But, as researchers have pointed out, most concealed-carry permits are issued to white men outside of urban areas, so Lott was measuring two separate trends—an increase in violent crime associated with the crack epidemic in urban areas, and an increase in concealed-carry permits in rural areas—and then concluding they influenced each other. Lott did discuss the crack epidemic in a footnote in 1997, and the rural-urban issue in later research that appeared in the influential *American Economic Review*, but he continued defending his position long after the scholarly consensus rejected it.

"He's able to find things in data that most people don't," says <u>Daniel Webster</u>, a professor of health policy at the Johns Hopkins Bloomberg School of Public Health and the director of the school's <u>Center for Gun Policy and Research</u>, noting that Lott hasn't been peer-reviewed for his gun research in over a decade.

Other researchers have found it problematic that Lott's landmark 1997 paper depended on the state of Florida and incorporated crime data he collected from police departments on his own, as opposed to relying solely on data from the Federal Bureau of Investigation. If you take out Florida, Lott's claimed reductions in crime become much less dramatic.

After Sandy Hook, Evan DeFilippis and Devin Hughes, two young, independent gun researchers, noticed the usual slew of pro-gun arguments on Facebook from conservative family members and friends. They realized that nearly all who used statistics in an attempt to prove that guns were safe relied solely on Lott's work.

As undergraduates at the University of Oklahoma, DeFilippis and Hughes began looking carefully into Lott's research. They found that his models only worked under strict and often unrealistic conditions. Adding new variables often produced results that didn't match real-world observations—a conclusion other researchers have also reached.

"To debunk him, you have to dive down this rabbit hole [of data]," Hughes says. "People just don't want to go down that rabbit hole, and they don't realize how important he is to the entire pro-gun narrative."

In the four years immediately following the conclusion of Lott's 1997 study, 14 more jurisdictions passed concealed-carry laws. <u>Ian Ayres</u>, a lawyer and economist at Yale Law School, and Stanford Law School professor <u>John Donohue</u>, both of whom have published extensively on gun control, jointly wrote a 106-page takedown of Lott's work in 2002. They decided to add those 14 jurisdictions to Lott's models, and found that, in every jurisdiction, all categories of crime increased after concealed-carry laws were passed.

David Hemenway, a professor of health policy at Harvard, found that, if you increase the unemployment rates in Lott's models, homicides drop dramatically—the opposite of what research on gun violence and unemployment shows. And if you reduce the number of black women age 40 to 49 in Lott's models by 1 percent, homicides drop by 59 percent and rapes increase by 74 percent. Hemenway argued that such massive effects from such a tiny change in just one demographic suggest Lott's model is "no good." Lott, as he usually does when criticized, responded with a litany of blog posts, op-eds, and media appearances.

Ex. 20

The failure of variable-testing Hemenway identified in Lott's work is among the clearest signs that his methods are flawed. If research is strong, it should stand up to being tested and picked apart by other researchers, which is what the peer-review process is for. Lott's recent research hasn't gotten the same scrutiny most scientific researchers do, because if it did, other researchers told me, it would be torn apart.

"He is, perhaps, perceived by some [to have] the same credibility as myself or other people who have published tons of stuff in scientific, peer-reviewed literature and have been through rigorous academic vetting," Webster says. "He's just some guy who anointed himself as the pro-gun researcher."

"What I dislike is he says all these things that are clearly wrong, and his science is not very good at all," Hemenway says. John Donohue says Lott obfuscates with bad data, and won't admit when he's wrong. "Lott's work was mainstreamed very quickly because it did appeal to a powerful economic interest, and political interests, and so the work got more prominence more rapidly than it probably deserved."

"What I've found over the years, at least for me, is the best way to move forward is to kind of pretend he doesn't exist," says <u>Stephen Teret</u>, another professor at the Bloomberg School of Public Health who is familiar with Lott's work.

These researchers hold the majority opinion. Hemenway, with the help of his graduate students, compiled a list of academics in peer-reviewed journals who had published on gun safety within the last four years. Those surveyed came from various fields—criminology, economics, political science, public health, public policy. And there was a clear consensus: 84 percent concluded guns in the home increase the risk of suicide, 64 percent said guns make homes more dangerous in general, and 73 percent said guns are used for crimes more often than for self-defense. Perhaps most damningly for Lott, Hemenway's survey found that only 9 percent of researchers thought that concealed-carry laws reduced gun violence rates.

Lott countered with another survey showing that a smaller majority of researchers from only two fields (criminology and economics) agreed with him. "They only surveyed academics, and only three economists. That's their way of discrediting my research," he said. "They never mention all the published studies that confirm my results. They always want to make it seem like it's only me saying these things." Hemenway and others have disputed the results of Lott's survey too.

When challenged on his research, Lott has, in the past, resorted to odd behavior. He admitted to using an alternate online persona named Mary Rosh, who would defend Lott's articles. "I shouldn't have used it, but I didn't want to get directly involved with my real name because I could not commit large blocks of time to discussions," Lott said once the Rosh debacle was uncovered. Lott has also come under fire for writing an op-ed under the name of a real woman who had a stalker and became a gun advocate after her college would not provide her with adequate protection. Even pro-gun-rights outlets like Townhall and *Reason* have criticized these efforts.

And Lott has never publicly shared the data behind one of his most-cited statistics—that 98 percent of defensive gun use doesn't even require a gun to be fired, just pulled out to scare away the attacker or intruder. When the late sociologist Otis Dudley Duncan, who pioneered the field of human ecology at the University of Chicago, asked Lott for more raw data, Lott said he'd lost it in a hard drive crash. Lott then redid the survey with a sample of about 1,000 people, and found that 13 had used a gun in self-defense. Only one had actually fired the gun—not the largest sample, but even one out of 13 (7 percent) is far from the 2 percent that Lott has touted for most of his career.

For every attack lobbed at him, Lott has hit back with lengthy posts on his blog that attempt to dismantle his opponent's critiques. And in each one he dives deep into statistical analyses that seem designed to confuse more than elucidate. DeFilippis and Hughes call this "security through obscurity." Similarly, Rutgers University sociologist <u>Ted Goertzel</u> has said Lott's work "would never have been taken seriously if it had not been obscured by a maze of equations."

"I have been willing to debate other academics, and I have done so every time that I have been asked to do so," Lott wrote me in an email. "I have [asked people] many times to try to set up debates but it has been very difficult to get other academics to participate."

When DeFilippis debated Lott on a liberal radio show a few years ago, he experienced the deluge-of-data technique firsthand. "You end up getting into a high-level, technical debate, which is not going to persuade the lay audience," DeFilippis says. "You're fighting an uphill battle."

Outside of the Baylor auditorium, Lott told me about his journey to becoming the most prominent and most hated gun researcher: His interest in guns, he said, started when he was an economist at the Wharton School of the University of Pennsylvania. Students asked him about his thoughts on gun control, and so Lott started researching. He wasn't a gun expert then, just an economist. But he noticed that, despite the volume of gun research, there were few well-designed studies with large sample sizes. Around the same time, Lott had become disillusioned with the Clinton administration. He said he had been a Democrat, but found himself starting to align with more-conservative belief systems. He felt that the response to the research he'd started doing on guns encouraged his political transformation. It appeared to Lott that the liberal establishment had gotten everything wrong, and that, in their rush to prove their progressive fantasies, they had ignored the facts.

By the time Lott's first research came out, he was a researcher at the University of Chicago. But his new fascination with guns made him a pariah there. He says he began receiving death threats from gun-control advocates, and so his wife and kids moved back to Pennsylvania so they wouldn't be harmed if one of the threats ever materialized. Then, Lott says, under pressure from a gun-conscious mayor, the university terminated him because of his pro-gun views. (The University of Chicago declined to comment on the specifics of Lott's departure.)

Lott returned to Pennsylvania and eventually started the <u>Crime Prevention Research</u> <u>Center</u>, which is funded through small donations and operates with a limited budget. When he flies around the country giving talks, it's with his own money. His lifestyle does not appear lavish. He seems isolated, and he seems impassioned—doing this of his own volition, making a decent living but not an offensive one. The Crime Prevention Research Center is mostly run out of his house, in the suburbs of Philadelphia. He sleeps little, because he does his research at night.

Seeing Lott slouching in an uncomfortable, shiny lounge chair at Baylor made me wonder why he does this—when so many of his peers say he's wrong, when he's not being given obscene amounts of money for his work, when he's been essentially banished from academia, pushed to self-publishing and creating fake identities to advance his research.

After half an hour of me trying to figure out his motivations, Lott said he'd be late if we talked any longer, so he got up and opened the double doors to the auditorium, where he was introduced by a smiling student in a suit to a round of applause.

# Exhibit 21

Ex.21 Page 390

# Scholar Invents Fan To Answer His Critics

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# Body

Mary Rosh thinks the world of John R. Lott Jr., the controversial American Enterprise Institute scholar whose book "More Guns, Less Crime" caused such a stir a few years ago.

In postings on Web sites in this country and abroad, Rosh has tirelessly defended Lott against his harshest critics. He is a meticulous researcher, she's repeatedly told those who say otherwise. He's not driven by the ideology of the left or the right. Rosh has even summoned memories of the classes she took from Lott a decade ago to illustrate Lott's probity and academic gifts.

"I have to say that he was the best professor I ever had," Rosh gushed in one Internet posting.

Indeed, Mary Rosh and John Lott agree about nearly everything.

Well they should, because Mary Rosh is John Lott -- or at least that's the pseudonym he's used for three years to defend himself against his critics in online debates, Lott acknowledged this week.

"I probably shouldn't have done it -- I know I shouldn't have done it -- but it's hard to think of any big advantage I got except to be able to comment fictitiously," said Lott, an economist who has held senior research positions at the University of Chicago and Yale.

Moreover, the AEI resident scholar acknowledged on Friday that he permitted his 13-year-old son to write an effusive review of "More Guns, Less Crime" and then post it on the Amazon.com Web site. It was signed "Maryrosh."

His son gave the book five stars -- the highest possible rating.

"If you want to learn about what can stop crime or if you want to learn about many of the myths involving crime that endanger people's lives, this is the book to get," the review stated. "It was very interesting reading and Lott writes very well. He explains things in an understandable commonsense way. I have loaned out my copy a dozen times and while it may have taken some effort to get people started on the book, once they read it no one was disappointed."

Lott denied that he was the author of the review, an assertion made on various Web sites that have been tracking the controversy. He said his son wrote it, with some help from his wife. "They told me they had done it. They showed it to me. I wasn't going to tell them not to do it. Should I have?"

Lott's book, which argues that gun ownership deters crime, has been praised by gun advocates and attacked by those who favor gun control.

Lott also is a lesser player in the now-diminishing debate over the 2000 elections. In a study two years ago, Lott reported that the decision by the major television networks to call the Florida election for Al Gore before the polls had closed everywhere in the state led thousands of Republican-leaning voters in the Florida Panhandle not to vote. Other researchers dispute his findings, which have been embraced by conservatives as well as by critics of exit polling.

Lott said that he frequently has used the name "Mary Rosh" to defend himself in online debates. The name is an amalgam of the first two letters of his four sons' first names. In a posting to the Web site maintained by Tim Lambert, an Australian professor who has relentlessly attacked Lott's guns studies, "Mary Rosh" claims to be a former student of Lott at the University of Pennsylvania, where the economist taught between 1991 and 1995.

"I had him for a PhD level empirical methods class when he taught at the Wharton School at the University of Pennsylvania back in the early 1990s, well before he gained national attention, and I have to say that he was the best professor that I ever had. You wouldn't know that he was a 'right-wing' ideologue from the class. . . . There were a group of us students who would try to take any class that he taught. Lott finally had to tell us that it was best for us to try and take classes from other professors more to be exposed to other ways of teaching graduate material."

When a reporter attempted to read the posting to him over the telephone, Lott stopped him after the first few words. "I'm sure I did that. I shouldn't have done it."

Julian Sanchez, a Cato Institute staffer, is the cybersleuth who tracked Mary Rosh back to John Lott.

Sanchez is a blogger -- someone who maintains a Web site where they report and comment on the news -- who had been tracking the debate between Lott and critics of his gun research. He became suspicious about Rosh after he noticed that several of Rosh's online defenses of Lott seemed to track closely with arguments the scholar himself had made in private e-mails to Sanchez and other bloggers. He tracked Mary Rosh's IP address (the computer code translation of the standard e-mail address) to Pennsylvania.

"I compared that IP with the header of an email Dr. Lott had sent me from his home address. And by yet another astonishing coincidence, it had originated at the very same IP address. Now, what are the odds of that?" he wrote in a posting on his Web site. "Sarcasm aside, we're a little old to be playing dress up, aren't we Dr. Lott?"

Lott said he initially used his own name in online debates with critics. "But you just get into really emotional things with people. You also run into other problems." So he started using the name Mary Rosh. "I should not have done it, there is no doubt. But it was a way to get information into the debate."

Officials at the American Enterprise Institute declined to comment yesterday.

Load-Date: February 1, 2003

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# Exhibit 22

Ex. 22 Page 393





# Authors

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# A Study of the Pre-Attack Behaviors of Active Shooters in the United States Between 2000 and 2013

Acknowledgments4
Introduction
Key Findings7
Methodology
Findings
Shooter Demographics9
Planning and Preparation13
Firearms Acquisition14
Stressors15
Mental Health
Concerning Behaviors17
Primary Grievance21
Targeting23
Suicide: Ideation and Attempts24
Concerning Communications24
Limitations
Conclusion
Appendices



Click on a link above to jump to a page.

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The authors are exceptionally grateful to our many threat assessment colleagues who have partnered with and supported the BAU over several years. These professionals quietly and tirelessly work each day to prevent active shootings in our schools, universities, houses of worship, and businesses.





The authors and researchers from the FBI's Behavioral Analysis Unit involved in preparing this report are aware of the horrific impact these shootings have had on victims, survivors, families, and communities. We extend our deepest sympathies to those who have suffered the unimaginable tragedy of an active shooting, either personally or as a family member. We know that behind the statistics and numbers presented here are thousands of individuals with personal stories of grief, bravery, and resilience. In partnership with other law enforcement and threat assessment professionals, we remain committed to doing everything possible to prevent future attacks. Although much work remains, we present this report as a step towards disrupting those who would seek to inflict catastrophic harm.

# Introduction

In 2017 there were 30 separate active shootings in the United States, the largest number ever recorded by the FBI during a one-year period.<sup>1</sup> With so many attacks occurring, it can become easy to believe that nothing can stop an active shooter determined to commit violence. "The offender just snapped" and "There's no way that anyone could have seen this coming" are common reactions that can fuel a collective sense of a "new normal," one punctuated by a sense of hopelessness and helplessness. Faced with so many tragedies, society routinely wrestles with a fundamental question: can *anything* be done to prevent attacks on our loved ones, our children, our schools, our churches, concerts, and communities?

There is cause for hope because there *is* something that can be done. In the weeks and months before an attack, many active shooters engage in behaviors that may signal impending violence. While some of these behaviors are intentionally concealed, others are observable and — if recognized and reported — may lead to a disruption prior to an attack. Unfortunately, well-meaning bystanders (often friends and family members of the active shooter) may struggle to appropriately categorize the observed behavior as malevolent. They may even resist taking action to report for fear of erroneously labeling a friend or family member as a potential killer. Once reported to law enforcement, those in authority may also struggle to decide how best to assess and intervene, particularly if no crime has yet been committed.

By articulating the concrete, observable pre-attack behaviors of many active shooters, the FBI hopes to make these warning signs more visible and easily identifiable. This information is intended to be used not only by law enforcement officials, mental health care practitioners, and threat assessment professionals, but also by parents, friends, teachers, employers and anyone who suspects that a person is moving towards violence.

In 2014, the FBI published a report titled *A Study of Active Shooter Incidents in the United States Between 2000 and 2013*.<sup>2</sup> One hundred and sixty active shooter incidents in the United States occurring between 2000 and 2013 were included in the sample. In this first report, the FBI focused on the circumstances of the active shooting events (e.g., location, duration, and resolution) but did not attempt to identify the motive driving the offender, nor did it highlight observable pre-attack behaviors demonstrated by the offender. The 2014 report will be referred to as the "Phase I" study.

The present study ("Phase II") is the natural second phase of that initiative, moving from an examination of the parameters of the shooting events to assessing the pre-attack behaviors of the shooters themselves. This second phase, then, turns from the vitally important inquiry of "what happened during and after the shooting" to the pressing questions of "how do the active shooters behave *before* the attack?" and, if it can be determined, "why did they attack?" The FBI's objective here was to examine specific behaviors that may precede an attack and which might be useful in identifying, assessing, and managing those who may be on a pathway to deadly violence.

<sup>1 &</sup>lt;u>https://www.fbi.gov/file-repository/active-shooter-incidents-us-2016-2017.pdf/view</u>

<sup>2</sup> https://www.fbi.gov/file-repository/active-shooter-study-2000-2013-1.pdf/view

# Key Findings of the Phase II Study

- 1. The **63** active shooters examined in this study did not appear to be uniform in any way such that they could be readily identified prior to attacking *based on demographics alone*.
- 2. Active shooters take time to plan and prepare for the attack, with 77% of the subjects spending a week or longer planning their attack and 46% spending a week or longer actually preparing (procuring the means) for the attack.
- 3. A majority of active shooters obtained their firearms legally, with only very small percentages obtaining a firearm illegally.
- 4. The FBI could only verify that 25% of active shooters in the study had ever been diagnosed with a mental illness. Of those diagnosed, only three had been diagnosed with a psychotic disorder.
- 5. Active shooters were typically experiencing multiple stressors (an average of **3.6** separate stressors) in the year before they attacked.
- 6. On average, each active shooter displayed *4 to 5* concerning behaviors over time that were observable to others around the shooter. The most frequently occurring concerning behaviors were related to the active shooter's mental health, problematic interpersonal interactions, and leakage of violent intent.
- 7. For active shooters under age 18, school peers and teachers were more likely to observe concerning behaviors than family members. For active shooters 18 years old and over, spouses/domestic partners were the most likely to observe concerning behaviors.
- 8. When concerning behavior was observed by others, the most common response was to communicate directly to the active shooter (83%) or do nothing (54%). In 41% of the cases the concerning behavior was reported to law enforcement. Therefore, just because concerning behavior was *recognized* does not necessarily mean that it was *reported* to law enforcement.
- 9. In those cases where the active shooter's primary grievance could be identified, the most common grievances were related to an adverse interpersonal or employment action against the shooter (49%).
- 10. In the majority of cases (64%) at least one of the victims was specifically targeted by the active shooter.

\*All percentages in this report are rounded to the nearest whole number.

# Methodology

With the goal of carefully reviewing the pre-attack lives and behaviors of the active shooters, the FBI developed a unique protocol of 104 variables covering, among other things:

- Demographics
- Planning and preparation
- Acquisition of firearms in relation to the attack
- Stressors
- Grievance formation
- Concerning pre-attack behaviors and communications
- Targeting decisions
- Mental health

Whereas Phase I analyzed event circumstances that are typically well documented both in law enforcement incident reports and reliable open sources<sup>3</sup>, this second phase is substantially based on observations of what are often nuanced behavioral indicators demonstrated by the active shooter prior to the attack. Given the subtle nature of many of the factors relevant to the inquiry, the FBI decided to use data that have been verified to the greatest possible extent, relying almost exclusively on information contained in official law enforcement investigative files.<sup>4</sup> For this reason, Phase II includes only those cases where the FBI obtained law enforcement investigative files that contained "background" materials (e.g., interviews with family members, acquaintances, neighbors; school or employment records; writings generated by the subject) adequate to answer the protocol questions.<sup>5</sup> In addition, as Phase II focused on identifying pre-attack behaviors of those on a trajectory to violence, active shooting events which appeared to be spontaneous reactions to situational factors (e.g., fights that escalated) were excluded. This resulted in a final sample of 63 active shooting incidents included in the Phase II study.

The use of law enforcement investigative case files as the primary source of data makes this study unique in comparison to other reports that typically rely upon unverified data derived from open sources. The comprehensive evaluation of law enforcement case files for suitability and completeness also contributed to the substantial time it has taken to prepare and publish this study.

The FBI examined whether the 63 cases included in Phase II are representative of the entire Phase I sample (N = 160). To identify the differences in the samples between Phase I and Phase II (N = 160 versus N = 63), the FBI compared those cases that were *only* in Phase I (n = 97) to those cases included in Phase II (N = 63), assessing potential differences between the active shooters (e.g., race, gender, age, and whether the offender committed suicide subsequent to the attack), as well as potential differences in the characteristics of the incidents (number of victims killed, number of law enforcement officers killed, location of the incident, active shooter movement during the event, and if the event concluded prior to the arrival of law enforcement).

<sup>3</sup> Incident overview (e.g., date, location), incident specifics (weapon(s) used, duration of event), and incident outcome (deaths, injuries, resolution).

<sup>4</sup> For one incident, the study relied on publicly available official reports which were based on the complete law enforcement investigative files.

<sup>5</sup> The investigative files did not contain uniform amounts of subject-related behavioral information, as the depth and breadth of investigations varied based on several factors, including available resources, the prospect or not of trial, and the complexity of the event.

As compared to the 97 cases that were only in Phase I, the 63 cases in Phase II had the following characteristics:

- Had a higher number of victims killed on average during each shooting;
- Were more likely to end before law enforcement arrived;
- Were more likely to include offenders who identified with Asian and Caucasian ethnicity, with active shooters
  identified with African American and Hispanic ethnicity generally underrepresented as compared to Phase I;
- Were more likely to occur in an educational facility or a house of worship; and
- Were more likely to end with the active shooter committing suicide.

After cases were identified, a three-stage coding process was utilized. First, two researchers read all case materials and independently coded each of the cases across all protocol variables. The researchers took a conservative approach to coding, declining to definitively answer any question that was not supported by record evidence. Second, another experienced coder (the "reviewer") also read each investigative file. In the final stage, the coders and the reviewer met for each of the 63 cases, compared answers, discussed disagreements, and produced a single reconciled set of data.

# SHOOTER DEMOGRAPHICS

The sample comprised individuals who varied widely along a range of demographic factors making it impossible to create a demographic profile of an active shooter. Indeed, the findings and conclusions of this study should be considered in light of the reality that these 63 active shooters did not appear to be uniform in any way such that they could be readily identified prior to attacking *based on demographics alone*.

## Age:

The youngest active shooter was 12 years old and the oldest was 88 years old with an average age of 37.8 years. Grouping the active shooters by age revealed the following:



**Gender and Race:** 

The sample was overwhelmingly male (94%, n = 59), with only four females in the data set (6%, n = 4), and varied by race as shown in Figure 2:<sup>6</sup>



**Highest Level of Education**<sup>7</sup>:

None of the active shooters under the age of 18 had successfully completed high school, and one (age 12) had not yet entered high school. When known, the highest level of education of adults varied considerably, as shown in Figure 3:



6 Descriptors of active shooters' races were obtained from law enforcement records.

7 Active shooters under the age of 18 (n=8) were excluded in analyses for those variables not typically pertaining to juveniles (e.g., marital status, higher education).

## **Employment:**

The active shooters who were under 18 years old were all students. As featured in Figure 4, nearly equal percentages of the adult active shooters 18 years or older were employed as were unemployed, and 7% (n = 4) were primarily students. The rest of the adults were categorized as retired, disabled/receiving benefits, or other/unknown.



# Military:

Of the active shooters 18 and older, 24% (n = 13) had at least some military experience, with six having served in the Army, three in the Marines, two in the Navy, and one each in the Air Force and the Coast Guard.

# **Relationship Status:**

The active shooters included in the Phase II study were mostly single at the time of the offense (57%, n = 36). Thirteen percent (n = 8) were married, while another 13% were divorced. The remaining 11% were either partnered but not married (n = 7) or separated (6%, n = 4).

## **Criminal Convictions and Anti-Social Behavior**<sup>8</sup>:

Nineteen of the active shooters aged 18 and over (35%) had adult convictions prior to the active shooting event. As visualized in Figure 5, the convictions can be categorized as crimes against society, property, or persons. The category of "crimes against society" included offenses such as driving under the influence, disorderly conduct and the possession of drug paraphernalia. Both the misdemeanor and felony "crimes against property" involved non-violent offenses, such as conspiracy to commit theft, theft, possession of stolen property, and criminal mischief. The misdemeanor "crimes against persons" were not inherently dangerous, but the felony "crimes against persons" involved convictions for criminal sexual assault of a family member, aggravated stalking, and endangering a person (although no active shooter was convicted of more than one crime against a person).

<sup>8</sup> The study does not include juvenile adjudications; therefore, we did not run the analyses on those aged 17 and younger.



In sum, the active shooters had a limited history of adult convictions for violent crime and a limited history of adult convictions for crime of any kind.

Because formal criminal proceedings may not capture the full range of anti-social behaviors in a person's background, the FBI also looked for evidence of behaviors that were abusive and/or violent, but which did not result in a criminal charge. For some active shooters, no evidence of these behaviors was found, but given that these actions by definition did not involve the formal criminal justice system, it is possible that more violent incidents occurred than are reported here.

We found evidence that 62% (n = 39) of the active shooters had a history of acting in an abusive, harassing, or oppressive way (e.g., excessive bullying, workplace intimidation); 16% (n = 10) had engaged in intimate partner violence; and 11% (n = 7) had engaged in stalking-related conduct.<sup>9</sup>

# Considerations

There were very few demographic patterns or trends (aside from gender) that could be identified, reinforcing the concept that there is no one "profile" of an active shooter. Perhaps most noteworthy is the absence of a pronounced violent criminal history in an overwhelming majority of the adult active shooters. Law enforcement and threat management professionals assessing a potentially violent person may therefore wish to avoid any reliance on demographic characteristics or on evidence (or lack thereof) of prior criminal behavior in conducting their assessments.

<sup>9</sup> This number may be underrepresented given the high percentage of unknown responses as related to stalking behaviors (68%).

# PLANNING AND PREPARATION

This study examined two related but separate temporal aspects of the active shooters' pre-attack lives — total time spent *planning* the attack and total time spent *preparing* for the attack.<sup>10,11,12</sup> The purpose in analyzing these chronologies was to establish the broad parameters during which active shooters were moving toward the attack and to identify behaviors that may have been common during these time periods.

In this context, planning means the full range of considerations involved in carrying out a shooting attack. This includes the decision to engage in violence, selecting specific or random targets, conducting surveillance, and addressing all ancillary practical issues such as victim schedules, transportation, and site access. Planning is more specific than a general intent to act violently and involves the thought processes necessary to bring about an intended outcome. Since planning may primarily be an internal thought process, it was often difficult to find objective, observable indications of an active shooter's planning. In nearly half of the cases, the total time spent planning is unknown. However, this is different than declaring that there was no evidence of planning at all, because in every case there was at least some evidence that the active shooter planned the attack; the challenge was ascertaining when the planning began.

In establishing the total duration of planning, the FBI looked for evidence of behaviors that were observable (e.g., conversations, conducting surveillance) as well as in materials that were private to the active shooter (e.g., journals, computer hard drives) and likely unknowable to others until after the attack. As demonstrated in Figure 6, there was a wide range of planning duration in the 34 cases where the time spent planning could reasonably be determined.



With regard to specific planning activities, care should be taken in the interpretation of the data. For instance, our study indicates that few active shooters overall approached or conducted surveillance on their target (14%, n = 9), and fewer still researched or studied the target site where the attack occurred (10%, n = 6). While this could indicate that the active shooters were uninterested in knowing about their targets or attack sites in advance or engaged in little tactical planning, this is inconsistent with the operational experience of the FBI. The likely reason for this finding is that the active shooters often attacked people and places with which they were already familiar. There was

<sup>10</sup> Calhoun, T., & Weston, S., (2003). Contemporary threat management. San Diego: Specialized Training Services;

<sup>11</sup> Fein, R. & Vossekuil, B. (1999). Assassination in the United States: an operational study of recent assassins, attackers, and near-lethal approachers. Journal of Forensic Sciences.

<sup>12</sup> Vossekuil, B., Fein, R., Reddy, M., Borum, R., & Modzeleski, W. (2004). The final report and findings of the safe school initiative: Implications for the prevention of school attacks in the United States. Washington, DC: U.S. Secret Service and the U.S. Department of Education.

a known connection between the active shooters and the attack site in the majority of cases (73%, n = 46), often a workplace or former workplace for those 18 and older (35%, n = 19), and almost always a school or former school for those younger than 18 (88%, n = 7), indicating that in most cases the active shooter was already familiar with both the attack site as well as the persons located at the site. Conversely, those active shooters with no affiliation to the targeted site behaved differently. Active shooters with no known connection to the site of their attack were more likely to conduct surveillance (p < .05) and research the site (p < .01). With routine contact, pre-attack surveillance could presumably be conducted concurrent to normalized activity and eliminate the need for a more formalized or detectable reconnaissance of a chosen target.

The investigative files also demonstrated that only some active shooters researched or studied past attacks by others (21%, n = 13). This is not to say that other active shooters were unaware of past attacks — it is difficult to imagine that they did not have at least some basic knowledge of prior infamous shootings that received national media coverage. The FBI again suspects that this behavior may be underrepresented in the study sample, especially as we could not determine if active shooters researched past attacks in 46% of the cases.

*Preparing* was narrowly defined for this study as actions taken to procure the means for the attack, typically items such as a handgun or rifle, ammunition, special clothing and/or body armor. The focus was on activities that could have been noticed by others (e.g., a visit to a gun store, the delivery of ammunition) and which were essential to the execution of the plan. The FBI was able to find evidence of time spent preparing in more cases than for time spent planning (likely reflecting the overt nature of procuring materials as opposed to the presumably largely internal thought process of planning). As Figure 7 demonstrates, in more than half of the cases where the time spent preparing was known, active shooters spent one week or less preparing for the attack.



# **FIREARMS ACQUISITION**

As part of the review of the active shooter's preparations, the FBI explored investigative records and attempted to identify how each active shooter obtained the firearm(s) used during the attack. Most commonly (40%, n = 25), the active shooter purchased a firearm or firearms legally and specifically for the purpose of perpetrating the attack. A very small percentage purchased firearms illegally (2%, n = 1) or stole the firearm (6%, n = 4). Some (11%, n = 7) borrowed or took the firearm from a person known to them. A significant number of active shooters (35%, n = 22) already possessed a firearm and did not appear (based on longevity of possession) to have obtained it for the express purpose of committing the shooting.





## Considerations

Active shooters generally take some time to plan and carry out the attack. However, retrospectively determining the exact moment when an active shooter decided to engage in violence is a challenging and imprecise process. In reviewing indicators of planning and preparing, the FBI notes that most active shooters (who demonstrated evidence of these processes in an observable manner) spent days, weeks, and sometimes months getting ready to attack. In fact, in those cases where it could be determined, 77% of the active shooters (n = 26) spent a week or longer planning their attack, and 46% (n = 21) spent a week or longer preparing. Readers are cautioned that simply because some active shooters spent less than 24 hours planning and preparing, this should not suggest that potential warning signs or evidence of an escalating grievance did not exist before the initiation of these behaviors. In the four cases where active shooters took less than 24 hours to plan and prepare for their attacks, all had at least one concerning behavior and three had an identifiable grievance.

Perhaps unsurprisingly, active shooters tended to attack places already familiar to them, likely as a result of a personal grievance which motivated the attack and/or as a result of operational comfort and access. A unique challenge for safety, threat assessment, and security professionals will be to identify "outside" active shooters who are not already operating within the target environment. Pre-attack site surveillance by an outsider may be one observable behavior in physical or online worlds indicative of planning and preparation activities.

# STRESSORS

Stressors are physical, psychological, or social forces that place real or perceived demands/pressures on an individual and which may cause psychological and/or physical distress. Stress is considered to be a well-established correlate of criminal behavior.<sup>13</sup> For this study, a wide variety of potential stressors were assessed, including financial pressures, physical health concerns, interpersonal conflicts with family, friends, and colleagues (work and/or school), mental health issues, criminal and civil law issues, and substance abuse.<sup>14</sup>

<sup>13</sup> Felson, R.B., Osgood, D.W., Horney, J. & Wiernik, C. (2012). Having a bad month: General versus specific effects of stress on crime. *Journal of Quantitative Criminology, 28,* 347-363 for a discussion of various theories describing the relationship between stress and crime.

<sup>14</sup> See Appendix A.

The FBI recognizes that most (if not all) people in some way confront similar issues on a regular basis in their daily lives, and that most possess adequate personal resources, psychological resiliency, and coping skills to successfully navigate such challenges without resorting to violence. Therefore, the FBI focused on identifying stressors that appeared to have more than a minimal amount of adverse impact on that individual, and which were sufficiently significant to have been memorialized, shared, or otherwise noted in some way (e.g., in the active shooter's own writings, in conversation with family or friends, work files, court records). Given the fluid nature of some (although not all) of the stressors, the analysis was limited to the year preceding the attack.

The variables were treated as binary, that is, either the stressor was present or not, without regard for the number of separate circumstances giving rise to the stressor. So, an active shooter who had conflict with *one* family member and a shooter who had conflicts with *several* family members were both coded as "yes" for "conflict with other family members."

Overall, the data reflects that active shooters were typically experiencing multiple stressors (an average of 3.6 separate stressors) in the year before they attacked. For example, in the year before his attack, one active shooter was facing disciplinary action at school for abuse of a teacher, was himself abused and neglected at home, and had significant conflict with his peers. Another active shooter was under six separate stressors, including a recent arrest for drunk driving, accumulating significant debt, facing eviction, showing signs of both depression and anxiety, and experiencing both the criminal and civil law repercussions of an incident three months before the attack where he barricaded himself in a hotel room and the police were called.

The only stressor that applied to more than half the sample was mental health (62%, n = 39). Other stressors that were present in at least 20% of the sample were related to financial strain, employment, conflicts with friends and peers, marital problems, drug and alcohol abuse, other, conflict at school, and physical injury.

Stressors	Number	%
Mental health	39	62
Financial strain	31	49
Job related	22	35
Conflicts with friends/peers	18	29
Marital problems	17	27
Abuse of illicit drugs/alcohol	14	22
Other (e.g. caregiving responsibilities)	14	22
Conflict at school	14	22
Physical injury	13	21
Conflict with parents	11	18
Conflict with other family members	10	16
Sexual stress/frustration	8	13
Criminal problems	7	11
Civil problems	6	10
Death of friend/relative	4	6
None	1	2

TABLE 1: STRESSORS

# **MENTAL HEALTH**

There are important and complex considerations regarding mental health, both because it is the most prevalent stressor and because of the common but erroneous inclination to assume that anyone who commits an active shooting must de facto be mentally ill. First, the *stressor* "mental health" is not synonymous with a *diagnosis* of mental illness. The stressor "mental health" indicates that the active shooter appeared to be struggling with (most commonly) depression, anxiety, paranoia, etc. in their daily life in the year before the attack. There may be complex interactions with other stressors that give rise to what may ultimately be transient manifestations of behaviors and moods that would not be sufficient to warrant a formal diagnosis of mental illness. In this context, it is exceedingly important to highlight that the FBI could only verify that 25% (n = 16) of the active shooters in Phase II were known to have been diagnosed by a mental health professional with a mental illness *of any kind* prior to the offense.<sup>15</sup> The FBI could not determine if a diagnosis had been given in 37% (n = 23) of cases.

Of the 16 cases where a diagnosis prior to the incident could be ascertained, 12 active shooters had a mood disorder; four were diagnosed with an anxiety disorder; three were diagnosed with a psychotic disorder; and two were diagnosed with a personality disorder. Finally, one active shooter was diagnosed with Autism spectrum disorder; one with a developmental disorder; and one was described as "other." Having a diagnosed mental illness was unsurprisingly related to a higher incidence of concurrent mental health stressors among active shooters.

# Considerations

It is clear that a majority of active shooters experienced multiple stressors in their lives before the attack. While the active shooters' reactions to stressors were not measured by the FBI, what appears to be noteworthy and of importance to threat assessment professionals is the active shooters' ability to navigate conflict and resiliency (or lack thereof) in the face of challenges. Given the high prevalence of financial and job-related stressors as well as conflict with peers and partners, those in contact with a person of concern at his/her place of employment may have unique insights to inform a threat assessment.

In light of the very high lifetime prevalence of the symptoms of mental illness among the U.S. population, formally diagnosed mental illness is not a very specific predictor of violence of any type, let alone targeted violence.<sup>16,17,18</sup> Some studies indicate that nearly half of the U.S. population experiences symptoms of mental illness over their lifetime, with population estimates of the lifetime prevalence of diagnosable mental illness among U.S. adults at 46%, with 9% meeting the criteria for a personality disorder.<sup>19,20</sup> Therefore, absent specific evidence, careful consideration should be given to social and contextual factors that might interact with any mental health issue before concluding that an active shooting was "caused" by mental illness. In short, declarations that all active shooters must simply be mentally ill are misleading and unhelpful.

# **CONCERNING BEHAVIORS**

Concerning behaviors are *observable* behaviors exhibited by the active shooter. For this study, a wide variety of concerning behaviors were considered, including those related to potential symptoms of a mental health disorder, interpersonal interactions, quality of the active shooter's thinking or communication, recklessness, violent media usage, changes in hygiene and weight, impulsivity, firearm behavior, and physical aggression.<sup>21</sup> Although these may be related to stressors in the active shooter's life, the focus here was not on the internal, subjective experience of

21 See Appendix B.

<sup>15</sup> The number of documented, diagnosed mental illness may be the result of a number of factors, including those related to situational factors (access to health care) as well as those related to the study factors (access to mental health records).

<sup>16</sup> Elbogen, E.B., & Johnson, S.C. (2009). The intricate link between violence and mental disorder. Arch Gen Psychiatry, 66(2), 152-161.

<sup>17</sup> Glied, S.A., and Frank, R.G. (2014). Mental illness and violence: Lessons from the evidence. American Journal of Public Health, 104, e5-e6 doi:10.2015/AJPH.2013.301710

<sup>18</sup> Monahan, J., Steadman, H. J., Silver, E., Applebaum, P.S., Clark Robbins, P., Mulvey, E. P., & Banks, S. (2001). Rethinking Risk Assessment: The MacArthur Study of Mental Disorder and Violence. Oxford, UK: Oxford University Press

<sup>19</sup> Kessler, R.C., Berglund, P., Demler, O., Jin, R., Merikangas, K.R., Walters, E.E. Lifetime prevalence and age-of-onset distributions of DSM-IV disorders in the National Comorbidity Survey Replication. Arch Gen Psychiatry. 2005:62(6): 593-602.

<sup>20</sup> Lenzweger, M.F., Lane, M.C., Loranger, A.W., Kessler, R.C., DSM-IV personality disorders in the National Comorbidity Survey Replication. Biol Psychiatry. 2007;62(6): 553-564.

the active shooter, but rather on what was *objectively knowable* to others. So, while the assessment of stressors is meant to provide insight into the active shooter's inner turmoil, the examination of concerning behaviors addresses a related but separate issue — the possibility of identifying active shooters before they attack by being alert for observable, concerning behaviors. The FBI looked for documented confirmation that someone noticed a facet of the shooter's behavior causing the person to feel a "more than minimal" degree of unease about the well-being and safety of those around the active shooter.

Before examining what behaviors were observable by others, it is useful to address the widespread perception that active shooters tend to be cut off from those around them. In general, the active shooters in Phase II were not completely isolated and had at least some social connection to another person. While most of the active shooters age 18 and older were single/never married (51%, n = 28) or separated/divorced (22%, n = 12) at the time of the attack, the majority did live with someone else (68%, n = 43). This percentage was slightly less (64%, n = 35) for only those active shooters who were 18 years or older. Most had significant in-person social interactions with at least one other person in the year before the attack (86%, n = 54), and more than a quarter of them had significant online interactions with another person within a year of the attack (27%, n = 17). All active shooters either: a) lived with someone, or b) had significant in-person or online social interactions.

Since the observation of concerning behaviors offers the opportunity for intervention prior to the attack, this study examines not only what was observed, but when the observations were made, who made them, and what if anything the person(s) did with regard to these observations. To better serve threat assessment teams, mental health professionals, community resources, and law enforcement officials, the FBI expanded the inquiry to capture behaviors that may have been observed at any point (in many cases beyond one year) before the attack.

Overall, active shooters showed concerning behaviors in multiple ways, with an average of 4.7 concerning behaviors per active shooter. Behaviors observed in more than half of the sample were related to the shooter's mental health<sup>22</sup>, interpersonal interactions, leakage (the communication to a third-party of an intent to harm someone, discussed with threats in a separate section), and the quality of the active shooter's thinking or communication.

Of note was that contextually inappropriate firearms behavior was noted in approximately one fifth of the active shooters, while drug and alcohol abuse figured even less prominently in the sample (for the purposes of the study, contextually inappropriate firearms behavior was defined as interest in or use of firearms that appeared unusual given the active shooter's background and experience with firearms).

Concerning Behavior	Number	%
Mental health	39	62
Interpersonal interactions	36	57
Leakage	35	56
Quality of thinking or communication	34	54
Work performance*	11	46
School performance**	5	42
Threats/confrontations	22	35
Anger	21	33
Physical aggression	21	33
		Continues on next nage

#### TABLE 2: CONCERNING BEHAVIORS

22 Thirty-nine active shooters were experiencing a mental health stressor, and 39 active shooters showed concerning behaviors related to mental health, but the same 39 active shooters did not appear in each category; there were five active shooters who had a mental health stressor but who did not show a concerning behavior, and five other active shooters who showed a mental health-related concerning behavior but for whom there was no evidence of mental health stress.

Risk-taking	13	21
Firearm behavior	13	21
Violent media usage	12	19
Weight/eating	8	13
Drug abuse	8	13
Impulsivity	7	11
Alcohol abuse	6	10
Physical health	6	10
Other (e.g. idolizing criminals)	5	8
Sexual behavior	4	6
Quality of sleep	3	5
Hygiene/appearance	2	3

\* Based on the 24 active shooters who were employed at the time of the offense

\*\* Based on the 12 active shooters who were students at the time of the offense

# When Were the Concerning Behaviors Noticed?

Since the overwhelming majority of active shooters (all but three) displayed at least two concerning behaviors, there are a number of different ways to assess the data. One way is to examine the data by active shooter and to observe the first instance that any concerning behavior was noticed (this could not be determined for three active shooters). Figure 9 shows this data and helps frame the longest time before a shooting during which others were concerned about the active shooter's behavior.



Again, this chart shows the first instance of *any* concerning behavior, and it should be kept in mind that this behavior might not have been the type that by *itself* would cause a reasonable person to be alarmed or to report it to others. For example, a co-worker who noticed that an active shooter had more than the normal amount of conflict with a supervisor might be unlikely to take any action. Perhaps only after an attack and with the benefit of hindsight would this singular behavior be considered to be — in and of itself — troubling or concerning. Yet, on average, each active shooter displayed *four to five* concerning behaviors over time. While it may only be the interaction and cumulative effect of these behaviors that would cause alarm, early recognition and detection of growing or interrelated problems may help to mitigate the potential for violence.

# In What Way Were the Concerning Behaviors Noticed?

Concerning behaviors came to the attention to others in a variety of ways, with some far more common than others. The most prevalent way in which concerning behaviors were noticed was verbal communication by the active shooter (95%, n = 60), followed by observing the physical actions of the active shooter (86%, n = 54), written communication (27%, n = 17), and finally instances where concerning behavior was displayed online (16%, n = 10). A large majority of active shooters (89%, n = 56) demonstrated concerning behaviors that were noticed in multiple ways.

# Who Noticed the Concerning Behaviors?

At least one person noticed a concerning behavior in *every* active shooter's life, and on average, people from three different groups noticed concerning behaviors for each active shooter. As shown below, classmates (for those who were students), partners (for those in relationships), family members and friends most frequently noticed concerning behavior, followed by co-workers, other, and law enforcement:

Who Noticed	Number	%
Schoolmate*	11	92
Spouse/domestic partner**	13	87
Teacher/school staff*	9	75
Family member	43	68
Friend	32	51
Co-worker	25	40
Other (e.g. neighbors)	23	37
Law enforcement	16	25
Online individual	6	10
Religious mentor	3	5

## TABLE 3: WHO NOTICED CONCERNING BEHAVIORS

\* Percentage calculated only with those active shooters who were students at the time of the offense

\*\* Percentage calculated only with those active shooters who were in a relationship at the time of the offense

# What, If Anything, Did the Concerned Party Do?

If the person recognizes behaviors as problematic but takes no action, the opportunity for intervention is missed. Whether and how a person responds to an active shooter's concerning behavior is likely influenced by a host of personal and situational factors (e.g., whether the behavior is threatening to the observer or others, the relationship of the observer and active shooter, avenues for anonymous reporting, and/or confidence in authorities or others to address the behavior).

In this study, even in cases where an active shooter displayed a variety of concerning behaviors that might indicate an intent to act violently, the observer(s) of that information did not necessarily pass it along to anyone else. As shown above, the people most likely to notice concerning behaviors were those who knew the active shooter best — family, friends and classmates. For the very reason they are the people most likely to take note of concerning behaviors, they are also people who may feel constrained from acting on these concerns because of loyalty, disbelief, and/or fear of the consequences.<sup>23</sup>

<sup>23</sup> Borum, R. (2013). Informing Lone-Offender Investigations. *Criminology & Public Policy*, 12(1), 103-112.

Again, keeping in mind that active shooters displayed multiple concerning behaviors and those who observed these behaviors might have responded in different ways to each, the most common response was to communicate directly to the active shooter (83%, n = 52) or do nothing (54%, n = 34). Thus, in many instances, the concern stayed between the person who noticed the behavior and the active shooter.

The next most common responses were: report the active shooter to a non-law enforcement authority (51%, n = 32); discuss the concerning behavior with a friend or family member (49%, n = 31); and, report the active shooter to law enforcement authority (41%, n = 26).

## Considerations

The analysis above is not intended to, nor could it, encompass the innumerable ways in which the observer of a concerning behavior might react. Nor does it suggest that every concerning behavior warrants assertive intervention; many of the concerning behaviors that registered with others likely would not have presaged deadly violence to a reasonable person. The FBI is aware that in retrospect certain facts may take on a heightened degree of significance that may not have been clear at the time.

Nevertheless, understanding that there are often opportunities before a shooting to recognize concerning behaviors that may suggest progression toward violence, the FBI is highlighting the most common behaviors displayed in the sample. There is no single warning sign, checklist, or algorithm for assessing behaviors that identifies a prospective active shooter. Rather, there appears to be a complex combination of behaviors and interactions with bystanders that may often occur in the days, weeks, and months leading up to an attack. Early recognition *and* reporting of concerning behaviors to law enforcement or threat assessment professionals may initiate important opportunities for mitigation.

# **PRIMARY GRIEVANCE**

A grievance is defined for this study as the cause of the active shooter's distress or resentment; a perception — not necessarily based in reality — of having been wronged or treated unfairly or inappropriately.<sup>24,25,26</sup> More than a typical feeling of resentment or passing anger, a grievance often results in a grossly distorted preoccupation with a sense of injustice, like an injury that fails to heal. These thoughts can saturate a person's thinking and foster a pervasive sense of imbalance between self-image and the (real or perceived) humiliation. This nagging sense of unfairness can spark an overwhelming desire to "right the wrong" and achieve a measure of satisfaction and/or revenge. In some cases, an active shooter might have what appeared to be multiple grievances but, where possible, the FBI sought to determine the primary grievance. Based on a review of the academic literature and the facts of the cases themselves, the FBI identified eight categories of grievances, with an additional category of "other" for grievances that were entirely idiosyncratic.

As shown in the following table, the FBI could not identify a primary grievance for 13 (21%) of the active shooters, either because they did not have one or because there was insufficient evidence to determine whether one existed. While it may be particularly difficult to understand the motivation(s) for attacks that do not appear to be based on identifiable grievances, these active shooters still displayed concerning behaviors, were under identifiable stressors, and engaged in planning and preparation activities. For example, for the active shooters where no grievance could be identified, all had at least two behaviors (with an average of 5.4 behaviors) that were noted to be concerning by others.

<sup>24</sup> Calhoun, T., & Weston, S., (2003).

<sup>25</sup> Fein, R., & Vossekuil, B. (1999).

<sup>26</sup> Vossekuil, B., Fein, R., Reddy, M., Borum, R., & Modzeleski, W. (2004).

The majority (79%, n = 50) of the active shooters did appear to be acting in accord with a grievance of some kind. Of course, the grievance itself may not have been reasonable or even grounded in reality, but it appeared to serve as the rationale for the eventual attack, giving a sense of purpose to the shooter. Most of these grievances seem to have originated in response to some specific action taken regarding the active shooter. Whether interpersonal, employment, governmental, academic, or financial, these actions were (or were perceived to be) directed against the active shooter personally. In contrast, grievances driven by more global or broad considerations — such as ideology or hatred of a group — account for less than 7% of the overall cases. In general then, active shooters harbored grievances that were distinctly personal to them and the circumstances of their daily lives.

Primary Grievance	Number	%
Adverse interpersonal action against the shooter	21	33
Adverse employment action against the shooter	10	16
Other (e.g. general hatred of others)	6	10
Adverse governmental action against the shooter	3	5
Adverse academic action against the shooter	2	3
Adverse financial action against the shooter	2	3
Domestic	2	3
Hate crime	2	3
ldeology/extremism	2	3
Unknown	13	21

## TABLE 4: PRIMARY GRIEVANCE

## **Precipitating Events**

Of the 50 active shooters who had an identifiable grievance, nearly half of them experienced a precipitating or triggering event related to the grievance (44%, n = 22). Seven active shooters (14%) did not experience a precipitating event, and the FBI could not determine whether the remaining 21 (42%) did. Precipitating events generally occurred close in time to the shooting and included circumstances such as an adverse ruling in a legal matter, romantic rejection, and the loss of a job.

These precipitating events were of more consequence in the timing of the attack, and while they appear to have accelerated the active shooter's movement on the trajectory to violence, they did not by themselves appear to set the course.

## **Considerations**

Of course, many people have grievances and never act violently. What caused the active shooters in this study to act the way they did cannot be explained simply by the presence of a grievance. There was likely the interaction of a variety of operational considerations and psychological stressors that eventually crystallized in the decision to ignore non-violent options and choose to attack. However, the types of grievances most commonly experienced by the active shooters in this study may be important considerations for the many threat assessment teams and law enforcement professionals who work each day to assess a subject's progression along the pathway to violence.

# TARGETING

For this study, a target is defined as a person or group of people who were *identifiable before the shooting occurred* and whom the active shooter intended to attack. It was not necessary that the active shooter knew the target by name; intending to attack a person holding a position at or affiliated with a business, educational facility, or in a governmental agency sufficed. The target could be a group, so long as members of that group could have been identified prior to the attack.

In cases where the victims could not reasonably have been identified prior to the shooting, the active shooter was deemed to have selected the victims at random. While there is *some* element of selection in any attack where there is more than one potential victim (unless the active shooter literally does not aim at all), the FBI considered victims to be random where there was: 1) no known connection between the active shooter and the victims, and 2) the victims were not specifically linked to the active shooter's grievance.

In many cases, there was a mix of targeted and random victims in the same shooting. The typical circumstance occurred when an active shooter went to a location with targets in mind and also shot others who were at the same location, either because they presented some obstacle in the attack or for reasons that could not be identified.



The overall numbers for targeted and random victims are listed below:

\*Does not sum to 100% due to rounding.

# Considerations

While approximately one-third of active shooters in this sample victimized only random members of the public, most active shooters arrive at a targeted site with a specific person or persons in mind. Awareness of targeting behaviors can provide valuable insight for threat assessment professionals. Relatedly, the FBI has observed that when an active shooter's grievance generalizes — that is, expands beyond a desire to punish a specific individual to a desire to punish an institution or community — this should be considered to be progression along a trajectory towards violence and ultimately a threat-enhancing characteristic.

# SUICIDE: IDEATION AND ATTEMPTS

For this study, "suicidal ideation" was defined as thinking about or planning suicide, while "suicide attempt" was defined as a non-fatal, self-directed behavior with the intent to die, regardless of whether the behavior ultimately results in an injury of any kind. Although these definitions are broad, the FBI concluded that an active shooter had suicidal ideation or engaged in a suicide attempt only when based on specific, non-trivial evidence.

Nearly half of the active shooters had suicidal ideation or engaged in suicide-related behaviors at some time prior to the attack (48%, n = 30), while five active shooters (8%) displayed no such behaviors (the status of the remaining 28 active shooters was unknown due to a lack of sufficient evidence to make a reasonable determination).

An overwhelming majority of the 30 suicidal active shooters showed signs of suicidal ideation (90%, n = 27), and seven made actual suicide attempts (23%). Nearly three-quarters (70%, n = 21) of these behaviors occurred within one year of the shooting.

# Considerations

The high levels<sup>27</sup> of pre-attack suicidal ideation — with many appearing within 12 months of the attack — are noteworthy as they represent an opportunity for intervention. If suicidal ideation or attempts in particular are observed by others, reframing bystander awareness within the context of a mass casualty event may help to emphasize the importance of telling an authority figure and getting help for the suicidal person. Without stigmatizing those who struggle with thoughts of self-harm, researchers and practitioners must continue to explore those active shooters who combined suicide with externalized aggression (including homicidal violence) and identify the concurrent behaviors that reflect this shift.

# **CONCERNING COMMUNICATIONS**

One useful way to analyze concerning communications is to divide them into two categories: *threats/confrontations* and *leakage of intent*.

# **Threats/Confrontations**

Threats are *direct communications to a target* of intent to harm and may be delivered in person or by other means (e.g., text, email, telephone). For this study, threats need not be verbalized or written; the FBI considered in-person confrontations that were intended to intimidate or cause safety concerns for the target as falling under the category of threats as well.

More than half of the 40 active shooters who had a target made threats or had a prior confrontation (55%, n = 22). When threats or confrontations occurred, they were almost always in person (95%, n = 21) and only infrequently in writing or electronically (14%, n = 3). Two active shooters made threats both in person and in writing/electronically.

## Leakage

Leakage occurs when a person intentionally or unintentionally reveals clues to a *third-party* about feelings, thoughts, fantasies, attitudes or intentions that may signal the intent to commit a violent act.<sup>28</sup> Indirect threats of harm are included as leakage, but so are less obvious, subtle threats, innuendo about a desire to commit a violent attack, or boasts about the ability to harm others. Leakage can be found not only in verbal communications, but

<sup>27</sup> The National Survey on Drug Use and Health (2015) shows that in 2015: 4% of adults had serious thoughts of suicide, 1.1% made serious plans, and 0.6% attempted suicide (https://www.samhsa.gov/data/sites/default/files/NSDUH-DR-FFR3-2015/NSDUH-DR-FFR3-2015.htm)

<sup>28</sup> Meloy, J. R. & O'Toole, M. E. (2011). The concept of leakage in threat assessment. Behavioral Sciences and the Law, 29, 513-527

also in writings (e.g., journals, school assignments, artwork, poetry) and in online interactions (e.g., blogs, tweets, texts, video postings). Prior research has shown that leakage of intent to commit violence is common before attacks perpetrated by both adolescents and adults, but is more common among adolescents.<sup>29,30,31</sup>

Here, too, leakage was prevalent, with over half of the active shooters leaking intent to commit violence (56%, n = 35). In the Phase II sample, 88% (n = 7) of those active shooters age 17 and younger leaked intent to commit violence, while 51% (n = 28) of adult active shooters leaked their intent. The leaked intent to commit violence was not always directed at the eventual victims of the shootings; in some cases what was communicated was a more general goal of doing harm to others, apparently without a particular person or group in mind. For example, one active shooter talked to a clerk at a gas station about killing "a family" and another expressed interest in becoming a sniper like a character featured in *The Turner Diaries*. In 16 of the 40 cases (40%) where the active shooter had a target, however, the leaked intent to act violently was directly pertaining to that target. In these cases, the leakage was generally a statement to a third-party of the intent to specifically harm the target.

#### **Legacy Tokens**

Finally, the FBI considered whether or not an active shooter had constructed a "legacy token" which has been defined as a communication prepared by the offender to claim credit for the attack and articulate the motives underlying the shooting.<sup>32</sup> Examples of legacy tokens include manifestos, videos, social media postings, or other communications deliberately created by the shooter and delivered or staged for discovery by others, usually near in time to the shooting. In 30% (n = 19) of the cases included in this study, the active shooter created a legacy token prior to the attack.

#### Considerations

Although more than half of the active shooters with pre-attack targets made threats (n = 22), in the majority (65%) of the overall cases no threats were made to a target, and the FBI cautions that the absence of a direct threat should not be falsely reassuring to those assessing the potential for violence raised by other circumstances and factors. Nor should the presence of a threat be considered conclusive. There is a significant amount of research and experience to demonstrate that direct threats are not correlated to a subsequent act of targeted violence.<sup>33,34,35,36,37,38</sup>

It is important to highlight that in this Phase II study the overwhelming majority of direct threats were verbally delivered by the offender to a future victim. Only a very small percentage of threats were communicated via writing or electronically. In many ways this is not surprising. Written, directly communicated threats against a target (e.g., "I'm going to shoot and kill everyone here on Tuesday") often spark a predictable response that includes a heightened law enforcement presence and the enhancement of security barriers. These responses are highly undesirable to an offender planning an active shooting.<sup>39</sup> Verbal threats issued directly to another person appear to be far more common among the active shooters included in the Phase II study.

<sup>29</sup> Hemple, A., Meloy, J.R., & Richards, T. (1999). Offender and offense characteristics of a nonrandom sample of mass murderers. *Journal of the American Academy of Psychiatry and the Law*, 27, 213-225. Meloy, J.R., Hoffman, J., Guldimann, A., & James, D. (2011). The role of warning behaviors in threat assessment: An exploration and suggested typology. Behavioral Sciences and the Law, 30, 256-279.

<sup>30</sup> Meloy, J. R. & O'Toole, M. E. (2011).

<sup>31</sup> Meloy, J.R., Hoffman, J., Guldimann, A., & James, D. (2011). The role of warning behaviors in threat assessment: An exploration and suggested typology. *Behavioral Sciences and the Law, 30,* 256-279.

<sup>32</sup> Simons, A., & Tunkel, R. (2014). The assessment of anonymous threatening communications. In J.R. Meloy & J. Hoffman (Eds.), *International handbook of threat assessment* (pp. 195-213). New York: Oxford University Press.

<sup>33</sup> Borum, R., Fein, R. Vossekuil, B., & Berglund, J. (1999). Threat assessment: Defining an approach for evaluating risk of targeted violence. Behavioral Sciences and the Law, 17, 323-337.

<sup>34</sup> Calhoun, F. (1998). Hunters and howlers: Threats and violence against federal judicial officials in the United States, 1789-1993. Arlington, VA: US Marshals Service.

<sup>35</sup> Calhoun T. & Weston, S. (2003).

<sup>36</sup> Dietz, P., Matthews, D., Martell, D., Stewart, T., Hrouda, D., & Warren, J. (1991a). Threatening and otherwise inappropriate letters to members of the United States Congress. *Journal of Forensic Sciences*, *36*, 1445-1468.

<sup>37</sup> Dietz, P., Matthews, D., Van Duyne, C., Martell, D., Parry, C., Stewart, T., et al. (1991b). Threatening and otherwise inappropriate letters to Hollywood celebrities. *Journal of Forensic Sciences, 36*, 185-209.

<sup>38</sup> Meloy, J.R. (2000). Violence risk and threat assessment. San Diego: Specialized Training Services.

<sup>39</sup> Simons A. & Tunkel, R. (2014)

Whether verbal or written, concerning communications are challenging as those on the receiving end must assess sometimes ominously vague or nebulous verbiage. Such confusion can create doubt in the listener's mind as to the communicator's true intent toward violence.<sup>40</sup> As law enforcement agencies continue to remind bystanders if they "see something, say something" it becomes relevant to use this data (particularly regarding leakage behaviors) to lower the internal threshold for reporting, even in the face of ambiguous language. It is troubling to note that no bystanders reported instances of leakage to law enforcement, perhaps out of a fear of overreacting or perhaps due to a lack of understanding as to what law enforcement's response would be. This suggests that more robust efforts need to be made to educate bystanders (especially students and adolescents) on the nature of leakage and its potential significance.

# Limitations

The findings presented in this report reflect a thorough and careful review of the data derived almost exclusively from law enforcement records. Nevertheless, there are limitations to the study which should be kept in mind before drawing any conclusions based on the findings.

First, the Phase I study on which the present analysis is based included only a specific type of event. Shootings must have been (a) in progress in a public place and (b) law enforcement personnel and/or citizens had the potential to affect the outcome of the event based on their responses. The FBI acknowledges there is an inherent element of subjectivity in deciding whether a case meets the study criteria. Moreover, while every effort was made to find all cases between 2000 and 2013 which met the definition, it is possible that cases which should have been included in the study were not identified. Overall, as with the Phase I study, the incidents included in the Phase II study were not intended to and did not comprise all gun-related violence or mass or public shootings occurring between 2000 and 2013.

Second, although the FBI took a cautious approach in answering protocol questions and limited speculation by relying on identifiable data, there was some degree of subjectivity in evaluating which of the original 160 cases had sufficient data to warrant inclusion in the study.

Third, while reliance on official law enforcement investigative files was reasonable based on the study's objectives, the level of detail contained in these files was not uniform throughout and the FBI was not able to definitively answer all protocol questions for all subjects.

This is a purely descriptive study. With the exception of mental health and suicidal behaviors, the FBI did not make any comparisons to the general population or to criminals who were not active shooters. Therefore, we cannot postulate on the probability as to whether some of the behaviors and characteristics seen here would also have been seen in other populations. Furthermore, the FBI cautions readers to not treat the observed behaviors as having predictive value in determining if a person will become violent or not, as the findings and observations presented herein are not a "checklist" but instead are offered to promote awareness among potential bystanders and for consideration in the context of a thorough, holistic threat assessment by trained professionals. Future research may benefit from comparisons between those who completed active shooting attacks and those who planned to attack but were disrupted prior to the offense, and/or in comparison to those individuals who may have displayed concerning behaviors but had no true intent to commit an act of targeted violence.

<sup>40</sup> The FBI noted that there were four cases where threats were made and someone notified law enforcement (out of 22 cases where a threat was made, or 14%)

# Conclusion

The ability to utilize case files (as compared to open-source documents) allowed the FBI to carefully examine both the internal issues experienced and the behaviors demonstrated by active shooters in the weeks and months preceding their attacks. What emerges is a complex and troubling picture of individuals who fail to successfully navigate multiple stressors in their lives while concurrently displaying four to five observable, concerning behaviors, engaging in planning and preparation, and frequently communicating threats or leaking indications of an intent to attack. As an active shooter progresses on a trajectory towards violence, these observable behaviors may represent critical opportunities for detection and disruption.

The information contained in this Phase II report can be utilized by myriad safety stakeholders. The successful prevention of an active shooting frequently depends on the collective and collaborative engagement of varied community members: law enforcement officials, teachers, mental health care professionals, family members, threat assessment professionals, friends, social workers, school resource officers...and many others. A shared awareness of the common observable behaviors demonstrated by the active shooters in this study may help to prompt inquiries and focus assessments at every level of contact and every stage of intervention.

While many dedicated professionals work to thwart active shootings, the FBI suspects that future active shooters themselves are looking for ways to avoid detection and maximize damage as they plan and prepare for their acts of violence. The prevention of these future attacks will depend on our ability to remain agile and recognize evolving pre-attack behaviors. To that end, the FBI continues to study active shooters to better inform all safety stakeholders and to support the development of sound threat mitigation strategies.

As tragically seen from current events, active shootings continue to impact our nation. The FBI hopes that the information contained in this Phase II study will help in efforts to promote safety across all communities.
## **Appendix A:**

## STRESSORS

Abuse of illicit drugs or alcohol: difficulties caused by the effects of drugs/alcohol and/or frustrations related to obtaining these substances.

Civil legal problems: being party to a non-trivial lawsuit or administrative action.

**Conflict with friends/peers:** general tension in the relationship beyond what is typical for the active shooter's age or specific instances of serious and ongoing disagreement.

**Conflict with other family members:** general tension in the relationship beyond what is typical for the active shooter's age, or specific instances of serious and ongoing disagreement.

**Conflict with parents:** general tension in the relationship beyond what is typical for the active shooter's age, or specific instances of serious and ongoing disagreement.

Criminal legal problems: arrests, convictions, probation, parole.

Death of friend/relative: death that caused emotional or psychological distress.

**Financial strain:** related to job loss, debt collection, potential or actual eviction, inability to pay normal and usual daily bills.

**Job-related problems:** ongoing conflicts with co-workers or management, pervasive poor performance evaluations, or disputes over pay or leave.

**Marital problems/conflict with intimate partner(s)/divorce or separation:** difficulties in the relationship that were a consistent source of psychological distress and/or which did or were likely to lead to the end of the relationship or the desire to end the relationship.

**Mental health problems:** symptoms of anxiety, depression, paranoia, or other mental health concerns that have a negative effect on daily functioning and/or relationships.

**Other:** any other circumstance causing physical, psychological, or emotional difficulties that interfere in a non-trivial way with normal functioning in daily life.

**Physical injury:** physical condition/injury that significantly interfered with or restricted normal and usual activities.

**School-related problems:** conflicts with teachers and staff that go beyond single instances of minor discipline; pervasive frustration with academic work; inability to follow school rules.

Sexual stress/frustration: pronounced and ongoing inability to establish a desired sexual relationship.

## **Appendix B:**

## **CONCERNING BEHAVIORS**

Amount or quality of sleep: unusual sleep patterns or noticeable changes in sleep patterns.

Anger: inappropriate displays of aggressive attitude/temper.

**Change, escalation, or contextually inappropriate firearms behavior:** interest in or use of firearms that appears unusual given the active shooter's background and experience with firearms.

Changes in weight or eating habits: significant weight loss or gain related to eating habits.

Hygiene or personal appearance: noticeable and/or surprising changes in appearance or hygiene practices.

Impulsivity: actions that in context appear to have been taken without usual care or forethought.

**Interpersonal interactions:** more than the usual amount of discord in ongoing relationships with family, friends, or colleagues.

Leakage: communication to a third-party of the intent to harm another person.

Mental health: indications of depression, anxiety, paranoia or other mental health concerns.

**Other:** any behavior not otherwise captured in above categories that causes more than a minimal amount of worry in the observer.

Physical aggression: inappropriate use of force; use of force beyond what was usual in the circumstances.

Physical health: significant changes in physical well-being beyond minor injuries and ailments.

Quality of thinking or communication: indications of confused or irrational thought processes.

Risk-taking: actions that show more than a usual disregard for significant negative consequences.

School performance: appreciable decrease in academic performance; unexplained or unusual absences.

Sexual behavior: pronounced increases or decreases in sexual interest or practices.

**Threats/Confrontations:** direct communications to a target of intent to harm. May be delivered in person or by other means (e.g., text, email, telephone).

Use of illicit drugs or illicit use of prescription drugs: sudden and/ recent use or change in use of drugs; use beyond social norms that interferes with the activities of daily life.

Use or abuse of alcohol: sudden and/or recent use or changes in use of alcohol; use beyond social norms that interferes with the activities of daily life.

Violent media usage: more than a usual age-appropriate interest in visual or aural depictions of violence.

Work performance: appreciable decrease in job performance; unexplained or unusual absences.



**U.S. Department of Justice** Federal Bureau of Investigation

> Ex. 22 Page 423