IN THE UNITED STATES DISTRICT COURT
FOR THE DISTRICT OF MARYLAND

RAYMOND WOOLLARD, *et al.*,	*	
Plaintiffs,	*	
v.	*	Civil Case No. 1:10-cv-2068-BEL
MARCUS BROWN, *et al.*,	*	
Defendants.	*	

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SUPPLEMENTAL BRIEF IN SUPPORT OF MOTION FOR STAY PENDING APPEAL

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INTRODUCTION

Pursuant to the Court's March 30, 2012 Order (ECF No. 63), the defendants submit this supplemental brief in support of their motion for stay (ECF Nos. 54, 67), and to address three questions posed by the Court during the March 22, 2012 conference call.[1]

The Court should enter a stay pending appeal because of:

(1) the compelling public interest in public safety that the General Assembly determined, and law enforcement officials have confirmed, is served by the good and substantial reason requirement, *see* ECF No. 54 at 11-13; *see also* Defendants' Memorandum in Support of Cross-Motion for Summary Judgment (ECF No. 26) at 10-16, 34-38; Bealefeld Decl. (ECF No. 26-5); Sheridan Decl. (ECF No. 26-6); Johnson Decl. (ECF No. 26-7); Cook Decl. (ECF No. 26-4);

(2) the resulting irreparable harm if the good and substantial reason requirement cannot be enforced, *see* ECF No. 54 at 17-19;

(3) the likelihood of success on the merits, *see* ECF No. 54 at 13-17; and

(4) the balance of the equities, *see* ECF No. 54 at 18-19.

With respect to the balance of the equities, Maryland law already protects the core Second Amendment right, namely "'self-defense in the home by a law-abiding citizen.'" Memorandum Opinion, ECF No. 52, at 8 (quoting *United States v. Masciandaro*, 638 F.3d 458, 470 (4th Cir. 2011)). That core right is not at issue here. *Id.* at 9. Moreover, Maryland law allows the wearing and carrying of handguns without a permit in the home and many other locations, *see* ECF No. 26 at 6-7, 33-34, and further generally allows the

[1] At the end of the March 22 call, the Court asked whether Mr. Woollard could get a permit even if other relief were stayed. The defendants note that the injunction (ECF No. 63) contains two paragraphs, one generally prohibiting enforcement of good and substantial reason and the second prohibiting consideration of good and substantial reason with respect to Mr. Woollard's application. It is within the Court's discretion to grant a stay of the first paragraph, but not the second. In that case, MSP would promptly process Mr. Woollard's application.

open wearing and carrying of long guns in public, *see* ECF No. 26 at 7. Thus, staying the injunction pending appeal would neither interfere with a core constitutional right nor prevent citizens from keeping and bearing firearms for self-defense either inside or outside the home. The equities to be balanced, therefore, are the plaintiffs' desire to wear and carry, in public, a particular type of firearm—which happens to be the type of firearm most frequently used in criminal activity, *see* ECF No. 26 at 10-14—against the State's significant interest in protecting its citizens from harm flowing from the public carry of that particular weapon by individuals without good and substantial reason to do so.

I. The Failure to Enter a Stay Could Result in Harm to Individuals Eligible to Receive a Permit Under Existing Maryland Law.

The first question posed by the Court is what would happen if the injunction were not stayed pending appeal, but the Fourth Circuit later reversed. If the injunction were not stayed, and the Maryland State Police ("MSP") was therefore precluded from enforcing the good and substantial reason requirement, MSP will not necessarily know who among those receiving permits while the injunction is in effect (the "Interim Period") have good and substantial reason. If the Fourth Circuit were later to reverse, all permits that had been issued to individuals who had not demonstrated good and substantial reason during the Interim Period would be inconsistent with valid Maryland law. MSP, a law enforcement agency, would therefore be required to revoke those permits. Ex. A, Declaration of Marcus Brown, April 18, 2012 ("Brown Decl."), ¶ 5.

In this scenario, the greatest impact of denial of a stay would fall on individuals *with* good and substantial reason to wear and carry a handgun in public, those individuals

who, by definition, have the greatest need for a permit. Although MSP would process applications received from individuals whose permits were revoked as soon as reasonably practicable, there would almost certainly be delays for individuals with good and substantial reason as a result of the likely glut of applications to process. *Id.* ¶ 11.

One particular category of individuals who would be impacted includes those whose good and substantial reason is employment-related, such as security guards, armored car drivers, private detectives, special police officers, and people who need to transport valuable goods for their businesses.[2] *Id.* ¶ 9. Because some of these individuals are required to have a permit as a condition of their employment, revocation of permits could lead to a loss of that employment. *Id.* ¶ 10. Similarly, in the absence of a stay, MSP would not necessarily know which permit recipients fell into other categories of individuals currently eligible for permits, including those who obtain permits because of a demonstrable need for personal protection. *Id.* ¶ 13. For these individuals, the absence of a permit pending the reapplication process could have safety implications. *Id.* ¶ 14.

If there is no stay, MSP would attempt to mitigate these potential consequences by asking, during the Interim Period, that applicants who have good and substantial reason voluntarily provide it and cooperate with MSP's investigation. *Id.* ¶ 6. MSP would not deny permits to individuals who decline to provide any such reason, but would keep records so that if the Fourth Circuit reverses, MSP would not be required to revoke permits of individuals who had demonstrated good and substantial reason. *Id.* However,

[2] There are currently 5,091 permits issued to security guards, armored car drivers, private detectives and special police officers, and 896 permits issued to people who transport valuable items in the regular course of business. Brown Decl. ¶ 9.

in light of the strong feelings surrounding this issue, MSP nonetheless expects that a significant number of applicants who have good and substantial reason may decline to provide it during the Interim Period as a matter of principle. *Id.*

Individuals who lack good and substantial reason would not be eligible for a permit if the Fourth Circuit reverses. Although MSP expects that many such individuals would comply with its directions and return their permits, MSP anticipates that some will not comply. *Id.* ¶ 15. Because it would be impractical for MSP to track down and recover all of the permits that would not be returned, a number of permits would remain in circulation that would appear facially valid, but that had been revoked. *Id.* Police would therefore be significantly hindered in their ability to enforce the law.

Finally, a failure to stay the injunction pending appeal would adversely affect the processing of permit applications for individuals who have good and substantial reason. MSP resources for processing permit applications are already strained, and would become much more so if a large number of new permit applications need to be processed. *Id.* ¶ 16.[3] As a result of the need to comply with State policies with respect to creating new positions, as well as the need to train and certify new employees, it would take a minimum of several months, and possibly much longer, to add a new position to assist with processing applications. *Id.* ¶ 17. Even if new positions are added, processing times

[3] Even if MSP is not required to investigate good and substantial reason, it is still required to investigate whether an applicant satisfies the other requirements of Md. Code Ann., Pub. Safety § 5-306(a), including whether the applicant has exhibited a "propensity for violence or instability." As a result, processing applications will not take significantly less time.

would likely increase, a problem that would particularly affect those who, under existing law, have a demonstrable reason to wear and carry a handgun in public. *Id.* ¶ 17.

II. Evidence Regarding the Impact of "Shall Issue" Laws Supports a Stay.

The defendants previously presented evidence, *inter alia*, as to the problem of handgun violence in Maryland, the conclusions of law enforcement that the good and substantial reason requirement is an important component of the effort to stem that violence, and the importance of the good and substantial reason requirement to public safety. *See generally* ECF No. 26 at 10-16, 34-37. The question now posed by the Court is whether there is data demonstrating the impact on crime of the adoption of "shall issue" handgun permit laws elsewhere. The answer is yes, subject to caveats.

Identifying causal trends in crime data is notoriously difficult in any circumstance because of the multiplicity of variables that impact crime and the different effects of those variables in different places and on different people.[4] While some have claimed that the passage of "shall issue" laws has actually decreased crime, the studies on which those claims are based failed to consider important variables that contribute to crime rates and have failed to hold up under scrutiny.[5] The most prominent such study claiming that "shall issue" laws decrease crime rates is a 1997 study by John Lott and David Mustard.

[4] *See, e.g.*, Alfred Blumstein & Joel Wallman, eds., THE CRIME DROP IN AMERICA, 2 (2006) (extensive analysis of potential factors leading to national drop in crime "leads to the conclusion that there is no single explanation but that a variety of factors, some independent and some interacting in a mutually supportive way, have been important.").

[5] *See, e.g.*, National Research Council, FIREARMS & VIOLENCE: A CRITICAL REVIEW, 150-51 (2004) ("NRC Report") (excerpts at Ex. B); Ex. C, Daniel Webster and Jens Ludwig, *Myths About Defensive Gun Use and Permissive Gun Carry Laws*, Berkeley Media Studies Group, 3-4 (2000) (attributing difference in crime rates to concealed carry law is likely misleading "when in fact part or all of the difference will be due to other unmeasured differences across states").

Numerous studies have since refuted the study's conclusion, taking issue with the methodology, the failure to control for certain important factors influencing crime rates, the failure of the conclusion to hold up when additional years of data were added, the dependence of the conclusion on the experience of only one or two states, and outright errors.[6] Subsequent studies reached the contrary conclusion that passage of "shall issue" laws in fact led to an increase in crime rates.[7]

In 2004, a panel of national experts assembled by the National Research Council of the National Academy of Science undertook to identify any conclusions that could be drawn from the available data with respect to a number of issues related to firearms and violence. *See* Ex. B, NRC Report. The panel concluded that the then-existing data was not sufficient to identify an impact of "shall issue" laws on crime to a scientific certainty. *Id.* at 7-8. The report did not conclude that there is no causal link between adoption of

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[6] *See, e.g.*, Ex. D, Abhay Aneja, John J. Donohue III, Alexandria Zhang, *The Impact of Right-to-Carry Laws and the NRC Report: Lessons for the Empirical Evaluation of Law and Policy*, 13:2 AMERICAN LAW AND ECONOMICS REVIEW 565 (Fall 2011); Ex. B, NRC Report, 2-3, 7, 120-51 (2004); Ian Ayres & John J. Donohue III, *Shooting Down the "More Guns Less Crime" Hypothesis*, 55 STAN. L. REV. 1193 (2003); *see also* Blumstein & Wallman, 327-28 ("[F]ew researchers have been able to corroborate [Lott's] findings, and a number of scholars have shown his studies to be seriously flawed.") For example, one recent analysis of data from 25 different states that had passed "shall issue" laws demonstrated that if data from only two states were excluded, the data from the remaining 23 states showed a "highly pernicious" impact of "shall issue" laws on murder rates. Ex. D, Aneja, Donohue & Zhang at 610-11. Similarly, the conclusions of the original Lott & Mustard analysis appear to be explained much better by the greater impact of the crack cocaine epidemic on crime rates in states that did not adopt "shall issue" laws than by those laws. *Id.* at 601-06.

[7] *See, e.g.*, John J. Donohue, *The Impact of Concealed-Carry Laws*, *in* EVALUATING GUN POLICY EFFECTS ON CRIME AND VIOLENCE 289, 320 (2003) (states enacting "shall issue" laws appear to "experience increases in violent crime, murder, and robbery when [those] laws are adopted"); Jens Ludwig, *Concealed-Gun-Carrying Laws and Violent Crime: Evidence from State Panel Data*, 18 INT'L REV. L. & ECON. 239 (1998) (laws allowing concealed carrying of weapons "have resulted, if anything, in an increase in adult homicide rates").

"shall issue" laws and crime rates, or that identification of such a link is not possible, just that then-current data and studies were not sufficiently robust to do so to a scientific certainty. *Id*. at 150-51.

More recently, in 2011, Aneja, Donohue and Zhang published a study that extensively reviewed the existing data, updated that data, and corrected certain errors in it. Ex. D, Aneja, Donohue & Zhang at 578-615. Although the authors agreed with the NRC Report that without further evidence the available data are not sufficient to identify, to a scientific certainty, a causal link between "shall issue" laws and crime rates, they determined that the conclusion that followed from the updated and corrected data they analyzed was that "shall issue" laws "likely increase the rate of aggravated assaults." *Id.* at 615-16.

In light of the existence of studies identifying a positive correlation between "shall issue" laws and increases in certain crimes, especially aggravated assaults, other studies concluding that further research is needed, and the clear significance of state-specific factors, it is particularly significant that the Maryland General Assembly identified a public safety need for a good and substantial reason requirement, and that Maryland law enforcement officials, among others, have concluded that such a requirement is important to public safety. Notably, as of 2010, Maryland's level of violent crime was the lowest ever recorded for both overall violent crime and homicide. *See* Maryland Governor's Office of Crime Control & Prevention, Maryland 2010 Crime Totals, *available at* http://www.goccp.maryland.gov/msac/crime-statistics.php.

III. Evidence Reveals that Many Permit Holders Commit Crimes, Including Murder.

Although comprehensive data on the law abidingness of permit holders is difficult to obtain because that data is frequently shielded from public view, available information demonstrates that, while the majority of handgun permit holders have not been charged with crimes, many crimes, including murders, are committed by permit holders, particularly permit holders in "shall issue" states.[8] Since May 2007, the Violence Policy Center has used news and police reports to identify 270 non-suicide killings by concealed carry permit holders.[9] The vast majority of these killings were committed by individuals who obtained a concealed carry permit in a "shall issue" state, including the recent killing of Trayvon Martin by Florida concealed permit holder George Zimmerman. *Id.* The report identified only one non-suicide killing by a Maryland permit holder since May 2007, *id.* at 62, whereas two "shall-issue" states that border it—Pennsylvania and Virginia—have had 44 non-suicide killings (22 each) by permit holders, including 6 killings of law enforcement officers. *Id.* at 120-36 & 164-175. Significantly, it was a Virginia concealed carry permit holder who was responsible for the tragic murder-suicide at Johns Hopkins Hospital in 2010. *Id.* at 63.

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[8] At least 28 states have laws or regulations that prevent public access to information about gun owners. *See* Reports Committee for Freedom of the Press, Open Government Guide (2011), *available at*: http://www.rcfp.org/open-government-guide. Other states, such as Virginia, interpret their state public records acts to exempt carry permit data from public inspection. *See* Kelsey M. Swanson, *Comment: The Right to Know: An Approach to Gun Licenses and Public Access to Government Records*, 56 UCLA L. Rev. 1579, 1584-85 (2009); *see* Va. Code § 18.2-308(K). This shielding of data has been a legislative priority for certain advocacy groups. *See, e.g.*, http://www.ammoland.com/tag/gun-owner-privacy/#axzz1s2dHLqb5.

[9] Violence Policy Center, *Total People Killed By Concealed Handgun Permit Holders* (March 2012), *available* at http://www.vpc.org/fact_sht/ccwtotalkilled.pdf.

The limited data that are available from "shall issue" states are not comforting:

- In Florida, 5,021 concealed weapon or firearm license holders had their licenses revoked or suspended due to a disqualifying arrest or domestic violence injunction between July 1, 2010, and June 30, 2011. Florida Dep't of Agric. & Consumer Servs., Div. of Licensing, Concealed Weapon or Firearm License Report (2011), *available at*: http://licgweb.doacs.state.fl.us/stats/07012010_06302011_cw_annual.pdf.

- In Michigan, in the year ending June 30, 2011, 2,711 criminal charges were filed against concealed carry license holders, with four convictions for second-degree murder and 161 convictions for some form of assault (15 for assault with a deadline weapon), and 349 licenses were revoked due to a felony or misdemeanor charge. Michigan State Police, Concealed Pistol Licensure Annual Report 2, 22, 32 & 34 (2011), *available at*: http://www.michigan.gov/documents/msp/2011_CPL_Report_376632_7.pdf.

- Texas, which only reports convictions, reports that 101 license holders were convicted of crimes—including one for murder, four for terroristic threat, three for sexual assault of a child, 19 for deadly conduct and 45 for some other form of assault—in 2009, the most recent year for which data are available. Texas Dep't of Pub. Safety, Reg. Servs. Div., Conviction Rates for Concealed Handgun License Holders (2009), *available at*: http://www.txdps.state.tx.us/administration/crime_records/chl/ConvictionRatesReport2009.pdf. Before Texas law limited reporting to convictions, Texas had reported that license holders were arrested for 5,314 crimes from January 1, 1996 through August 31, 2001. Karen Brock & Marty Langley, *License to Kill IV, More Guns, More Crime*, Violence Policy Center, 2 (2002).

- In Utah, more than 1,000 concealed carry permit holders had their permits revoked just during 2011. Concealed Firearm Permit and Brady Bill Statistical Data (2012), *available at* http://publicsafety.utah.gov/bci/documents/2012Q1.pdf.

These reports, of course, are only as good as the monitoring and reporting mechanisms of the states at issue, and a New York Times investigation of monitoring by one "shall issue" state, North Carolina, found serious and disturbing shortcomings. Ex. E, Michael Luo, *Guns in Public, and Out of Sight*, N.Y. Times, Dec. 26, 2011. The investigation, which identified convictions of felonies or non-traffic misdemeanors by

more than 2,400 North Carolina permit holders between 2007 and 2011, including more than 200 gun- or weapon-related crimes, found that in "about half of the felony convictions, the authorities failed to revoke or suspend the holder's permit, including for cases of murder, rape and kidnapping." *Id.* There is no reason to believe monitoring in these other states is any better. In fact, reporting by the Florida Sentinel in 2007 found the opposite to be true in Florida. *See* Exs. F, G (finding that licenses were issued to hundreds of people found "responsible for assaults, burglaries, sexual battery, drug possession, child molestation – even homicide").

CONCLUSION

The defendants request that the Court stay the effect of its March 5 Order (ECF No. 53), as amended by its March 30 Order (ECF No. 63), pending appeal to the United States Court of Appeals for the Fourth Circuit.

Respectfully submitted,

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April 19, 2012

Attorneys for Defendants

EXHIBIT A:

Declaration of Marcus Brown

IN THE UNITED STATES DISTRICT COURT
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RAYMOND WOOLLARD, *et al.*,	*	
Plaintiffs,	*	
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MARCUS BROWN, *et al.*,	*	
Defendants.	*	

* * * * * * * * * * * * *

DECLARATION OF MARCUS L. BROWN

I, Marcus L. Brown, am competent to state and testify to the following, based on my personal knowledge.

1. I am the Secretary of the Maryland State Police ("MSP"), a position I have held since August 1, 2011.

2. As Secretary, my responsibilities include supervising and directing the affairs and operation of the MSP.

3. I have been informed by counsel that the Court has asked what difficulties would arise if there is no stay of the Court's injunction pending appeal, but the injunction is later overturned by an appellate court. This declaration is submitted in response to that question, and is therefore based on the hypothetical scenario posed by the Court. I refer to the time between when the injunction would go into effect and the date of a ruling by the appellate court overturning the injunction as the "Interim Period."

4. It is my understanding that if a stay is not granted pending appeal, the injunction entered by the Court would prohibit the MSP from enforcing the requirement that an applicant for a handgun wear and carry permit demonstrate a "good and substantial reason" to wear, carry, and transport a handgun in public in Maryland ("GSR"). In that case, the MSP would not necessarily know whether a recipient of a handgun wear and carry permit during the Interim Period had GSR.

5. In the event the MSP is precluded during the Interim Period from enforcing the GSR requirement, and the injunction is then overturned on appeal, the MSP would be obligated to revoke any permit issued during the Interim Period where GSR was not considered. Under those circumstances, Maryland law would once again require GSR as a condition for having a lawful handgun wear and carry permit, and the MSP could not, consistent with its obligation to uphold the laws of Maryland, allow permits issued in violation of what would then be a valid law to remain outstanding.

6. In the absence of a stay during the Interim Period, the MSP would attempt to avoid the need to revoke permits of individuals who have GSR by giving applicants the option of identifying a GSR, and cooperating with MSP's investigation of that GSR, voluntarily. The MSP would not deny permits to individuals who decline to provide any such reason or who fail to demonstrate GSR, but would keep records of individuals who do demonstrate GSR so that if the appellate court vacates the injunction, the MSP would not be required to revoke permits of individuals who had demonstrated GSR during the Interim Period. Nevertheless, in light of the strong feelings surrounding this issue, the

MSP expects that a significant number of applicants who have GSR will decline to provide it during the application process as a matter of principle.

7. Revoking permits would involve sending letters to all recipients of permits during the Interim Period who had not demonstrated GSR, informing each that their permit had been revoked and demanding that they immediately return the permit to MSP. Those individuals would be informed that they could re-apply for a new permit, subject to a determination of GSR.

8. For purposes of discussing the effect of revocation, people who will get permits during the Interim Period would fall into one of three broad categories:

a. Category 1: Individuals who, in the absence of an injunction, would have received a permit restricted to the activities providing their GSR, such as while working as a security guard, armored car driver, private detective, special police officer, or engaged in a business requiring that person to transport valuable items. During the Interim Period, those individuals would presumably qualify for unrestricted permits.[1]

b. Category 2: Individuals who, in the absence of an injunction, would have received an unrestricted permit because the nature of their GSR. This category includes individuals who demonstrate a need for personal protection, including those who have been threatened and certain police officers, correctional officers, judges, prosecutors, and public defenders.

[1] By "unrestricted" I mean permits that are not limited by the individual's GSR. Permit holders would remain subject to generally-applicable restrictions such as, for example, prohibitions under State law from carrying guns on public school property.

During the Interim Period, they would also receive an unrestricted permit; and

c. Category 3: Individuals who, in the absence of an injunction, would not have received a permit because they lack GSR.

9. Individuals falling within Category 1 are individuals who would be eligible for a permit under existing law, but subject to restrictions tailored to their GSR. *See* Md. Code Ann., Pub. Safety § 5-307(b) (permitting authority to "limit the geographic area, circumstances, or times of the day, week, month, or year in which a [handgun wear and carry] permit is effective"). This would include security guards sponsored by their employers, private detectives, armored car drivers, special police officers, and people in business who transport valuable items in the regular course of business. There are currently 5,091 permits issued to security guards, private detectives, armored car drivers, and special police officers, and 896 to people in business who transport valuable items in the regular course of business.

10. If MSP is not permitted to enforce the GSR requirement during the Interim Period, it will not necessarily know who falls into Category 1 and will be obligated to revoke all permits issued during the Interim Period to individuals who had not voluntarily demonstrated GSR. For individuals in Category 1, this could have potentially significant consequences, including the loss of employment if their jobs require the wear and carry of a handgun.

11. Although the MSP would process new applications as promptly as reasonably possible after revoking permits issued during the Interim Period, it is expected

that there will be a significant number of applications that need to be processed and limited resources available to process them, including conducting the necessary investigations into GSR. The average processing time for an initial permit application is currently 2-3 months, and the average processing time for a renewal permit application is currently 45 days. It is anticipated that the processing times following the Interim Period would be significantly longer, which will compound the problems associated with revocation for these individuals who, by definition, have GSR.

12. A further complication with respect to individuals in Category 1 who do voluntarily demonstrate GSR during the Interim Period is that the MSP would then be required to impose GSR-related restrictions after the Interim Period, which would require recovering the unrestricted permits and re-issuing new, restricted permits.

13. Individuals in Category 2 are individuals who have GSR that would qualify them for an unrestricted permit under the existing wear-and-carry permit statute. This would include individuals whose GSR is "personal protection," including individuals who have been threatened and those whose professions put them at heightened risk at all times—including certain police officers, correctional officers, judges, prosecutors, and public defenders. If the MSP is prohibited from enforcing GSR during the Interim Period, it would not necessarily know which permit recipients fall into this category, and would therefore have to revoke all permits issued without consideration of GSR.

14. Again, although the MSP would process applications as promptly as reasonably possible after the Interim Period, it is expected that there will be a significant number of applications that need to be processed and limited resources available to

process them, including conducting the necessary investigations into GSR. That, in turn, will lead to an increase in processing times that will compound the problems associated with revocation for these individuals who, by definition, have GSR.

15. With respect to individuals falling within Category 3, those without GSR, none of them would qualify for a permit under existing law. MSP expects that many such individuals will comply with the demand to return the permits, but that many others will not. It would be impractical for MSP to track down and recover every permit that would not be returned, so a number of permits would remain on the streets. That would hinder the ability of law enforcement officers to enforce Maryland law regarding the unlawful carrying of handguns in public because a number of facially-valid permits would remain in circulation. A law enforcement officer who encounters someone holding one of these permits will not necessarily have any reasonably practicable way of discerning its invalidity.

16. Enforcing the injunction during the Interim Period would also have significant repercussions for MSP resources. MSP resources available for processing handgun wear and carry permits, including both personnel and equipment, are already strained, and would be more so following an expected increase in the number of permit applications if the injunction is enforced during the Interim Period.

17. As a result of the need to comply with State policies with respect to creating new positions, as well as the need to train and obtain certifications for new employees in this area, it would take a minimum of several months, and possibly much longer, to add a new position to assist with processing applications. Even if new

positions are ultimately added, the MSP will likely face significantly increased processing times for applications until that can happen. During the Interim Period, those delays will most impact individuals with GSR, those who, by definition, are most in need of a permit.

18. Finally, if a stay is not granted during the Interim Period, and an appellate court later vacates the injunction, the need to revoke permits of individuals who have not demonstrated GSR, and to impose GSR-related restrictions on other permits, will likely lead to a significant increase in appeals to the Handgun Permit Review Board. That will further tax the resources of both the MSP and the Handgun Permit Review Board.

Pursuant to 28 U.S.C. § 1746, I declare under penalty of perjury that the foregoing is true and correct.

Executed on April 18, 2012, Baltimore, Maryland

Marcus L. Brown

EXHIBIT B:

Excerpts From National Research Council, Firearms & Violence: A Critical Review (2004)

FIREARMS AND VIOLENCE

A CRITICAL REVIEW

Committee to Improve Research Information and Data on Firearms
Charles F. Wellford, John V. Pepper, and Carol V. Petrie, editors
Committee on Law and Justice
Division of Behavioral and Social Sciences and Education

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Executive Summary

There is hardly a more contentious issue in American politics than the ownership of guns and various proposals for gun control. Each year tens of thousands of people are injured and killed by firearms; each year firearms are used to defend against and deter an unknown number of acts of violence; and each year firearms are widely used for recreational purposes. For public authorities to make reasonable policies on these matters, they must take into account conflicting constitutional claims and divided public opinion as well as facts about the relationship between guns and violence. And in doing so they must try to strike what they regard as a reasonable balance between the costs and the benefits of private gun ownership.

Adequate data and research are essential to judge both the effects of firearms on violence and the effects of different violence control policies. Those judgments are key to many important policy questions, among them: Should regulations restrict who may possess and carry a firearm? Should regulations differ for different types of firearms? Should purchases be delayed and, if so, for how long and under what circumstances? Should restrictions be placed on the number or types of firearms that can be purchased? Should safety locks be required? While there is a large body of empirical research on firearms and violence, there is little consensus on even the basic facts about these important policy issues.

Given the importance of these issues and the continued controversy surrounding the debate on firearms, the Committee to Improve Research Information and Data on Firearms was charged with providing an assessment of the strengths and limitations of the existing research and data on gun violence

and identifying important gaps in knowledge; describing new methods to put research findings and data together to support the design and implementation of improved prevention, intervention, and control strategies for reducing gun-related crime, suicide, and accidental fatalities; and utilizing existing data and research on firearms and firearm violence to develop models of illegal firearms markets. The charge also called for examining the complex ways in which firearm violence may become embedded in community life and considering whether firearm-related homicide and suicide have become accepted as ways of resolving problems, especially among youth. However, there is a lack of empirical research to address these two issues.

MAJOR CONCLUSIONS

Empirical research on firearms and violence has resulted in important findings that can inform policy decisions. In particular, a wealth of descriptive information exists about the prevalence of firearm-related injuries and deaths, about firearms markets, and about the relationships between rates of gun ownership and violence. Research has found, for example, that higher rates of household firearms ownership are associated with higher rates of gun suicide, that illegal diversions from legitimate commerce are important sources of crime guns and guns used in suicide, that firearms are used defensively many times per day, and that some types of targeted police interventions may effectively lower gun crime and violence. This information is a vital starting point for any constructive dialogue about how to address the problem of firearms and violence.

While much has been learned, much remains to be done, and this report necessarily focuses on the important unknowns in this field of study. The committee found that answers to some of the most pressing questions cannot be addressed with existing data and research methods, however well designed. For example, despite a large body of research, the committee found no credible evidence that the passage of right-to-carry laws decreases or increases violent crime, and there is almost no empirical evidence that the more than 80 prevention programs focused on gun-related violence have had any effect on children's behavior, knowledge, attitudes, or beliefs about firearms. The committee found that the data available on these questions are too weak to support unambiguous conclusions or strong policy statements.

Drawing causal inferences is always complicated and, in the behavioral and social sciences, fraught with uncertainty. Some of the problems that the committee identifies are common to all social science research. In the case of firearms research, however, the committee found that even in areas in which the data are potentially useful, the complex methodological prob-

lems inherent in unraveling causal relationships between firearms policy and violence have not been fully considered or adequately addressed.

Nevertheless, many of the shortcomings described in this report stem from the lack of reliable data itself rather than the weakness of methods. In some instances—firearms violence prevention, for example—there are no data at all. Even the best methods cannot overcome inadequate data and, because the lack of relevant data colors much of the literature in this field, it also colors the committee's assessment of that literature.

DATA RECOMMENDATIONS

If policy makers are to have a solid empirical and research base for decisions about firearms and violence, the federal government needs to support a systematic program of data collection and research that specifically addresses that issue. Adverse outcomes associated with firearms, although large in absolute numbers, are statistically rare events and therefore are not observed with great frequency, if at all, in many ongoing national probability samples (i.e., on crime victimization or health outcomes). The existing data on gun ownership, so necessary in the committee's view to answering policy questions about firearms and violence, are limited primarily to a few questions in the General Social Survey. There are virtually no ongoing, systematic data series on firearms markets. Aggregate data on injury and ownership can only demonstrate associations of varying strength between firearms and adverse outcomes of interest. Without improvements in this situation, the substantive questions in the field about the role of guns in suicide, homicide and other crimes, and accidental injury are likely to continue to be debated on the basis of conflicting empirical findings.

Emerging Data Systems on Violent Events

The committee reinforces recommendations made by past National Research Council committees and others to support the development and maintenance of the National Violent Death Reporting System and the National Incident-Based Reporting System. These data systems are designed to provide information that characterizes violent events. No single system will provide data that can answer all policy questions, but the necessary first step is to collect accurate and reliable information to describe the basic facts about violent injuries and deaths. The committee is encouraged by the efforts of the Harvard School of Public Health's Injury Control Research Center pilot data collection program and the recent seed money provided to implement a Violent Death Reporting System at the Centers for Disease Control and Prevention.

Ownership Data

The inadequacy of data on gun ownership and use is among the most critical barriers to better understanding of gun violence. Such data will not by themselves solve all methodological problems. However, its almost complete absence from the literature makes it extremely difficult to understand the complex personality, social, and circumstantial factors that intervene between a firearm and its use. Also difficult to understand is the effect, if any, of programs designed to reduce the likelihood that a firearm will cause unjustified harm, or to investigate the effectiveness of firearm use in self-defense. We realize that many people have deeply held concerns about expanding the government's knowledge of who owns guns and what type of guns they own. We also recognize the argument that some people may refuse to supply such information in any system, especially those who are most likely to use guns illegally. **The committee recommends a research effort to determine whether or not these kinds of data can be accurately collected with minimal risk to legitimate privacy concerns.**

A starting point is to assess the potential of ongoing surveys. For example, efforts should be undertaken to assess whether tracing a larger fraction of guns used in crimes, regularly including questions on gun access and use in surveys and longitudinal studies (as is done in data from the ongoing, yearly Monitoring the Future survey), or enhancing existing items pertaining to gun ownership in ongoing national surveys may provide useful research data. To do this, researchers need access to the data. **The committee recommends that appropriate access be given to data maintained by regulatory and law enforcement agencies, including the trace data maintained by the Bureau of Alcohol, Tobacco, and Firearms; registration data maintained by the Federal Bureau of Investigation and state agencies; and manufacturing and sales data for research purposes.**

In addition, researchers need appropriate access to the panel data from the Monitoring the Future survey. These data may or may not be useful for understanding firearms markets and the role of firearms in crime and violence. However, without access to these systems, researchers are unable to assess their potential for providing insight into some of the most important firearms policy and research questions. Concerns about security and privacy must be addressed in the granting of greater access to these data, and the systems will need to be continually improved to make them more useful for research. Nevertheless, there is a long-established tradition of making sensitive data available with appropriate safeguards to researchers.

Methodological Approaches

Difficult methodological issues exist regarding how different data sets might be used to credibly answer the complex causal questions of interest.

The committee recommends that a methodological research program be established to address these problems. The design for data collection and analysis should be selected in light of particular research questions. For example, how, if at all, could improvements in current data, such as firearms trace data, be used in studies of the effects of policy interventions on firearms markets or any other policy issue? What would the desired improvements contribute to research on policy interventions for reducing firearms violence? Linking the research and data questions will help define the data that are needed. **We recommend that the results of such research be regularly reported in the scientific literature and in forums accessible to investigators.**

RESEARCH RECOMMENDATIONS

Firearms, Criminal Violence, and Suicide

Despite the richness of descriptive information on the associations between firearms and violence at the aggregate level, explaining a violent death is a difficult business. Personal temperament, the availability of weapons, human motivation, law enforcement policies, and accidental circumstances all play a role in leading one person but not another to inflict serious violence or commit suicide.

Because of current data limitations, researchers have relied primarily on two different methodologies. First, some studies have used case-control methods, which match a sample of cases, namely victims of homicide or suicide, to a sample of controls with similar characteristics but who were not affected by violence. Second, some "ecological" studies compare homicide or suicide rates in large geographic areas, such as counties, states, or countries, using existing measures of ownership.

Case-control studies show that violence is positively associated with firearms ownership, but they have not determined whether these associations reflect causal mechanisms. Two main problems hinder inference on these questions. First and foremost, these studies fail to address the primary inferential problems that arise because ownership is not a random decision. For example, suicidal persons may, in the absence of a firearm, use other means of committing suicide. Homicide victims may possess firearms precisely because they are likely to be victimized. Second, reporting errors regarding firearms ownership may systemically bias the results of estimated associations between ownership and violence.

Ecological studies currently provide contradictory evidence on violence and firearms ownership. For example, in the United States, suicide appears to be positively associated with rates of firearms ownership, but homicide is not. In contrast, in comparisons among countries, the association between

rates of suicide and gun ownership is nonexistent or very weak but there is a substantial association between gun ownership and homicide. These cross-country comparisons reflect the fact that the suicide rate in the United States ranks toward the middle of industrialized countries, whereas the U.S. homicide rate is much higher than in all other developed countries.

The committee cannot determine whether these associations demonstrate causal relationships. There are three key problems. First, as noted above, these studies do not adequately address the problem of self-selection. Second, these studies must rely on proxy measures of ownership that are certain to create biases of unknown magnitude and direction. Third, because the ecological correlations are at a higher geographic level of aggregation, there is no way of knowing whether the homicides or suicides occurred in the same areas in which the firearms are owned.

In summary, the committee concludes that existing research studies and data include a wealth of descriptive information on homicide, suicide, and firearms, but, because of the limitations of existing data and methods, do not credibly demonstrate a causal relationship between the ownership of firearms and the causes or prevention of criminal violence or suicide. The issue of substitution (of the means of committing homicide or suicide) has been almost entirely ignored in the literature. What sort of data and what sort of studies and improved models would be needed in order to advance understanding of the association between firearms and suicide? Although some knowledge may be gained from further ecological studies, the most important priority appears to the committee to be individual-level studies of the association between gun ownership and violence. Currently, no national surveys on ownership designed to examine the relationship exist. **The committee recommends support of further individual-level studies of the link between firearms and both lethal and nonlethal suicidal behavior.**

Deterrence and Defense

Although a large body of research has focused on the effects of firearms on injury, crime, and suicide, far less attention has been devoted to understanding the defensive and deterrent effects of firearms. Firearms are used by the public to defend against crime. Ultimately, it is an empirical question whether defensive gun use and concealed weapons laws generate net social benefits or net social costs.

Defensive Gun Use

Over the past decade, a number of researchers have conducted studies to measure the prevalence of defensive gun use in the population. However, disagreement over the definition of defensive gun use and uncertainty over the

accuracy of survey responses to sensitive questions and the methods of data collection have resulted in estimated prevalence rates that differ by a factor of 20 or more. These differences in the estimated prevalence rates indicate either that each survey is measuring something different or that some or most of them are in error. Accurate measurement on the extent of defensive gun use is the first step for beginning serious dialogue on the efficacy of defensive gun use at preventing injury and crime.

For such measurement, the committee recommends that a research program be established to (1) clearly define and understand what is being measured, (2) understand inaccurate response in the national gun use surveys, and (3) apply known methods or develop new methods to reduce reporting errors to the extent possible. A substantial research literature on reporting errors in other contexts, as well as well-established survey sampling methods, can and should be brought to bear to evaluate these response problems.

Right-to-Carry Laws

A total of 34 states have laws that allow qualified adults to carry concealed handguns. Right-to-carry laws are not without controversy: some people believe that they deter crimes against individuals; others argue that they have no such effect or that they may even increase the level of firearms violence. This public debate has stimulated the production of a large body of statistical evidence on whether right-to-carry laws reduce or increase crimes against individuals.

However, although all of the studies use the same basic conceptual model and data, the empirical findings are contradictory and in the committee's view highly fragile. Some studies find that right-to-carry laws reduce violent crime, others find that the effects are negligible, and still others find that such laws increase violent crime. The committee concludes that it is not possible to reach any scientifically supported conclusion because of (a) the sensitivity of the empirical results to seemingly minor changes in model specification, (b) a lack of robustness of the results to the inclusion of more recent years of data (during which there were many more law changes than in the earlier period), and (c) the statistical imprecision of the results. The evidence to date does not adequately indicate either the sign or the magnitude of a causal link between the passage of right-to-carry laws and crime rates. Furthermore, this uncertainty is not likely to be resolved with the existing data and methods. If further headway is to be made, in the committee's judgment, new analytical approaches and data are needed. (One committee member has dissented from this view with respect to the effects of these laws on homicide rates; see Appendix A.)

Interventions to Reduce Violence and Suicide

Even if it were to be shown that firearms are *a* cause of lethal violence, the development of successful programs to reduce such violence would remain a complex undertaking, because such interventions would have to address factors other than the use of a gun. Three chapters in this report focus specifically on what is known about various interventions aimed at reducing firearms violence by restricting access, or implementing prevention programs, or implementing criminal justice interventions. These chapters focus largely on what is known about the effects of different interventions on criminal violence. Although suicide prevention rarely has been the basis for public support of the passage of specific gun laws, such laws could have unintended effects on suicide rates or unintended by-products. **Thus, in addition to the recommendations related to firearms and crime below, the committee also recommends further studies of the link between firearms policy and suicide.**

Restricting Access

Firearms are bought and sold in markets, both formal and informal. To some observers this suggests that one method for reducing the burden of firearm injuries is to intervene in these markets so as to make it more expensive, inconvenient, or legally risky to obtain firearms for criminal use or suicide. Market-based interventions intended to reduce access to guns by criminals and other unqualified persons include taxes on weapons and ammunition, tough regulation of federal firearm licensees, limits on the number of firearms that can be purchased in a given time period, gun bans, gun buy-backs, and enforcement of laws against illegal gun buyers or sellers.

Because of the pervasiveness of guns and the variety of legal and illegal means of acquiring them, it is difficult to keep firearms from people barred by law from possessing them. The key question is substitution. In the absence of the pathways currently used for gun acquisition, could individuals have obtained alternative weapons with which they could have wrought equivalent harm? Substitution can occur in many dimensions: offenders can obtain different guns, they can get them from different places, and they can get them at different times.

Arguments for and against a market-based approach are now largely based on speculation, not on evidence from research. It is simply not known whether it is actually possible to shut down illegal pipelines of guns to criminals nor the costs of doing so. Answering these questions is essential to knowing whether access restrictions are a possible public policy. The committee has not attempted to identify specific interventions, research strategies, or data that might be suited to studying market interventions, substitu-

tion, and firearms violence. **Rather, the committee recommends that work be started to think carefully about possible research and data designs to address these issues.**

Prevention Programs and Technology

Firearm violence prevention programs are disseminated widely in U.S. public school systems to children ages 5 to 18, and safety technologies have been suggested as an alternative means to prevent firearm injuries. The actual effects of a particular prevention program on violence and injury, however, have been little studied and are difficult to predict. For children, firearm violence education programs may result in *increases* in the very behaviors they are designed to prevent, by enhancing the allure of guns for young children and by establishing a false norm of gun-carrying for adolescents. Likewise, even if perfectly reliable, technology that serves to reduce injury among some groups may lead to increased deviance or risk among others.

The committee found little scientific basis for understanding the effects of different prevention programs on the rates of firearm injuries. Generally, there has been scant funding for evaluation of these programs. For the few that have been evaluated, there is little empirical evidence of positive effects on children's knowledge, attitudes, beliefs, or behaviors. Likewise, the extent to which different technologies affect injuries remains unknown. Often, the literature is entirely speculative. In other cases, for example the empirical evaluations of child access prevention (CAP) laws, the empirical literature reveals conflicting estimates that are difficult to reconcile.

In light of the lack of evidence, the committee recommends that firearm violence prevention programs should be based on general prevention theory, that government programs should incorporate evaluation into implementation efforts, and that a sustained body of empirical research be developed to study the effects of different safety technologies on violence and crime.

Criminal Justice Interventions

Policing and sentencing interventions have had recent broad bipartisan support and are a major focus of current efforts to reduce firearms violence. These policies generally do not affect the ability of law-abiding citizens to keep guns for recreation or self-defense, and they have the potential to reduce gun violence by deterring or incapacitating violent offenders. Descriptive accounts suggest that some of these policies may have had dramatic crime-reducing effects: homicide rates fell dramatically after the implementation of Boston's targeted policing program, Operation Ceasefire, and Richmond's sentencing enhancement program, Project Exile.

Despite these apparent associations between crime and policing policy, however, the available research evidence on the effects of policing and sentencing enhancements on firearm crime is limited and mixed. Some sentencing enhancement policies appear to have modest crime-reducing effects, while the effects of others appear to be negligible. The limited evidence on Project Exile suggests that it has had almost no effect on homicide. Several city-based quasi-random interventions provide favorable evidence on the effectiveness of targeted place-based gun and crime suppression patrols, but this evidence is both application-specific and difficult to disentangle. Evidence on Operation Ceasefire, perhaps the most frequently cited of all targeted policing efforts to reduce firearms violence, is limited by the fact that it is a single case at a specific time and location. Scientific support for the effectiveness of the Boston Gun Project and most other similar types of targeted policing programs is still evolving.

The lack of research on these potentially important kinds of policies is an important shortcoming in the body of knowledge on firearms injury interventions. These programs are widely viewed as effective, but in fact knowledge of whether and how they reduce crime is limited. Without a stronger research base, policy makers considering adoption of similar programs in other settings must make decisions without knowing the true benefits and costs of these policing and sentencing interventions.

The committee recommends that a sustained, systematic research program be conducted to assess the effect of targeted policing and sentencing aimed at firearms offenders. Additional insights may be gained from using observational data from different applications, especially if combined with more thoughtful behavioral models of policing and crime. City-level studies on the effect of sentencing enhancement policies need to engage more rigorous methods, such as pooled time-series cross-sectional studies that allow the detection of short-term impacts while controlling for variation in violence levels across different areas as well as different times. Another important means of assessing the impact of these types of targeted policing and sentencing interventions would be to conduct randomized experiments to disentangle the effects of the various levers, as well as to more generally assess the effectiveness of these targeted policing programs.

6

Right-to-Carry Laws

This chapter is concerned with the question of whether violent crime is reduced through the enactment of *right-to-carry-laws,* which allow individuals to carry concealed weapons.[1] In all, 34 states have right-to-carry laws that allow qualified adults to carry concealed handguns. Proponents of these laws argue that criminals are deterred by the knowledge that potential victims may be carrying weapons and therefore that the laws reduce crime. However, it is not clear a priori that such deterrence occurs. Even if it does, there may be offsetting adverse consequences. For example, increased possession of firearms by potential victims may motivate more criminals to carry firearms and thereby increase the amount of violence that is associated with crime. Moreover, allowing individuals to carry concealed weapons may increase accidental injuries or deaths or increase shootings during arguments. Ultimately, it is an empirical question whether allowing individuals to carry concealed weapons generates net social benefits or net social costs.

The statistical analysis of the effects of these laws was initiated by John Lott and David Mustard (1997) and expanded by Lott (2000) and Bronars and Lott (1998) (hereinafter referred to simply as Lott). Lott concludes that the adoption of right-to-carry laws substantially reduces the prevalence of violent crime. Many other researchers have carried out their own statistical analyses using Lott's data, modified versions of Lott's data, or expanded

[1]The laws are sometimes called *shall-issue* laws because they require local authorities to issue a concealed-weapons permit to any qualified adult who requests one. A qualified adult is one who does not have a significant criminal record or history of mental illness. The definition of a nonqualified adult varies among states but includes adults with prior felony convictions, drug charges, or commitments to mental hospitals.

data sets that cover the more recent time period not included in the original analysis.[2]

Because the right-to-carry issue is highly controversial, has received much public attention, and has generated a large volume of research, the committee has given it special attention in its deliberations. This chapter reviews the existing empirical evidence on the issue. We also report the results of our own analyses of the data. We conclude that, in light of (a) the sensitivity of the empirical results to seemingly minor changes in model specification, (b) a lack of robustness of the results to the inclusion of more recent years of data (during which there are many more law changes than in the earlier period), and (c) the imprecision of some results, it is impossible to draw strong conclusions from the existing literature on the causal impact of these laws. Committee member James Q. Wilson has written a dissent that applies to Chapter 6 only (Appendix A), and the committee has written a response (Appendix B).

DESCRIPTION OF THE DATA AND METHODS

Researchers studying the effects of right-to-carry laws have used many different models. However, all of the analyses rely on similar data and methodologies. Accordingly, we do not attempt to review and evaluate each of the models used in this literature. Instead, we describe the common data used and

[2]Two other general responses to Lott's analysis deserve brief mention. First, some critics have attempted to discredit Lott's findings on grounds of the source of some of his funding (the Olin Foundation), the methods by which some of his results were disseminated (e.g., some critics have claimed, erroneously, that Lott and Mustard, 1997, was published in a student-edited journal that is not peer reviewed), and positions that he has taken on other public policy issues related to crime control. Much of this criticism is summarized and responded to in Chapter 7 of Lott (2000). The committee's view is that these criticisms are not helpful for evaluating Lott's data, methods, or conclusions. Lott provides his data and computer programs to all who request them, so it is possible to evaluate his methods and results directly. In the committee's view, Lott's funding sources, methods of disseminating his results, and opinions on other issues do not provide further information about the quality of his research on right-to-carry laws.

A second group of critics have argued that Lott's results lack credibility because they are inconsistent with various strongly held a priori beliefs or expectations. For example, Zimring and Hawkins (1997:59) argue that "large reductions in violence [due to right-to-carry laws] are quite unlikely because they would be out of proportion to the small scale of the change in carrying firearms that the legislation produced." The committee agrees that it is important for statistical evidence to be consistent with established facts, but there are no such facts about whether right-to-carry laws can have effects of the magnitudes that Lott claims. The beliefs or expectations of Lott's second group of critics are, at best, hypotheses whose truth or falsehood can only be determined empirically. Moreover, Lott (2000) has argued that there are ways to reconcile his results with the beliefs and expectations of the critics. This does not necessarily imply that Lott is correct and his critics are wrong. The correctness of Lott's arguments is also an empirical question about which there is little evidence. Rather, it shows that little can be decided through argumentation over a priori beliefs and expectations.

focus on the common methodological basis for all of them. In particular, we use the results presented in Tables 4.1 and 4.8 of Lott (2000) to illustrate the discussion. We refer to these as the "dummy variable" and "trend" model estimates, respectively. Arguably, these tables, which are reproduced in Table 6-1 and Table 6-2, contain the most important results in this literature.

Data

The basic data set used in the literature is a county-level panel on annual crime rates, along with the values of potentially relevant explanatory variables. Early studies estimated models on data for 1977-1992, while more recent studies (as well as our replication exercise below) use data up to 2000. Between 1977 and 1992, 10 states adopted right-to-carry laws.[3] A total of 8 other states adopted right-to-carry laws before 1977. Between 1992 and 1999, 16 additional states adopted such laws.

The data on crime rates were obtained from the FBI's Uniform Crime Reports (UCR). Explanatory variables employed in studies include the arrest rate for the crime category in question, population density in the county, real per capita income variables, county population, and variables for the percent of population that is in each of many race-by-age-by-gender categories. The data on explanatory variables were obtained from a variety of sources (Lott, 2000: Appendix 3).

Although most studies use county-level panels on crime rates and demographic variables, the actual data files used differ across studies in ways that sometimes affect the estimates. The data set used in the original Lott study has been lost, although Lott reconstructed a version of the data, which he made available to other researchers as well as the committee. This data set, which we term the "*revised original data set,*" covers the period 1977-1992.[4] More recently, Lott has made available a data set covering the

[3]There is some disagreement over when and whether particular states have adopted right-to-carry laws. Lott and Mustard, for example, classify North Dakota and South Dakota as having adopted such laws prior to 1977, but Vernick and Hepburn (2003) code these states as having adopted them in 1985. Likewise, Lott and Mustard classify Alabama and Connecticut as right-to-carry states adopting prior to 1977, yet Vernick codes these states as not having right-to-carry laws. See Ayres and Donohue (2003a:1300) for a summary of the coding conventions on the adoption dates of right-to-carry laws.

[4]There are 3,054 counties observed over 16 years in the revised original data. In the basic specifications, there are a number of sample restrictions, the most notable of which is to drop all counties with no reported arrest rate (i.e., counties with no reported crime). This restricts the sample to approximately 1,650 counties per year (or approximately 26,000 county-year observations). In specifications that do not involve the arrest rate, Lott treats zero crime as 0.1 so as not to take the log of 0. Black and Nagin (1998) further restrict the sample to counties with populations of at least 100,000, which limits the sample to 393 counties per year. In some regressions, Duggan (2001) and Plassmann and Tideman (2001) estimate models that include data on the over 2,900 counties per year with nonmissing crime data.

period 1977-2000 that corrects acknowledged errors in data files used by Plassmann and Whitley (2003). We term this file the *"revised new data set."* [5] We make use of both of these data sets in our replication exercises.

Dummy Variable Model

For expository purposes it is helpful to begin by discussing the dummy variable model without "control" variables.[6] The model (in Lott, 2000: Table 4.1) allows each county to have its own crime level in each category. Moreover, the crime rate is allowed to vary over time in a pattern that is common across all counties in the United States. The effect of a right-to-carry law is measured as a change in the level of the crime rate in a jurisdiction following the jurisdiction's adoption of the law. Any estimate of a policy effect requires an assumption about the "counterfactual," in this case what would have happened to crime rates in the absence of the change in the law. The implicit assumption underlying this simple illustrative dummy variable model is that, in the absence of the change in the law, the crime rate in each county would, on average, have been the county mean plus a time-period adjustment reflecting the common trend in crime rates across all counties.

Dummy variable models estimated in the literature are slightly more complicated than the above-described model. First, they typically include control variables that attempt to construct a more realistic counterfactual. For example, if crime rates vary over time with county economic conditions, then one can construct a more credible estimate of what would have happened in the absence of the law change by including the control variables as a determinant of the crime rate. Most estimates in the literature use a large number of control variables, including local economic conditions, age-gender population composition, as well as arrest rates.

Second, some estimates in the literature model the time pattern of crime differently. In particular, some studies allow each region of the country to have its own time pattern, thereby assuming that in the absence of the law change, counties in nearby states would have the same time pattern of crime rates in a crime category. We term this the "region-interacted time pattern model," in contrast to the "common time pattern" dummy variable model above.

[5]These data were downloaded by the committee from www.johnlott.org on August 22, 2003.

[6]This no-control model is often used as a way to assess whether there is an association between the outcome (crime) and the law change in the data. The committee estimates and evaluates this model below (see Tables 6-5 and 6-6, rows 2 and 3).

Mathematically, the common time pattern dummy variable model takes the form

$$(6.1)\quad Y_{it} = \sum_{t=1977}^{1992} \alpha_t YEAR_t + \beta X_{it} + \delta LAW_{it} + \gamma_i + \varepsilon_{it} \ ,$$

where Y_{it} is the natural logarithm of the number of crimes per 100,000 population in county i and year t, $YEAR_t = 1$ if the year is t and $YEAR_t = 0$ otherwise, X_{it} is a set of control variables that potentially influence crime rates, $LAW_{it} = 1$ if a right-to-carry law was in effect in county i and year t and $LAW_{it} = 0$ otherwise, γ_i is a constant that is specific to county i, and ε_{it} is an unobserved random variable. The quantities α_t, β, and δ are coefficients that are estimated by fitting the model to data. The coefficient δ measures the percentage change in crime rates due to the adoption of right-to-carry laws. For example, if $\delta = -0.05$ then the implied estimate of the adoption of a right-to-carry law is to reduce the crime rate by 5 percent. The coefficients α_t measure common time patterns across counties in crime rates that are distinct from the enactment of right-to-carry laws or other variables of the model.

The vector X_{it} includes the control variables that may influence crime rates, such as indicators of income and poverty levels; the density, age distribution, and racial composition of a county's population; arrest rates; and indicators of the size of the police force. The *county fixed effect* γ_i captures systematic differences across counties that are not accounted for by the other variables of the model and do not vary over time. The values of the parameters α_t, β, and δ are estimated separately for each of several different types of crimes. Thus, the model accounts for the possibility that right-to-carry laws may affect different crimes differently.

Trend Model

While the dummy variable model measures the effect of the adoption of a right-to-carry law as a one-time shift in crime rates, one can alternatively estimate the effect as the change in time trends. The following trend model, which generated the results in Lott's Table 4.8, allows right-to-carry laws to affect trends in crime:

$$(6.2)\quad Y_{it} = \sum_{t=1977}^{1992} \alpha_t YEAR_t + \beta X_{it} + \delta_B YRBEF_{it} + \delta_A YRAFT_{it} + \gamma_i + \varepsilon_{it}$$

In this model, $YRBEF_{it}$ is a variable equal to 0 if year t is after the adoption of a right-to-carry law and the number of years until adoption if year t precedes adoption. $YRAFT_{it}$ is 0 if year t precedes adoption of a right-to-carry law and is the number of years since adoption of the law otherwise. The other variables are defined as in Model 6.1. The effect of adoption on the trend in crime is measured by $\delta_A - \delta_B$.

The interpretation of the "trend" model is slightly complicated, since the model already includes year effects to accommodate the time pattern of crime common across all counties. To see what this model does, consider a more flexible model with a series of separate dummy variables, for each number of years prior to—and following—the law change for adopting states (see the figures illustrating the section later in the chapter called "Extending the Baseline Specification to 2000"). Thus, for example, a variable called *shall_issue_minus_1* is 1 if the observation corresponds to a county in a state that adopts the law in the following year, 0 otherwise. Similarly, *shall_issue_plus_5* is 1 if the observation corresponds to a county in a state that adopted five years ago, 0 otherwise. And so on.

The coefficient on each of these variables shows how adopting states' time patterns of crime rates move, relative to the national time pattern, surrounding the respective states' law adoption. Note that the time pattern in question is not calendar time but rather time relative to local law adoption, which occurs in different calendar years in different places.

The trend model in equation 6.2 constrains the adopting states' deviations to fall on two trend lines, one for years before and one for years after adoption. Thus, the model restricts the yearly movements in the deviations to fall on trend lines with break points at the time of law adoption.

STATISTICAL ANALYSES OF RIGHT-TO-CARRY LAWS

In this section, we review the basic empirical findings on the effects of right-to-carry laws. We begin with a discussion of Lott's original estimates of Models 6.1 and 6.2 and the committee's efforts to replicate these findings. We then discuss results from other studies that estimate the effects of right-to-carry laws on crime.

Lott's Results

Table 6-1 (first row) displays Lott's estimates from Model 6.1. Lott finds that where they have been adopted, right-to-carry laws have reduced homicide by about 8 percent, rapes by about 5 percent, and aggravated assaults by about 7 percent (Lott, 2000:51). Lott also finds that adoption of right-to-carry laws may increase the rates of nonviolent property crimes (burglary, larceny, auto theft). In theory, this is possible, as criminals substitute away from crimes that involve contact with victims toward crimes that do not involve encounters with victims.

Rows 2 and 3 of Table 6-1 report the results of the committee's replication of these estimates. In row 2, we use the *revised original data set* and Lott's computer programs. The committee was unable to replicate Lott's estimate of the reduction in the murder rate, although the estimates are

TABLE 6-1 Dummy Variable Model with Common Time Pattern, Original and Revised Data[a]

	Sample	Years	Violent Crime	Murder	Rape
1. Lott (2000)	Original 1992	1992	–4.9%	–7.7%	–5.3%
2. Committee replication	Revised 1992[b]	1992	–4.91	–7.30	–5.27
SE			(0.98)**	(1.57)**	(1.22)**
3. Committee replication	Revised 2000[c]	1992	–1.76	–9.01	–5.38
SE			(1.07)	(1.70)**	(1.33)**

[a]The regressions use the covariates and specification from the original Lott and Mustard (1997) models that do not control for state poverty, unemployment, death penalty execution rates, or regional time trends. The controls include the arrest rate for the crime category in question (AOVIOICP), population density in the county, real per capita income variables (RPCPI RPCUI RPCIM RPCRPO), county population (POPC), and variables for the percentage of the population that is in each of many race x age x gender categories (e.g., PBM1019 is the percentage of the population that is black, male, and between ages 10 and 19). The "no

close and consistent with the conclusion that right-to-carry laws reduce the incidence of murder. Through communication with Lott, the committee learned that the data used to construct Table 4.1 of Lott (2000) were lost and that the data supplied to the committee are a reconstruction and not necessarily identical to the original data.

Row 3 displays estimates using the *revised new data set* restricted to period 1977-1992. The estimates from these revised data are substantially different from those originally reported by Lott (2000). In the dummy variable model, the magnitude of the estimated reduction in the rates of violent crime and aggravated assault was reduced, the estimated reduction in the murder rate increased, and the sign of the estimated effects of right-to-carry laws on robbery reversed. Moreover, the effects of right-to-carry laws on violent crime are no longer statistically significantly different from zero at the 5 percent significance level. Finally, the estimated increase in the rates of all property crimes increased substantially.

Table 6-2 presents estimates of the trend model. The first row displays Lott's estimates. Lott finds the passage of right-to-carry laws to be associated with changes in the crime trend. He finds a 0.9 percent reduction in the annual rate of growth of violent crime overall, and a 0.6 percent reduction in the rate of growth of property crimes. Row 2 of Table 6-2 shows the committee's attempt to replicate Lott's results using the *revised original data set*. The committee was unable to replicate most of the results in Lott's Table 4.8. Through communication with Lott, the committee learned that

Aggravated Assault	Robbery	Property Crimes	Auto Theft	Burglary	Larceny
–7.0%	–2.2%	2.7%	7.1%	0.05%	3.3%
–7.01 (1.14)**	–2.21 (1.33)	2.69 (0.72)**	7.14 (1.14)**	0.05 (0.76)	3.34 (0.89)**
–5.60 (1.25)**	1.17 (1.45)	5.84 (0.76)**	10.28 (1.24)**	4.12 (0.83)**	6.82 (0.82)**

controls" specification" includes county fixed effects, year dummies, and the dummy for whether the state has a right-to-carry law.

[b]Using Lott's reconstruction of his original 1977-1992 data.

[c]Using the revised new data set, which contains observations, 1977-2000, even though the estimates in this row use data only through 1992.

NOTE: All samples start in 1977. SE = standard error. Standard errors are in parentheses, where * = significant at 5% and ** = significant at 1%.

this is because there are many misprints in Table 4.8. Nonetheless, Lott's and the committee's results have the same signs for all crimes except aggravated assault. Row 3 displays estimates using the *revised new data set* restricted to the period 1977-1992. These new results tend to show larger reductions in the violent crime trends than those found using the revised original data.

Other Statistical Evaluations of Right-to-Carry Laws

Researchers have estimated the effects of right-to-carry laws using Lott's or related data and models. Many of these studies have found that the use of plausible alternative data, control variables, specifications, or methods of computing standard errors, weakens or reverses the results. Tables 6-3 and 6-4 display estimates from selected studies that illustrate variability in the findings about the effects of right-to-carry laws. The committee does not endorse particular findings or consider them to provide better estimates of the effects of right-to-carry laws than do Lott's results. Moreover, the committee recognizes that several independent investigators have used alternative models or data to obtain results that are consistent with Lott's. These investigators include Bartley and Cohen (1998) and Moody (2001). We focus on the conflicting results in this section because they illustrate a variability of the findings that is central to the committee's evaluation of their credibility.

TABLE 6-2 Trend Model with Common Time Pattern, 1977-1992[a]

	Sample	Years	Violent Crime	Murder	Rape
1. Lott (2000)	Original 1992	1992	–0.9%	–3.0%	–1.4%
2. Committee replication	Revised 1992[b]	1992	–0.50	–4.25	–1.37
SE			(0.41)	(0.65)**	(0.51)**
3. Committee replication	Revised 2000[c]	1992	–2.15	–3.41	–3.37
SE			(0.39)**	(0.62)**	(0.48)**

[a]The regressions use the covariates and specification from the original Lott and Mustard (1997) models that do not control for state poverty, unemployment, death penalty execution rates, or regional time trends. The controls include the arrest rate for the crime category in question (AOVIOICP), population density in the county, real per capita income variables (RPCPI RPCUI RPCIM RPCRPO), county population (POPC), and variables for the percentage of the population that is in each of many race × age × gender categories (e.g., PBM1019 is the percentage of the population that is black, male, and between ages 10 and 19).

Control Variables and Specification

The most common modifications to Lott's original analyses of right-to-carry laws has been to assess the sensitivity of the findings to variation in the control variables or the specification of the model. Lott's basic model relies on dozens of controls, but concerns have been raised that some controls may be missing, others may be unnecessary, and still others may be endogenous (that is, related to the unobserved determinates of county crime rates).

Duggan (2001), for example, raises concerns that county-level control variables may not be precisely measured on an annual basis and that the arrest rate control variable, which includes the crime rate in the denominator, may bias the estimates. In response to these concerns, Duggan estimated a simple dummy variable model that controls only for year and county fixed effects.[7] Duggan drops all other covariates from the model. When estimated on all county-year observations with nonmissing crime

[7]Duggan also changed the coding of the dates of adoption of right-to-carry laws, although this had only a minimal effect on the estimates. According to Duggan (2001) and others (see, for example, Ayres and Donohue, 2003a), there is an inconsistency in the coding used by Lott and Mustard. Duggan finds that in 8 of the 10 right-to-carry states, the adoption date is defined as the year the law was passed, but in 2 states, Florida and Georgia, the adoption date is set to the calendar year after the law was passed. Lott, in personal communications, maintains that the dates are coded correctly. The committee does not take a stand on which coding is correct.

Aggravated Assault	Robbery	Property Crimes	Auto Theft	Burglary	Larceny
–0.5%	–2.7%	–0.6%	–0.1%	–0.3%	–1.5%
0.46 (0.48)	–2.72 (0.56)**	–0.69 (0.30)*	–0.31 (0.48)	–1.58 (0.32)**	–0.11 (0.37)
–2.63 (0.45)**	–3.02 (0.53)**	–1.13 (0.27)**	0.25 (0.45)	–1.80 (0.30)**	–0.84 (0.30)**

[b]Using Lott's reconstruction of his original 1977-1992 data.

[c]Using the revised new data set, which contains observations, 1977-2000, even though the estimates in this row use data only through 1992.

NOTE: All samples start in 1977. SE = standard error. Standard errors are in parentheses, where * = significant at 5% and ** = significant at 1%.

data, this reduced the magnitude of the estimated reduction in the rates of murder and aggravated assault, and it reversed the signs of the estimated effects of right-to-carry laws on rape, robbery, and all violent crime. That is, according to Duggan's estimates, adoption of right-to-carry laws increases the frequencies of rape, robbery, and violent crime as a whole. Moreover, Duggan found there is no statistically significant effect of right-to-carry laws on violent crimes (at the 5 percent significance level).

Other researchers have varied the specification of the model, allowing for the effects of right-to-carry laws to be more heterogeneous. Black and Nagin (1998), for example, estimated a dummy variable model in which the effects of right-to-carry laws are allowed to vary among states (that is, the coefficient δ is allowed to take different values for different states). Plassmann and Tideman (2001) estimate a nonlinear Poisson regression model with a restricted set of covariates, but otherwise similar to Model 6.1. Ayres and Donohue (2003a) combined Models 6.1 and 6.2, thereby obtaining a hybrid model in which adoption of right-to-carry laws can affect both the level and the trend of crime. The results from these analyses, which vary the way in which right-to-carry laws can effect crime, are highly variable, with some suggesting that the laws increase crime, others suggesting that they decrease crime, and many being statistically insignificant.

In Black and Nagin (1998), for example, only Florida has a statistically significant decrease in the murder rate following adoption of a right-to-carry law, and only West Virginia has a statistically significant increase in

TABLE 6-3 Summary of Selected Studies: Dummy Variable Model (percentage) (shaded cells indicate a positive coefficient)

Source	Modification	Violent Crime	Murder	Rape
Lott (2000)	Original specification and data	–5*	–8*	–5*
Moody	Unweighted	–6*	–4*	–5*
	State-level analysis	–11	15	–22*
Duggan[a]	County and time effects only	–1	–6	3
	All counties	0	–1	6
Black and Nagin	Large counties		–9*	–4
	Exclude Florida		–1	1
	Florida		–27.7*	–17*
	Georgia		–5.2	–5
	Idaho		–21	–10
	Maine		7.2	4
	Mississippi		5.4	32*
	Montana		–36.7	–97*
	Oregon		–5.9	4
	Pennsylvania		–8.9	4
	Virginia		3.9	–8
	West Virginia		72*	–29*
Plassmann and Tideman	No control for arrest rate		–7*	–6*
	All counties		–2	–5
	Count model (Poisson)		–11*	–4*
	Florida		–24*	–16*
	Georgia		–8*	–16*
	Idaho		–6	10*
	Maine		1	–2
	Mississippi		5	11*
	Montana		–7	–4
	Oregon		–10*	–2
	Pennsylvania		–5	14*
	Virginia		8*	–3
	West Virginia		5	–1
Ayres and Donohue (2003a)	State trends	0	–9*	–2
	1977-1997 data	2	0	3
	State level analysis			
	State and time effects only	–3	–8	–1
	1977-1999 data	9*	–2	6*
Plassmann and Whitley[a,b]	Regional trend + others			
	1977-2000 data	–3	–6*	–7*
Ayres and Donohue (2003b)[a,b]	Regional trends + other controls			
	1977-2000 corrected data	0	–4	–5

Aggravated Assault	Robbery	Property Crimes	Auto Theft	Burglary	Larceny
–7*	–2	3*	7*	0	3*
–9*	–1	3*	3	1	4*
–18*	–10	1	–9	4	3
–6	4	6*	9*	8*	5
–5	10	7*	11*	10*	5
–7*	–3				
–6*	–5				
–7	7				
–4	8				
–31*	–64*				
–52*	–33*				
–45*	10				
–71*	–14				
–17*	–4				
7*	–5				
–16*	–12				
–3	9				
	–1				
	2				
	6*				
	–3*				
	1				
	–41*				
	–22*				
	25*				
	–27*				
	–48*				
	–14*				
	–5*				
	–9*				
3	–8	–1*	–1*	–4*	1
7*	0	–1	4	1	4
–10	–5	7*	9*	9*	7*
4*	16*	16*	23*	14*	16*
–2	–5	4	9*	0	6
1	–3	6*	11*	2	8*

continued

TABLE 6-3 Continued

Source	Modification	Violent Crime	Murder	Rape
	Standard errors			
Lott (2000)	Unadjusted standard errors	0.98	1.57	1.22
Duggan	State clustered standard errors	2.31	2.95	2.32
Helland and Tabarrok	Placebo standard errors	4.9	6.4	5.6

[a]Uses clustered sampling standard errors.
[b]Added covariates for state poverty, unemployment, death penalty execution rates, and regional time trends.

TABLE 6-4 Summary of Selected Studies: Trend and Hybrid Variable Model (shaded cells indicate a positive coefficient)

Source	Modification	Violent Crime	Murder	Rape
Lott (2000)	Original specification and data	2*	–3*	–1*
Lott (2000)[a]	1977-1996	–2*	–2*	–3*
Ayres and Donohue (2003a)	Hybrid model: Level	7*	3	7*
	Trend	–2*	–5*	–3*
	1977-1997 data: Level	0	7*	6*
	Trend	–2*	–4*	–3*
Plassmann and Whitley[a,b]	Regional trend + others 1977-2000 data	–1	–2	–3*
Ayres and Donohue (2003b)[a,b]	Regional trends + other controls 1977-2000 corrected data	0	–2	–2

[a]Added covariates for state poverty, unemployment, death penalty execution rates, and regional time trends.
[b]Standard errors adjusted for state clustering.

its murder rate. The estimated changes in the murder rates of other states that adopted right-to-carry laws are sometimes positive (three cases) and sometimes negative (five cases) and are not statistically significantly different from zero. Black and Nagin also report variations in the directions and statistical significance of changes in the rates of rape and aggravated assault. They report no statistically significant increases in robberies, but only 2 of the 10 states that adopted right-to-carry laws had statistically signifi-

Aggravated Assault	Robbery	Property Crimes	Auto Theft	Burglary	Larceny
1.14	1.33	0.72	1.14	0.76	0.89
2.77	3.34	1.89	2.59	2.29	2.27
6.6	7.5	5.1	6.5	5.7	5.7

NOTES: Shaded cells indicate a positive coefficient estimate and * indicates the estimate is statistically significant at the 5% significance level. Unless otherwise noted, the standard errors are not adjusted for state-level clustering. Exceptions: Duggan, Plassmann and Tideman, Ayres and Donohue.

Aggravated Assault	Robbery	Property Crimes	Auto Theft	Burglary	Larceny
–1*	–3*	–1*	0*	–2*	0
–3*	–3*	–2*	–3*	–1*	–2*
10*	–3	0	0	–3	0
–2	–1	0	0	0	1
6*	4	–1	9*	4*	5*
–3*	–4*	0	–2*	–3*	–2*
–2	–3*	0	0	–2	–1
–1	–2	0	0	–1	0

NOTES: Shaded cells indicate a positive coefficient estimate and * indicates the estimate is statistically significant at the 5% significance level. Unless otherwise noted, the standard errors are not adjusted for state-level clustering. Exceptions: Duggan, Plassmann and Tideman, Ayres and Donohue.

cant decreases. In summary, according to Black and Nagin, adoption of a right-to-carry law may increase, decrease, or have no discernible effect on the crime rate depending on the crime and the state that are involved.[8]

[8]To avoid selection problems associated with using counties with positive crime rates, Black and Nagin also restricted their analysis to counties with populations of 100,000 or more. This was done to mitigate a possible bias arising from Lott's use of the arrest rate as an explanatory variable. The arrest rate is the number of arrests divided by the number of crimes

Plassmann and Tideman (2001) document similar variability in the estimates. To account for the fact that county-level crime data include a large number of observations for which the outcome variable equals zero, Plassmann and Tideman estimate a nonlinear count data model. Using data from all counties with reported crime figures, the resulting estimates on murder and rape are consistent with Lott's findings, but the sign of the estimated effect of right-to carry laws on robbery is reversed. Furthermore, when the effects of right-to-carry laws are allowed to vary among states, Plassmann and Tideman found that adoption of a right-to-carry law may increase, decrease, or have no effect on the crime rate depending on the crime and state that are involved. Consider, for example, murder. Right-to-carry laws are estimated to have a statistically significant decrease in the murder rate in Florida, Georgia, and Oregon following adoption of a right-to-carry law. Virginia has a statistically significant increase in its murder rate. The changes in the murder rates of other states that adopted right-to-carry laws are not statistically significantly different from zero. Plassmann and Tideman conclude by noting the fragility in the estimated effects of right-to-carry laws: "While this ambiguous result is somewhat discouraging, it is not very surprising. Whenever the theoretically possible and in practice plausible effects of public policy are ambiguous, it can be expected that the effects of such a policy will differ across localities that are clearly different from each other" (p. 797).

Finally, the added flexibility of the hybrid model estimated by Ayres and Donohue (2003a) produces estimation results that are different from Lott's.[9] The results found when using the revised original data (1977-

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and is undefined in counties that report no crimes of the types analyzed. Therefore, these counties are not included in Lott's analysis. Because the denominator of the arrest rate variable contains the dependent variable in Lott's models, it is possible that dropping no-crime counties biases the results of his analysis. Nearly all of the low-crime counties have populations below 100,000. Therefore, use of only counties with larger populations largely overcomes the problem of missing arrest rate data without creating a bias.

Lott (1999:8-9; 2000:142-143), however, has argued that Black's and Nagin's results are unreliable because they eliminated 85 percent of the counties in the nation (all the counties with populations of less than 100,000). In particular, they used only one county in West Virginia. Lott (2000: Table 4.9) presents his own estimation results according to which his findings are largely unaffected by disaggregating the right-to-carry effect by state. However, Lott does not report the details of his analysis or the statistical significance levels of his estimates. Moreover, his response does not explain why Black and Nagin found statistically significant increases in some crime rates for some states following passage of right-to-carry laws.

[9]The committee takes no position on whether the hybrid model provides a correct description of crime levels or the effects of right-to-carry laws. The important feature of the hybrid model is that it nests Models 6.1 and 6.2.

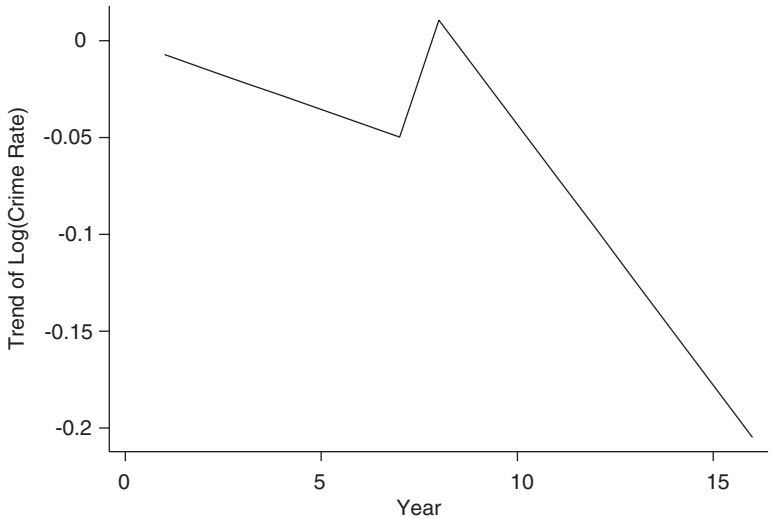


FIGURE 6-1 Trend in the logarithm of the violent crime rate.

1992) are illustrated in Figure 6-1, which shows the "relative trend" in the logarithm of the violent crime rate obtained from the Ayres and Donohue model for a hypothetical county in which a right-to-carry law is adopted in year 8. The relative trend is the difference between the crime trend in the adopting county and the trend in a nonadopting county with the same values of the explanatory variables *X*. According to the figure, adoption of the law increased the level of violent crime but accelerated a decreasing (relative) trend. Ayres and Donohue obtained similar results for rape and aggravated assault. For murder, the shift in the level is not statistically significant, but there is a statistically significant downward shift in the trend. There is no statistically significant effect on either the level or the trend for robbery and property crimes. Ayres and Donohue also report estimates from an expanded data set that includes the years 1977-1999. The results found using these data, which are reported in Table 6-4, are similar.

Updated Sample Endpoint

Several researchers, including Lott, have assessed whether the basic findings from Models 6.1 and 6.2 continue to hold when using more recent data. In the epilogue to the second edition of his book, Lott (2000: Table 9.1) analyzes data covering the period 1993-1996. Plassmann and Whitley (2003) use data through 2000. In addition to updating the data, these

researchers also change the model specification. In particular, these analyses include additional covariates (i.e., state poverty, unemployment and death penalty execution rates) and allow for region-interacted time patterns, as opposed to a common time trend used in the original Lott models (Lott 2000:170).

With these new models and the updated sample endpoints, Lott found that the basic conclusions from the trend model are robust to the additional years of data covering the periods 1977-1996. Likewise, Plassmann and Whitley (2003) found that when the data are updated to cover the period 1977-2000, the trend model estimates of the effects of right-to-carry laws on crime continue to be negative, but only the estimates for rape and robbery are statistically significant. In the dummy variable model, Plassmann and Whitley found negative coefficient estimates for the right-to-carry coefficient for each violent crime category and positive coefficients for each of the property categories.

Ayres and Donohue (2003b), however, document a number of errors in the data used by Plassmann and Whitley, and Lott's revised new data correct these errors. Plassmann, in communications with the committee, has agreed that the changes to these data are appropriate. Using the revised new data, the committee exactly replicated the results reported by Ayres and Donohue (2003b).

In particular, Ayres and Donohue (2003b) found that rerunning the dummy variable model regressions using the corrected data reduced the magnitude of the estimated reduction in the rates of violent crime, murder, rape, and robbery, and it reversed the sign of the estimated effects of right-to-carry laws on aggravated assault. Moreover, none of the negative estimates is statistically significant, while effects for larceny, auto theft, and property crime overall are positive and significant. Likewise, the changes in the crime trends are generally small in absolute value, and none of the changes is significantly different from zero (see Table 6-4).[10]

Maltz and Targonski (2002) do not update the data but instead assess the quality of the county crime data used in the empirical research on right-to-carry laws. In particular, they note that not all police jurisdictions report their crime levels to the FBI and argue that there is systematic underreporting in the UCR. Maltz and Targonski (2002:298) conclude that "county-level crime data, as they are currently constituted, should not be used, especially in policy studies." However, Maltz and Targonski do not estimate the magnitude of the effects of underreporting on the results obtained by Lott and others. Thus, it is not known whether correcting for underreporting, if it were possible, would change any of the results.

[10]Both Ayres and Donohue (2003b) and Plassmann and Whitley (2003) use standard errors that account for state clustering.

Lott and Whitley (2002: Figure 5) report estimates of the effects of right-to-carry laws that are obtained by dropping from the data counties with large fractions of missing UCR reports. Lott's and Whitley's figure shows estimated trends in crime levels before and after adoption of right-to-carry laws, and they claim that these trends support the conclusion that adoption of right-to-carry laws reduces crime. The committee disagrees. According to Figure 5b of Lott and Whitley (2002), the murder rate peaks and begins to decrease at an accelerating rate approximately 5 years before the adoption of right-to-carry laws. Aggravated assault decreases prior to adoption and then increases for approximately 3 years following adoption before starting to decrease again (Figure 5e). Adoption has no effect on rape (Figure 5c). The rate of violent crimes as a whole decreases up to the time of adoption and then remains unchanged until approximately 3 years after adoption before beginning a steeper decline (Figure 5a). Among violent crimes, only robbery displays a decrease immediately following adoption (Figure 5d). However, this followed a period during which the robbery rate first increased and then remained constant for approximately 5 years. In summary, the committee concludes that it is at least possible that errors in the UCR data may account for some of Lott's results.

Standard Errors

A final point that has been argued in the literature is that conventional standard errors reported by Lott and others are not appropriate. The statistical analyses of dummy variable and trend models are conducted using a county-year pair as the unit of analysis. Right-to-carry laws, however, almost always vary only at the state level. Consequently, some investigators believe that treating the county-level observations as if they are statistically independent may lead to estimates of the standard errors that underestimate their true magnitude. These investigators make adjustments for state-level clustering that inflate their standard errors. For example, the standard error for the dummy variable model estimate of the effect of right-to-carry laws on violent crime increases from 0.98 when reporting the unadjusted standard error, to 2.31 when estimating clustered sampling standard errors (Duggan, 2001), to 4.9 when using the methods advocated by Helland and Tabarrok (2004) (see Table 6-3). The fact that the adjustments in most cases greatly increase the standard errors is a reason for concern. Once the standard errors have been adjusted for clustering, very few of the point estimates, in any of the models, using any of the data sets, are statistically different from zero.

However, investigators reporting cluster-adjusted standard errors do not formally explain the need for these adjustments. These adjustments, in fact, are not supported in the basic models specified in Equations 6.1 and

6.2. Instead, those who argue for presenting clustered standard errors often cite Moulton (1990) as the source of their belief that adjustments are needed. Moulton considered a model in which there is an additive source of variation (or additive effect) that is the same for all observations in the same cluster. He showed that ignoring this source of variation leads to standard errors that are too low. Investigators who make clustering corrections usually consider the counties in a state to constitute one of Moulton's clusters and appear to believe that the absence of state-level additive effects in their models causes standard errors to be too low. The models estimated in this literature, including those of Lott and his critics, typically contain county-level fixed effects (the constants γ_i in equations 6.1 and 6.2). Every county is always in the same state, so, any state-level additive effect simply adds a constant to the γ_i's of the counties in that state. The constant may vary among states but is the same for all counties in the same state. The combined county- and state-level effects are indistinguishable from what would happen if there were no state-level effects but each γ_i for the counties in the same state were shifted by the same amount. Therefore, state-level effects are indistinguishable from county-level effects. Any state-level effects are automatically included in the γ_i's. There is no need for adjustments for state-level clustering.

Other observationally equivalent but different models can support the use of adjusted standard errors. If, for example, the effects of right-to-carry laws (or other explanatory variables) vary across states, then the assumption of independence across counties would be incorrect. Adjustments to the standard errors can allow for uncertainty arising from the possibility that the coefficients of variables in the model that are not allowed to vary across states, in fact, vary randomly across states. The adjustments made by Duggan and Plassmann and Whitley, for example, can be used to correct estimated standard errors for this possibility (see Wooldridge, 2003).

These alternative models have not been discussed in the literature or by the committee. Thus, it is not clear whether the models that would support using clustered-sampling-adjusted standard errors are appropriate to evaluate the effects of right-to-carry laws. At the most basic level, researchers need to assess whether models that support clustering are of interest.[11] If, for example, coefficients can vary randomly among states, Models 6.1 and 6.2 reveal the mean coefficients. In other words, if different states have different coefficients, then researchers estimate an average over states. It is

[11]There are also important technical issues to consider. For example, a commonly used method for making these corrections is reliable only when the number of "clusters" (here states) is large, and there is reason to think that the 50 states do not constitute a large enough set of clusters to make these methods reliable.

not clear why anyone should care about this average, which is not related in any obvious way to (for example) nationwide benefits of right-to-carry laws. If coefficients vary among states, then it may be much more useful to estimate the coefficients for each state. It is entirely possible that the effects of right-to-carry laws vary among states, even after controlling everything else that is in the model. If they do, it may be much more useful to know which states have which coefficients, to see the magnitude of the variation, and to have a chance of finding out whether it is related to anything else that is observable. Of course, a number of the studies summarized above have varied Lott's model by allowing the effect of right-to-carry laws to differ by states (see, for example, Black and Nagin, 1998, and Plassmann and Tideman, 2001). A model in which coefficients are estimated separately for each state does not require adjustment of standard errors.

In summary, whether adjustment of standard errors is needed depends on the details of the effects that are being estimated and the model that is used to estimate them. These issues have not been investigated in studies of right-to-carry laws to date. Adjusted standard errors are not needed for Models 6.1 and 6.2. The precision of estimates from these models should be evaluated using unadjusted standard errors.

COMMITTEE'S ANALYSIS: ARE THE ESTIMATES ROBUST?

This section presents the results of the committee's own analysis of Lott's revised new data covering the period 1977-2000. The purpose of the analysis is to clarify and illustrate some of the causes of the conflicting results. The committee has not attempted to form our own estimates of the effects of right-to-carry laws. Rather, our analysis is directed toward gaining a better understanding of the fragility of the estimates. We begin by illustrating the sensitivity of the findings to extending the sample period to cover the years 1993-2000. We then demonstrate that the basic qualitative results are sensitive to variations in the explanatory variables. In all cases, we use the *revised new data set*. There is a consensus that these revised data, covering the periods 1977-2000, are correct.

Horowitz discusses this problem in further detail and provides a statistical explanation for the fragility in the estimates in Appendix D. This appendix describes two fundamental sources of difficulty in causal inference that are especially relevant to studies of right-to-carry laws. One is the difficulty of choosing the right explanatory variables for a statistical model. The second is the difficulty of estimating the relation among crime rates, a large number of potential explanatory variables, and the adoption of right-to-carry laws. Even if the correct explanatory variables were known, it would be hard to specify a model correctly, especially in high dimensional settings with many explanatory variables. The committee drew on some of

TABLE 6-5 Dummy Variable Model with Common Time Pattern, 2000 Data

	Years	Controls[a]	Violent Crime	Murder	Rape
0. Committee replication	1992[b]	Yes	–1.76	–9.01	–5.38
SE			(1.07)	(1.70)**	(1.33)**
1. Comm estimate w/ covariates	2000	Yes	4.12	–8.33	–0.16
SE			(0.71)**	(1.05)**	(0.83)
2. Comm estimate w/o covariates	1992[b]	No	–0.12	–1.22	1.39
SE			(1.29)	(2.65)	(2.24)
3. Comm estimate w/o covariates	2000	No	12.92	–1.95	17.91
SE			(0.78)**	(1.48)	(1.39)**

[a]The regressions use the covariates and specification from the original Lott and Mustard (1997) models that do not control for state poverty, unemployment, death penalty execution rates, or regional time trends. The controls include the arrest rate for the crime category in question (AOVIOICP), population density in the county, real per capita income variables (RPCPI RPCUI RPCIM RPCRPO), county population (POPC), and variables for the percentage of the population that is in each of many race × age × gender categories (e.g., PBM1019 is the percentage of the population that is black, male, and between ages 10 and 19). The "no

these ideas in our deliberations but did not adopt them in total as part of our consensus report. This statistical argument is presented to stimulate further discussion and dialogue on these issues.

Extending the Baseline Specification to 2000

Extending the sample to cover the period 1977-2000 provides an important test of the robustness of the estimates for two reasons. First, the number of observations from states with right-to-carry laws in effect more than triples when the additional years are included. Second, 16 additional states enacted right-to-carry laws during the period 1993-1999, thereby providing additional data on the effects of these laws.

Another reason for the importance of the extended data is that aggregate crime trends differ greatly between the periods 1977-1992 and 1993-1997. The first period was one of rising crime, especially in large urban areas, which tend to be in states that did not adopt right-to-carry laws during 1977-1992. The period 1993-1997 was one of declining crime. Any differences in estimation results between the 1977-1992 and 1977-1997

Aggravated Assault	Robbery	Property Crimes	Auto Theft	Burglary	Larceny
–5.60	1.17	5.84	10.28	4.12	6.82
(1.25)**	(1.45)	(0.76)**	(1.24)**	(0.83)**	(0.82)**
3.05	3.59	11.48	12.74	6.19	12.40
(0.80)**	(0.90)**	(0.52)**	(0.78)**	(0.57)**	(0.55)**
–4.17	9.18	8.47	11.98	8.53	8.56
(1.54)**	(2.17)**	(0.79)**	(1.48)**	(0.94)**	(0.93)**
12.34	19.99	21.24	23.33	19.06	22.58
(0.90)**	(1.21)**	(0.53)**	(0.85)**	(0.61)**	(0.59)**

controls" specification includes county fixed effects, year dummies, and the dummy for whether the state has a right-to-carry law.

[b]Using the revised new data set, which contains observations, 1977-2000, even though the estimates in this row use data only through 1992.

NOTE: All samples start in 1977. SE = standard error. Standard errors are in parentheses, where * = significant at 5% and ** = significant at 1%.

data constitute evidence of model misspecification (e.g., because the model cannot account for the change in the aggregate crime trend) and raise the possibility (although do not prove) that the estimated effects of right-to-carry laws are artifacts of specification errors. This is a particularly important concern because states that pass right-to-carry laws are not representative of the nation as a whole on important dimensions (e.g., percentage rural) that are correlated with rising crime in the 1977-1992 period and falling crime in the years 1993-2000.

The first row of Table 6-5 reports the results of extending the dummy variable model (6.1) to the new data covering the period 1977-2000. The specifications estimated are identical to the original model, with the only difference being that the number of years has been expanded. Compared with the model estimated on the original (1977-1992) sample period (see Table 6-5, Row 0), the results have now changed rather substantially. Only the coefficient on murder is negative and significant, while seven coefficients are positive and significant (violent crime overall, aggravated assault, robbery, property crime overall, auto theft, burglary, and larceny). The dummy variable results that were apparent with the earlier data set and

TABLE 6-6 Trend Model with Common Time Pattern, 2000 Data

	Years	Controls[a]	Violent Crime	Murder	Rape
0. Committee replication	1992[b]	Yes	–2.15	–3.41	–3.37
SE			(0.39)**	(0.62)**	(0.48)**
1. Comm estimate w/ covariates	2000	Yes	–0.95	–2.03	–2.81
SE			(0.18)**	(0.26)**	(0.20)**
2. Comm estimate w/o covariates	1992[b]	No	–1.41	–1.52	–3.45
SE			(0.47)**	(0.97)	(0.82)**
3. Comm estimate w/o covariates	2000	No	–0.62	0.12	–2.17
SE			(0.17)**	(0.32)	(0.30)**

[a]The regressions use the covariates and specification from the original Lott and Mustard (1997) models that do not control for state poverty, unemployment, death penalty execution rates, or regional time trends. The controls include the arrest rate for the crime category in question (AOVIOICP), population density in the county, real per capita income variables (RPCPI RPCUI RPCIM RPCRPO), county population (POPC), and variables for the percentage of the population that is in each of many race x age x gender categories (e.g., PBM1019 is the percentage of the population that is black, male,

earlier sample periods almost completely disappear with the extension of the sample to 2000. The committee views the failure of the original dummy variable model to generate robust predictions outside the original sample as important evidence of fragility of the model's findings.[12]

These results are also substantially different from those found when using the expanded set of control variables first adopted by Lott (2000: Table 9.1). As described above, Ayres and Donohue (2003b) estimate a dummy variable model using the revised new data (see Table 6-3). As in Lott (2000, Table 9.1) and Plassmann and Whitley (2003), they modify the original specification to include additional covariates (i.e., state poverty, unemployment, and death penalty execution rates) and region-interacted time patterns, as opposed to a common time trend used in the original Lott models (Lott 2000:170). These seemingly minor adjustments cause sub-

[12]In light of the variability in the estimates, statistical tests might aid in determining whether particular specifications can be rejected by the data. It is not possible to test empirically whether a proposed set of explanatory variables is the correct one. It is possible to test for specification, given a set of controls (see Horowitz, Appendix D). None of the models examined by the committee passes a simple specification test (i.e., Ramsey's 1969 RESET test).

Aggravated Assault	Robbery	Property Crimes	Auto Theft	Burglary	Larceny
−2.63	−3.02	−1.13	0.25	−1.80	−0.84
(0.45)**	(0.53)**	(0.27)**	(0.45)	(0.30)**	(0.30)**
−1.92	−2.58	−0.01	−0.49	−2.13	−0.73
(0.20)**	(0.22)**	(0.13)	(0.19)*	(0.14)**	(0.13)**
−2.02	−0.44	−1.33	1.62	−2.50	−1.27
(0.57)**	(0.79)	(0.29)**	(0.54)**	(0.34)**	(0.34)**
−0.65	−0.88	−0.81	0.57	−1.99	−0.71
(0.20)**	(0.26)**	(0.11)**	(0.19)**	(0.13)**	(0.13)**

and between ages 10 and 19). The "no controls" specification includes county fixed effects, year dummies, and th dummy for whether the state has a right-to-carry law.

[b]Using the revised new data set, which contains observations, 1977-2000, even though the estimates in this row use data only through 1992.

NOTE: All samples start in 1977. SE = standard error. Standard errors are in parentheses, where * = significant at 5% and ** = significant at 1%.

stantial changes to the results. For example, right-to-carry laws are estimated to decrease murder by about 4 percent using the revised specification, but about 8 percent using the original specification. The estimated effects for the eight other crime categories decrease between 2 and 6 points when moving from the original to the revised specification.

We also estimate the trend model extending the sample to 2000 (row 1, Table 6-6). Relative to the estimates in row 0 (using only data to 1992), the estimates are mostly smaller but remain negative and statistically significant. Thus, the trend specification continues to show reductions in the rate of growth of crime following right-to-carry passage.

To explore why the updated dummy variable and trend models give conflicting results, we do two things. First, we estimate a more flexible year-by-year specification, a variant of Model 6.1, the dummy variable model. Second, we reanalyze the trend model (Model 6.2) by varying the number of years after the law's adoption to estimate its effects on crime. In each of these cases, we use the revised new Lott data through 2000 and we include the original controls used by Lott and Mustard (1997). In each of these cases, except for sampling variability, the changes should not affect the results if the trend model in equation 6.2 is properly specified.

In the first exercise, we replace the right-to-carry dummy with a series of dummies for each of the possible numbers of years prior to—and following—adoption. We summarize the estimated coefficients in three figures. These figures show the estimated coefficients normalized on the year of adoption and multiplied by 100 (so the y-axis is a percentage), and the associated 95 percentage confidence intervals.[13] The vertical line marks the adoption year, while the horizontal line marks 0.

Figure 6-2 shows the time pattern of coefficients from the violent crime model. For years preceding adoption, violent crime is increasing in ultimately adopting states (relative to the national time pattern). Following adoption, the increase relative to trend continues, reverses, then reverses twice again. For property crimes, in Figure 6-2, the upward trend for years prior to adoption continues following adoption.

Figure 6-3 and Figure 6-4 show graphs for individual violent and property crime categories, respectively. The obvious striking feature of these figures is that the big reductions in crime occur roughly 9 years after adoption. Otherwise, the postadoption estimates are generally small and sometimes positive and are, in general, both statistically insignificant and statistically indistinguishable from the preadoption estimates. The trend model essentially fits a line with constant slope through the postadoption portions of these graphs, and the line's slope is affected by years long after adoption. These time patterns raise serious questions about whether the reductions in crime documented in the trend model are reasonably attributed to the change in the law.

In the second exercise, to further explore the sensitivity of the trend model estimates, we reestimate the baseline trend model (Model 6.2) using revised new Lott data on the period 1977-2000. Table 6-7, row 1, repeats the estimates from Table 6-6, row 1 which includes all years for all states, regardless of the amount of time elapsed since the law change. Subsequent rows include observations that occur certain numbers of years after the law change. (Row 2, labeled "6 years," includes the year of the law change and the 5 following years, and so on.) These estimates show that including 5 years or fewer reverses the signs of the estimated effects of right-to-carry laws on murder and property crime (from negative to positive) and reduces the magnitude of the estimated reduction in the rates of rape, aggravated assault, robbery, and violent crime. Moreover, there are fewer statistically significant changes in crime trends. One needs to include at least 6 years following the prelaw-change period to find statistically significant reductions in the violent crime and murder trends.

The trend results rely on changes in crime trends occurring long after the law changes, again raising serious questions about whether one can

[13]That is, we subtract the year 0 coefficient from each year's coefficient.

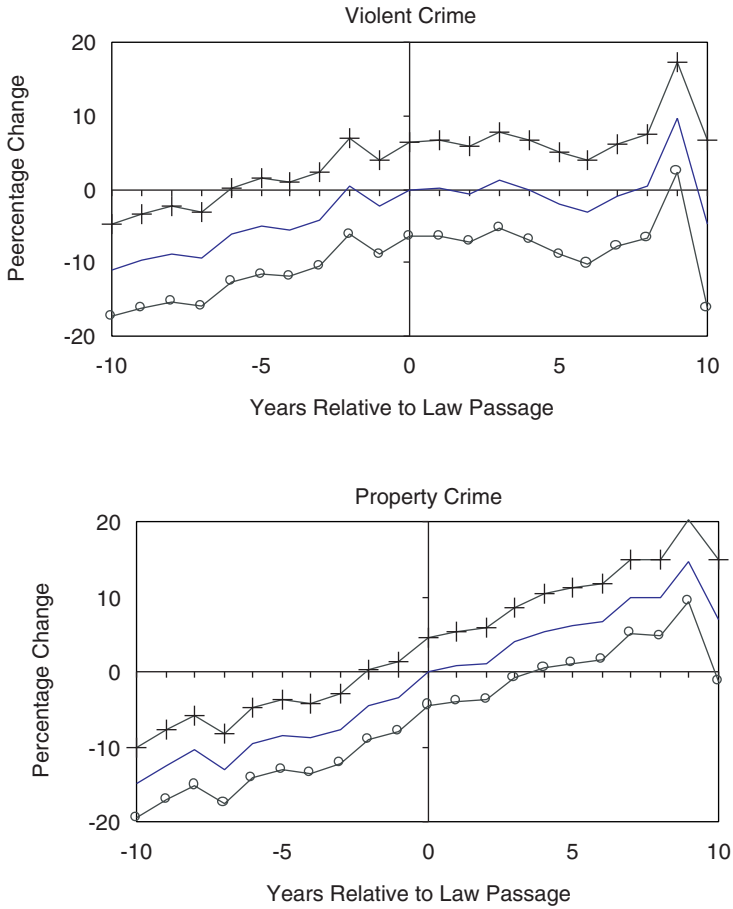
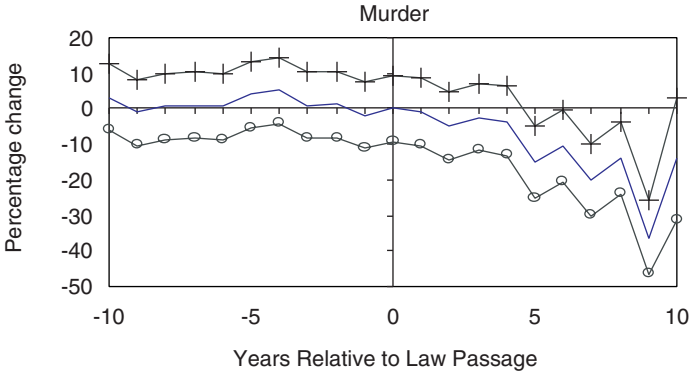


FIGURE 6-2 Year-by-year estimates of the percentage change in aggregate crime (normalized to adoption date of right-to-carry law, year 0).
—— Estimate, —o— bottom of 95% confidence interval (CI), —+—Top of 95% CI

sensibly attribute the estimates from trend models in the literature to the adoption of right-to-carry laws.

Are the Results Sensitive to Controls?

The final two rows of Table 6-5 present two sets of results obtained by the committee when estimating models identical to those of Model 6.1, but excluding socioeconomic and demographic controls. We include only the



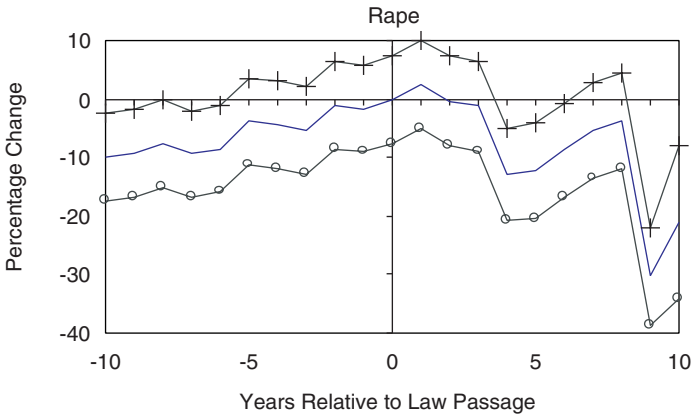
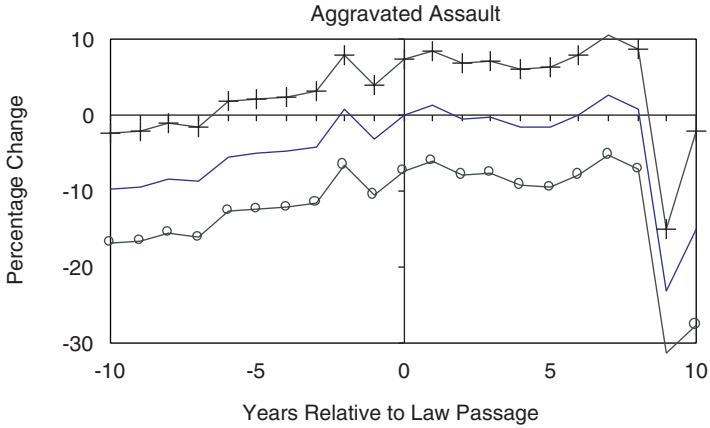
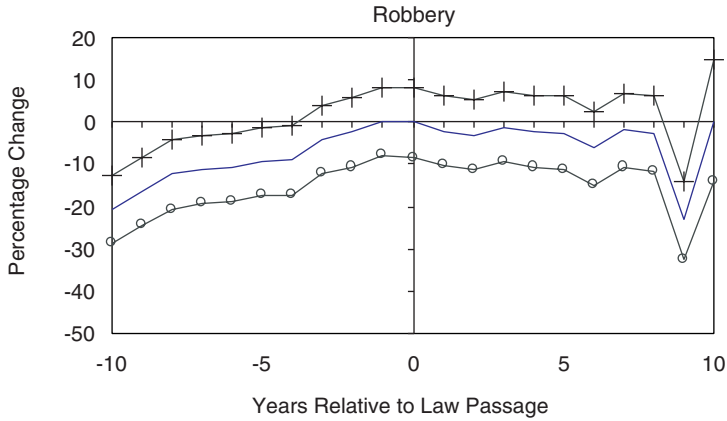


FIGURE 6-3 Year-by-year estimates of the percentage change in disaggregate violent crimes (normalized to adoption date of right-to-carry law, year 0).
—— Estimate, —o— bottom of 95% confidence interval (CI), —+— Top of 95% CI

right-to-carry variable, year dummies, and county fixed effects. These estimates tell us how crime has changed in states that have adopted the right-to-carry laws before and after the law change, relative to national time patterns in crime. It is important to stress that the committee is not arguing that excluding all socioeconomic and demographic covariates is an appropriate method of identifying the effects of right-to-carry laws. Rather, we are simply assessing whether such laws are associated with a decline in the level of crime. If not, then detecting the effect, if any, of right-to-carry laws





requires controlling for appropriate confounding variables and thereby reliance on a model such as those used by Lott and others.

The results without controls are quite different. Using the earlier sample period and the new data, one finds three negative coefficients, only one of them statistically significant. When the sample is extended to 2000, only one of nine coefficients is negative, and it is insignificantly different from zero. For example, the violent crime coefficient with controls is 4.1 percent, while it is 12.9 percent without controls. These results show that states that

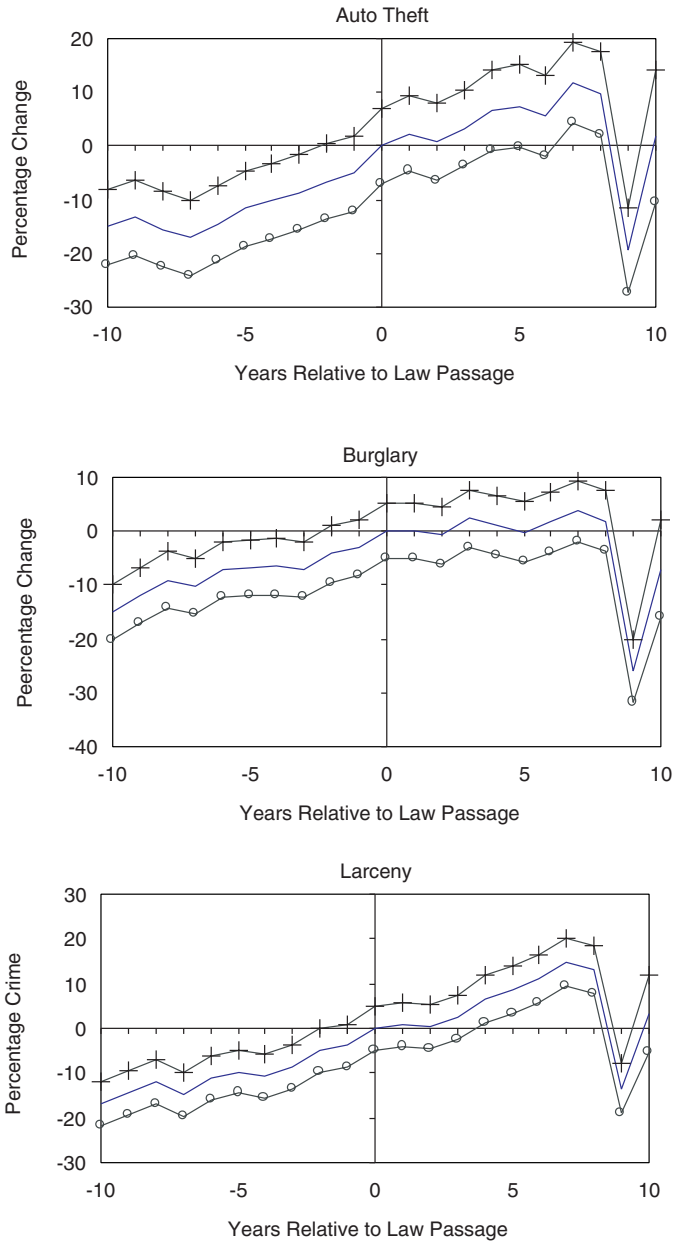


FIGURE 6-4 Year-by-year estimates of the percentage change in disaggregate property crimes (normalized to adoption date of right-to-carry law, year 0).
—— Estimate, —o— bottom of 95% confidence interval (CI), —+—Top of 95% CI

passed right-to-carry laws did not on average experience statistically significant crime declines relative to states that did not pass such laws.

There are two points to make about the no-controls results. First, the no-controls results provide a characterization of the data that shows that, if there is any effect, it is not obvious in the dummy variable model. What do estimates from that model mean? The model says that crime rates differ across counties and, moreover, that they change from one year to the next in the same proportionate way across all counties in the United States. Over and above this variation, there is a one-time change in the mean level of crime as states adopt right-to-carry laws. So these estimates indicate that, for the period 1977-1992, states adopting right-to-carry laws saw roughly no change in their violent crime rates and 8.5 percent increases in their property crime rates, relative to national time patterns. Estimating the model using data to 2000 shows that states adopting right-to-carry laws saw 12.9 percent increases in violent crime—and 21.2 percent increases in property crime—relative to national time patterns. The first-blush evidence provided by these no-controls models is thus not supportive of the theory that right-to-carry laws reduce crime.

A final lesson to draw from the no-controls dummy variable results is that the results are sensitive to the inclusion of controls. That is, whether one concludes that right-to-carry laws increase or decrease crime based on models of this sort depends on which control variables are included. Such laws have no obvious effect in the model without controls (and therefore no clear level effect in the raw data). Moreover, as demonstrated above, seemingly minor changes to the set of control variables substantially alter the estimated effects. Given that researchers might reasonably argue about which controls belong in the model and that the results are sensitive to the set of covariates, the committee is not sanguine about the prospects for measuring the effect of right-to-carry laws on crime. Note that this is distinct from whether such laws affect crime. Rather, in our view, any effect they have on crime is not likely to be detected in a convincing and robust fashion.

Estimates from the trend model are less sensitive to the inclusion of controls. While the no-control point estimates displayed in the third and fourth rows of Table 6-6 are smaller than in the model with controls, most of these estimates are negative and statistically significant. The trend model without controls shows reductions in violent and property crime trends following the passage of right-to-carry laws for both sample endpoints. For murder, however, the results are positive when using the 2000 endpoint, negative when using the 1992 endpoint, and statistically insignificant in both cases.

TABLE 6-7 Trend Model with Varying Postlaw Change Durations

	Years	Controls[a]	Violent Crime	Murder	Rape
1. Baseline comm estimate[b] from row 1 of Table 6-6	2000	Yes	–0.95	–2.03	–2.81
SE			(0.18)**	(0.26)**	(0.20)**
2. 6 years	2000	Yes	–0.97	–1.11	–2.90
SE			(0.29)**	(0.42)**	(0.33)**
3. 5 years	2000	Yes	–0.65	0.05	–2.45
SE			(0.35)	(0.50)	(0.40)**
4. 4 years	2000	Yes	–0.27	0.48	–0.74
SE			(0.44)	(0.63)	(0.50)

[a]The regressions use the covariates and specification from the original Lott and Mustard (1997) models that do not control for state poverty, unemployment, death penalty execution rates, or regional time trends. The controls include the arrest rate for the crime category in question (AOVIOICP), population density in the county, real per capita income variables (RPCPI RPCUI RPCIM RPCRPO), county population (POPC), and variables for the percentage of the population that is in each of many race × age × gender categories (e.g., PBM1019 is the percentage of the population that is black, male, and between ages 10 and 19).

CONCLUSIONS

The literature on right-to-carry laws summarized in this chapter has obtained conflicting estimates of their effects on crime. Estimation results have proven to be very sensitive to the precise specification used and time period examined. The initial model specification, when extended to new data, does not show evidence that passage of right-to-carry laws reduces crime. The estimated effects are highly sensitive to seemingly minor changes in the model specification and control variables. No link between right-to-carry laws and changes in crime is apparent in the raw data, even in the initial sample; it is only once numerous covariates are included that the negative results in the early data emerge. While the trend models show a reduction in the crime growth rate following the adoption of right-to-carry laws, these trend reductions occur long after law adoption, casting serious doubt on the proposition that the trend models estimated in the literature reflect effects of the law change. Finally, some of the point estimates are imprecise. Thus, the committee concludes that 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.

Aggravated Assault	Robbery	Property Crimes	Auto Theft	Burglary	Larceny
–1.92	–2.58	–0.01	–0.49	–2.13	–0.73
(0.20)**	(0.22)**	(0.13)	(0.19)*	(0.14)**	(0.13)**
–1.06	–1.88	0.11	1.40	–1.13	0.33
(0.32)**	(0.36)**	(0.21)	(0.31)**	(0.23)**	(0.22)
–0.83	–1.63	0.28	1.83	–0.77	0.36
(0.39)*	(0.43)**	(0.25)	(0.37)**	(0.27)**	(0.26)
–0.34	–1.36	0.44	2.03	–0.47	0.31
(0.49)	(0.55)*	(0.32)	(0.47)**	(0.35)	(0.33)

[b]Using the revised new data set, for the full available time period (1977-2000).
NOTES: All samples start in 1977. All estimates use the trend model. Rows 2 through 4 of this table restrict the sample to include only years falling fixed numbers of years past the law change. For example, row 2 includes all the prelaw-change years, the year of the law change (year 0), plus 5 additional years, for a total of 6 years after the prelaw-change period. SE = standard error. Standard errors are in parentheses, where * = significant at 5% and ** = significant at 1%.

It is also the committee's view that additional analysis along the lines of the current literature is unlikely to yield results that will persuasively demonstrate a causal link between right-to-carry laws and crime rates (unless substantial numbers of states were to adopt or repeal right-to-carry laws), because of the sensitivity of the results to model specification. Furthermore, the usefulness of future crime data for studying the effects of right-to-carry laws will decrease as the time elapsed since enactment of the laws increases.

If further headway is to be made on this question, new analytical approaches and data sets will need to be used. For example, studies that more carefully analyze changes in actual gun-carrying behavior at the county or even the local level in response to these laws may have greater power in identifying the impact of such laws. Surveys of criminals or quantitative measures of criminal behavior might also shed light on the extent to which crime is affected by such laws.

EXHIBIT C:

Daniel Webster And Jens Ludwig,
Myths About Defensive Gun Use And Permissive Gun Carry Laws,
Berkeley Media Studies Group (2000)

BERKELEY MEDIA STUDIES GROUP

Myths about Defensive Gun Use and Permissive Gun Carry Laws

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Myths about Defensive Gun Use and Permissive Gun Carry Laws

In 1998, economist John Lott, Jr. published a book with the provocative title *More Guns, Less Crime*[1] in which he presents and interprets data to support his thesis that communities are safer when its residents are free of government restrictions on gun ownership and carrying. The book focuses primarily on two of his studies. The first, conducted with David Mustard, estimates the effects on crime attributable to state laws that allow virtually all eligible gun buyers to obtain a permit to carry a gun in public.[2] The second, conducted with William Landes, examines the effects of permissive gun carrying laws on mass shootings.[3] In each case, the authors conclude that permissive gun carrying laws result in substantial reductions in violent crime.

Another study that examines the benefits of gun ownership and carrying was conducted by Florida State University criminologists Gary Kleck and Marc Gertz,[4] and was designed to estimate the frequency with which would-be-victims of crime in the U.S. use guns to successfully defend themselves. Kleck and Gertz estimate that 2.5 million citizens use guns in self-defense each year in the U.S., a figure that exceeds the annual number of gun crimes committed (around 1 million, according to government victimization surveys).

Lott and Kleck, as well as pro-gun activists, have used these studies to argue that policies that could potentially make guns less available to citizens may cause violent crime to increase by preventing more defensive gun uses than gun crimes. This paper summarizes some of the key problems with these studies and the authors' interpretations of their findings.

Evidence That Permissive Gun Carrying Laws Reduce Violent Crime

Currently, 31 states have laws that require local law enforcement authorities to issue permits to carry concealed handguns to any adult applicant who does not have a felony conviction or a history of serious mental illness. Prior to the implementation of such laws, local police had discretion in issuing such permits. Because most police officers are nervous about the possibility that every traffic stop or drunk-and-disorderly might be armed, law enforcement officials in states that allow police discretion in the issuance of gun carrying permits had typically issued only a limited number of such permits.

The argument by Lott and other proponents of permissive gun-carrying laws is that if more people could legally carry guns in public spaces, the chances that criminal predators encounter well-armed would-be victims will increase. This heightened risk faced by potential attackers will in turn dissuade them from committing violent crimes in the future.

The potential costs of these laws come from the possible misuse of guns by those with concealed-carry permits, and the potential complications that such laws may pose for police efforts to prevent illegal gun carrying. Another cost from these laws comes from the possibility of an "arms race" between criminals and law-abiding citizens. Previous research suggests that this is a plausible concern. Currently, a full 75% of robbers do not use guns to commit their crimes.[5] If more potential victims start carrying handguns, those robbers who continue to perpetrate street muggings may be more likely to use guns to commit their crimes. When they do, these robbers may be more likely to shoot first and ask questions later in an attempt

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[1] Lott JR Jr. *More Guns, Less Crime.* Chicago: University of Chicago Press, 1998.

[2] Lott JR Jr, Mustard D. Crime, deterrence and right-to-carry concealed handguns. *Journal of Legal Studies* 1997; 26:1–68.

[3] Lott JR Jr, Landes WM. Multiple-victim public shootings, bombings, and right-to-carry concealed handgun laws. University of Chicago Law School Working Paper, 1997.

[4] Kleck G, Gertz M. Armed resistance to crime: The prevalence and nature of self-defense with a gun. *Journal of Criminal Law and Criminology* 1995 (Fall); 86:150–187.

[5] Rennison CM. Criminal Victimization 1998: Changes 1997–98 with Trends 1993–98. (NCJ 176353) Bureau of Justice Statistics, U.S. Department of Justice, Washington D.C., July 1999.

to preempt an armed victim response. In fact, research by Philip Cook confirms that cities where more robbers use guns to commit their crimes also have higher robbery-murder rates.[6]

Since both positive and negative effects from these laws are in principle possible, what are the net effects on the overall rate of violent crime? The results of John Lott's research (or at least his interpretation of his findings) point one way, made clear by the book's title — *More Guns, Less Crime*. But, as we will demonstrate, the evidence that permissive gun carrying laws lead to substantial reductions in crime is shaky at best.

Much of Lott's book focuses on his and David Mustard's study that was designed to estimate the effects that permissive gun carrying laws had in the first 10 states that adopted them in the U.S. To estimate the impact of these laws, Lott analyzed data on crime trends from 1977 through 1992 for 3,054 counties across the U.S. His research approach was to identify the effects of permissive gun carrying laws by comparing changes in crime rates over time in states that adopted permissive concealed-carry laws with states that did not alter their usually more restrictive laws governing the issuing of permits to carrying concealed guns. These comparisons in trends statistically control for a number of differences across counties that may affect crime; for example, he controls for differences in the age, race, and income levels of populations. Some analyses also control for the presence of laws requiring waiting periods for handgun purchases and laws requiring mandatory minimum sentences for persons convicted of committing a violent crime with a gun.

The methods used in Lott's study are relatively sophisticated and, in some ways, are an improvement on previous evaluations of gun laws. But it is very difficult to derive valid estimates of the effects of 10 state gun laws due to the need to control for other factors that influence crime trends that may also be correlated with the passage of permissive gun carrying laws. The errors made in this study, several inconsistencies in the findings, the implausible estimates that are generated, and subsequent research on the effects of permissive gun carrying laws provide convincing evidence that Lott's methods do not adequately control for these other confounding factors.

We will not describe in detail all of the errors contained in *More Guns, Less Crime*. Readers are referred to the work of Professor Tim Lambert of the University of New South Wales for an extensive review of these errors, and our previous explanation of errors made in the classification of certain states' gun carrying laws.

Errors aside, the fundamental problem with Lott's research can be summarized by the old social science adage "correlation is not causation." Many variables may be related to one another yet not cause one another. For example, there is a significant association between a child's shoe size and the child's writing ability. But this correlation, of course, does not prove that large shoes improve writing ability.[7]

A similar inferential challenge lies at the heart of most policy evaluations, including Lott's study of the effects of permissive concealed-carry laws. If Florida has a lower crime rate than California, and Florida has a permissive concealed-carry law, can we conclude that the difference in crime rates is due to the gun-carrying legislation? In reality Florida and California differ along a number of dimensions, and attributing the difference in crime rates between the

[6] Cook PJ. "The effect of gun availability on robbery and robbery murder: A cross-section study of fifty cities." In *Policy Studies Review Annual, Volume 3*, RH Haveman and GG Zellner (eds.). Beverly Hills, CA: Sage, 1979.

[7] Kuzma JW. *Basic Statistics for the Health Sciences*. Mountain View, CA: Mayfield Publishing Company, 1984, page 159.

two states to any one factor is quite difficult. The obvious concern is that we will mistakenly attribute the difference in crime rates between Florida and California to the presence of a permissive concealed-carry law in the former, when in fact part or all of the difference will be due to other unmeasured differences across states. Lott does control for some differences between states that would explain some of the differences in crime rates. But he does not adequately control for many other factors that are almost surely relevant for a state's crime rate, including poverty, drugs (and in particular crack use and selling, which is widely thought to have been responsible for the dramatic increase in violent crime in America starting in the mid-1980's), gang activity, and police resources or strategies.

Lott tries to overcome this problem by comparing the *changes* in crime rates over time in states with versus without permissive concealed-carry laws. The idea is that unmeasured factors may cause California to have a higher crime rate than Florida, so focusing on the *change* in crime rates in Florida around the time of this state's gun-carrying law with the change observed in California around the same time will not be affected by the fact that California always has higher crime rates than Florida for reasons unrelated to the law. This research strategy assumes that the trend in crime rates in states like California and Florida would have been identical had Florida not enacted a permissive concealed-carry law.

But research by Dan Black at Syracuse University and Dan Nagin at Carnegie-Mellon show that: (1) states with permissive concealed-carry laws have violent crime trends that were different from other states even before the gun-carrying laws are enacted in that violence was increasing more in states the adopted permissive gun carrying laws than in other states in the years leading up to the permissive gun carrying law; and (2) the variables included in Lott's statistical models do a poor job of controlling for these differences in trends. As a result, differences in crime trends between states with and without permissive gun-carrying laws around the time of these laws cannot be attributed to the laws themselves, because all or part of the difference in trends around the time of the laws will be due to the unmeasured factors that caused the trends to be different before the laws went into effect. Crime trends in any particular area tend to be cyclical and regress to some long-term mean (average) after going up or down. Therefore, the reductions in violent crime observed after the introduction of permissive gun carrying laws may actually be simple regression to the mean, rather than the effects of the laws, as Lott suggests.

To his credit, Lott recognizes the potential problem with his crime-trend analysis. He attempts to remedy the problem in some of his analyses by using a more complicated statistical technique for identifying causal effects known as instrumental variables. Instrumental variables analyses are dependent on several crucial assumptions that may or may not hold in the crime data, though Lott presents none of the diagnostic tests that might help readers determine whether these assumptions are met. Instrumental variables require that the analyst identify a variable that is correlated with a state's gun carrying law, but is otherwise uncorrelated with differences across states in crime rates. One such variable that Lott uses is the proportion of a state's population that belongs to the National Rifle Association (NRA). While this variable is correlated with state concealed-carry laws, most people can recognize that

NRA representation within a state is likely to be correlated with crime rates for other reasons as well, since heavy NRA states are more likely than average to be rural and to support many other "tough on crime" measures. Lott uses other instrumental variables as well, though all of them have similar problems. In fact, the statistical problems with many of his instruments were discussed in a report issued on criminal deterrence by the National Academy of Sciences in 1978.[8]

Unlike most of the other findings that Lott describes in his book, he does not translate the results from the instrumental variable analyses into estimates of the percentage reduction in violent crime associated with the adoption of permissive gun carrying laws. When Lott's findings from these analyses are translated in this manner, the estimates suggest that enacting a permissive gun carrying law will, on average, reduce homicides by 67 percent, rapes by 65 percent, and assaults by 73 percent. If true, these results suggest that if every state in the union enacted a permissive gun carrying law, our murder rate would be reduced to levels not seen in this country since 1910, roughly similar to the rate currently observed in Finland. These implausibly large estimates of the laws' effects are strong evidence that Lott's efforts to address the problem with his crime trend comparisons was unsuccessful.

Lott's other study of the effects of permissive gun carrying laws on multiple-victim public shootings uses the same research approach at the study discussed above, and thus suffers from the same inferential problems. This study also produces estimates of the law effects that most would consider implausibly large — an 89% reduction in multiple-victim public shootings. One indicator of the implausibility of these estimates of the effects of permissive carry laws is Gary Kleck's skepticism that permissive gun carrying laws could produce the much more modest reductions in violent crime (usually 2%–8%) that Lott more commonly trumpets. Kleck (generator of implausibly large estimates of the number of successful defensive gun uses in the U.S.) states that Lott's conclusions that permissive gun carrying laws led to substantial reductions in violent crime

> ...could be challenged, in light of how modest the intervention was. The 1.3% of the population in places like Florida who obtained permits would represent at best only a slight increase in the share of potential crime victims who carry guns in public places. And if those who got permits were merely legitimating what they were already doing before the new laws, it would mean that there was no increase at all in carrying or in actual risks to criminals.... More likely, the declines in crime coinciding with relaxation of carry laws were largely attributable to other factors not controlled in the Lott and Mustard analysis.[9]

Indeed, a subsequent survey of new permit holders in North Carolina indicates that most had been taking a gun outside the home, in their vehicles, or on their person prior to obtaining the permit with little or no increased frequency in carrying after obtaining the permit.[10]

The study that Lott references to argue that permit holders are rarely arrested for crimes of violence also indicates that permit holders very rarely successfully use a gun to ward off a criminal attacker. This study examined data collected by the Dade County, Florida police dur-

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[8] Blumstein A, Cohen J, Nagin D. Eds. *Deterrence in Incapacitation: Estimating the Effects of Criminal Sanctions on Crime Rates.* Washington, DC: National Academy Press, 1978.

[9] Kleck G. *Targeting Guns: Firearms and Their Control.* New York: Aldine de Gruyter, 1997.

[10] Robuck-Mangum G. "Concealed Weapon Permit Holders in North Carolina: A Descriptive Study of Handgun Carrying Behaviors." Unpublished Master's Thesis, University of North Carolina — Chapel Hill, School of Public Health, 1997.

ing the first five years after Florida's permissive gun carrying law went into effect. During this period there were only three incidents in which a permit holder successfully used a gun in defense against a criminal attack outside the permit-holder's home.[11] [12] Considering that about 100,000 violent crimes were reported to Dade County police during the five-year study period, it is hard to argue that criminals are likely to have noticed a significant change in their risk of facing a victim armed with a gun.

Another way to assess whether the decreases in violent crime that Lott finds are associated with permissive gun carrying laws are actually attributable to the laws and not to unmeasured confounding factors is to see if the crime reductions are most pronounced for robberies than for other types of crimes because robberies are most likely to be committed against strangers in public places. But Lott's own research indicates that the violent crime category for which permissive gun carrying law effects were weakest (and often nonexistent) was robbery. Because even permissive gun carrying laws do not allow juveniles to legally carry guns, one should see greater reductions for victimizations of adults than of juveniles. Again, Lott's research as well as subsequent research[13] indicates that permissive gun carrying laws were not associated with greater reductions in murders of adults than of murders of juveniles.

The Myth of 2.5 Million Defensive Gun Uses Per Year

Kleck and Gertz's claim of 2.5 million defensive gun uses per year is derived from a telephone survey of 5,000 American adults conducted in 1992. Fifty-six respondents to this survey reported that they had used a gun in self-defense during the past year. Kleck and Gertz multiply the proportion of respondents in their survey who report a defensive gun use (X / 5,000 = Y percent) by the number of adults in the U.S. (around 200 million) and the number of defensive gun uses equals 2.5 million per year. They estimate that in 670,000 of these incidents the would-be victims used guns when they were away from their homes.

Many people are amazed that projections about national phenomena can be made based on a telephone survey of a few thousand adults. While many surveys of this type can provide useful information about national phenomena, in this particular case the public's skepticism is warranted. The primary problem is that, even if the Kleck and Gertz's estimates were accurate, defensive gun use is a relatively rare occurrence in that only 1% of respondents reported a defensive gun use during the previous 12 months. As David Hemenway of Harvard University has pointed out, inaccurate reporting of these events by a relatively small number of respondents could lead to population projections that are orders of magnitude different from the true incidence.[14] For example, if one-half of one percent of the survey respondents incorrectly reported that they had used a gun to defend themselves against a criminal attack during the past year, the estimated number of defensive gun uses would be twice as high the true number.

There are many reasons that respondents' reports of defensive gun use might be exaggerated. In some cases, respondents may have misjudged the level of danger they faced when they drew their gun. Survey researchers are also familiar with two types of response bias, "telescoping" and social desirability bias, that could lead to an overstated incidence of

[11] There were also three incidents in which permit holders unsuccessfully attempted to use a gun in defense against a criminal attack outside the home, including one case in which a robber took the permit holder's gun away.

[12] Data cited in: Cramer CE, Kopel DB. Shall issue: The new wave of concealed handgun permit laws. *Tennessee Law Review*, 1995; 62:679–758.

[13] Ludwig J. Concealed-gun-carrying laws and violent crime — evidence from state panel data. *International Review of Law and Economics*, 1998; 18:239–254.

[14] Hemenway D. Survey research and self-defense gun use: An explanation of extreme overestimates. *Journal of Criminal Law and Criminology* 1998.

reported events such as defensive gun use. Telescoping refers to the tendency of respondents to report that salient events such as a crime victimization or a defensive gun use occurred more recently than was the case. Evidence that the Kleck-Gertz survey respondents are telescoping their recollections of their crime victimizations comes from the estimated number of robbery victimizations it produces that is nearly five times as high as the estimate derived from the National Crime Victimization Survey (NCVS). The NCVS minimizes telescoping by using shorter recall periods and a panel design that re-surveys respondents multiple times over a three-year period.

Social desirability bias refers to the tendency of respondents to over-report their actions they believe others would find admirable such as an heroic act to defend oneself or others against a criminal. There is no way to definitively determine the degree to which social desirability bias may have influenced the Kleck-Gertz estimates of defensive gun use. However, it seems likely that the nearly half of the respondents reporting defensive gun uses who indicated that they believe their defensive gun use saved their life or the life of someone else probably thought of their actions as heroic. Such incidents are regularly reported in *American Rifleman*, a monthly magazine distributed to all members of the National Rifle Association, in a manner that unequivocally portrays the incidents as heroic acts.

Given these possible sources of error, it is not surprising that surveys sometimes produce quite puzzling results. For example, in his discussion of the pitfalls of using the Kleck-Gertz survey to make population projections about the incidence of defensive gun use, David Hemenway of Harvard University cites a 1994 phone survey of 1,500 adults living in the U.S. Six percent of the respondents to this survey reported having had personal contact with aliens from another planet. This six percent could be explained, in part, by the series of questions that led up to question about contact with aliens that set up the respondent to expect that the interviewer was hoping for some alien-contact answers. In addition, some small yet non-negligible percentage of survey respondents could be expected to have mental conditions that impair their perceptions and lead them to report defensive gun incidents that did not actually happen.

Not surprisingly, the combined effects of these problems can produce population estimates that are grossly out of line with other measures of violent crime. For example, the Kleck-Gertz projection for the number of assailants wounded by armed citizens in 1992 is more than twice as high as the estimate from another study of the *total* number of people treated for gunshot wounds in a nationally representative sample of hospitals in 1994. Finally, the Kleck-Gertz survey data suggest that, in serious crimes, the victim was four times more likely than the offender to have and use a gun, a highly implausible finding given the much higher rate of gun carrying among criminals compared with other citizens.

A Re-evaluation of the Science on Guns and Violent Crime is Not Warranted

The idea that the availability of guns increased the lethality of violent crime was first established by a 1968 study of crime in Chicago by Franklin Zimring, currently a law professor at the University of California at Berkeley. Zimring showed that most homicides and other assaults stem from arguments between people, rather than premeditated gangland-style executions. In addition, he found that assaults with a firearm were much more lethal than those in which the attacker uses a knife, even though the circumstances of gun and knife attacks closely resemble each other in most respects.[15] If the number of wounds inflicted is a reflection of the attackers' homicidal intentions, assailants using knives actually demonstrated greater intent to kill their victims than did the assailants who used guns. A similar conclusion was reached when Duke University professor Philip Cook compared gun and non-gun robberies in a series of studies during the '70's and '80's.[6, 16, 17] The implication is that more guns mean more death, and policies that can keep guns from violence-prone individuals should reduce the number of homicides.

In addition to increasing the lethality of violent acts against individuals, guns enhance assailants ability to, within seconds, wound or kill many people, including children and other innocent by-standers. It is no surprise that incidents in which assailants seriously injure or kill many people with weapons other than firearms are quite rare in the U.S. where firearms are so plentiful.

As a result, policy makers and researchers have struggled to identify ways to keep guns away from those who are most likely to misuse them, while preserving access to guns for most law-abiding adults. Among the gun control measures that are designed to reduce the availability of guns to potentially dangerous individuals include regulations that require background checks to screen eligible from ineligible buyers, registration of firearms, licensing of firearm owners, and restrictions on the number of firearms that can be legally purchased. Most of these measures have not be adequately evaluated, however, there is some evidence that background checks requirements for handgun sales have some effect in reducing violent behavior by convicted felons. Policy makers have also sought to regulate gun design with the objective of minimizing public health costs associated with gun misuse. Examples of this approach include bans on guns with fully-automatic firing mechanisms and proposals to require all new handguns to come equipped with devices that prevent unauthorized use. There is also evidence that restrictions on carrying of guns in public places, particularly in high-risk settings and often with stepped-up enforcement, can significantly reduce gun violence.[18, 19]

Although research by John Lott and Gary Kleck has challenged the prevailing view that gun regulations can reduce lethal crimes, the many limitations of Lott's and Kleck's research indicate that there is no reason to move from view of guns and violence backed by research in previous decades. Until proven otherwise, the best science indicates that more guns will lead to more deaths.

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[15] Zimring FE. Is gun control likely to reduce violent killings? *The University of Chicago Law Review* 1968; 35:721–737.

[16] Cook PJ. Reducing injury and death rates in robbery. *Policy Analysis* 1980; 6:21–45.

[17] Cook PJ. Robbery violence. *Journal of Criminal Law and Criminology* 1987; 78:357–376.

[18] Sherman LW, Shaw JW, Rogan DP. *The Kansas City gun experiment*. National Institute of Justice Research in Brief. Washington, D.C.: U.S. Dept. of Justice, Office of Justice Programs, National Institute of Justice, January 1995.

[19] Fagan J; Zimring FE; Kim J. Declining homicide in New York City: A tale of two trends. *Journal of Criminal Law and Criminology*, 1998 Summer; 88(4):1277–1323.

EXHIBIT D:

Abhay Aneja, John J. Donohue III, Alexandria Zhang, *The Impact of Right-to-Carry Laws and the NRC Report: Lessons for the Empirical Evaluation of Law and Policy*, 13:2 AMERICAN LAW AND ECONOMICS REVIEW 565 (Fall 2011)

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 public—so-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.[1] 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

1. 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[2]. 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

2. 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 law-abiding citizens during criminal acts and by deterring potential criminals from ever committing offenses for fear of encountering an armed response.[3] 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

3. 143 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

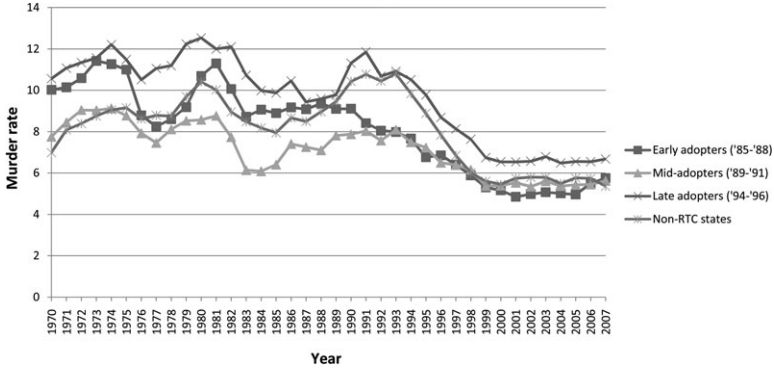


Figure 1. Murder Trends in RTC versus Non-RTC States—Weighted Average of Murder Rates per 100,000 Residents (1970–2007).

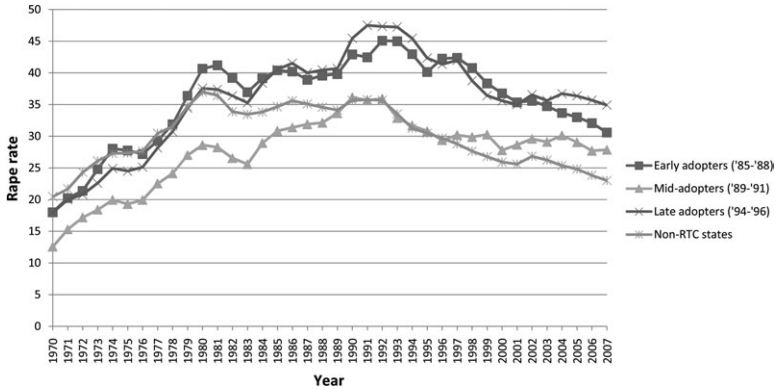


Figure 2. Rape Trends in RTC versus Non-RTC States—Weighted Average of Rape Rates per 100,000 Residents (1970–2007).

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

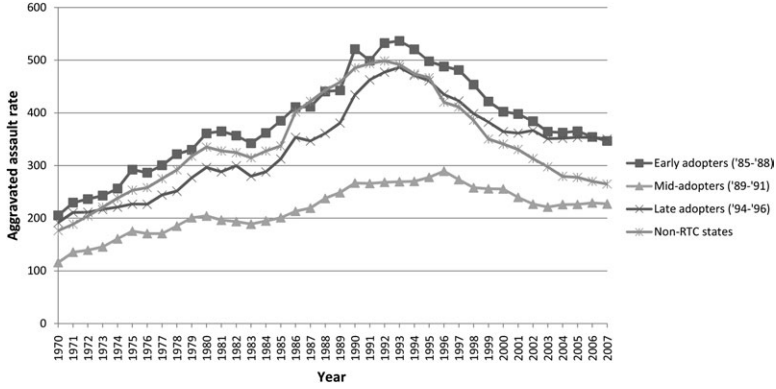


Figure 3. Assault Trends in RTC versus Non-RTC States—Weighted Average of Assault Rates per 100,000 Residents (1970–2007).

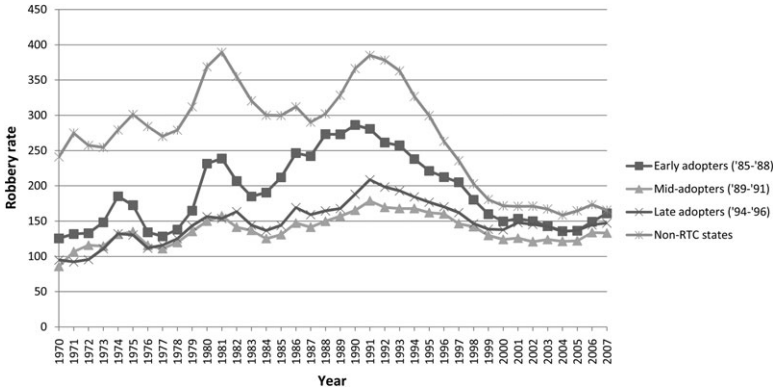


Figure 4. Robbery Trends in RTC versus Non-RTC States—Weighted Average of Robbery Rates per 100,000 Residents (1970–2007).

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

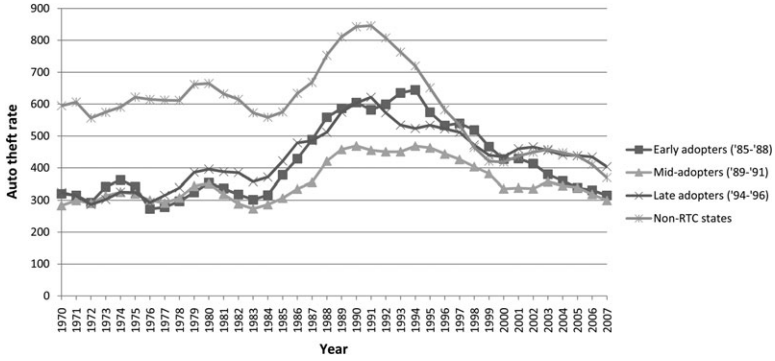


Figure 5. Auto Theft Trends in RTC versus Non-RTC States—Weighted Average of Auto Theft Rates per 100,000 Residents (1970–2007).

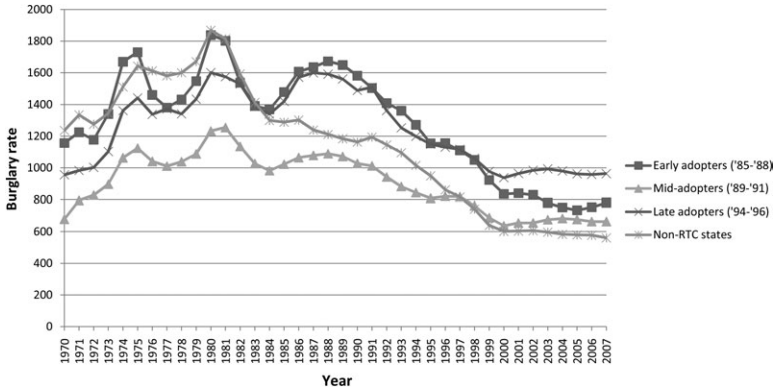


Figure 6. Burglary Trends in RTC versus Non-RTC States—Weighted Average of Burglary Rates per 100,000 Residents (1970–2007).

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.[4]

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

4. 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).

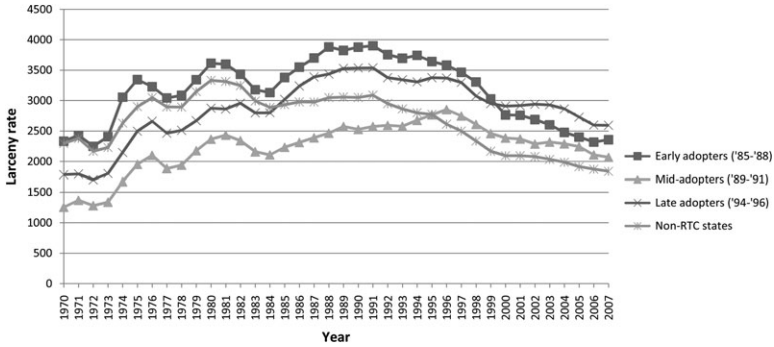


Figure 7. Larceny Trends in RTC versus Non-RTC States—Weighted Average of Larceny Rates per 100,000 Residents (1970–2007).

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 intra-panel 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.[5] 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.[6] 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

5. 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).

6. 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)[a]

	Murder (%)	Rape (%)	Aggravated Assault (%)	Robbery (%)	Auto Theft (%)	Burglary (%)	Larceny (%)
1. Dummy variable model	−1.95	17.91***	12.34***	19.99***	23.33***	19.06***	22.58***
	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***

[a]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 1b. Estimated Impact of RTC Laws—Using NRC County Data—No Controls, All Crimes, 1977—2000[a]

	Murder (%)	Rape (%)	Aggravated Assault (%)	Robbery (%)	Auto Theft (%)	Burglary (%)	Larceny (%)
1. Dummy variable model	−2.58	18.40***	12.60***	19.70***	22.80***	19.00***	22.60***
	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 dummy	−0.06	16.20***	11.90***	17.40***	16.80***	17.70***	18.50***
	2.33	2.22***	1.69***	1.88***	1.86***	1.34***	1.20***
Trend effect	−0.56	0.58	0.22	0.51	1.32***	0.28	0.98***
	0.43	0.40	0.30	0.35	0.35***	0.27	0.25***

[a]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 1c. Estimated Impact of RTC Laws—Using Updated 2009 County-Level Data—No Controls, All Crimes, 1977–2000 (without 1993 Data)[a]

	Murder (%)	Rape (%)	Aggravated Assault (%)	Robbery (%)	Auto Theft (%)	Burglary (%)	Larceny (%)
1. Dummy variable model	−2.20	27.80***	16.40***	19.50***	23.90***	22.80***	28.10***
	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 dummy	−7.99***	12.00***	−3.50	8.91***	5.50**	1.44	3.26
	2.19***	3.08***	2.72	2.32***	2.70**	2.60	2.98
Trend effect	1.34***	3.66***	4.60***	2.44***	4.27***	4.94***	5.75***
	0.33***	0.37***	0.32***	0.30***	0.32***	0.31***	0.35***

[a]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.[7] 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).[8] This data extension also gives us an opportunity to explore how the NRC's results are

7. 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.

8. 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).[9]

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.[10] 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 overwhelmingly 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

9. 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.

10. 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.[11] 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:

$$Y_{it} = \eta \mathrm{RTC}_{jt} + \alpha_i + \theta_t + \beta_{jt} + \gamma X_{ijt} + \epsilon_{it}, \tag{1}$$

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_{jt}. 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).[12]

The variable α_i indicates county-level fixed effects (unobserved county traits) and θ_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 β_{jt} indicating state-specific trends, which are introduced in selected models. Since neither Lott and Mustard (1997) nor the NRC (2005) examines state

11. 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.

12. 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).[13] 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).

Table 2a. Estimated Impact of RTC Laws—Published NRC Estimates—Lott-Mustard Controls, All Crimes, 1977–2000[a]

	Murder (%)	Rape (%)	Aggravated Assault (%)	Robbery (%)	Auto Theft (%)	Burglary (%)	Larceny (%)
1. Dummy variable model	−8.33***	−0.16	3.05***	3.59***	12.74***	6.19***	12.40***
	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***

[a]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%.

13. 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.

Table 2b. Estimated Impact of RTC Laws—Using NRC Data—with Lott-Mustard Controls, All Crimes, 1977–2000[a]

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

[a]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)[a]

	Murder (%)	Rape (%)	Aggravated Assault (%)	Robbery (%)	Auto Theft (%)	Burglary (%)	Larceny (%)
1. Dummy variable model	−3.80**	9.82***	8.96***	5.44***	13.60***	4.36***	12.90***
	1.87**	2.74***	1.34***	1.45***	1.40***	0.95***	0.88***
2. Spline model	−0.26	0.48	1.10***	0.26	1.50***	0.30**	1.16***
	0.28	0.33	0.18***	0.21	0.19***	0.15**	0.14***
3. Hybrid model							
Post-passage dummy	−3.98*	11.40***	6.34***	6.39***	10.60***	4.53***	11.80***
	2.22*	2.62***	1.48***	1.66***	1.57***	1.05***	0.94***
Trend effect	0.04	−0.38	0.63***	−0.23	0.70***	−0.04	0.28*
	0.33	0.30	0.20***	0.25	0.22***	0.16	0.15*

[a]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).[14] We do not know if the committee dropped this variable before conducting its analysis, but we drop it in our own analysis.[15] 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.[16]

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

14. 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.

15. We found that whether or not we include this variable, we cannot replicate the NRC's results (in Table 2a).

16. 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 that are underestimated. Lott, however, suggests that clustered 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.

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)[a]

		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

[a]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)[a]

		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

[a]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)[a]

	Murder (%)	Rape (%)	Aggravated Assault (%)	Robbery (%)	Auto Theft (%)	Burglary (%)	Larceny (%)
1. Dummy variable model	−3.80	9.82	8.96*	5.44	13.60**	4.36	12.90***
	6.25	11.20	5.33*	5.53	5.83**	3.58	3.97***
2. Spline model	−0.26	0.48	1.10	0.26	1.50*	0.30	1.16
	0.80	1.22	0.81	0.85	0.83*	0.50	0.82
3. Hybrid model							
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	−0.38	0.63	−0.23	0.70	−0.04	0.28
	0.89	0.86	0.76	0.81	0.77	0.49	0.65

[a]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).[17]

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

17. 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.

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)[a]

	Murder (%)	Rape (%)	Aggravated Assault (%)	Robbery (%)	Auto Theft (%)	Burglary (%)	Larceny (%)
1. Dummy variable model	−6.17	−10.80	3.00	−5.31	0.21	−5.19	−0.40
	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
3. Hybrid model							
Post-passage dummy	−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

[a]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).[18]

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).

18. 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)[a]

	Murder (%)	Rape (%)	Aggravated Assault (%)	Robbery (%)	Auto Theft (%)	Burglary (%)	Larceny (%)
1. Dummy variable model	−5.44	10.40	11.40**	3.10	14.40**	7.48*	12.90***
	5.91	13.20	4.84**	4.47	6.65**	3.85*	3.96***
2. Spline model	−0.28	0.61	1.05	0.39	0.99	0.44	1.07**
	0.60	1.03	0.69	0.54	0.61	0.43	0.51**
3. Hybrid model							
Post-passage dummy	−5.35	9.77	8.39**	1.69	12.60**	6.99*	10.10***
	6.05	12.00	3.48**	5.43	5.91**	3.99*	3.68***
Trend effect	−0.02	0.14	0.65	0.30	0.39	0.10	0.59
	0.61	0.74	0.63	0.65	0.47	0.44	0.49

[a]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)[a]

	Murder (%)	Rape (%)	Aggravated Assault (%)	Robbery (%)	Auto Theft (%)	Burglary (%)	Larceny (%)
1. Dummy variable model	−4.45	−13.00	3.44	−0.22	3.81	−0.77	1.51
	4.44	8.14	3.13	5.48	4.84	3.53	3.10
2. Spline model	−0.96	−4.51	1.72*	−0.95	−0.91	−0.82	−0.66
	0.96	3.74	0.94*	1.60	1.10	1.04	0.87
3. Hybrid model							
Post-passage dummy	−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	−4.26	1.66*	−0.96	−1.01	−0.82	−0.70
	0.98	3.69	0.93*	1.62	1.08	1.07	0.89

[a]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.[19]

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.[20] 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

19. For extended discussion on the abundant problems with this pseudo arrest rate, see Donohue and Wolfers (forthcoming).

20. 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.[21]

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.[22] 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

21. 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.

22. These results are not presented here since standard errors clustered on state are clearly needed. The authors can provide these results upon request.

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)[a]

	Murder (%)	Rape (%)	Aggravated Assault (%)	Robbery (%)	Auto Theft (%)	Burglary (%)	Larceny (%)
1. Dummy variable model	−0.44	21.30	21.60	19.30	24.80	26.60	29.50
	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 dummy	−2.72	12.60	7.40	7.92	12.00	11.10	10.90
	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*

[a]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 7b. Estimated Impact of RTC Laws—Using Updated 2009 County-Level Data—with Preferred Controls, with Clustered Standard Errors and State Trends, All Crimes, 1977–2006 (without 1993 Data)[a]

	Murder (%)	Rape (%)	Aggravated Assault (%)	Robbery (%)	Auto Theft (%)	Burglary (%)	Larceny (%)
1. Dummy variable model	−3.11	−15.50	0.02	1.15	1.89	−3.98	−3.22
	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	−13.00	−0.22	1.48	2.29	−3.25	−2.35
	5.08	9.98	10.30	7.64	10.40	11.50	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

[a]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.[23] 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 shortcomings 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

23. 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.[24] 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.[25] 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

24. 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.

25. 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.

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[a]

	Murder (%)	Rape (%)	Aggravated Assault (%)	Robbery (%)	Auto Theft (%)	Burglary (%)	Larceny (%)
1. Dummy variable model	−4.94	−5.04**	1.44	−6.96**	0.31	−4.97**	2.32
	3.61	2.29**	4.11	2.90**	3.98	2.22**	1.58
2. Spline model	−0.03	−0.49	0.80	−0.16	−0.87**	−0.44	0.40
	0.54	0.33	0.66	0.60	0.42**	0.45	0.29
3. Hybrid model							
Post-passage dummy	−5.62	−3.77	−1.69	−7.41**	4.00	−3.92*	1.03
	4.25	2.36	3.26	3.59**	4.88	2.03*	1.80
Trend effect	0.19	−0.35	0.86	0.12	−1.02**	−0.29	0.36
	0.58	0.36	0.64	0.64	0.50**	0.46	0.32

[a]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[a]

	Murder (%)	Rape (%)	Aggravated Assault (%)	Robbery (%)	Auto Theft (%)	Burglary (%)	Larceny (%)
1. Dummy variable model	−3.32	−3.33	−1.12	−3.36	2.64	−1.93	1.21
	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 dummy	−3.83	−3.78	−3.33	−3.90	4.51	−2.33	0.92
	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

[a]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 9a. Estimated Impact of RTC Laws—Using Updated 2009 State-Level Data—with Preferred Controls, with Clustered Standard Errors, All Crimes, 1977–2006[a]

	Murder (%)	Rape (%)	Aggravated Assault (%)	Robbery (%)	Auto Theft (%)	Burglary (%)	Larceny (%)
1. Dummy variable model	−2.93	−0.62	5.05	5.36	7.03	2.24	6.72**
	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	1.71	0.15	3.09	6.29	2.82	5.22*
	3.75	3.52	3.56	4.74	5.49	3.21	3.05*
Trend effect	−0.04	−0.52	1.09*	0.50	0.17	−0.13	0.34
	0.63	0.56	0.63*	0.83	0.56	0.43	0.50

[a]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 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[a]

	Murder (%)	Rape (%)	Aggravated Assault (%)	Robbery (%)	Auto Theft (%)	Burglary (%)	Larceny (%)
1. Dummy variable model	0.54	−3.61*	−2.03	2.40	8.17*	1.51	1.89
	2.72	1.83*	3.05	3.67	4.16*	2.18	1.83
2. Spline model	0.83	0.08	3.10**	0.51	−1.84**	−0.22	−0.15
	0.87	0.79	0.81**	1.29	0.82**	0.88	0.74
3. Hybrid model							
Post-passage dummy	0.11	−3.70*	−3.68	2.17	9.26**	1.65	1.99
	2.86	1.96*	3.15	3.96	4.24**	2.41	1.97
Trend effect	0.83	0.19	3.21***	0.44	−2.11**	−0.27	−0.20
	0.89	0.79	0.82***	1.35	0.84**	0.91	0.77

[a]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.[26]

26. 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

Table 10a. Estimated Impact of RTC Laws—Using Updated 2009 State-Level Data—with Preferred Controls, with Clustered Standard Errors, All Crimes, 1977–1992[a]

	Murder (%)	Rape (%)	Aggravated Assault (%)	Robbery (%)	Auto Theft (%)	Burglary (%)	Larceny (%)
1. Dummy variable model	−3.69	−12.10***	−6.55	−4.85	7.28	−3.73	0.12
	3.81	3.41***	4.66	4.07	4.73	2.45	1.52
2. Spline model	−0.88	−2.87***	0.52	−2.28***	0.51	−0.34	−0.10
	1.44	0.80***	1.70	0.72***	1.13	0.83	0.33
3. Hybrid model							
Post-passage dummy	−2.32	−7.59**	−11.80**	1.08	9.07*	−4.37	0.54
	4.70	3.01**	5.64**	5.32	4.61*	3.87	1.82
Trend effect	−0.56	−1.83***	2.13	−2.42**	−0.73	0.26	−0.17
	1.67	0.59***	1.47	1.08**	0.85	0.97	0.42

[a]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[a]

	Murder (%)	Rape (%)	Aggravated Assault (%)	Robbery (%)	Auto Theft (%)	Burglary (%)	Larceny (%)
1. Dummy variable model	−5.61	−4.14	−2.02	−3.78	−0.04	−3.05	1.28
	3.57	3.61	3.70	4.25	3.84	2.23	1.96
2. Spline model	−5.41**	0.27	−0.05	−4.35*	−1.62	−2.36	0.37
	2.45**	1.11	1.17	2.48*	2.20	1.43	1.15
3. Hybrid model							
Post-passage dummy	2.47	−6.67*	−2.89	3.08	3.17	0.18	1.16
	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

[a]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 11a. Estimated Impact of RTC Laws—Using Updated 2009 State-Level Data—with Preferred Controls, with Clustered Standard Errors, All Crimes, 1993–2006[a]

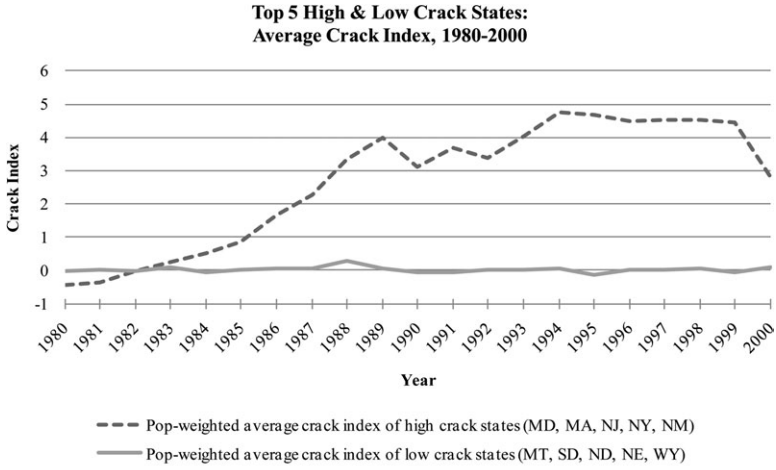
	Murder (%)	Rape (%)	Aggravated Assault (%)	Robbery (%)	Auto Theft (%)	Burglary (%)	Larceny (%)
1. Dummy variable model	3.12	−3.47	1.36	3.64	2.46	3.58	0.27
	3.61	2.47	3.54	4.89	4.50	2.57	2.74
2. Spline model	1.11*	−0.21	1.91**	1.78**	−0.30	0.35	0.08
	0.63*	0.68	0.74**	0.87**	0.80	0.71	0.55
3. Hybrid model							
Post-passage dummy	2.36	−3.35	0.03	2.42	2.70	3.37	0.22
	3.82	2.46	4.05	4.73	4.33	2.57	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

[a]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[a]

	Murder (%)	Rape (%)	Aggravated Assault (%)	Robbery (%)	Auto Theft (%)	Burglary (%)	Larceny (%)
1. Dummy variable model	3.12	0.27	2.38	3.81	2.83	0.89	0.33
	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	−0.75	0.79	4.04	5.12	1.20	0.93
	3.87	2.46	2.40	3.48	3.43	2.29	1.98
Trend effect	−2.44	2.69**	4.25**	−0.62	−6.10**	−0.84	−1.59
	2.10	1.14**	1.61**	1.95	2.99**	1.80	1.42

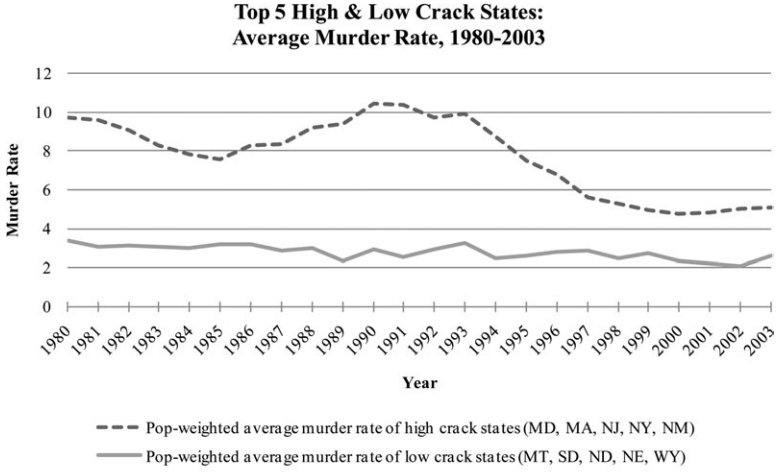
[a]Estimations include year and county fixed effects, and are weighted by county population. Robust standard errors are provided beneath point estimates. The control variables for this "preferred" specification include incarceration and police rates (lagged one year to avoid potential endogeneity issues), 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).

Figure 8. Prevalence of Crack in the Five Most and the Five Least Crack-Affected States.

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.



Source: Bureau of Justice Statistics (2009).

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).[27] 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.[28]

Table 12. Population-Weighted Statistics of RTC-Adopting States between 1977 and 1990[a]

State	Year of RTC 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

Notes: Source—Fryer et al. (2005) and Bureau of Justice Statistics (2009).
[a]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.)

27. 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).

28. 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 year-by-year specification, adding pre- and post-passage dummy variables to the analysis.[29] 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

29. In Appendix C, we further analyze the issue of misspecification and model fit by analyzing residuals from the regression analysis.

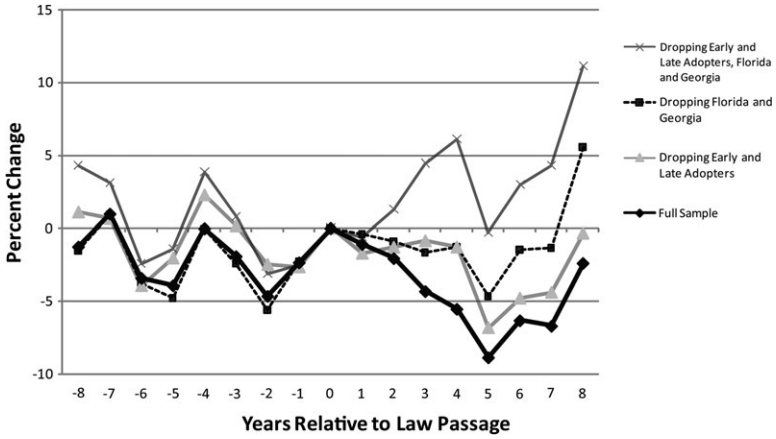


Figure 10. Normalized Year-by-Year Estimates of the Impact of RTC Laws on Murder.
Notes: Estimations include year and county fixed effects, state trends, and are weighted by county population. The control variables include incarceration and police rates, unemployment rate, poverty rate, county population, 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

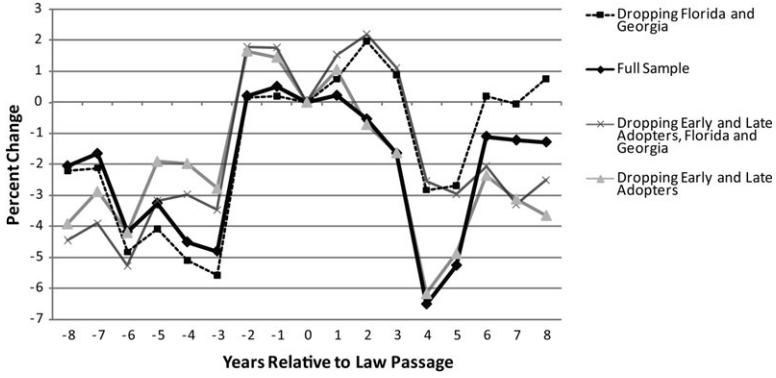


Figure 11. Normalized Year-by-Year Estimates of the Impact of RTC Laws on Rape.
Notes: Estimations include year and county fixed effects, state trends, and are weighted by county population. The control variables include incarceration and police rates, unemployment rate, poverty rate, county population, population density, per capita income, and six demographic composition measures.

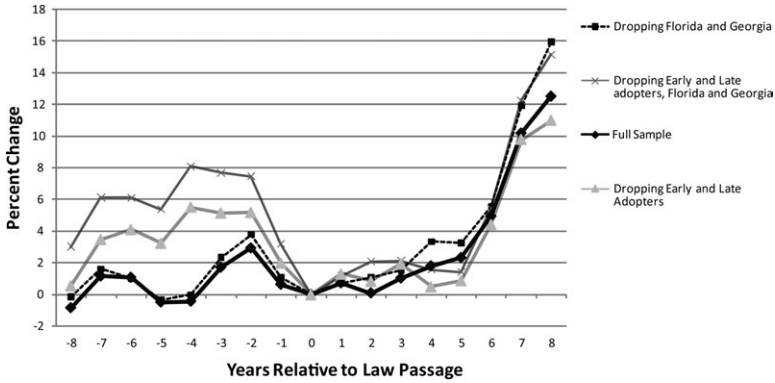


Figure 12. Normalized Year-by-Year Estimates of the Impact of RTC Laws on Assault.
Notes: Estimations include year and county fixed effects, state trends, and are weighted by county population. The control variables include incarceration and police rates, unemployment rate, poverty rate, county population, 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

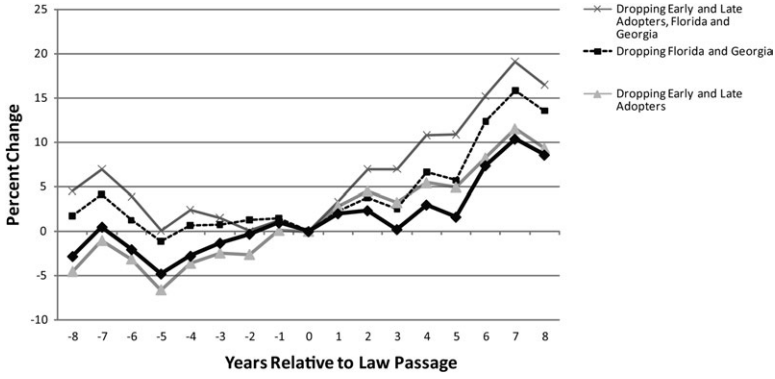


Figure 13. Normalized Year-by-Year Estimates of the Percent Change in Robbery.
Notes: Estimations include year and county fixed effects, state trends, and are weighted by county population. The control variables include incarceration and police rates, unemployment rate, poverty rate, county population, 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.[30]

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).[31]

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

30. 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).

31. 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.[32]

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 pre-law 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

32. 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 gun-related 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

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[a]

	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*

[a]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![33] 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

33. 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.[34] 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

34. 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.

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)

		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 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 crime-*increasing* 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

Table B1. Estimated Impact of RTC Laws—Using Updated 2009 County-Level Data—No Controls, with Clustered Standard Errors, All Crimes, 1977–2006[a]

	Murder (%)	Rape (%)	Aggravated Assault (%)	Robbery (%)	Auto Theft (%)	Burglary (%)	Larceny (%)
1. Dummy variable model	−0.55	33.10	27.30	25.50*	33.50	35.90	38.00
	8.30	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 dummy	−3.48	21.40	14.30	14.30	21.40	21.50	21.30
	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*

[a]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 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[a]

	Murder (%)	Rape (%)	Aggravated Assault (%)	Robbery (%)	Auto Theft (%)	Burglary (%)	Larceny (%)
1. Dummy variable model	−2.80	−13.10	5.02	3.10	5.58	1.50	2.98
	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	−10.50	3.94	3.35	5.73	1.97	3.48
	5.22	10.10	10.20	8.27	10.20	11.40	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

[a]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

Table B3. Estimated Impact of RTC Laws—Using Updated 2009 State-Level Data—No Controls, with Clustered Standard Errors, All Crimes, 1977–2006[a]

	Murder (%)	Rape (%)	Aggravated Assault (%)	Robbery (%)	Auto Theft (%)	Burglary (%)	Larceny (%)
1. Dummy variable model	−1.79	8.33	11.70**	20.00**	24.70**	18.30***	16.60***
	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 dummy	−3.22	5.90	5.36	13.30*	19.60**	12.70**	11.00***
	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**

[a]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[a]

	Murder (%)	Rape (%)	Aggravated Assault (%)	Robbery (%)	Auto Theft (%)	Burglary (%)	Larceny (%)
1. Dummy variable model	−0.31	−4.66**	0.62	3.43	8.38	1.10	0.92
	3.73	2.00**	3.36	4.92	5.28	2.93	2.37
2. Spline model	0.78	−0.54	2.46***	0.29	−0.16	−0.20	−0.46
	0.93	0.92	0.91***	1.39	1.71	0.80	0.63
3. Hybrid model							
Post-passage dummy	−0.80	−4.39**	−0.90	3.30	8.63	1.25	1.24
	3.67	2.03**	3.37	5.30	5.17	3.23	2.55
Trend effect	0.80	−0.44	2.48***	0.21	−0.39	−0.23	−0.49
	0.93	0.91	0.92***	1.43	1.70	0.84	0.67

[a]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.[35] In particular, we found that the residual variance was high for smaller states, even when we do not weight our regressions by population.[36] 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 high–residual 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

35. 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.

36. 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.

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)[a]

	Murder (%)	Rape (%)	Aggravated Assault (%)	Robbery (%)	Auto Theft (%)	Burglary (%)	Larceny (%)
1. Dummy variable model	−3.53	−0.98	4.33	5.04	6.80	1.38	5.75*
	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 dummy	−3.69	1.65	−1.21	2.53	5.26	1.69	3.94
	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

[a]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)[a]

	Murder (%)	Rape (%)	Aggravated Assault (%)	Robbery (%)	Auto Theft (%)	Burglary (%)	Larceny (%)
1. Dummy variable model	−4.99	−0.28	3.94	5.80	8.13	2.86	6.75**
	4.23	4.28	2.40	4.97	6.60	3.20	3.23**
2. Spline model	−0.16	−0.50	0.84*	0.90	0.71	0.29	0.71
	0.66	0.59	0.47*	0.83	0.70	0.37	0.50
3. Hybrid model							
Post-passage dummy	−5.38	2.53	0.15	2.09	6.16	1.91	4.39
	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

[a]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 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)[a]

	Murder (%)	Rape (%)	Aggravated Assault (%)	Robbery (%)	Auto Theft (%)	Burglary (%)	Larceny (%)
1. Dummy variable model	1.17	−3.56	−0.13	2.28	7.82**	1.31	1.77
	2.95	2.16	2.82	3.75	3.26**	2.03	1.66
2. Spline model	0.80	0.15	2.83***	0.32	−2.01**	−0.31	−0.21
	0.91	0.81	0.82***	1.37	0.83**	0.91	0.79
3. Hybrid model							
Post-passage dummy	0.73	−3.71	−1.77	2.14	9.13***	1.51	1.93
	3.12	2.32	2.80	4.04	3.23***	2.31	1.84
Trend effect	0.77	0.27	2.89***	0.25	−2.29***	−0.35	−0.27
	0.95	0.83	0.84***	1.42	0.83***	0.95	0.83

[a]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)[a]

	Murder (%)	Rape (%)	Aggravated Assault (%)	Robbery (%)	Auto Theft (%)	Burglary (%)	Larceny (%)
1. Dummy variable model	2.09	−2.88	−1.35	4.63	8.94***	1.42	2.41
	2.97	2.29	2.78	3.44	3.18***	2.14	1.68
2. Spline model	0.92	0.25	2.42***	0.63	−2.11**	−0.43	−0.12
	0.97	0.83	0.80***	1.44	0.88**	0.99	0.83
3. Hybrid model							
Post-passage dummy	1.69	−3.03	−2.50	4.39	10.00***	1.63	2.50
	3.09	2.40	2.83	3.71	3.18***	2.40	1.87
Trend effect	0.88	0.32	2.48***	0.53	−2.35**	−0.47	−0.18
	1.01	0.84	0.81***	1.50	0.87**	1.02	0.87

[a]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).[37] 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.[38] 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

37. 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.

38. 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.

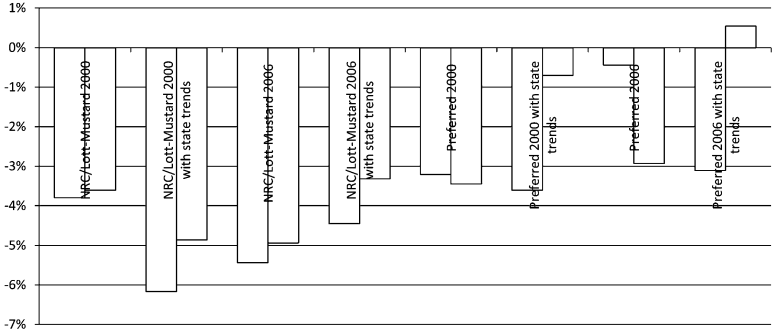


Figure D1. Various Murder Estimates (Dummy Model).

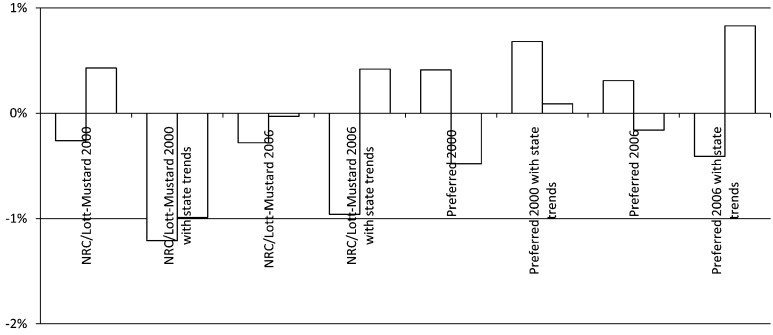


Figure D2. Various Murder Estimates (Spline Model).

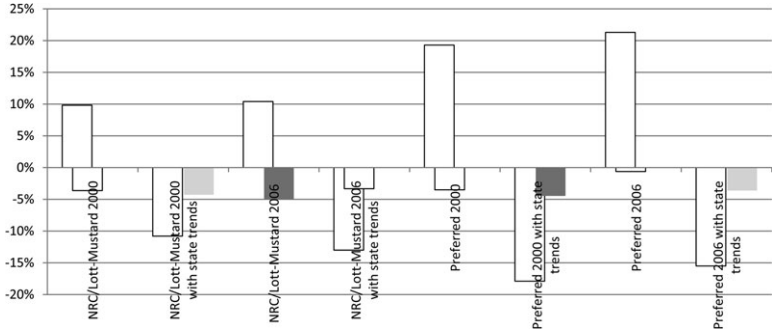


Figure D3. Various Rape Estimates (Dummy Model).

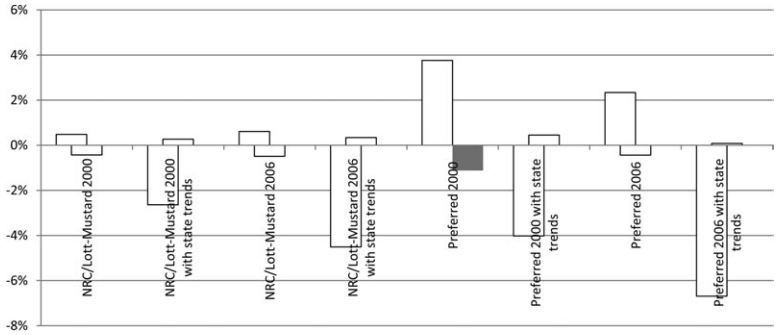


Figure D4. Various Rape Estimates (Spline Model).

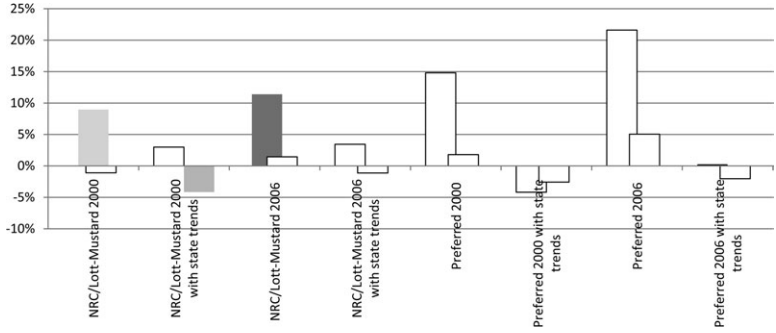


Figure D5. Various Assault Estimates (Dummy Model).

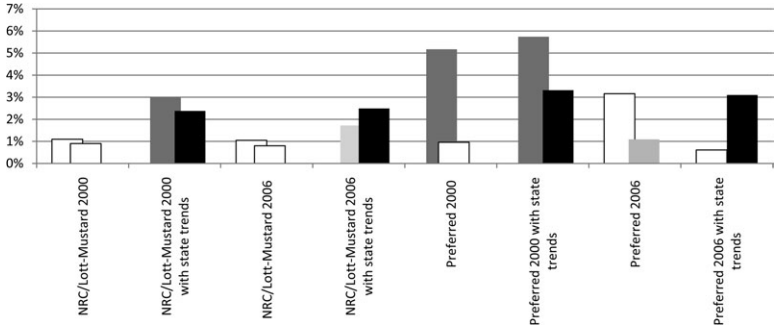


Figure D6. Various Assault Estimates (Spline Model).

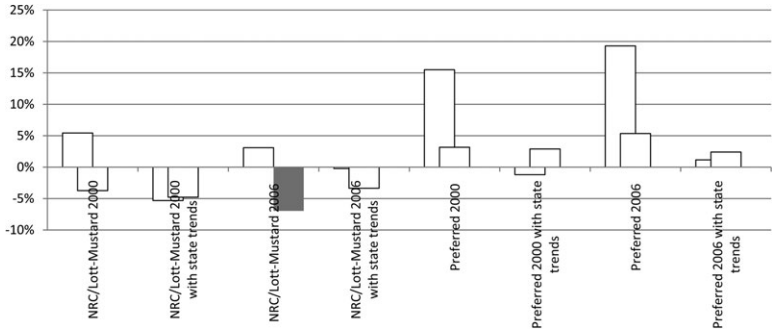


Figure D7. Various Robbery Estimates (Dummy Model).

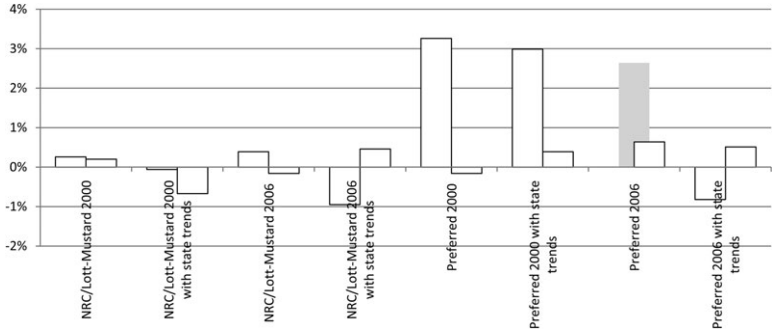


Figure D8. Various Robbery Estimates (Spline Model).

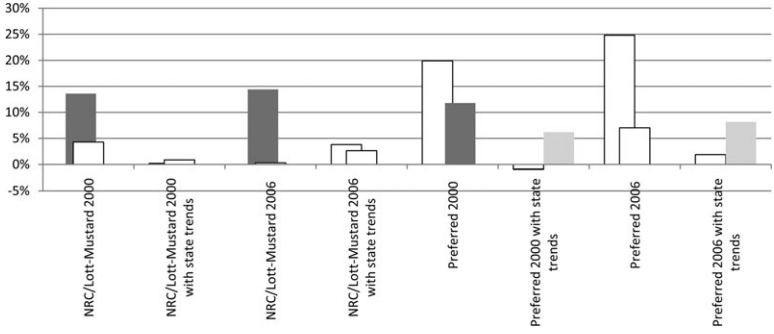


Figure D9. Various Auto Theft Estimates (Dummy Model).

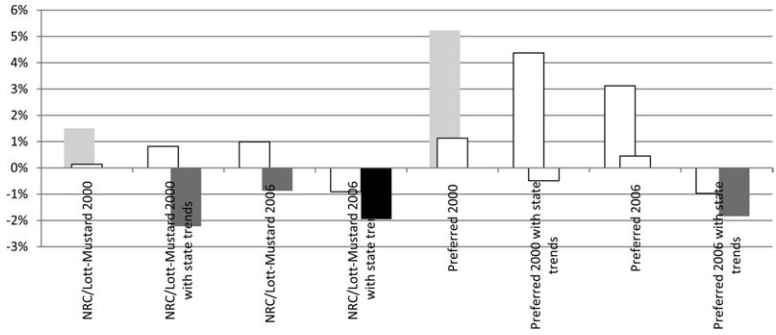


Figure D10. Various Auto Theft Estimates (Spline Model).

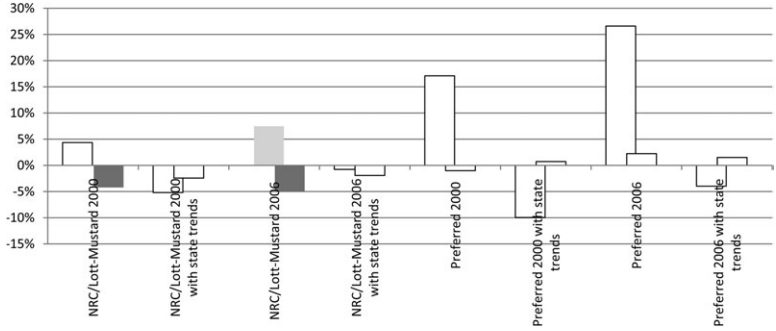


Figure D11. Various Burglary Estimates (Dummy Model).

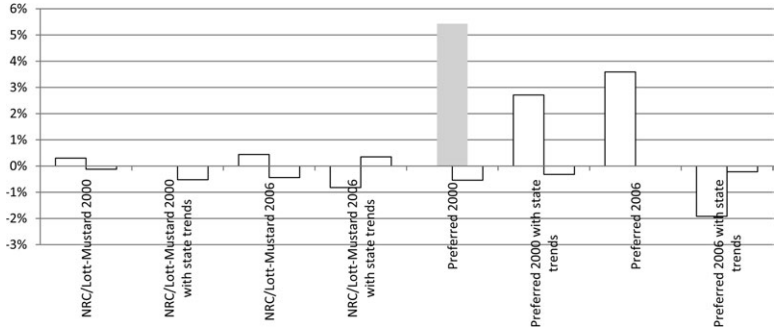


Figure D12. Various Burglary Estimates (Spline Model).

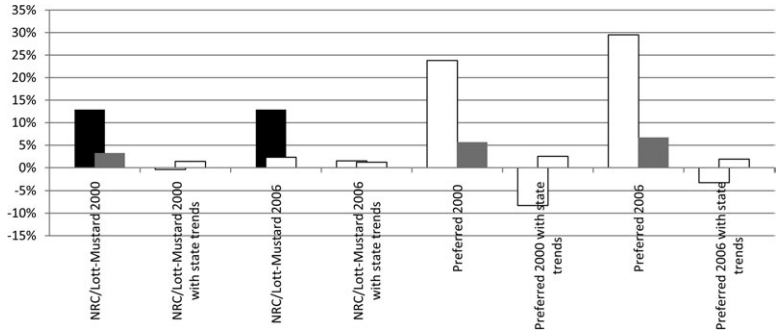


Figure D13. Various Larceny Estimates (Dummy Model).

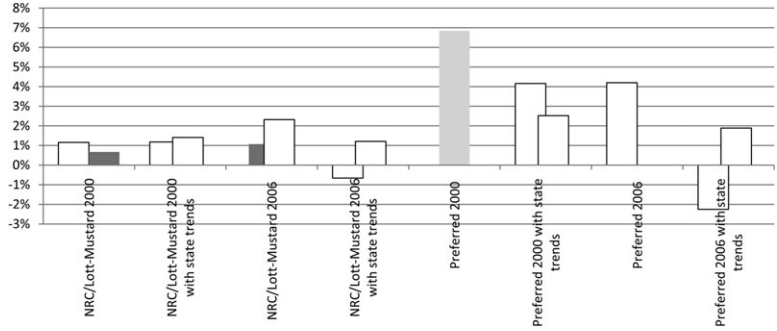


Figure D14. Various Larceny Estimates (Spline Model).

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 E:

Michael Luo, *Guns in Public, and Out of Sight*, N.Y. TIMES, Dec. 26, 2011

December 26, 2011

Guns in Public, and Out of Sight

By MICHAEL LUO

Alan Simons was enjoying a Sunday morning bicycle ride with his family in Asheville, N.C., two years ago when a man in a sport utility vehicle suddenly pulled alongside him and started berating him for riding on the highway.

Mr. Simons, his 4-year-old son strapped in behind him, slowed to a halt. The driver, Charles Diez, an Asheville firefighter, stopped as well. When Mr. Simons walked over, he found himself staring down the barrel of a gun.

“Go ahead, I’ll shoot you,” Mr. Diez said, according to Mr. Simons. “I’ll kill you.”

Mr. Simons turned to leave but heard a deafening bang. A bullet had passed through his bike helmet just above his left ear, barely missing him.

Mr. Diez, as it turned out, was one of more than 240,000 people in North Carolina with a permit to carry a concealed handgun. If not for that gun, Mr. Simons is convinced, the confrontation would have ended harmlessly. “I bet it would have been a bunch of mouthing,” he said.

Mr. Diez, then 42, eventually pleaded guilty to assault with a deadly weapon with intent to kill.

Across the country, it is easier than ever to carry a handgun in public. Prodded by the gun lobby, most states, including North Carolina, now require only a basic background check, and perhaps a safety class, to obtain a permit.

In state after state, guns are being allowed in places once off-limits, like bars, college campuses and houses of worship. And gun rights advocates are seeking to expand the map still further, pushing federal legislation that would require states to honor other states’ concealed weapons permits. The House approved the bill last month; the Senate is expected to take it up next year.

The bedrock argument for this movement is that permit holders are law-abiding citizens who should be able to carry guns in public to protect themselves. “These are people who have proven themselves to be among the most responsible and safe members of our community,” the federal legislation’s author, Representative Cliff Stearns, Republican of Florida, said on the House floor.

To assess that claim, The New York Times examined the permit program in North Carolina, one of a dwindling number of states where the identities of permit holders remain public. The review, encompassing the last five years, offers a rare, detailed look at how a liberalized concealed weapons law has played out in one state. And while it does not provide answers, it does raise questions.

More than 2,400 permit holders were convicted of felonies or misdemeanors, excluding traffic-related crimes, over the five-year period, The Times found when it compared databases of recent criminal court cases and licensees. While the figure represents a small percentage of those with permits, more than 200 were convicted of felonies, including at least 10 who committed murder or manslaughter. All but two of the killers used a gun.

Among them was Bobby Ray Bordeaux Jr., who had a concealed handgun permit despite a history of alcoholism, major depression and suicide attempts. In 2008, he shot two men with a .22-caliber revolver, killing one of them, during a fight outside a bar.

More than 200 permit holders were also convicted of gun- or weapon-related felonies or misdemeanors, including roughly 60 who committed weapon-related assaults.

In addition, nearly 900 permit holders were convicted of drunken driving, a potentially volatile circumstance given the link between drinking and violence.

The review also raises concerns about how well government officials police the permit process. In about half of the felony convictions, the authorities failed to revoke or suspend the holder’s permit, including for cases of murder, rape and kidnapping. The apparent oversights are especially worrisome in North Carolina, one of about 20 states where anyone with a valid concealed handgun permit can buy firearms without the federally mandated criminal background check. (Under federal law, felons lose the right to own guns.)

Ricky Wills, 59, kept his permit after recently spending several months behind bars for terrorizing his estranged wife and their daughter with a pair of guns and then shooting at their house while they, along with a sheriff’s deputy who had responded to a 911 call, were inside. “That’s crazy, absolutely crazy,” his wife, Debra Wills, said in an interview when told that her husband could most likely still buy a gun at any store in the state.

Mr. Wills's permit was revoked this month, after The Times informed the local sheriff's office.

Growing National Trend

Gun laws vary across the country, but in most states, people do not need a license to keep firearms at home. Although some states allow guns to be carried in public in plain sight, gun rights advocates have mostly focused their efforts on expanding the right to carry concealed handguns.

The national movement toward more expansive concealed handgun laws began in earnest in 1987, when Florida instituted a "shall issue" permit process, in which law enforcement officials are required to grant the permits as long as applicants satisfy certain basic legal requirements.

The authorities in shall-issue states deny permits to certain applicants, like convicted felons and people who have been involuntarily committed to a mental health institution, unless their gun rights have been restored. North Carolina, which enacted its shall-issue law in 1995, also bars applicants who have committed violent misdemeanors and has a variety of other disqualifiers; it also requires enrollment in a gun safety class.

Today, 39 states either have a shall-issue permit process or do not require a permit at all to carry a concealed handgun. Ten others are "may issue," meaning law enforcement agencies have discretion to conduct more in-depth investigations and exercise their judgment. For example, the authorities might turn down someone who has no criminal record but appears to pose a risk or does not make a convincing case about needing to carry a gun. Gun rights advocates argue, however, that such processes are rife with the potential for abuse.

For now, the permits are good only in the holder's home state, as well as others that recognize them. The bill under consideration in Congress would require that permits be recognized everywhere, even in jurisdictions that might bar the holder from owning a gun in the first place.

In recent years, a succession of state legislatures have also struck down restrictions on carrying concealed weapons in all sorts of public places. North Carolina this year began allowing concealed handguns in local parks, and next year the legislature is expected to consider permitting guns in restaurants.

Efforts to evaluate the impact of concealed carry laws on crime rates have produced contradictory results.

Researchers acknowledge that those who fit the demographic profile of a typical permit holder — middle-age white men — are not usually major drivers of violent crime. At the same time, several states have produced statistical reports showing, as in North Carolina, that a small segment does end up on the wrong side of the law. As a result, the question becomes whether allowing more people to carry guns actually deters crime, as gun rights advocates contend, and whether that outweighs the risks posed by the minority who commit crimes.

Gun rights advocates invariably point to the work of John R. Lott, an economist who concluded in the late 1990s that the laws had substantially reduced violent crime. Subsequent studies, however, have found serious flaws in his data and methodology.

A few independent researchers using different data have come to similar conclusions, but many other studies have found no net effect of concealed carry laws or have come to the opposite conclusion. Most notably, Ian Ayres and John J. Donohue, economists and law professors, concluded that the best available data and modeling showed that permissive right-to-carry laws, at a minimum, increased aggravated assaults. Their data also showed that robberies and homicides went up, but the findings were not statistically significant.

In the end, most researchers say the scattershot results are not unexpected, because the laws, in all likelihood, have not significantly increased the number of people carrying concealed weapons among those most likely to commit crimes or to be victimized.

Crimes by Permit Holders

Gun advocates are quick to cite anecdotes of permit holders who stopped crimes with their guns. It is virtually impossible, however, to track these episodes in a systematic way. By contrast, crimes committed by permit holders can be.

The shooting at the Hogs Pen Pub in Macclesfield, N.C., in August 2008 took place after two men, Cliff Jackson and Eddie Bordeaux, got into a scuffle outside the bar. John Warlick, who was there with his wife, helped separate them, only to see Eddie's brother, Bobby Ray, fatally shoot Mr. Jackson in the back of the head. Mr. Bordeaux then shot Mr. Warlick in the upper torso, wounding him.

Bobby Ray Bordeaux had obtained a concealed carry permit in 2004 and used to take a handgun everywhere. He was also an alcoholic and heavy user of marijuana with a long history of depression, according to court records. He had been hospitalized repeatedly for episodes related to his drinking, including about a year before, when he shot himself in the chest with a pistol while drunk in an apparent suicide attempt. Mr. Bordeaux, then 48,

started drinking heavily at age 13. He had been taking medication for depression but had not taken it the day of the shooting, he later told the police. He also said he had 15 beers and smoked marijuana that night and claimed to have no memory of what occurred. He was eventually convicted of first-degree murder and assault with a deadly weapon inflicting serious injury.

John K. Gallaher III, a permit holder since 2006, was also an alcoholic with serious mental health issues, said David Hall, the assistant district attorney who prosecuted him for murder. In May 2008, Mr. Gallaher, then 24, shot and killed a friend, Sean Gallagher, and a woman, Lori Fioravanti, with a .25-caliber Beretta after an argument at his grandfather's home. The police found 22 guns, including an assault rifle, at his home. Mr. Gallaher pleaded guilty to two counts of first-degree murder last year.

Among the other killings: three months after receiving his permit in July 2006, Mark Stephen Thomas killed Christopher Brynarsky with a handgun after an argument at Mr. Brynarsky's custom detail shop. In 2007, Jamez Mellion, a permit holder since 2004, killed Capt. Paul Burton Miner III of the Army by shooting him 10 times with two handguns after finding him with his estranged wife. William Littleton, who obtained a permit in 1998 and was well known to the police because of complaints about him, shot his neighbor to death with a rifle in 2008 over a legal dispute.

More common were less serious gun-related episodes like these: in July 2008, Scotty L. Durham, who got his permit in 2006, confronted his soon-to-be ex-wife and another man in the parking lot of Coffee World in Durham and fired two shots in the air with a .45-caliber Glock. Antoine Cornelius Whitted, a permit holder since 2009, discharged his semiautomatic handgun during a street fight in Durham last year. Jerry Maurice Thomas, a permit holder since 2009 whose drinking problems were well known to the authorities, held a gun to his girlfriend's head at his house in Asheville last year, prompting a standoff with the police.

Falling Through the Cracks

Gun rights advocates in North Carolina, as well as elsewhere, often point to the low numbers of permit revocations as evidence of how few permit holders break the law. Yet permits were often not suspended or revoked in North Carolina when they should have been.

Charles Dowdle of Franklin was convicted of multiple felonies in 2006 for threatening to kill his girlfriend and chasing her to her sister's house, where he fired a shotgun round through a closed door. He then pointed the gun at the sister, who knocked it away, causing it to fire

again. Mr. Dowdle was sentenced to probation, but his concealed handgun permit remained active until it expired in 2009.

Mr. Dowdle, 63, said in a telephone interview that although he gave away his guns after his conviction, no one had ever done anything about his permit. He said he "could probably have purchased" a gun with it but had not done so because federal law forbade it.

Besides felons like Mr. Dowdle, The Times also found scores of people who kept their permits after convictions for violent misdemeanors. They included more than half of the roughly 40 permit holders convicted in the last five years of assault by pointing gun and nearly two-thirds of the more than 70 convicted of a common domestic violence charge, assault on a female.

Precisely how these failures of oversight occurred is not clear. The normal protocol would be for the local sheriff's office to suspend and eventually revoke a permit after a holder is arrested and convicted of a disqualifying crime, the authorities said. The State Bureau of Investigation, which maintains a computerized database of permits, also tries to notify individual sheriffs when it discovers that a holder has been arrested for a serious crime, according to a spokeswoman, but the process is not formalized.

In Ricky Wills's case, he not only threatened his wife and daughter last May with a handgun and a rifle, but he shot at their house while a Union County sheriff's deputy was inside. It led to convictions on two charges: assault with a deadly weapon with intent to kill and assault on a police officer.

Soon after the shooting, Mr. Wills's wife obtained a restraining order, which also should have led to his permit being suspended.

Sgt. Lori Pierce, who handles concealed handgun permits in Union County, said no one ever notified her about Mr. Wills, who was released from prison in November. And as the sole person handling permits in her county, she said, she does not have time to conduct regular criminal checks on permit holders, unless they are up for a five-year renewal.

As it is, she said, she can barely keep up with issuing permits. She has granted about 1,300 this year.

Tom Torok contributed reporting.

This article has been revised to reflect the following correction:

Correction: December 29, 2011

An article on Tuesday about North Carolina's concealed-handgun permit program misstated the name of the town that is home to the Hogs Pen Pub, the site of a fatal shooting in 2008 by the holder of one such permit. It is Macclesfield, not Macclesville.

EXHIBIT F:

License To Carry: Florida's Flawed Concealed Weapon Law, SOUTH FLORIDA SUN-SENTINEL, Jan. 28, 2007

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LICENCE TO **CARRY**
FLORIDA'S FLAWED CONCEALED WEAPON LAW

Garth F. Bailey, of Pembroke Pines, pleaded no contest to manslaughter in 1988 for shooting his girlfriend in the head while she cooked breakfast. Eight years later, the state of Florida gave him a **license** to **carry** a gun.

John P. Paxton Jr., then of Deerfield Beach, pleaded guilty to aggravated child abuse in 1993 for grabbing his 4-year-old nephew by the neck, choking and slapping him for flicking the lights on and off. Eight years later, the state gave him a **license** to **carry** a gun.

John M. Corporal, of Lake Worth, pleaded guilty to aggravated assault in 1998 for pulling a chrome revolver from his waistband and placing it against his roommate's head during an argument. In 2002, he pleaded guilty to grand theft. In February 2006, the state gave him a **license** to **carry** a gun.

Florida has given concealed weapon licenses to hundreds of people who wouldn't have a chance of getting them in most other states because of their criminal histories. Courts have found them responsible for assaults, burglaries, sexual battery, drug possession, child molestation -- even homicide.

In an investigation of the state's concealed weapon system, the South Florida Sun-Sentinel found those licensed to carry guns in the first half of 2006 included:

More than 1,400 people who pleaded guilty or no contest to felonies but qualified because of a loophole in the law.

216 people with outstanding warrants, including a Tampa pizza deliveryman wanted since 2002 for fatally shooting a 15-year-old boy over a stolen order of chicken wings.

128 people with active domestic violence injunctions against them, including a Hallandale man who was ordered

by a judge to stay away from his former son-in-law after pulling a handgun out of his pocket and telling the man: "I'll blow you away, you son of a b----."

Six registered sex offenders.

"I had no idea," said Baker County Sheriff Joey Dobson, who sits on an advisory panel for the state Division of Licensing, which issues concealed weapon permits. "I think the system, somewhere down the line, is broken. I guarantee you the ordinary person doesn't know [that] ... and I'd venture to guess that 160 legislators in Florida don't know that, either."

The National Rifle Association, the prime mover behind the state's nearly 20-year-old concealed weapon law, says it's the court system that is broken, not the gun -licensing system.

The problem rests with gaps within law enforcement and with "bleeding-heart, criminal-coddling judges and prosecutors," said Marion P. Hammer, Tallahassee lobbyist for the NRA and its affiliate, the Unified Sportsmen of Florida.

"What you need to understand is the NRA and the sportsmen's group are law-abiding people," she said. "We don't want bad guys to have guns."

But the Violence Policy Center, a national nonprofit group dedicated to reducing gun violence, studied Florida's concealed weapon program in 1995 and concluded that it "puts guns into the hands of criminals."

"The people who are intimately familiar with these laws, the people at the NRA, they know exactly what's going on," said Kristen Rand, the center's legislative director. Florida's gun lobby and the program's administrators "know they're permitting some bad people, but they don't want the general public to know that."

Last summer, the Legislature made it even harder for the public to find out. It made the names of licensed gun carriers a secret.

Pistol Packin' Paradise

From the country's founding, the protection of house, home, life, land and liberty has come at the point of a muzzle. Few other constitutional rights are more cherished, or more controversial, than the right to bear arms.

Nationwide, Florida has long been considered a gun enthusiast's paradise -- a place where licensed gun dealers outnumber golf courses; where gun ranges are protected from lawsuits for contaminating groundwater with lead bullets; and where Hammer, the NRA lobbyist, holds so much sway in Tallahassee lawmakers are, in the opinion of one adversary: "scared to hell of her."

"If she says they're anti-gun, their political life is over," said Arthur C. Hayhoe, executive director of the Florida Coalition to Stop Gun Violence.

In fact, Florida often is referred to as the Gunshine State.

The label dates back to 1987, when the state passed a law to "ensure that no honest, law-abiding person who qualifies" for a **license** to **carry** a gun would be denied the right. The legislation became the model for so-called "shall-issue" concealed weapon laws nationwide.

The licenses enable people to carry handguns -- hidden usually under clothing or in a purse or briefcase -- in most public places.

"We need to send a strong message to criminals in the state of Florida that the next time you rob someone or rape them or try to kill them, that they very well may be armed, and they very well may be able to protect themselves," former state Rep. Ron Johnson, a Panama City Democrat and sponsor of the bill, argued on the House floor nearly 20 years ago.

At the time, counties had differing rules for who could or could not get a **license** to **carry** a gun.

Under the 1987 law, the rules became uniform statewide. People who wanted to carry a gun didn't need a reason other than basic self-protection.

The numbers skyrocketed. Before the law took effect, 25 people in Broward County had concealed weapon licenses. As of Dec. 31, there were 35,884.

"That's an alarming increase," said Coral Springs Police Chief Duncan Foster. "I don't view that as a positive trend. I view that as a negative. The more guns on the street, the more prone people are to violence."

During the same time, in Palm Beach County, the number of licenses went from 1,400 to 28,478; and in Miami-Dade, from 2,200 to 42,521. The numbers grew statewide from fewer than 25,000 to more than 410,000 today.

When the system changed in 1987, authors of the law promised that the state would give gun licenses only to "law-abiding citizens."

It hasn't worked out that way.

The Sun-Sentinel found that people who violate the law do get -- and at times keep -- **licenses** to **carry** guns. It happens because of loopholes in the law, bureaucratic errors, poor communication with cops and courts, and a

loose suspension process.

As of July 1, the Legislature ended access to records of the licensees without a court order. Lawmakers made the change after an Orlando television station, in 2005, published the identities of Central Florida licensees on the Internet, infuriating some who were named.

The Sun-Sentinel obtained the state's database of licensees twice, once in March and again in late June, in 2006, before the new privacy law took effect.

The records provide a last public look at who is sanctioned to carry a gun in Florida.

And they are not all the "law-abiding" citizens the state promised.

One man, 22 arrests

Robert E. Rodriguez, a 71-year-old retired Tampa bar owner, was arrested 22 times between 1960 and 1998, according to the Florida Department of Law Enforcement.

In four cases, he pleaded guilty or no contest and received probation or fines but judges "withheld" formal convictions: in 1982 for trafficking 10,000 pounds of marijuana; in 1983 for aggravated assault, and in 2000 for aggravated assault and for firing a weapon into an unoccupied building.

As of late June, he had a valid **license** to **carry** a gun.

Numerous other states, including South Dakota, Texas and Maryland, likely would not permit Rodriguez to carry a gun given his rap sheet.

Rodriguez acknowledged that he has a long arrest record but told the Sun-Sentinel: "I've never been convicted of anything."

Convicted felons cannot get gun licenses under state and federal law.

But a loophole in Florida law allows people charged with felonies to obtain **licenses** to **carry** guns three years after they complete their sentences so long as a judge "withholds adjudication."

In a sort of legal no man's land, the defendants plead guilty or no contest. They serve probation, complete community service, obtain counseling, pay fines or fulfill other requirements. When they successfully complete the terms, they have no criminal record.

The break often is given to first- or second-time offenders in instances in which the state's case is weak, and a plea deal appears to be the best option for defendants without the money to mount a strong defense, legal experts said. But it also has aided people accused of violent felonies, sometimes repeatedly.

The Sun-Sentinel found more than 1,400 people, such as Rodriguez, with gun licenses who had felony convictions "withheld" from their records.

"That's incredible," said state Sen. Gwen Margolis, D-Aventura. "I just can't believe it. It's outrageous."

"We should not have loopholes for these things," said state Sen. Nan Rich, D-Weston. "It's too dangerous. I think the Legislature should look at that."

"If we need to plug up those holes, let's have that debate and let's plug up those holes," said state Rep. Ellyn Bogdanoff, R-Fort Lauderdale.

But Hammer, the NRA lobbyist, says the courts, not the gun law, should change.

"What you're talking about is taking away the rights of people who have not been convicted and punished for crimes because the court decided to give them a pass," she said. "How can a state agency take rights away from people when a court refuses to?"

Broward Chief Judge Dale Ross said the NRA's stance is inconsistent.

"The NRA is mad at judges because more people are able to own guns?" he said. "I thought they were advocates of gun usage. They want less people to have guns?"

The judge said he was unaware that Florida's gun law permitted people who have had formal convictions withheld to later obtain **licenses** to **carry** guns.

The Sun-Sentinel found six registered sex offenders with valid concealed weapon licenses at the time of its review. Five of the six had convictions "withheld" by the courts, making them qualified for gun licenses.

State Rules Questioned

Overall, the rules governing who gets a **license** to **carry** a gun in Florida are a mishmash of contradictions and provisos.

Convicted of a misdemeanor crime of violence, such as stalking, assault or battery, against a member of your

family? License denied.

Convicted of a misdemeanor crime of violence against someone outside your family? License approved (so long as three years have passed since the completion of the sentence).

Convicted of driving under the influence? License approved.

Convicted of driving under the influence twice within the past three years? License denied.

Subjected to a current domestic violence restraining order? License denied.

Subjected to a prior domestic violence restraining order? License approved.

Have a long rap sheet but no convictions? License approved.

The state's licensing methods bother some in law enforcement.

"I believed, and still believe, the system in place now is not strong enough to discern the credibility of the person applying for it," said Broward Sheriff Ken Jenne, who voted against the 1987 law when he was in the state Senate. Broward County, he said, would be far stricter in issuing the licenses than the state is.

"Some of these people make your head spin," said Lt. Tundra King, spokeswoman for the Lauderhill Police Department. "You know what you've arrested them for, yet they still produce a legal concealed weapon permit. ... It's kind of scary. ... I don't know if I even understand all of the loopholes."

Finding Loopholes

The loopholes enabled John Corporal to obtain a **license** to **carry** a gun.

The Lake Worth man pleaded guilty and received 18 months probation in 1998 for pulling a gun on his roommate and threatening to kill him, according to court records. The judge withheld a formal conviction.

In 2002, he pleaded guilty to grand theft. He was accused of stealing $96,058 from a medical imaging company he worked for, according to a police report. Again, a judge withheld a formal conviction.

He was sentenced to 10 years probation, which ended early in March 2005.

The state gave him a **license** to **carry** a gun in 2006.

On June 6, two weeks after the Sun-Sentinel asked the state for information about Corporal, the Florida Division of Licensing moved to revoke the license because three years had not passed from the completion of his sentence, as required by law.

The loopholes also helped John Paxton, 36, the Deerfield Beach man who pleaded guilty to aggravated child abuse. He had his conviction withheld by the courts.

A judge sentenced him to one year of community control, two years probation and 120 days in the Broward County Stockade Facility, with work-release privileges. The court also ordered Paxton to complete a family violence prevention program.

Paxton, now living in Lake Placid, Highlands County, told the Sun-Sentinel that he took a parenting class as part of his court sentence but disagreed with much of the teachings.

"If you're punishing a child and you send a child to cut his own switch ... that's not allowed," he said. "That's mental cruelty. ... If he cusses, you can't wash his mouth out with soap. You can't use Tabasco sauce when they talk back to you."

The state gave Paxton a **license** to **carry** a gun in 2001.

The loopholes also benefited Garth Bailey, the Pembroke Pines man who killed his 20-year-old girlfriend in 1985 as she cooked breakfast.

"It was an accident, it was an accident," Bailey insisted, telling police he was showing the 9 mm Smith & Wesson to Marie A. Lue Young when it fired. There were no witnesses.

A search turned up 10 pounds of marijuana in the trunk of a car, court records state.

Prosecutors charged Bailey, now 40, with drug possession and manslaughter, arguing that he showed a "reckless disregard for human life."

He pleaded no contest to the drug charge in 1986 and got 18 months probation.

In 1988, he pleaded no contest to manslaughter in Young's death and was sentenced to four years probation. The court withheld a formal conviction on each charge.

Four years after the shooting, in October 1989, Bailey got into trouble with the law again.

Miramar police stopped him for speeding and charged him with carrying a concealed weapon without a license. Police found a samurai knife under a paper on the passenger's seat and two firearms in the glove box.

Bailey pleaded no contest. The court withheld a formal conviction again.

He applied for and received a **license** to **carry** a gun in 1996.

As of late June, it was listed in state records as valid until June 2009.

Tomorrow: Fugitives, mistakes and an inmate.

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ABOUT THIS SERIES

The South Florida Sun-Sentinel begins a four-part investigation into Florida's gun laws and who's licensed to carry concealed weapons.

Today: Hundreds of Floridians carry guns despite criminal histories.

Monday: Bureaucratic errors and narrow laws let people keep **licenses** to **carry** guns.

Tuesday: Licenses reinstated, time and again.

Wednesday: Who's packing heat is now a state secret.

HOW WE DID IT

The South Florida Sun-Sentinel examined the state's concealed weapons program, comparing the identities of 443,425 license holders with databases of felony convictions, domestic violence injunctions, arrests, warrants, clemency proceedings, jail bookings and sex offender registries. Reporters also analyzed 21,180 disciplinary actions the Florida Division of Licensing has taken against permit holders. The findings represent only a sampling.

The newspaper did not have access to the records of more than 760,000 people licensed to carry guns since 1987 because the state destroys the records of expired or revoked licenses after two years.

Also, the Florida Department of Law Enforcement would not make its statewide database of misdemeanor arrests and convictions available for the newspaper's review without an estimated $23 million fee for one year's worth of records.

WANT A GUN? FLORIDA MAKES IT EASY

GENERAL FLORIDA GUN RULES

No license or permit is needed to purchase a handgun, rifle or shotgun.

Must be 18 to purchase a rifle or shotgun, and 21 to purchase a handgun from a licensed dealer. To buy a handgun from a private person, you can be 18.

Must pass a criminal background check to purchase from a licensed firearms dealer.

Must wait three days between buying a handgun from a licensed dealer and taking it home.

Multiple firearms can be purchased at one time.

Firearms do not need to be registered with police and can be kept in the home without a permit or a concealed weapon license.

A firearm can be transported in a vehicle without a permit or concealed weapon license as long as it is "securely encased," such as in a glove compartment, or "not immediately accessible." It cannot be carried on a person in a vehicle without a concealed weapon license.

CONCEALED WEAPON LICENSES IN FLORIDA

Licenses are required to carry a concealed weapon in public places.

Concealed weapons are defined as: handguns, knives, electronic weapons or devices, billy clubs, tear gas guns.

Licenses are issued by the Florida Division of Licensing, in the Department of Agriculture and Consumer Services. (Go to http://licgweb.doacs.state.fl.us/ weapons/index.html)

Applicants must submit: notarized application, photo, fingerprints, a fee of $117 and verification of firearm training. The state recommends that applicants also include certified copies of court documents to expedite the processing.

Must be at least 21, a U.S. citizen or lawful permanent resident alien, and must pass a criminal background check.

No justification for a license is required; "self-defense" is sufficient.

Licenses are valid for five years.

License holders don't have to wait three days between buying and taking home a handgun.

License is valid in 30 other states.

Concealed weapons generally are prohibited in police stations, jails, courtrooms, polling places, government meeting rooms, schools, bars and airline passenger terminals.

Due to a recent change in law, licensees can now carry concealed weapons in Florida's state parks, including beaches, as well as in Florida's national forests outside of hunting season.

Sources: Florida Division of Licensing, Florida Department of Law Enforcement, The National Rifle Association, The Brady Campaign Against Gun Violence.

BEFORE AND AFTER

Number of concealed weapon licenses, before and after the current law took effect in 1987.

County	1987	2006
Broward	25	35,884
Palm Beach	1,400	28,478
Miami-Dade	2,200	42,521

Florida under 25,000 410,392*

* 85 percent of current licensees are men. Their average age is 53 and 88 percent are white.

Sources: Florida Division of Licensing; Archives, South Florida Sun-Sentinel

BEARING ARMS

Here are some people qualified to carry guns in Florida who would not be eligible to obtain concealed weapon licenses in many other states.

Edward L. Caldwell, 33, Bradenton

Obtained a **license** to **carry** a gun in September 2004. He has been a registered sex offender since 1997 after being sentenced to seven months in jail and five years probation for sexual battery on a woman who gave him a ride home from a Bradenton bar. "He threatened to shoot her if she didn't comply," a police report states. A judge withheld a formal conviction, making Caldwell eligible for the gun license. His legal history included a 2001 acquittal for lewd and lascivious acts on a child under 16; three domestic violence injunctions between 1998 and 2001, all dismissed; and a domestic violence restraining order in place from January 2002 to August 2003. A warrant issued for him in January 2006 for failing to update his address as a sex offender states: "There is information in the file he has firearms [note: his tag number] and has also been known to make comments reference "suicide by cop" ... USE CAUTION." Caldwell's license tag: Glock40. Authorities arrested him on the warrant on Feb. 13, 2006. On May 4, the state moved to suspend his gun license.

Roderick "Shorty" Barber, 33, Lauderhill

Arrested in 1993 for possessing crack cocaine, referred to Drug Court for treatment. Skipped a court date and a warrant was issued. Charged in 1996 with attempted robbery after a fender-bender. According to the police report, Barber demanded the victim pay him $30 for the damage, grabbed his throat and slammed him into a metal box, requiring stitches to the man's head. Barber pleaded no contest to the attempted robbery and the 1993 drug charge. A judge withheld a formal conviction, sentencing him to 30 months probation. He obtained a **license** to **carry** a gun in 2002. In an interview, Barber told the Sun-Sentinel: "Occasionally, I go to the gun range. I figure if I've got a gun in the car, and I got a permit, I don't have to bother with the police harassing me." He said the drug charges stemmed from him "being in the wrong place at the wrong time."

Robert E. Rodriguez, 71, Tampa

A bar owner, he was arrested 22 times between 1960 and 1998, according to the Florida Department of Law Enforcement. Nearly half of the arrests were for carrying concealed weapons without a license and various liquor violations, including allowing nude dancing, according to FDLE. The other half were for graver crimes, includ-

ing homicide, arson and aggravated battery -- all dismissed, the records state. He also was acquitted of assault and battery.

Four arrests resulted in court sentences.

In January 1982, he pleaded no contest to trafficking 10,000 pounds of marijuana. A judge withheld a formal conviction and sentenced him to 12 years probation. In March 1983, St. Petersburg police arrested him for aggravated assault for threatening a man in a bar with a broken cue stick, sticking a gun in his side and escorting him out in a conflict over a toppled motorcycle. He pleaded no contest and a judge withheld a formal conviction, sentencing him to five years probation, according to FDLE and the Pinellas circuit court clerk.

He obtained a concealed weapon license in September 1989.

In January 2000, Rodriguez pleaded no contest to shooting a gun into an unoccupied cafe. A formal conviction was withheld and he was fined $1,250, Hillsborough County court records show.

In November 2000, Rodriguez pleaded guilty to aggravated assault for firing a gun when a man turned into his driveway, police said. The court withheld a formal conviction, and he received two years probation. In 2002, the state revoked his gun license.

He reapplied in 2004 and was denied. He won an appeal and the state issued a new license in February 2005. It is valid until 2010.

Rodriguez referred questions to his daughter, Amy Pickford, who told the Sun-Sentinel that "the bar business is a very rough business" and that at times he had to defend himself against "bikers and gangs."

"It's not like he was a hit man for the Mafia," she said.

Manuel de Jesus Castro, 45, Miami Beach

Obtained a **license** to **carry** a gun in 2000, despite police and court records warning of possible family violence. Twice in 1994 his wife, Belinda Boquin, tried to get a domestic violence restraining order from a Miami judge. The court denied the petitions. Months later, her brother succeeded in obtaining a restraining order against Castro. The injunction expired in 1995.

In 1997, Miami-Dade police responded to Castro's home after Boquin said he hit her in the face and chest, "grabbed a machete" and threatened to kill her and their two children, a police report states. Prosecutors did not file charges.

The couple divorced. In May 2002, Castro was arrested for shooting Boquin's new husband and her 17-year-old son to death. "He got into an argument with my husband," she said. "He started shooting."

Authorities told Boquin her son died from ricocheting bullets. He was about to graduate from high school.

In October 2003, a judge sentenced Castro to 30 years in prison. Five months later, the state revoked his **license** to **carry** a gun.

MOST STATES DRAW TOUGHER LINES ON GUNS

Most states are stricter than Florida in determining who can have a **license** to **carry** a gun.

The Sun-Sentinel, in a survey of 50 states and the District of Columbia found one -- Mississippi -- with a law comparable to Florida's, and two with looser restrictions: Alaska and Vermont.

Arizona denies concealed weapon permits to anyone who had a felony conviction "set aside or vacated."

Texas disqualifies anyone who had an adjudication withheld on a felony. "It's a no go," said Tela Mange, spokeswoman for the Texas Department of Public Safety.

South Dakota denies a gun license to anyone who has pleaded guilty or no contest to a felony.

South Carolina denies applicants with six or more traffic violations in five years. "It shows a person's character, you know," said South Carolina Law Enforcement Division Capt. Clifton Weir, who administers the concealed weapon permit program.

Oklahoma excludes people for "significant character defects." "This is somebody who is not a big-time person, but time after time they get in trouble on misdemeanors," said Trent Baggett, associate executive coordinator of the Oklahoma District Attorneys Council.

Wisconsin prohibits people from carrying concealed weapons altogether. The state's governor, Jim Doyle, twice vetoed bills to legalize the practice.

Marion P. Hammer, National Rifle Association lobbyist in Tallahassee, is comfortable with Florida's law.

"I don't care what other states have. I live in Florida," she said. "I'm raising my family in Florida. Our laws in Florida, that the NRA has had anything to do with, are pretty good laws."

She rejects arguments that other states gauge an applicant's temperament and judgment on more than formal convictions. Maine, for example, excludes people who have shown "reckless or negligent conduct." Maryland denies people who exhibit a "propensity for violence."

The Florida Division of Licensing, Hammer said, cannot be burdened with that level of scrutiny.

"What are you going to do, psychoanalyze everybody to be sure they'll be safe when they buy a set of kitchen knives?" she said. "That they're not going to play coroner and do autopsies on people with kitchen knives? Come on!"

PHOTO: LETTER OF THE LAW: Albert Johnson sorts license requests at the state Division of Licensing in Tallahassee. More than 410,000 Floridians have a permit to carry a concealed weapon. Staff photo/Jim Rassol
Garth F. Bailey. Pleaded no contest to manslaughter in 1988 for shooting his girlfriend in the head. He received a concealed weapon permit eight years later.
John M. Corporal. Pleaded guilty to aggravated assault in 1998. In 2002, he pleaded guilty to grand theft. In February 2006, the state gave him a **license** to **carry** a gun.
John P. Paxton, Jr. Pleaded guilty to aggravated child abuse in 1993 for grabbing, choking and slapping his 4-year-old nephew. In 2001, Florida gave him a concealed weapon permit.
Caldwell
Barber
Rodriguez
Castro CHART: License ratios by county. Source: Florida division of Licensing. Staff graphic
CHART: New Licenses roar. Source: Florida Department of Agriculture and Consumer Services, Division of Licensing. Staff graphic

---- INDEX REFERENCES ---

COMPANY: NATIONWIDE MUTUAL INSURANCE CO

NEWS SUBJECT: (Violent Crime (1VI27); Legal (1LE33); Social Issues (1SO05); Legislation (1LE97); Government (1GO80); Police (1PO98); Crime (1CR87); Non-Profit Organizations (1NO22); Property Crime (1PR85); Judicial (1JU36); Criminal Law (1CR79); Sex Crimes (1SE01); Economics & Trade (1EC26))

INDUSTRY: (Smuggling & Illegal Trade (1SM35))

REGION: (South Dakota (1SO07); South Carolina (1SO63); Maryland (1MA47); Americas (1AM92); Oklahoma (1OK58); North America (1NO39); Texas (1TE14); USA (1US73); Florida (1FL79))

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OTHER INDEXING: (AGRICULTURE; ARMS; BRADY CAMPAIGN AGAINST GUN VIOLENCE; CONSUMER SERVICES; CORAL SPRINGS POLICE CHIEF; DEPARTMENT OF AGRICULTURE; DRUG COURT; FDLE; FLORIDA; FLORIDA DEPARTMENT; GENERAL FLORIDA GUN; GUN; GUNSHINE STATE; HALLANDALE; LAUDERHILL; LAUDERHILL POLICE DEPARTMENT; LEGISLATURE; LICENCE; NATIONAL RIFLE ASSOCIATION; NATIONWIDE; NRA; OKLAHOMA DISTRICT ATTOR-

NEYS COUNCIL; PINELLAS; STATE RULES; STOP GUN VIOLENCE; SUN SENTINEL; TEXAS DEPARTMENT OF PUBLIC; UNIFIED SPORTSMEN; VIOLENCE POLICY CENTER) (Amy Pickford; Arizona; Arthur C. Hayhoe; Bailey; Barber; Belinda Boquin; Boquin; Caldwell; Castro; Clifton Weir; CONCEALED WEAPON LICENSES; Corporal; Dale Ross; Due; Edward L. Caldwell; Ellyn Bogdanoff; Garth Bailey; Garth F. Bailey; Gwen Margolis; Hammer; Jesus Castro; Jim; Jim Doyle; Joey Dobson; John; John Corporal; John M. Corporal; John P. Paxton; John P. Paxton Jr.; John Paxton; Johnson; Ken Jenne; Kristen Rand; Lue Young; Manuel; Marie A.; Marion P. Hammer; Maryland; Megan O'Matz; Miramar; Multiple; Nan Rich; Numerous; Oklahoma; Paxton; Pistol Packin; RassolGarth F. Bailey; Robert E. Rodriguez; Roderick "Shorty" Barber; Rodriguez; Ron Johnson; Skipped; Smith Wesson; Staff; STATES DRAW TOUGHER LINES; Tela Mange; Tomorrow: Fugitives; Trent Baggett; Wisconsin; Young)

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EXHIBIT G:

System Under The Gun: Errors, Laws Keeping Weapons In Questionable Hands, SOUTH FLORIDA SUN-SENTINEL, Jan. 29, 2007

less misdemeanors, and at least one prison inmate, Arthur W. White, of Okaloosa County.

White began serving 35 years in 2002 for sexual battery on a child. He had a **license** to **carry** a gun from the day he entered prison until May 2006, when the license expired, state records show.

Dennis Henigan, legal director of the Washington-based Brady Center to Prevent Gun Violence, the nation's leading gun control organization, calls the Sun-Sentinel's findings "really shocking," given that the state has been overseeing people with concealed weapon licenses since 1987.

"That's almost 20 years. And yet this kind of sloppiness and negligence still pervades the licensing of concealed weapon holders in Florida," said Henigan.

The people who oversee the weapons program at the Florida Division of Licensing say they are following the rules as set by the Legislature.

"I don't know of a systemic problem," said Licensing Director W.H. "Buddy" Bevis. "I know of problems here and there."

Defining Violence

The state is supposed to suspend a concealed weapon license if the holder is arrested or formally charged with a felony. Or a drug crime. Or if a judge imposes a restraining order against the licensee for domestic violence.

If the charge is dismissed, the person cleared, or the restraining order dropped, the license is restored.

If the person is found guilty of a crime of violence, the state will revoke the license.

Under the rules, people charged with or convicted of "non-violent" misdemeanors keep their licenses.

A misdemeanor can include something as minor as smoking in an elevator or loitering, or as grave as stalking, simple assault or battery.

What is a "violent" misdemeanor?

"Cases are reviewed by the division on a case-by-case basis," said Mary Kennedy, a Division of Licensing bureau chief.

The Sun-Sentinel found that the state does not commonly suspend or revoke licenses for firearm-related misde-

meanors that show recklessness but are not considered violent, such as bringing a loaded gun through airport security or firing a gun into the air to celebrate the Fourth of July.

"People make mistakes. Very stupid mistakes, quite regularly, where they're not intending to be violent," Bevis said.

In 2002, Leroy Cerny, the Miramar man, kept his license after firing from his kitchen into a backyard ficus. He pleaded no contest to culpable negligence, a misdemeanor the state considers to be non-violent, and was given one year of probation.

In an interview with the Sun-Sentinel, Cerny, 47, said he repairs guns and had fired one into the tree to test it. A neighbor complained to police. "Before I fired it, I made sure nobody was in the backyard, mine and his," Cerny said. "But he said I was trying to kill his kids."

At times, the system frustrates prosecutors, police and others who urge the state to suspend a license immediately but find their pleas rejected.

"I've tried to get them to revoke people's permits, and they'll tell me they'll only start proceedings once they get convictions," said Melissa Steinberg, an assistant state attorney in Broward County. "I send them the convictions, and I hope they're doing what has been asked of them."

Steinberg was referring to multiple instances in which people with concealed weapon licenses are caught with guns in their bags at security checkpoints at the Fort Lauderdale-Hollywood International Airport or the Broward County Courthouse.

When they have concealed weapon licenses, they are charged with misdemeanors, not felonies.

And unless they fire the gun, point it at someone or threaten to use it, the crime typically is not considered violent, said Bevis.

So their licenses remain valid, despite the efforts of public officials such as Steinberg.

Recently, Steinberg took the unusual step of asking a judge to order the state to revoke a man's gun license.

Joseph Damgajian, 35, of Coral Springs, was charged in August with bringing a Walther P99 handgun into the Broward courthouse in downtown Fort Lauderdale.

Damgajian, who had reported for jury duty, had the gun in a backpack as he went through security. He "kept repeating that he forgot, and it was an honest mistake," a Broward Sheriff's Office report states.

Along with the gun, security officers found that he had three loaded magazines, a holster, a "Koran, [the] sacred book of Muslims," and "approximately 20 DVDs of extremely violent tactical games, some in English and some in Arabic," the report states.

The sheriff's report lists his occupation as a salesman for Life Extension, an anti-aging foundation in Fort Lauderdale. He was not charged with any other offenses. He has no prior record in Florida, according to the Florida Department of Law Enforcement.

As a result of the courthouse incident, Damgajian pleaded no contest in November to a weapon permit violation, a "non-violent" misdemeanor that, under state law, would not require revocation of his gun license.

Broward County Judge Gary Cowart "withheld" a formal conviction, sentencing Damgajian to six months probation, six months of wearing an electronic tracking device and ordered him to forfeit the gun -- and surrender his gun license to the state. Damgajian declined to comment.

Fugitives Licensed

Bevis said the law is clear on when the agency can and can't suspend or revoke a license. If the agency veers from those rules, the licensee can appeal the decision through the courts.

"We will lose," Bevis said. "We're wasting money then."

Case in point: fugitives. Under federal law, they can't own firearms.

The Sun-Sentinel matched the state's database of concealed weapon licensees with databases of open warrants kept by FDLE and the sheriffs in Broward, Miami-Dade and Palm Beach counties. The newspaper found 216 licensees with active warrants at the time of the paper's review.

The warrants ranged from minor offenses, such as shoplifting and trespassing, to more serious charges, including assault, battery, arson, drug possession and firing a gun in public.

The Division of Licensing could get warrant information from FDLE and the state's 67 sheriffs but doesn't.

"We wouldn't be aware when a warrant is issued," Bevis said. "The law does not allow us to suspend or revoke on a warrant."

Had the department obtained warrant information it would have seen that John A. Brandley, 34, of Margate, had been arrested for aggravated battery, a felony.

Brandley was licensed to carry a gun in April 2001. Four months later, he was arrested for beating a man so badly at a Pompano nightclub the victim needed reconstructive surgery on his face. In April 2002, a Broward judge issued a warrant for Brandley's arrest after he skipped a court date.

Over the next four years the gun permit was neither suspended nor revoked, Licensing Division records show. It expired in April 2006.

"I think it's seriously messed up that he would still be allowed to carry that," said the victim, Kenneth Collins, now 30. He said he was dating Brandley's ex-girlfriend when Brandley, who is still being sought, allegedly knocked him on his head.

"He's about 6 foot, 3 inches, 285 pounds. Big body builder type," Collins recalled. "He continued to smash my face while I was on the floor. The entire left side of my face was shattered. ..."

Marion P. Hammer, Tallahassee lobbyist for the National Rifle Association, supports the law as written. She argues that the state should not suspend or revoke gun licenses simply because someone is wanted on an arrest warrant.

"The issuance of a warrant doesn't mean that anybody is guilty," she said. "It means that someone is a suspect."

Gaps in the System

Florida's Division of Licensing has about 140 employees. In the past year the division has issued, on average, more than 1,000 new concealed weapon licenses a week, not including renewals. The agency also licenses security guards, private investigators and repo agents.

The division's greatest challenge, Bevis said, is its workload.

Even with more employees, however, the system would not be perfect, he said. "If you gave us twice as much money and two times as many people, we're still going to make mistakes."

The division's four lawyers rely on law enforcement to share criminal record data on the more than 410,000 people licensed to carry guns.

Each week, FDLE provides the division with lists of arrestees from around the state. Each night, FDLE provides a list of people subjected to domestic violence injunctions. And once a month, the Department of Corrections provides the division with names of inmates.

Computers match the names to the roster of people licensed to carry guns. The results are given to the licensing division's lawyers to review.

The division has suspended 699 licenses and revoked 113 since July 1, according to the latest state figures.

"I promise you, our attorneys are absolutely, tongue-hanging-out tired every day," Bevis said.

The data the lawyers receive is not always complete, leading to instances in which licenses are not suspended.

Gaps in the process can occur anywhere in the criminal record-gathering system: at the police station, sheriff's office, courthouse or elsewhere.

"No system is perfect," says Joe Waldron, executive director of the Citizens Committee for the Right to Keep and Bear Arms, a gun rights group headquartered in Washington state.

The Brady Center's Henigan says that's not good enough.

"Where the stakes are so high, where a mistake in granting a license means a dangerous person carrying a hidden handgun on the street ..., the public cannot afford mistakes."

Tomorrow: The Suspension Turnstile

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John Maines can be reached at jmaines@sun-sentinel.com or 954-356-4737

DESPITE LEGAL PROBLEMS, THEY KEPT GUN LICENSES

Here are some people who retained their gun licenses because of bureaucratic errors or narrowly drawn laws.

Adel R. Ahmad 26, Tampa

In June 2002, Tampa police issued a warrant for Ahmad, a pizza deliveryman wanted for manslaughter. He is still missing, but his **license** to **carry** a gun was valid for four years -- until May 24, 2006, when it expired, state Division of Licensing records show. According to police reports, Ahmad got lost making a delivery and stopped to ask for directions. Tyrone Stephens, 15, approached Ahmad's car, reached in and took an order of chicken wings. Ahmad told police the teen began to run away but six to 10 other men approached the vehicle, frightening him. He fired two rounds from a 9 mm Glock, killing Stephens. Police made a copy of Ahmad's concealed

weapon permit and let him go. A week later, prosecutors decided to charge him with aggravated manslaughter. By then he was gone.

Nathaniel A. Ferguson, 47, Lake Mary, Seminole County

He still had a **license** to **carry** a gun, according to state records, as of late June -- a year and a half after his arrest for shooting a woman in a parking lot outside a Seminole County bar. In January 2005, Ferguson got into an argument with a group of patrons in the bar's parking lot, grabbed a revolver from his SUV and fired. Melanie Abbott, 30, of Longwood, suffered a flesh wound to the hip. "It was terrifying. I could have died. I have a 10-year-old son," she said. Ferguson claimed self-defense. "They were trying to attack my wife and I," he told the Longwood Police Department. In January 2006, he pleaded no contest to attempted manslaughter. A judge "withheld adjudication" and sentenced him to 180 days in jail, two years of house arrest and three years probation. Under Florida law, Ferguson's gun license should have been suspended after his arrest and revoked after his sentencing. Ferguson's wife, Patricia, told the Sun-Sentinel that her husband returned the license after his sentencing in February 2006. State records, however, show the license was still valid in June.

Barry Cogen 24, Sunrise

State officials blamed human error on their failure to take action against Cogen.

He obtained a **license** to **carry** a gun June 7, 2005. The following day, he was arrested for aggravated stalking, a felony. His concealed weapon license should have been suspended. A year later, however, it was still valid, according to Division of Licensing records. The criminal case against him is pending.

According to the Broward Sheriff's Office, between August 2002 and July 2004, Cogen and another man harassed an Oakland Park-area family, vandalizing their lawn and two cars with firebombs, eggs, burning papers, concrete blocks, an acid bomb, Molotov cocktail and fireworks. Cogen told authorities he was "getting even" with the victim's son, a former middle school classmate. In June, the state said it would begin actions to suspend Cogen's license. Asked about his gun license, Cogen told the Sun-Sentinel: "I have no idea what you're talking about. I got no comment."

Lyglenson Lemorin 32, Miami

Now an accused terrorist, Lemorin got a **license** to **carry** a gun in May 1996. He did not lose the license after two arrests in 1997 and 1998. In each case, the charges were dropped.

In one, Lemorin allegedly threw a beer bottle at an ex-girlfriend, striking her in the neck as she walked along with two children. And in the second, he punched a pregnant former girlfriend, flashed a handgun and warned: "... I'll kill you," police reports state. The state suspended his gun license in February 2000, a year after he was arrested for carrying a concealed firearm with a domestic violence injunction against him. The state lifted the

suspension in March 2000, then suspended it again in June 2006 after he was arrested with six other South Florida men on terrorism conspiracy charges. Federal authorities claim the group swore allegiance to al-Qaida and plotted to blow up buildings in Miami and Chicago, including the Sears Tower.

Michael Zappia 50, Boca Raton

Retained his **license** to **carry** a gun after being charged in late 2005 with disorderly conduct and resisting arrest, both misdemeanors, while at the Pompano Beach Booby Trap strip club. A dancer had complained that Zappia, a retired Boca Raton police sergeant, was drunk and claiming he had a gun. A police affidavit says Zappia refused to leave the club and struggled with officers. He pleaded no contest in March. A judge withheld a formal conviction and ordered him to pay court costs and fines of $426. His gun license was still valid as of late June, according to the latest publicly available records. In a telephone interview, Zappia denied the incident happened.

LICENSES TO **CARRY** A CONCEALED WEAPON IN FLORIDA

Licenses are required to carry concealed weapons in public places.

Concealed weapons are defined as: handguns, knives, electronic weapons or devices, billy clubs, tear gas guns.

Licenses are issued by the Florida Division of Licensing, within the Department of Agriculture and Consumer Services.

For rules, go to http://licgweb.doacs.state.fl.us/weapons/index.html

ABOUT THIS SERIES

The South Florida Sun-Sentinel continues a four-part investigation into Florida's gun laws and who's licensed to carry concealed weapons.

Sunday: Hundreds of Floridians carry guns despite criminal histories.

Today: Bureaucratic errors and narrow laws let people keep **licenses** to **carry** guns.

Tuesday: Licenses reinstated, time and again.

Wednesday: Who's packing heat is now a state secret.

Online: Sun-Sentinel.com/ guns

PHOTO: VIOLENCE ERUPTS: Kenneth Collins, holding his daughter Emily, 3, accused John Brandley, left, of beating him in 2001. Despite Brandley's arrest, he kept his concealed weapon permit until it expired in 2006. "I think it's seriously messed up that he would still be allowed to carry that [gun]," Collins said. Staff photo/Michael Laughlin
KEEPING HIS GUN: Melanie Abbott was shot outside a Longwood bar in 2005. Eighteen months later, her attacker, Nathaniel Ferguson, still had a gun license. Staff photo/Michael Laughlin
Ahmad
Ferguson
Cogen
Lemorin
Zappia

---- INDEX REFERENCES ---

COMPANY: STATE FORTIFICATION STS JAKSN MS L L C; SUNRISE

NEWS SUBJECT: (Violent Crime (1VI27); Insurance Fraud (1IN81); Crime (1CR87); Legal (1LE33); Social Issues (1SO05); Criminal Law (1CR79); Technology Law (1TE30); Government (1GO80); Economics & Trade (1EC26))

REGION: (USA (1US73); Americas (1AM92); Florida (1FL79); North America (1NO39))

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OTHER INDEXING: (BARRY; BRADY CENTER; CITIZENS COMMITTEE; CONSUMER SERVICES; DEFINING VIOLENCE; DEPARTMENT OF AGRICULTURE; DEPARTMENT OF CORRECTIONS; FDLE; FLORIDA; FLORIDA DEPARTMENT; LIFE EXTENSION; LONGWOOD; LONGWOOD POLICE DEPARTMENT; LYGLENSON; NATIONAL RIFLE ASSOCIATION; OAKLAND PARK; POMPANO; POMPANO BEACH BOOBY TRAP; PREVENT GUN VIOLENCE; STATE; SUN SENTINEL; SUNRISE; SUV) (Abbott; Adel R. Ahmad; Ahmad; Arthur W. White; Bear Arms; Bevis; Big; Boca Raton; Brandley; Cerny; Cogen; Collins; Computers; Damgajian; Dennis Henigan; Eighteen; Ferguson; Gaps; Gary Cowart; Henigan; Joe Waldron; John; John A. Brandley; John Brandley; Joseph Damgajian; Kenneth Collins; Lacho; Leroy Cerny; Leroy W. Cerny; Licensing; Licensing Director; Lubomir Lacho; Marion P. Hammer; Mary; Mary Kennedy; Megan O'Matz; Melanie; Melanie Abbott; Melissa Steinberg; Michael LaughlinAhmadFergusonCogenLemorinZappia; Michael LaughlinKEEPING; Michael Zappia; Nathaniel A. Ferguson; Nathaniel Ferguson; Patricia; Staff; Steinberg; Stephens; Sunday; Tyrone Stephens; White; Zappia)

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