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No. 12-1437

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IN THE UNITED STATES COURT OF APPEALS
FOR THE FOURTH CIRCUIT

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RAYMOND WOOLLARD, *et al.*,

Plaintiffs-Appellees,

v.

DENIS GALLAGHER, *et al.*,

Defendants-Appellants.

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On Appeal from the United States District Court
for the District of Maryland
(Benson E. Legg, District Judge)

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SUPPLEMENTAL APPENDIX IN SUPPORT OF MOTION FOR STAY

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July 27, 2012

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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.	*	

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

**MOTION FOR CLARIFICATION OR AMENDMENT OF
ORDER AND FOR IMMEDIATE STAY PENDING APPEAL**

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March 7, 2012   Attorneys for Defendants

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INTRODUCTION

The defendants, through counsel, move: (1) under Rule 59(e), for an order clarifying or amending its March 2, 2012 order (ECF No. 53) (the "Order")[1] granting the plaintiffs' motion for summary judgment (ECF No. 21) and denying the defendants' cross-motion for summary judgment (ECF No. 25); and (2) under the Court's inherent power and Rule 62(c), for an order granting an immediate stay of the Order, as well as any subsequent, related order, pending appeal.[2]

First, under Rule 59(e), the defendants seek clarification or amendment of the Order to identify the extent, if any, to which the Court intended to grant the plaintiffs' request for injunctive relief. The plaintiffs' amended complaint (ECF No. 19) requests several forms of injunctive relief. Although the plaintiffs' memorandum in support of their motion for summary judgment (ECF No. 22) does not discuss their request for injunctive relief other than to request that the defendants "be enjoined" from violating the plaintiffs' constitutional rights, the proposed order accompanying the plaintiffs' motion

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[1] Although dated March 2, 2012, the order was not entered into ECF, or known to the defendants, until March 5, 2012.

[2] In the late morning of March 7, 2012, counsel for the defendants conferred with counsel for the plaintiffs with respect to the relief sought in this motion, as well as with respect to the relief requested in the accompanying consent motion for an expedited briefing schedule. Counsel for the plaintiffs represented in a subsequent voicemail that: (1) the plaintiffs consent to the deadline for their response to this motion being Friday, March 16, 2012, but only if this motion were filed today, March 7, 2012; (2) the plaintiffs do not consent to the defendants' request for a stay; and (3) the plaintiffs could not consent to a request for clarification at this time, but were open to further discussion. The defendants intend to pursue that additional discussion, but did not believe it feasible to do so before filing this motion in light of the time-sensitive nature of the issues raised, as well as the plaintiffs' condition that the motion be filed today for purposes of their agreement to the expedited briefing schedule.

for summary judgment (ECF No. 21-1) contains the same injunctive relief provisions requested in the amended complaint. However, neither the Order (ECF No. 53) nor the accompanying memorandum opinion (ECF No. 52) discuss the plaintiffs' request for injunctive relief. Instead, the Order grants the plaintiffs' motion for summary judgment, and denies that of the defendants, without specifying the relief awarded. As a result, the defendants seek clarification regarding the extent, if any, to which the Court intended to grant the plaintiffs' request for an injunction.

Second, whether or not the Court intended to award injunctive relief, the defendants request an immediate stay of the Order—including any subsequent clarification or amendment thereof—pending appeal to the United States Court of Appeals for the Fourth Circuit. A stay pending appeal would permit the Fourth Circuit to resolve the issues presented in this case, which are issues of first impression in this judicial circuit, before enforcing the extraordinary step of invalidating a portion of a long-standing statute and requiring the issuance by the Maryland State Police of permits to wear and carry handguns in public under circumstances that the Maryland General Assembly has determined will imperil public safety. The seriousness of the dispute over the admittedly difficult legal questions presented by this litigation is demonstrated by the fact that the only other courts to have ruled on constitutional challenges to similar handgun permit statutes have ruled in favor of the defendants' position. Because this case presents a new issue of constitutional law, in a largely "unmapped" legal context, (ECF No. 52, at 9), clarification from the appellate courts would be prudent in light of the potentially significant adverse consequences for public safety if a stay is not entered. As

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the Fourth Circuit has previously recognized, the issues at stake in this type of litigation are "serious business," with significant potential consequences for public safety, and courts should be careful to make certain they have interpreted the Second Amendment correctly before rejecting laws designed to protect the public against gun violence: "We do not wish to be even minutely responsible for some unspeakably tragic act of mayhem because in the peace of our judicial chambers we miscalculated as to Second Amendment rights." *United States v. Masciandaro*, 638 F.3d. 458, 475 (4th Cir. 2011).

A stay would also permit the Maryland General Assembly to consider whether to enact legislation in response to this Court's decision that might address, in whole or in part, the grounds on which this Court has held the law unconstitutional while providing more protection for public safety than would a wholesale abandonment of the "good and substantial reason" requirement for permits to wear and carry handguns.

I.   **The Court Should Clarify or Amend Its Order to Identify the Extent, if Any, to Which the Court Intended to Grant the Plaintiffs' Request for Injunctive Relief.**

The defendants seek clarification or amendment of the Order to identify the extent, if any, to which the Court intended to grant the plaintiffs' request for injunctive relief. Rule 59(e) permits the filing of a "motion to alter or amend a judgment . . . no later than 28 days after the entry of the judgment." A court may amend a judgment in three circumstances: "(1) to accommodate an intervening change in controlling law; (2) to account for new evidence not available at trial; or (3) to correct a clear error of law or prevent manifest injustice." *See Brown v. Hovatter*, 525 F. Supp. 2d 754, 757 (D. Md.

2007) (quoting *Pacific Ins. Co. v. Am. Nat'l Fire Ins. Co.*, 148 F. 3d 396, 403 (4th Cir. 1998)). In addition to this power of amendment, a district court may "clarify a previous judgment under the auspice of Rule 59(e) without necessarily amending it, so long as it is filed within" the time provided by the rule. *Brown*, 525 F. Supp. 2d at 757.

In their amended complaint, the plaintiffs challenge the constitutionality of § 5-306(a)(ii) of the Public Safety Article of the Maryland Code.   That provision conditions issuance of a handgun wear and carry permit on a determination that the applicant "has good and substantial reason to wear, carry, or transport a handgun, such as a finding that the permit is necessary as a reasonable precaution against apprehended danger." Md. Code Ann., Pub. Safety ("PS") § 5-306(a)(ii). The plaintiffs' prayer for relief requests the following:

> 1.   An order permanently enjoining defendants, their officers, agents, servants, employees, and all persons in active concert or participation with them who receive actual notice of the injunction, from enforcing Maryland Public Safety Code § 5-306(a)(5)(ii);

> 2.   An order permanently enjoining defendants, their officers, agents, servants, employees, and all persons in active concert or participation with them who receive actual notice of the injunction, from denying a permit to carry firearms on grounds that the applicant does not face a level of danger higher than that which an average person would reasonably expect to encounter.

> 3.   An order commanding Defendants to renew Plaintiff Woollard's permit to carry a handgun;

> 4.   Costs of suit, including attorney fees and costs pursuant to 42 U.S.C. § 1988;

> 5.   Declaratory relief consistent with the injunction; and

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6. Any other further relief as the Court deems just and appropriate.

Amended Complaint (ECF No. 19) at 7-8.

With respect to the declaratory relief requested in paragraph 5, the Memorandum Opinion holds that PS § 5-306(a)(ii) is unconstitutional. (ECF No. 52, at 20.)

Neither the Court's opinion (ECF No. 52) nor its Order (ECF No. 53) expressly addresses the plaintiffs' requests for injunctive relief. Neither document uses the terms "enjoin" or "injunction," nor does the Order satisfy the requirements of Rule 65(d), which requires that every order granting an injunction be "specific in terms" and "describe in reasonable detail, and not by reference to the complaint or other document, the act or acts sought to be restrained." Fed. R. Civ. P. 65(d). That requirement is "'designed to prevent precisely the sort of confusion'" that can arise from an unclear order. *Gunn v. Univ. Comm. to End the War in Vietnam*, 399 U.S. 383, 389 (1970) (quoting *Int'l Longshoremen's Ass'n v. Philadelphia Marine Trade Ass'n*, 389 U.S. 64, 74 (1967)). Given the "extraordinary" nature of an injunctive order, "'Congress . . . require[d] that a federal court frame its orders so that those who must obey them will know what the court intends to require and what it means to forbid.'" *Gunn*, 399 U.S. at 389 (quoting *Int'l Longshoremen's Ass'n*, 389 U.S. at 76). The Supreme Court has observed that this requirement is "absolutely vital" in cases concerning the validity of "a law duly enacted by a sovereign State." *Gunn*, 399 U.S. at 389.

The Order does not specifically state whether the Court intended to grant or deny some or all of the plaintiffs' requested injunctive relief or, if the Court intended to grant

that relief, on what terms. There are important reasons why the Court may have decided to enter only a declaratory judgment and deny one or more of the plaintiffs' requests for injunctive relief. The plaintiffs' summary judgment briefs fail to discuss, much less demonstrate that the plaintiffs meet, the criteria necessary to obtain injunctive relief. That is significant because "[a]n injunction is a matter of equitable discretion; it does not follow from success on the merits as a matter of course." *Winter v. Natural Resources Defense Council, Inc.*, 555 U.S. 7, 32 (2008). As a result, beyond prevailing on the merits, the plaintiffs were required to demonstrate that they met all of the criteria required for issuance of an injunction, including demonstrating irreparable injury, inadequacy of legal remedies, that an equitable remedy is warranted in light of balance of hardships, and that the public interest would not be disserved by an injunction. *See, e.g.*, *eBay Inc. v. MercExchange, L.L.C.*, 547 U.S. 388, 391 (2006). The plaintiffs did not do so.

Moreover, the only one of the plaintiffs' three requests for injunctive relief that accords with this Court's opinion is the first request, which would enjoin enforcement of PS § 5-306(a)(ii), the provision the Court has held unconstitutional. The second requested injunction appears to be an attempt to restate the first request, but in language that is less precise, more subject to ambiguity, and therefore less appropriate for an injunction. And the third requested injunction seeks an order requiring the defendants to "renew" a permit that expired in 2009 without any review to determine whether Mr. Woollard continues to meet the other requirements for a handgun wear and carry permit, requirements that this Court did not find to be unconstitutional. In *Heller*, by contrast, the Supreme Court required the District of Columbia to grant Mr. Heller a permit to have

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a gun in his home only if he was "not disqualified from the exercise of Second Amendment rights," presumably after undergoing a regular application process. *District of Columbia v. Heller*, 554 U.S. 570, 635 (2008).

To clarify the specific relief the Court intended to award, as well as to facilitate appellate review, the defendants request that the Court clarify or amend the Order. To the extent the Court intended to deny the plaintiffs' requests for injunctive relief, the defendants request that the Court clarify the Order by so stating. If the Court intended to grant injunctive relief, the defendants request that the Court amend the Order to meet the requirements of Rule 65(d). As discussed below, the defendants also request that any form of relief ordered by the Court, whether declaratory or injunctive, be stayed pending appeal.

II.   The Court Should Enter a Stay Pending Appeal.

PS § 5-306(a)(ii) was enacted as part of a comprehensive effort by the State legislature to deter crime and protect citizens from handgun violence, violence which tragically results in hundreds of shooting deaths and hundreds more shooting injuries every year in Maryland. Given, as this Court found, the State's unquestioned compelling interest in promoting public safety and preventing crime, and given the increased public safety risks that may result from an inability to enforce PS § 5-306(a)(ii), a stay allowing continued operation of the statute pending appeal of the Order represents a sensible exercise of judicial discretion.

As the Fourth Circuit recently observed in a firearm regulation case: "This is serious business. We do not wish to be even minutely responsible for some unspeakably tragic act of mayhem because in the peace of our judicial chambers we miscalculated as to Second Amendment rights." *United States v. Masciandaro*, 638 F.3d 458, 475 (4th Cir. 2011). The danger of firearm violence, as the Fourth Circuit recognized, may "rise exponentially" from an expansion of the right to bear arms "from the home to the public square." *Id.* at 476.   Thus, courts must take great care in their adjudication of that right to balance the effort to define its proper contours against the State's need to maintain adequate protections against violence from its misuse. *See Piszczatoski v. Filko*, 2012 U.S. Dist. LEXIS 4293, No. 10-cv-06110 (WHW) *3 (D.N.J. Jan. 12, 2012) (courts should be "careful—most careful—to ascertain the reach of the Second Amendment right" because that right, unlike any other constitutional right, "permits the user of a firearm to cause serious personal injury—including the ultimate injury, death—to other individuals, rightly or wrongly"). In the present case, a stay preserving the long-standing status quo would be the appropriate means of ensuring that sufficient attention is paid to both the serious constitutional issues and the compelling public interests at stake.

Federal Rule of Civil Procedure 62(c) provides that "[w]hile an appeal is pending . . . the court may suspend, modify, restore, or grant an injunction on terms . . . that secure the opposing party's rights." The purpose of this rule is to enable the Court to maintain the status quo pending final resolution of the case when the case presents "an admittedly difficult legal question and when the equities of the case suggest that the status quo should be maintained." *Goldstein v. Miller*, 488 F. Supp. 156, 172-73 (D. Md.

1980) (quoting *Washington Metropolitan Area Transit Comm'n v. Holiday Tours, Inc.*, 559 F.2d 841, 843-45 (D.C. Cir. 1977)); *see Smith v. Old Angus, Inc.*, 1973 U.S. Dist. LEXIS 15221, at *4-*5 (D. Md. 1973) ("[T]he purpose of Rule 62(c) is to preserve the status quo where, in its sound discretion, the Court deems the circumstances so justify.") (citing 7 Moore, Federal Practice, § 62.05, at 62-19 (2d ed. 1972)).

Rule 62(c) applies, by its terms, "[w]hile an appeal is pending." In this case, the defendants have not yet filed a notice of appeal only because they do not want to hinder the Court's ability to address the defendant's request under Rule 59(e) to clarify or amend the Order. The defendants do not believe this precludes entry of a stay because the Court inherently has the power to stay or delay the effect of its Order at any point before the filing of a notice of appeal. *See, e.g.*, *Shinholt v. Angle*, 90 F.2d 297, 298 (5th Cir. 1937) (noting "inherent power" of courts in equity to preserve the status quo pending an appeal from an order disallowing injunctive relief); *Paramount Plastering, Inc. v. Local No. 2 of the Operative Plasterers & Cement Masons Int'l Ass'n*, 195 F. Supp. 287, 298 (C.D. Cal. 1961) (exercising "inherent power" to stay execution and enforcement of judgment until the judgment "shall have become final" in light of the "novelty of the problem" and harm to the defendants). Rule 62(c)'s grant of power during the pendency of an appeal does not limit the Court's inherent power to suspend, modify, restore or grant an order prior to a party noting an appeal.

Moreover, even if the Court intended only to enter a declaratory judgment in this case, a stay pending appeal is appropriate. *See Florida v. United States Dep't of Health & Hum. Servs.*, 780 F. Supp. 2d 1307, 1320 (N.D. Fla. 2011) (granting motion to clarify

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and stay declaratory judgment that ruled a statute unconstitutional without specifying any injunctive relief). Although the issuance of a declaratory judgment in the absence of an injunction does not legally compel the State to act in compliance with the judgment while the judgment is being appealed, *see Kennedy v. Mendoza-Martinez*, 372 U.S. 144, 155 (1963) ("the Government has been free to continue to apply [a] statute" following entry of a declaratory judgment declaring the statute unconstitutional); *accord, e.g.*, *Werner v. Hickey*, 920 F. Supp. 1257, 1259 (M.D. Fla. 1996) ("[A]bsent an injunctive sanction, a district court's declaration that a statute is unconstitutional does not bar the government's application of the statute pending appeal."), the State, as a matter of policy, generally seeks to comply with the terms of a declaratory judgment even while the judgment is being appealed. As a result, a declaratory judgment that is not stayed pending appeal can have the same practical effect as an injunction, and can be stayed on the same basis.

The specific factors governing a request for a stay pending appeal are "(1) whether the stay applicant has made a strong showing that he is likely to succeed on the merits; (2) whether the applicant will be irreparably injured absent a stay; (3) whether issuance of the stay will substantially injure the other parties interested in the proceeding; and (4) where the public interest lies." *Hilton v. Braunskill*, 481 U.S. 770, 776 (1987); *see also Long v. Robinson*, 432 F.2d 977, 979 (4th Cir. 1970). Each of these factors is discussed below.

A.   The Public Interests in Protecting Public Safety and Preventing Crime Favor Issuance of a Stay.

As this Court recognized, the public interests in protecting public safety and preventing crime are "substantial, indeed compelling, government interests." (ECF No. 52, at 18.) The Supreme Court has held that "the Government's regulatory interest in community safety can, in appropriate circumstances, outweigh an individual's liberty interest." *United States v. Salerno*, 481 U.S. 739, 748-50 (1987); *see also Schall v. Martin*, 467 U.S. 253, 264 (1984) ("The legitimate and compelling state interest in protecting the community from crime cannot be doubted." (citations and quotation marks omitted)); *Monroe v. City of Charlottesville*, 579 F.3d 380, 390 (4th Cir. 2009).

It was to advance these critical public interests, particularly as they pertain to handgun violence, that the Maryland legislature enacted the Permit Statute. Although this Court has concluded that PS § 5-603(a)(ii) is overly broad in its attempt to protect public safety (ECF No. 52, at 18), it is undisputed that the Maryland General Assembly enacted the Permit Statute to respond to devastating handgun violence that was tearing through Maryland communities. *See* Defendants' Memorandum in Support of Cross-Motion for Summary Judgment (ECF No. 26) ("Defendants' MSJ Memo."), at 13-14. The legislature made sure to include specific legislative findings about the public interest in handgun regulation in the text of the statute itself; one of these findings states: "additional regulations on the wearing, carrying, and transporting of handguns are necessary to preserve the peace and tranquility of the State and to protect the rights and liberties of the public." Md. Code Ann., Crim. Law § 4-202(5).

Again, although the Court concluded that PS § 5-603(a)(ii) is "insufficiently tailored to the State's interest in public safety and crime prevention" (ECF No. 52, at 23), the Court expressly acknowledged both the compelling nature of that governmental interest, *id.* at 18, as well as the "unquestionable threat to public safety" posed by scenarios the defendants identified as ones § 5-603(a)(ii) serves to mitigate, *id.* at 19. In this rapidly-evolving area of the law, and especially in light of the acknowledged lack of guidance provided by the Supreme Court in interpreting and applying its recently-announced understanding of the Second Amendment, the Fourth Circuit has strongly cautioned against court action that might adversely affect public safety without first ensuring that the court's interpretation of the scope of the Second Amendment right is not "miscalculated." *Masciandaro*, 638 F.3d at 475. This Court's invalidation of § 5-603(a)(ii) clearly falls within the category of laws covered by the Fourth Circuit's cautionary warning.

In addition to providing the opportunity for the appellate courts to address issues of first impression in this judicial circuit before implementing an order that could adversely affect public safety, a stay pending appeal could also permit the Maryland General Assembly to consider and, should it choose to do so, to enact changes to existing law designed to address public safety concerns in a manner that is consistent with this Court's holding. For example, the Court expressly declined to address whether a "good and substantial reason" requirement could be applied only to concealed carry of a handgun, *id.* at 20-21; the plaintiffs have expressly conceded that a complete ban on concealed carry of a handgun would be constitutional, *see* Plaintiffs' Memorandum of

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Points and Authorities in Opposition to Defendants' Motion for Summary Judgment (ECF No. 34) at 14-15; and the Supreme Court has recognized the permissibility of bans on the concealed carry of handguns, *Robertson v. Baldwin*, 165 U.S. 275, 281-82 (1897) ("the right of the people to keep and bear arms (article 2) is not infringed by laws prohibiting the carrying of concealed arms"); *see also Heller*, 554 U.S. at 626 (recognizing that courts have historically permitted bans on the concealed carry of handguns).   Entering a stay pending appeal might provide the General Assembly the opportunity to consider enacting a statute addressing this issue without creating a void in the statutory scheme that affects both open and concealed carry of handguns, thus partially alleviating the circumscription of "the scope of popular governance" against which the Fourth Circuit has cautioned. *Masciandaro*, 638 F.3d at 475; *see also Texaco, Inc. v. Hughes*, 572 F. Supp. 1, 9 (D. Md. 1982) (when ruling on the application of an injunction, finding that the "public interest in enforcing statutes passed by [the state] legislature must be taken into account").

B.   The State's Likelihood of Success on Appeal Supports a Stay.

The "likelihood-of-success standard does not mean that the trial court needs to change its mind or develop serious doubts concerning the correctness of its decision in order to grant a stay pending appeal." *Miller*, 488 F. Supp. at 172. Nor does the trial court need to determine "that ultimate success by the movant is a mathematical probability." *Id.* (quoting *Washington Metropolitan Area Transit Comm'n*, 559 F.2d at 843-45). Given the stage of litigation, it would be "illogical" to condition availability of

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a stay on a court concluding that its original decision was incorrect. *Miller*, 488 F. Supp.

at 173 (quoting *Evans v. Buchanan*, 435 F. Supp. 832, 843-44 (D. Del. 1977)). Rather,

this Court need only weigh the likelihood of success in a case presenting "an admittedly

difficult legal question" against the other factors (the public interest and the balance of

equities) and make a holistic determination as to whether a stay is appropriate. *Miller*,

499 F. Supp. at 172-73.

This case undoubtedly involves a difficult legal question, namely the nature and

extent of Second Amendment rights outside the home and the interaction of any such

rights with governmental interests in public safety.   As this Court acknowledged,

answering this question involves navigating a relatively "unmapped" area of the law.

(ECF No. 52, at 9.) Other courts to have faced the question have remarked that it brings

courts into a "new frontier" of Second Amendment jurisprudence, *Piszczatoski*, 2012

U.S. Dist. LEXIS 4293, at *12, one that is fraught with "uncertainty," *id.*, in part because

of "a lack of a clear directive from the Supreme Court," *Kachalsky v. Cacace*, 2011 U.S.

Dist. LEXIS 99837, No. 10-CV-5413 (CS), at *64 (S.D.N.Y. Sept. 2, 2011).

Against this background, it is significant that every court to have addressed the

constitutionality of similar handgun wear and carry permit statutes before this one has

upheld the constitutionality of the statutes.   That, at a minimum, raises a substantial

prospect of success for the defendants on the merits on appeal. In a case addressing a

permit statute that is one of the most similar to Maryland's, the United States District

Court for the Southern District of New York upheld the constitutionality of New York's

requirement that a permit applicant demonstrate that "proper cause exists for the

issuance" of the permit. *See Kachalsky*, 2011 U.S. Dist. LEXIS 99837, at *2. After first

undertaking a detailed analysis of the Second Amendment right in the wake of *Heller* and

McDonald, the court found that it was not at all clear that the Second Amendment would

even apply to New York's permit statute. *See id.* at *56-*82. The district court went on

to hold that, even if the New York statute implicated the Second Amendment, the statute

would survive intermediate scrutiny because the statute's restriction—which has been

interpreted similarly to Maryland's "good and substantial reason" requirement—was

substantially related to New York's compelling interest in protecting public safety. *See*

id. at *99 (observing that New York's "proper cause" requirement was not an outright

ban, but a requirement that individuals demonstrate an "actual and articulable" need for

self-defense).

Similarly, in *Piszczatoski*, the district court rejected a constitutional challenge to a

provision of New Jersey law that requires a handgun wear and carry permit applicant to

demonstrate a "justifiable need to carry a handgun" to a reviewing police officer and a

Superior Court judge. 2012 U.S. Dist. LEXIS 4293, at *2. In rejecting this challenge,

the court found that "[t]he Second Amendment does not protect an absolute right to carry

a handgun for self-defense outside the home," *id.* at *15, and ruled that the provision at

issue is the sort of "longstanding" licensing provision that the Supreme Court found to be

presumptively lawful in *Heller*, *id.* at *44. The court further ruled that the "justifiable

need" requirement did not unconstitutionally burden a Second Amendment right

"because this requirement is sufficiently tailored to address an important state interest."

Id. at *49.

Other courts have recently rejected constitutional challenges to other permit statutes based on similar analyses. For example, in *Kuck v. Danaher*, 2011 U.S. Dist. LEXIS 111793 (D. Conn. Sept. 29, 2011), the district court upheld a provision of Connecticut law requiring a handgun wear and carry permit applicant to demonstrate to a reviewing authority that he intends to use the handgun for a lawful purpose and that he is a "suitable person" to receive such a permit, without defining the term "suitable." *Id.* at *7. The district court declined to recognize a right to bear arms that extends outside the home, *id.* at *23, and found the provision to be constitutional as an exercise of the state's "strong and compelling interest in ensuring that firearm permits are not issued to those lacking the essential character or temperament necessary to be entrusted with a weapon," *id.* at *30 (citation and quotations marks omitted); *see also id.* at *36 ("[T]he Court holds that the statute at issue is substantially related to Connecticut's compelling interest in protecting the public from persons who could potentially pose a dangerous [sic] if entrusted with a firearm.").

Similarly, in *Hightower v. City of Boston*, the district court rejected a constitutional challenge to a provision of Massachusetts law that requires a firearm wear and carry permit applicant to demonstrate to a licensing authority that he is "a suitable person to be issued such license." 2011 U.S. Dist. LEXIS 111327, No. 08-cv-11955 (DJC), at *7 (D. Mass. Sept. 29, 2011). In addition to finding the challenge not to be ripe, *id.* at *43, the court also declined to recognize a right to bear arms that extends beyond the home, *id.* at *62, and ruled that the City of Boston "has a legitimate interest in protecting public safety, especially in light of the prevalence of gun violence in

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Massachusetts and especially in Boston," which is furthered by a regulation that "require[es] local licensing authorities . . . to determine whether an individual applicant appears unsuitable based on the content of her application materials, subject to judicial review." *Id.* at *68.

In contrast to this uniform line of decisions from sister jurisdictions, no court before this one has held a similar permit regime to be unconstitutional. As a result, these decisions, in addition to those discussed in the defendants' summary judgment briefs (ECF No. 26, at 22-42), satisfy the standard for demonstrating a "strong likelihood of success on the merits" in the context of a petition for a stay pending appeal. *See Miller*, 488 F. Supp. 172-73; *cf. United States v. Fourteen Various Firearms*, 897 F. Supp. 271, 273 (E.D. Va. 1995) ("[T]he court must determine whether there is a strong likelihood that the issues presented on appeal could be rationally resolved in favor of the party seeking the stay.").

C.   The Factors of Irreparable Harm to the Defendants and the Balance of Equities Favor Issuance of a Stay Pending Appeal.

Absent a stay, the defendants will be irreparably injured in multiple ways. First and foremost, as described above, their ability to protect public safety will be curtailed. This is not merely a formalistic injury; the State believes that the inability to enforce an important component of the handgun permit regulations will expose citizens to an increased risk of handgun violence, a risk that can be addressed in the near term only by a stay.

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Second, the absence of a stay will produce both significant, immediate administrative burdens and the possibility of widespread confusion depending on the ultimate resolution in the Fourth Circuit. The current process for reviewing applications for handgun wear and carry permit applications has been developed over the 40 years that the "good and substantial reason" requirement has been part of Maryland law. Even more significant than the administrative burden of revising the process and re-training staff is the significant confusion that would likely accompany the need to rescind permits granted during the pendency of the appeal to the Fourth Circuit if PS § 5-603(a)(ii) is ultimately upheld.

In contrast to the potential implications for public safety and the other harms to the defendants, the plaintiffs would continue during the pendency of the appeal to enjoy the core Second Amendment rights that this Court correctly noted are not implicated by PS § 5-603(a)(ii). (ECF No. 52, at 22.) Moreover, although this Court has now, for the first time, identified a non-core Second Amendment right extending beyond the home,[3] its decision on that point is contrary to the conclusions of the majority of courts to have addressed the issue even since the Supreme Court's decision in *Heller*. *See, e.g.*, Defendants' MSJ Memo. (ECF No. 26), at 24-27. It would, therefore, be prudent, and would impose only a relatively slight burden on the plaintiffs in contrast with the greater

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[3] Importantly, under Maryland law, no permit is required to wear and carry a handgun in numerous locations outside of one's home, including on any real estate owned by that person, on the premises of their business, on the premises of a business in which they are a supervisory employee, in a repair shop, on the site of an organized military activity, at the location of a target shoot or target practice, at a sport shooting event, hunt, or trap, or at a gun collector's exhibition; and an unloaded handgun can legally be transported between any of these locations. *See* Md. Code Ann., Crim. Law § 4-203(b).

burden on the defendants, to preserve the status quo while awaiting the views of the

Fourth Circuit.

<div align="center">

CONCLUSION

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The defendants request that the Court enter an order clarifying or amending its

March 5, 2012 order with respect to the plaintiffs' request for injunctive relief, and enter

a separate order staying the effect of the Court's March 2, 2012 Order, along with any

amendment, pending appeal to the United States Court of Appeals for the Fourth Circuit.

Respectfully submitted,

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March 7, 2012   Attorneys for Defendants

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IN THE UNITED STATES DISTRICT COURT
FOR THE DISTRICT OF MARYLAND

RAYMOND WOOLLARD, et al.,	)	Case No. 1:10-cv-02068-BEL
	)	
Plaintiffs,	)	MEMORANDUM OF POINTS AND
	)	AUTHORITIES IN OPPOSITION TO
v.	)	DEFENDANTS' MOTION TO
	)	CLARIFY AND FOR A STAY OF
MARCUS BROWN, et al.,	)	JUDGMENT
	)	
Defendants.	)	

_____

COME NOW the Plaintiffs, Raymond Woollard and Second Amendment Foundation, Inc.,

by and through undersigned counsel, and submit their Memorandum of Points and Authorities in

Opposition to Defendants' Motion to Clarify and for a Stay of Judgment.

Dated: March 16, 2012   Respectfully submitted,

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MEMORANDUM OF POINTS AND AUTHORITIES IN OPPOSITION TO DEFENDANTS'
MOTION TO CLARIFY AND FOR A STAY OF THE JUDGMENT

PRELIMINARY STATEMENT

Defendants' motion for clarification is itself unclear. Plaintiffs sought very specific relief, and the Court granted their motion without any apparent limitation, upon entry of an opinion that lucidly explains why Plaintiffs are entitled to relief.

But the motion to clarify the judgment does, at least, reveal several points. First, the Defendants do not believe themselves to be enjoined, and they are still enforcing the provision held unconstitutional by this Court.

Second, even though the Court has held that the challenged provision violates a fundamental right, and notwithstanding the fact that no factual disputes exist among the parties, Defendants assert that Plaintiffs have not established their entitlement to an injunction. This belief is clearly erroneous. Plaintiffs are entitled to an injunction as a matter of law. Constitutional violations are generally presumed to cause irreparable harm, and enforcing constitutional requirements is invariably in the public interest. Were the Court powerless to enjoin enforcement of an unconstitutional law, constitutional rights, and judicial review, would not amount to much.

In any event, Defendants would argue on appeal that the Court did not meet Rule 65's requirements in issuing injunctive relief, and perhaps, Rule 52's requirement of complete factual and legal findings. Rather than argue with the Defendants about the clarity of the Court's actions, in the context of enforcement proceedings and on appeal, some good could probably come of adding language to the Court's March 2 order [Dkt. 53] granting summary judgment. The Court's order should eliminate an unnecessary set of appellate issues by recounting the elements of injunctive relief, and dispelling any doubts as to the injunction's reach.

While the motion to clarify the injunction might yield some benefit, Defendants' request for a stay lacks merit. None of the requisite elements for a stay can be established.

First, merely to recount why Plaintiffs are entitled to injunctive relief—they are suffering irreparable harm owing to the violation of a fundamental constitutional right—suffices to eliminate most grounds for a stay. Nor are Defendants harmed by the injunction in any way, much less in a manner requiring a stay. The Court's decision leaves Maryland with a legal regime concerning the carrying of handguns that is more or less on par with that which prevails in *forty-three* other states, including every state in this circuit and each of Maryland's neighboring states. That is hardly a radical threat to public safety. And contrary to the Defendants' assertions, nothing in the Court's opinion, or in the terms of the injunction, prevents Maryland from continuing to regulate the carrying of handguns. If the state is contemplating additional or different regulations, it does not suddenly require this Court's permission to enact these in the first instance. The Plaintiffs never asked, and the Court never ordered, that the state be generally prevented from legislating on the subject of firearms. There is nothing to stay in this regard.

Finally, the decision is unlikely to be overturned. The Court merely interpreted the relevant constitutional text in the same way that it has already been defined by the Supreme Court, and in a manner consistent with the overwhelming weight of the right's historical understanding. If the Second Amendment secures a right to carry a gun outside the home, the decision to strike down a requirement that individuals prove their entitlement to this right flows inexorably.

STATEMENT OF FACTS

On February 23, 2011, Defendants filed a consent motion to schedule resolution of this case on cross-motions for summary judgment. Dkt. 23. Responding to Plaintiffs' motion for summary judgment, the Defendants failed to seek discovery under Rule 56(f). Defendants also offered no

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evidence challenging any of Plaintiffs' established facts regarding Woollard's permit history, or the

Second Amendment Foundation. Plaintiffs' summary judgment facts were thus uncontested. As the

Court found, "[t]he facts of the case are undisputed." Memorandum Op., Dkt. 52, at 1. Among the

uncontroverted, established facts, Woollard proved that his permit was denied *only* because he failed

to offer a "good and substantial reason" for its renewal. *See*, Pl. Exh. A, C, D.

The First Amended Complaint contained the following prayer for relief:

1.   An order permanently enjoining defendants, their officers, agents, servants, employees, and all persons in active concert or participation with them who receive actual notice of the injunction, from enforcing Maryland Public Safety Code § 5-306(a)(5)(ii);

2.   An order permanently enjoining defendants, their officers, agents, servants, employees, and all persons in active concert or participation with them who receive actual notice of the injunction, from denying a permit to carry firearms on grounds that the applicant does not face a level of danger higher than that which an average person would reasonably expect to encounter.

3.   An order commanding Defendants to renew Plaintiff Woollard's permit to carry a handgun;

4.   Costs of suit, including attorney fees and costs pursuant to 42 U.S.C. § 1988;

5.   Declaratory relief consistent with the injunction; and

6.   Any other further relief as the Court deems just and appropriate.

Dkt. 19.

In moving for summary judgment, Plaintiffs argued that Defendants "must be enjoined"

from enforcing the "good and substantial reason" prerequisite for a handgun carry license. Pl. Br.,

2/18/11, at 2, 21; Pl. Reply Br., 4/25/11, at 33. Plaintiffs proposed a summary judgment order, Dkt.

21-1, which offered, in pertinent part:

As there is no genuine, material dispute of fact, and as Plaintiffs are entitled to judgment as a matter of law, the motion is GRANTED.

> Defendants, their officers, agents, servants, employees, and all persons in active concert or participation with them who receive actual notice of the injunction, are permanently enjoined from enforcing Maryland Public Safety Code § 5-306(a)(5)(ii);
>
> Defendants, their officers, agents, servants, employees, and all persons in active concert or participation with them who receive actual notice of the injunction, are permanently enjoined from denying permits to carry firearms on grounds that an applicant does not face a level of danger higher than that which an average person would reasonably expect to encounter; and
>
> Defendants are directed to forthwith renew Plaintiff Raymond Woollard's permit to carry a handgun.

Dkt. 21-1.

Although Plaintiffs unambiguously sought a permanent injunction, Defendants never contested that Plaintiffs would be entitled to injunctive relief were the "good and substantial reason" requirement held unconstitutional. Defendants never explained how they could rebut Plaintiffs' presumptive entitlement to an injunction against a constitutional violation, nor addressed any argument to the elements of injunctive relief. Defendants never addressed the legal presumptions that constitutional violations cause irreparable harm, and that requiring government officials to obey the Constitution is in the public interest per se.

> Defendants now represent that
>
> the State, as a matter of policy, generally seeks to comply with the terms of a declaratory judgment even while the judgment is being appealed. As a result, a declaratory judgment that is not stayed pending appeal can have the same practical effect as an injunction, and can be stayed on the same basis.

Def. Br., 3/7/12, at 10. But regardless of what the State "generally" does, or whether a declaratory judgment "*can* have the same practical effect as an injunction" (emphasis added), at least for now, it is apparent that Defendants are still enforcing the "good and substantial reason" requirement pending the outcome of their motion for a stay. Woollard has still not received his renewal handgun carry permit. And on information and belief, based upon numerous reports from individuals who

have applied for handgun carry licenses in the wake of the Court's decision, lack of a "good and

substantial reason" is holding up the process. But Plaintiffs are legally entitled to injunctive relief.

<div align="center">ARGUMENT</div>

I.   PLAINTIFFS ARE ENTITLED TO PERMANENT INJUNCTIVE RELIEF AS A MATTER OF LAW.

Requisite elements for injunctive relief are sometimes presumed at law. *See, e.g. Elrod* v.

Burns, 427 U.S. 347, 373 (1976) (irreparable harm presumed when First Amendment violated).

Accordingly, when a party facing an unambiguous request for injunctive relief believes that its

opponent has failed to establish some necessary element of the claim, it should ordinarily be

expected to make that argument *before* the judgment is rendered. Defendants vigorously contested

the merits of Plaintiffs' claims, but never questioned Plaintiffs' entitlement to injunctive

relief—because there was never any doubt that Plaintiffs would be entitled to an injunction as a

matter of law if the challenged provision were held unconstitutional.

Defendants' equitable arguments would have failed even were they timely raised.

Defendants correctly point out that in some cases, injunctions "do[] not follow from success

on the merits as a matter of course." Def. Br., 3/7/12, at 6 (quoting *Winter* v. *Natural Resources

Defense Council, Inc.*, 555 U.S. 7, 32 (2008)).[1] But that argument is often unavailable where

defendants have violated constitutional rights.

> According to well-established principles of equity, a plaintiff seeking a permanent injunction
> must satisfy a four-factor test before a court may grant such relief. A plaintiff must
> demonstrate: (1) that it has suffered an irreparable injury; (2) that remedies available at law,
> such as monetary damages, are inadequate to compensate for that injury; (3) that,

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[1] *Winter* may not provide the best example of this principle. In *Winter*, the Supreme Court
reversed an injunction against the Navy's anti-submarine sonar training, issued for fear of
theoretically harming marine mammals. "Given that the ultimate legal claim is that the Navy must
prepare an EIS, not that it must cease sonar training, there is no basis for enjoining such training in a
manner credibly alleged to pose a serious threat to national security." *Winter*, 555 U.S. at 32-33.

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considering the balance of hardships between the plaintiff and defendant, a remedy in equity is warranted; and (4) that the public interest would not be disserved by a permanent injunction.

eBay Inc. v. *MercExchange, L.L.C.*, 547 U.S. 388, 391 (2006).

These factors are not always examined in constitutional cases where, as a matter of law, they are often presumed proven. *See*, *e.g. Parker* v. *District of Columbia*, 478 F.3d 370 (D.C. Cir. 2007), *aff'd sub nom District of Columbia* v. *Heller*, 554 U.S. 570 (2008).[2] This Court correctly did not perceive these factors to be in serious independent dispute. Memorandum Op., Dkt. 52, at 6 (setting out the questions before the Court).

Having prevailed on the merits of their claim, Plaintiffs have nothing left to prove with respect to their entitlement to injunctive relief.

A.   Plaintiffs Have Established An Irreparable Injury Lacking Remedies At Law.

"[T]he denial of a constitutional right, if denial is established, constitutes irreparable harm for purposes of equitable jurisdiction." *Ross* v. *Meese*, 818 F.2d 1132, 1135 (4th Cir. 1987) (citation omitted); *cf. Henry* v. *Greenville Airport Com.*, 284 F.2d 631, 633 (4th Cir. 1960) (per curiam) ("The District Court has no discretion to deny relief by preliminary injunction to a person who clearly establishes by undisputed evidence that he is being denied a constitutional right"); *see also Elrod*, supra (First Amendment violations constitute irreparable harm); *AFT–W. Va.* v. *Kanawha County Bd. of Educ.*, 592 F. Supp. 2d 883, 892 (S.D. W. Va. 2009) (same: Fourth Amendment).

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[2]In *Heller*, neither the D.C. Circuit nor the Supreme Court seriously questioned whether the prevailing plaintiff was entitled to injunctive relief, which was specifically requested by Heller in moving for summary judgment. *See* Pl. Summary Judgment Br., U.S. Dist. Ct. D.C. No. 03-213, Dkt. 4-1, pp. 2, 41; Proposed Order, *id.* Dkt. 4-2. The *Heller* defendants never questioned that injunctive relief would follow if they lost the case on the merits.

No constitutional right is so directly linked to one's immediate physical well-being as is the right to keep and bear arms. The interest in self-defense is the "*central component* of the [Second Amendment] right itself," *Heller*, 554 U.S. at 599 (emphasis original). Plainly, the inability to access constitutionally-protected arms impacts one's sense of security—to say nothing of the irreparable harm resulting from a successful criminal attack that might have been averted wit hacces to defensive arms.

Defendants do not merely view the Second Amendment as net socially harmful. Their motion completely fails to acknowledge *any* positive individual value secured by the Amendment. The law is otherwise. "[F]or some kinds of constitutional violations, irreparable harm is presumed. *See* 11A Charles Alan Wright et al., FEDERAL PRACTICE AND PROCEDURE § 2948.1 (2d ed. 1995) ('When an alleged deprivation of a constitutional right is involved, most courts hold that no further showing of irreparable injury is necessary.')." *Ezell* v. *City of Chicago*, 651 F.3d 684, 699 (7th Cir. 2011). As the Seventh Circuit explained,

> The loss of a First Amendment right is frequently presumed to cause irreparable harm based on "the intangible nature of the benefits flowing from the exercise of those rights; and the fear that, if those rights are not jealously safeguarded, persons will be deterred, even if imperceptibly, from exercising those rights in the future." The Second Amendment protects similarly intangible and unquantifiable interests. *Heller* held that the Amendment's central component is the right to possess firearms for protection. Infringements of this right cannot be compensated by damages.

Id. (citations and footnote omitted).

Considering precedents such as *Ross* and *Henry*, the Fourth Circuit is unlikely to break with *Ezell* on these points. The loss of Second Amendment rights is not a "slight burden." Def. Br., 3/7/12, p. 18. The violation of Second Amendment rights causes individuals severe and irreparable harm that is not compensable with money damages. There is simply no way to quantify or remedy the profound loss of security, and its possible consequences, when individuals are denied adequate

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means of self-defense. Plaintiffs agree that miscalculating as to Second Amendment rights can lead

to tragedy. Avoiding situations where law-abiding individuals are left defenseless against violent

crime is a core policy of the right to bear arms codified by the Second Amendment.

B.   The Balance of Hardships Tilts Decidedly in Plaintiffs' Favor.

In contrast to the obvious hardship inherent in the loss of a fundamental right necessary for

self-defense, Defendants will suffer no discernible harm in conforming their conduct to constitutional

standards. Defendants suggest that this Court's decision would leave them with a dangerously

inadequate handgun carry permitting scheme, a circumstance so dire and unusual that it warrants,

essentially, the suspension of a fundamental constitutional right. Not so. The Court's decision places

Maryland firmly in the national mainstream with respect to the right to carry handguns, and still

leaves the state with the Fourth Circuit's most restrictive regulatory landscape on the topic.

Apart from Maryland, the laws of forty-three states recognize that private citizens are

generally entitled to carry handguns for self-defense. Of these: Thirty-seven states require officials to

issue gun carry licenses to applicants meeting objective standards, barring proof of some positive

disability.[3] In some of these states, a license is required only if a gun is carried concealed. Four

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[3]Ark. Code Ann. § 5-73-309; Colo. Rev. Stat. Ann. § 18-12-203(1); Conn. Gen. Stat. § 29-32b(b); Fla. Stat. Ann. § 790.06(2); Ga. Code Ann. § 16-11-129(d)(4); Idaho Code Ann. § 18-3302(1); Ind. Code Ann. § 35-47-2-3(e); Iowa Code Ann. § 724.7(1); Kan. Stat. Ann. § 75-7c03(a); Ky. Rev. Stat. Ann. § 237.110(4); La. Rev. Stat. Ann. § 40:1379.3(A)(1); 25 Me. Rev. Stat. Ann. § 2003(1); Mich. Comp. Laws Ann. § 28.422(3); Minn. Stat. § 624.714, subdiv. 2(b); Miss. Code Ann. § 45-9-101(2); Mo. Ann. Stat. § 571.101(1); Mont. Code Ann. § 45-8-321(1); Neb. Rev. Stat. § 69-2430(3); Nev. Rev. Stat. Ann. § 202.3657(2); N.H. Rev. Stat. Ann. § 159.6(I); N.M. Stat. Ann. § 29-19-4(A); N.C. Gen. Stat. § 14-415.11(b); N.D. Cent. Code § 62.1-04-03(1); Ohio Rev. Code Ann. § 2923.125(D)(1); 21 Okla. Stat. Ann. § 1290.12(A)(12); Or. Rev. Stat. Ann. § 166.291(1); 18 Pa. Cons. Stat. Ann. § 6109(e); R.I. Gen. Laws § 11-47-11(a); S.C. Code Ann. § 23-31-215(A); S.D. Codified Laws § 23-7-7; Tenn. Code Ann. § 39-17-1351(b); Tex. Gov't Code § 411.177(a); Utah Code Ann. § 53-5-704(1)(a); Va. Code Ann. § 18.2-308(D); Wash. Rev. Code Ann. § 9.41.070(1); W. Va. Code Ann. § 61-7-4(f); Wis. Stat. § 175.60(2)(a).

states—Alaska, Arizona, Vermont and Wyoming—do not require permits to carry visible *or* concealed handguns, of at least some people (Wyoming requires permits of visitors), although Arizona, Alaska and Wyoming issue permits for reciprocity purposes.[4] Two states—Alabama and Delaware—have discretionary statutes for licensing the carry of concealed handguns, but do not, without more, ban private citizens from openly carrying handguns.[5]

Among Maryland's neighbors, only the District of Columbia bars the carrying of handguns for self-defense.[6] Pennsylvania, Virginia, West Virginia, and Delaware all generally allow the unlicensed open carrying of handguns, and of these, all but Delaware issue concealed carry licenses on a shall-issue basis. Within this circuit, Maryland now joins South Carolina as the only states that uniformly require a license to carry handguns, but issue licenses on a shall-issue basis.[7] The other Fourth Circuit states allow unlicensed open carry in most places and have shall-issue regimes for licensed concealed carry.

In short, Marylanders now enjoy a handgun carry licensing scheme that is, in its core structure, profoundly ordinary. It was the unconstitutional statute struck down by the Court, not the Court's decision, that effected a dangerous and unusual legal regime out of step with prevailing American practice. This is hardly the stuff of emergency stays or uncharted threats to public safety.

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[4]Alaska Stat. § 18.65.700(a); Ariz. Rev. Stat. § 13-3112(A); Wyo. Stat. Ann. § 6-8-104(b).

[5]Ala. Code §§ 13A-11-73, 75; *Morris* v. *State*, 342 So. 2d 417, 418 (Ala. Cr. App. 1977); 11 Del. Code Ann. §§ 1441-42; *In re Application of McIntyre*, 552 A.2d 500, 501 n.1 (Del. Super. 1988) .

[6]The District's unconstitutional handgun carry policies are being challenged. *Palmer* v. *District of Columbia*, U.S. Dist. Ct., D.C. No. 09-CV-1482-FJS.

[7]South Carolina forbids the open carrying of handguns.

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C.   The Public Interest Requires Injunctive Relief.

"Surely, upholding constitutional rights serves the public interest." *Newsom v. Albemarle County Sch. Bd.*, 354 F.3d 249, 261 (4th Cir. 2003) (citation omitted).

* * *

Plaintiffs are entitled to the injunctive relief they sought.

II.   ALTHOUGH THE COURT'S INTENTION MAY BE EASILY DISCERNABLE, THE COURT SHOULD NONETHELESS NARROW THE GROUNDS OF DISPUTE BY CLARIFYING ITS ORDER.

Rule 52 requires that the Court make findings of law and fact to support its judgment. Although an injunction order may recite factual and legal findings, the rule does not require that those findings of fact or conclusions of law be set forth in the injunction order itself. *CIENA Corp.* v. *Jarrard*, 203 F.3d 312, 321 (4th Cir. 2000).

The Court's Memorandum Opinion satisfies the requirements of Rule 52, except to the extent Defendants might claim that the Court made no specific findings regarding the requisite elements for injunctive relief discussed *supra*. Although, as Plaintiffs argued, there is frequently no need to recite the existence of these elements, and the opinion is not deficient for not doing so, Defendants have signaled this as a potential ground for appeal—which could be eliminated were the Court to briefly spell out its findings with respect to irreparable harm, lack of remedies at law, the balance of hardships, and the public interest.

Injunction orders must be set forth in a separate judgment. Fed. R. Civ. Proc. 58. *See, e.g. Reich* v. *ABC/York-Estes Corp.*, 64 F.3d 316, 319 (7th Cir. 1995) (no injunction where separate order not issued). This, the Court has done with Docket Entry 53. The Fourth Circuit would likely not "elevate flawless draftsmanship of court orders over the substantive concerns underlying Rule 65(d). Such perfection is not required . . . when the twin dangers of uncertainty and uninformed

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review are absent." *United States* v. *Fuller*, 919 F.2d 139, 1990 U.S. App. LEXIS 20896 at *10

(4th Cir. 1990) (unpublished); *SEC* v. *SBM Inv. Certificates, Inc.*, No. DKC-06-0866, 2012 U.S.

Dist. LEXIS 28175 (D. Md. March 2, 2012). All the same, Defendants signal that they perceive

appealable deficiencies in the form of that judgment. Again, whatever the merits of this claim, no

harm can come of definitively neutralizing this as an issue for appeal.

Fed. R. Civ. Proc. 65(d) (1) provides that "[e]very order granting an injunction . . . must:

> (A) state the reasons why it issued;
>
> (B) state its terms specifically; and
>
> (C) describe in reasonable detail--and not by referring to the complaint or other
> document--the act or acts restrained or required."

"The terms of Rule 65(d) 'are mandatory and must be observed in every instance.'" *CPC*

Int'l, Inc. v. *Skippy, Inc.*, 214 F.3d 456, 459 (4th Cir. 2000) (quoting *Thomas* v. *Brock*, 810 F.2d

448, 450 (4th Cir. 1987)). "Rule 65(d) . . . serves the twin purposes of providing fair notice of what

an injunction requires and of facilitating appellate review." *CPC*, 214 F.3d at 459 (citation omitted).

"[W]hile an injunction regarding the Act need not be laboriously detailed, it must say

something about the conduct enjoined." *Reich*, 64 F.3d at 320 (citations omitted). "Since one of the

purposes of these requirements is to aid the appellate court on review, it cannot be waived by the

parties unless the grounds for the court's rulings are clear from the record." *Federal Deposit Ins.*

Corp. v. *Jones*, 846 F.2d 221, 240 (4th Cir. 1988).

Again, the Court granted Plaintiffs' summary judgment motion without any apparent

limitation. And the relief Plaintiffs requested is fully supported by the record and relevant legal

presumptions. The Court's judgment, Dkt. 53, should thus be modified to specifically recount the

reasons for granting Plaintiffs injunctive relief, and spell out the terms of the injunction as offered by

Plaintiffs. The Fourth Circuit provided an example specifically upheld against a Rule 65(d) challenge in *CIENA*, 203 F.3d at 322.

The terms offered by Plaintiffs are fairly straightforward. First, the Defendants shall refrain from enforcing the unconstitutional law at issue in this case. Second, the Defendants shall not resurrect the forbidden practice encompassed in that law, under the guise of some other authority. And finally, Woollard should have his permit renewed, because it was only rejected for an unconstitutional reason, and Defendants never offered any other reason for denying the permit renewal. It is far too late in the day for Defendants to suggest that Woollard could be disqualified for some other reason independent of the issue in the litigation. Def. Br., 3/7/12, p. 6. Presumably, were Woollard disqualified on other grounds, Defendants would have raised that defense earlier. Of course, Defendants do not articulate exactly *how* Woollard might no longer be eligible for the permit they had already once renewed.[8]

III.   DEFENDANTS ARE NOT ENTITLED TO A STAY.

Consideration of a motion for stay under Fed. R. Civ. Proc. 62(c) requires the Court to balance "(1) whether the stay applicant has made a *strong showing* that he is *likely* to succeed on the merits; (2) whether the applicant will be irreparably injured absent a stay; (3) whether issuance of the stay will substantially injure the other parties interested in the proceeding; and (4) where the public interest lies." *Hilton* v. *Braunskill*, 481 U.S. 770, 776 (1987) (emphasis added); *Long* v.

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[8]Defendants' citation to *Heller*, to the effect that Woollard would need to re-apply to establish the factual predicate for demanding restoration of his permit, is inapposite. Heller only re-applied for his handgun registration permit because the District of Columbia had revamped its registration process and requirements. And Heller never applied for a permit to carry his handgun inside his home. His claim in this regard was that such permits, although contemplated by the D.C. Code, had never even been made available. In the wake of the Supreme Court's decision, the District of Columbia quickly repealed the criminal penalty for carrying handguns inside one's home without a permit, and repealed legislative authority for the issuance of handgun carry permits.

Robinson, 432 F.2d 977, 979 (4th Cir. 1970). "Defendants, as movants, have the heavy burden of

establishing that the aforementioned factors weigh in favor of a stay." *Cayuga Indian Nation* v. *Vill.*

of Union Springs, 317 F. Supp. 2d 152, 155 (N.D.N.Y. 2004).

As Defendants correctly note, they are currently ineligible for a stay of the Court's judgment

under Rule 62(c), which requires an actual notice of appeal, not merely a "possible appeal." *Corpus*

Christi Peoples' Baptist Church, Inc. v. *Texas Dep't of Human Resources*, 481 F. Supp. 1101,

1111-12 (S.D. Tex. 1979). As Defendants also correctly note, this Court nonetheless possesses an

inherent supervisory power to stay its judgment. That authority

> is not, however, without limitation. . . . proper use of this authority "calls for the exercise of
> judgment which must weigh competing interests and maintain an even balance." The party
> seeking a stay must justify it by clear and convincing circumstances outweighing potential
> harm to the party against whom it is operative. "The suppliant for a stay must make out a
> clear case of hardship or inequity in being required to go forward, if there is even a fair
> possibility that the stay for which he prays will work damage to someone else."

Williford v. *Armstrong World Industries, Inc.*, 715 F.2d 124, 127 (4th Cir. 1983) (citations

omitted).

As the Rule 62(c) standards are identical to those the Fourth Circuit would consider under

Fed. R. App. Proc. 8(a), and considering that Defendants could merely re-file their stay motion once

they have obtained a ruling on their Rule 59 motion, it may be most efficient to consider the motion

pursuant to the more detailed Rule 62(c) standards set forth in *Hilton* and *Long*.

Defendants cannot satisfy any of the four prerequisites of a stay, let alone a compelling

combination of these factors.

There is no need to revisit the questions of "whether issuance of the stay will substantially

injure the other parties interested in the proceeding; and . . . where the public interest lies." *Hilton*,

481 U.S. at 776. The Court found that Plaintiffs' fundamental Second Amendment rights are being

violated, a substantial harm which the public has a strong interest in seeing alleviated. The other two factors merit some additional discussion.

> A.   A Stay Is Not Required To Permit Further Legislation or Regulation.

As detailed *supra*, Defendants are not especially harmed by being required to license the carrying of handguns on the same terms already prevailing throughout the circuit and the Nation. Yet in seeking a stay, Defendants allege an additional harm they claim would be alleviated by a stay. "[A] stay pending appeal could . . . permit the Maryland General Assembly to consider and, should it choose to do so, to enact changes to existing law designed to address public safety concerns in a manner that is consistent with this Court's holding." Def. Br., 3/7/12, p. 12.

But no stay is required for the General Assembly to consider new legislation. The Court has not enjoined the General Assembly. Nothing in the Court's decision impedes Maryland's ability to enact further, different restrictions on the right to carry a gun, the constitutionality of which could only be evaluated if and when such restrictions are actually enacted. The Court's decision relates only as to whether individuals must prove their entitlement, generally, to carry a gun; it holds nothing with respect to how Maryland would otherwise regulate the exercise of this right.

Maryland has long opted to license the carrying of handguns, openly or concealed. If Maryland authorities truly wish to require all handgun carry permit holders to carry their handguns openly, Def. Br., 3/7/12, pp. 12-13, that option is not foreclosed by the Court's opinion, and no stay need be obtained to effectuate that policy choice. Maryland's legislature remains open for business, and indeed, is in session as of this writing through April 9, 2012. *See* http://mlis.state.md.us/ (last visited March 13, 2012). Moreover, Maryland law already allows Defendant Brown to limit the scope of permits to open or concealed carry. Md. Public Safety Code Ann. § 5-307(b). This provision enables Brown to adopt additional restrictions, which may or may not be constitutional

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depending on their terms. In any event, no stay is required to enable Defendant Brown to conjure handgun permit restrictions.

Notably, the Seventh Circuit rejected a similar claim that individuals should tolerate a Second Amendment violation because the government would be inconvenienced in pursuing its option to re-regulate. Faced with the prospect of losing its total ban on gun ranges, Chicago warned of a "regulatory vacuum" that would be left between an injunction and subsequent regulatory efforts. The court dismissed the concerns. "[W]e note that [Chicago] faced a similar dilemma after the Supreme Court decided *McDonald*. The sky did not fall. The City Council moved with dispatch and enacted the Ordinance just four days later." *Ezell*, 651 F.3d at 711. Chicago enacted its post-*Ezell* gun range regulations upon issuance of that opinion, the same day, and those regulations took effect before the District Court had an opportunity to enjoin the old prohibition on remand.

B.   Defendants Cannot Establish A "Strong Showing" That They Are "Likely" To Succeed On the Merits.

Courts have rejected the notion "that every time a case presents difficult questions of law a stay should be entered." *St. Agnes Hosp., Inc.* v. *Riddick*, 751 F. Supp. 75, 76 (D. Md. 1990) (citations omitted). It bears repeating that with respect to the first factor, a stay applicant must make "a *strong showing* that he is *likely* to succeed on the merits." *Hilton*, 481 U.S. at 776; *Long*, 432 F.2d at 979 ("likely prevail"). "Without such a substantial indication of probable success, there would be no justification for the court's intrusion into the ordinary processes of administration and judicial review." *Washington Metropolitan Area Transit Com*. v. *Holiday Tours, Inc*., 559 F.2d 841, 843 (D.C. Cir. 1977) (quoting *Virginia Petroleum Jobbers Association* v. *FPC*, 259 F.2d 921, 925 (1958)).

Defendants' arguments on the merits appear to be: (1) allowing the carrying of guns is dangerous, and (2) the courts should seek to avoid answering the Second Amendment question at the core of this case. Defendants advance no argument based on the text, history, or traditional application of the right to bear arms.

Defendants' statements that "every court to have addressed the constitutionality of similar handgun wear and carry permit statutes before this one has upheld the constitutionality of the statutes," Def. Br., 3/7/12, p. 14, and that "no court before this one has held a similar permit regime to be unconstitutional," *id.* at 17, are simply incorrect. At least four courts have held that officials cannot arbitrarily deny handgun carry licenses. "The exercise of a right guaranteed by the Constitution cannot be made subject to the will of the sheriff." *People* v. *Zerillo*, 219 Mich. 635, 639, 189 N.W. 927, 928 (1922). "The [provision] making it a crime for an unnaturalized, foreign-born resident to possess a revolver, unless so permitted by the sheriff, contravenes the guaranty of such right in the Constitution of the State and is void." *Id.* at 642, 929 (gun in car); *Kellogg* v. *City of Gary*, 562 N.E.2d 685, 694 (Ind. 1990); *State ex rel. City of Princeton* v. *Buckner*, 180 W. Va. 457, 462, 377 S.E.2d 139, 144 (1988); *Schubert* v. *De Bard*, 398 N.E.2d 1339, 1341 (Ind. App. 1980); *see also Mosby* v. *Devine*, 851 A.2d 1031, 1050 (R.I. 2004):

> [T]his Court will not countenance any system of permitting under the Firearms Act that would be committed to the unfettered discretion of an executive agency. . . One does not need to be an expert in American history to understand the fault inherent in a gun-permitting system that would allow a licensing body carte blanche authority to decide who is worthy of carrying a concealed weapon. The constitutional right to bear arms would be illusory, of course, if it could be abrogated entirely on the basis of an unreviewable unrestricted licensing scheme.

Moreover, in *Heller*, the Supreme Court discussed no fewer than four high court precedents that upheld a right to carry arms in public for self-defense: *State* v. *Reid*, 1 Ala. 612 (1840); *Nunn* v. *State*, 1 Ga. 243, 251 (1846); *Andrews* v. *State*, 50 Tenn. 165, 187-88 (1871); *State* v.

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Chandler, 5 La. Ann. 489, 490 (1850). There are others. *See, e.g. City of Las Vegas* v. *Moberg*, 82 N.M. 626, 627-28, 485 P.2d 737, 738-39 (N.M. Ct. App. 1971); *In re Brickey*, 8 Idaho 597, 599, 70 P. 609 (1902); *State* v. *Hogan*, 63 Ohio St. 202, 219, 58 N.E. 572, 575 (1900).

Heller did not merely define "bear arms" to mean "carry." *Heller* offered that "state constitutional provisions written in the 18th century or the first two decades of the 19th" were the examples "most prominent [and] most relevant to the Second Amendment" in defining the meaning of "bear arms." *Heller*, 554 U.S. at 584. None of these state constitutional provisions have been interpreted as relating solely to the home, but most were held to secure the public carrying of arms in at least some manner. "Justice James Wilson interpreted the Pennsylvania Constitution's arms-bearing right . . . as a recognition of the natural right of defense 'of one's person *or* house' — what he called the law of 'self preservation.'" *Heller*, 554 U.S. at 585 (emphasis added) (citing 2 COLLECTED WORKS OF JAMES WILSON 1142, and n.x (K. Hall & M. Hall eds., 2007)) (other citations omitted); *see also Reid*, supra, 1 Ala. 612 (interpreting Ala. Const. of 1819, art. I, § 27); *State* v. *Bailey*, 209 Conn. 322, 346, 551 A.2d 1206, 1218 (1988) (Conn. Const. art. I, § 15 (1819); *Bliss* v. *Commonwealth*, 12 Ky. 90 (1822) (Ky. Const. of 1799, art. XII, cl. 23); *State* v. *Schoultz*, 25 Mo. 128, 155 (1857) (Mo. Const. of 1820, art. XIII, § 3); *State* v. *Huntly*, 25 N.C. (3 Ired.) 418, 423 (1843) (N.C. Declaration of Rights § 17 (1776)); *Simpson* v. *State*, 13 Tenn. 356 (1833) (Tenn. Const. of 1796, art. XI, § 26); *State* v. *Rosenthal*, 55 A. 610 (Vt. 1903) (Vt. Const. c. 1, art. 16 (1777)).

Defendants note that some courts have recently taken a different view. But

> [t]he fact that courts may be reluctant to recognize the protection of the Second Amendment outside the home says more about the courts than the Second Amendment. Limiting this fundamental right to the home would be akin to limiting the protection of First Amendment freedom of speech to political speech or college campuses.

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United States v. *Weaver*, No. 2:09-CR-0222, 2012 U.S. Dist. LEXIS 29613 at *13-*14 n.7 (S.D.

W. Va. March 6, 2012). The *Weaver* court, writing simultaneously, reached the same conclusion

regarding the Second Amendment's scope as did this Court.

One state high court has already faced a claim similar to that asserted by Defendants, to the

effect that a constitutional right to arms provision must be limited to the home. In *State* v. *Kessler*,

289 Or. 359, 614 P.2d 94 (1980), Oregon's Supreme Court held that possession of a billy club was

secured by the constitutional guarantee that "[t]he people shall have the right to bear arms for the

defence of themselves . . ." Or. Const. art. I, § 27 (1857). The following year, prosecutors argued

that the right should be confined to *Kessler*'s facts, relating to home possession of a billy club, and

not to the public carrying of the same arm. The court disagreed:

> The text of the constitution is not so limited; the language is not qualified as to place except
> in the sense that it can have no effect beyond the geographical borders of this state . . . In
> *Kessler* we started from the premise that under § 27 a person has a right to bear arms for
> defense of self . . . We then moved from that general proposition to the more particular one
> that a person had the constitutional right to have a billy in his home for defense.

State v. *Blocker*, 291 Or. 255, 259, 630 P.2d 824, 825-26 (1981) (citation and footnote omitted).

Likewise, *Heller* announced a general proposition respecting constitutional protection for the

possession of handguns, and applied it to the home-bound facts of the case. *McDonald*'s description

of *Heller* neatly parallels *Blocker*'s description of *Kessler*:

> [I]n [*Heller*], we held that the Second Amendment protects the right to keep and bear arms
> for the purpose of self-defense, and we struck down a District of Columbia law that banned
> the possession of handguns in the home.

McDonald v. *City of Chicago*, 130 S. Ct. 3020, 3026 (2010).

There is no reason to suppose that the Supreme Court would suddenly limit *Heller* to its

facts—and every reason to suppose that it would decide future Second Amendment cases from the

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baseline proposition that the Amendment secures, generally, the right to keep and carry arms for

self-defense. "[T]he need for defense of self, family, and property is *most acute*" in the home,

Heller, 554 U.S. at 628 (emphasis added), and the Second Amendment right is secured "*most*

notably for self-defense within the home," *McDonald*, 130 S. Ct. at 3044 (emphasis added)—and in

this respect, the right to arms is no different than other rights. "[I]t is beyond dispute that the home is

entitled to special protection as the center of the private lives of our people." *Minnesota* v. *Carter*,

525 U.S. 83, 99 (1998) (Kennedy, J., concurring). Yet constitutional rights are not generally limited

to the home. The self-defense interest at the Second Amendment's core has never been so limited.

The overwhelming weight of historical sources, including treatises, precedent, and early state

constitutions—many invoked by the Supreme Court in *Heller*—confirm the traditional application

of the Second Amendment outside the home. Against these, Defendants can offer only the same post-

Heller cases that have already proven unpersuasive. Plaintiffs have previously addressed these cases

at length in supplemental filings, and there is no need to repeat all that has been written regarding

these cases' lack of merit. The gist of these cases, that the Second Amendment right is "unlike any

other constitutional right," Def. Br., 3/7/12, p. 8 (citing *Piszczatoski* v. *Filko*, No. 10-CV-06110,

2012 U.S. Dist. LEXIS 4293 (D.N.J. Jan. 12, 2012)), has already been rejected by the Supreme

Court. *See McDonald*, 130 S. Ct. at 3045 (rejecting argument that "Second Amendment differs

from all of the other provisions of the Bill of Rights because it concerns the right to possess a deadly

implement and thus has implications for public safety."); *see also Valley Forge Christian Coll.* v.

Ams. United for Separation of Church and State, Inc., 454 U.S. 464, 484 (1982) ("[W]e know of

no principled basis on which to create a hierarchy of constitutional values."). The Fourth Circuit

will follow *McDonald*.

And yet Plaintiffs do not rest their argument, as do Defendants, on an asserted numerical balance of authorities. The issue is not merely that Plaintiffs can cite to "x" number of sources confirming the right to bear arms exists outside the home, as against "y" contrary holdings invoked by Defendants. Rather, the issue is qualitative. The authorities relied upon to illustrate the right's public application reflect its historical understanding and logical application. The contrary cases are reactions to *Heller* and *McDonald* that misuse interest-balancing to substitute new policy preferences for those contained in the constitutional text. It is exceedingly unlikely that the Fourth Circuit will overrule this Court by offering that the right to bear arms is too dangerous to allow. Yet that appears to be Defendants' only argument on the merits.

The Court was not merely correct in securing the right to bear arms. The Court acted pursuant to, and consistent with, centuries of legal tradition.

CONCLUSION

If only to dispense with needless appellate issues, the Court should clarify its order to specify the terms of the injunction, and the fact that Plaintiffs have established the equitable predicates for obtaining that injunction. But the motion for a stay must be denied, as Defendants have failed to establish any of the factors justifying such an extraordinary request.

Dated: March 16, 2012   Respectfully submitted,

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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.   *

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

RENEWED MOTION FOR STAY PENDING APPEAL

The defendants, through counsel, hereby renew their motion under Rule 62(c) for

an order granting a stay of this Court's March 2, 2012 Order (entered March 5, 2012)

(ECF No. 53), as amended by this Court's March 30, 2012 Order (entered April 2, 2012)

(ECF No. 63).[1]   This motion is based on the reasons stated in the defendants' Motion for

Clarification or Amendment of Order and for Immediate Stay Pending Appeal (ECF No.

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[1] At the time the defendants filed the original motion for stay, they had not yet filed a notice of appeal. The notice of appeal (ECF No. 64) was filed on April 2, 2012. Because Rule 62(c) is applicable "[w]hile an appeal is pending," the defendants now renew that motion.

54), at 1-3 and 7-19, which is hereby incorporated by reference, as well as the reasons to

be stated in supplemental briefing pursuant to the March 30, 2012 Order (ECF No. 63).

Respectfully submitted,

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April 19, 2012   Attorneys for Defendants

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.	*	

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

**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

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[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,

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[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

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[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.

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[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").

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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").

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"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.

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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.

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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

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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,

DOUGLAS F. GANSLER
Attorney General of Maryland

_____/s/_____

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April 19, 2012   Attorneys for Defendants

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EXHIBIT A:

Declaration of Marcus Brown

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.   *

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

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

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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.

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[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.

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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

<div align="center">4</div>

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

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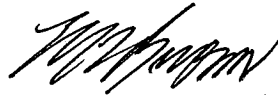
17.   As a result of the need to comply with State policies with respect to creating new positions, as well as the need to train and obtain certifications for new employees in this area, it would take a minimum of several months, and possibly much longer, to add a new position to assist with processing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

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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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Advisers to the Nation on Science, Engineering, and Medicine

The **National Academy of Sciences** is a private, nonprofit, self-perpetuating society of distinguished scholars engaged in scientific and engineering research, dedicated to the furtherance of science and technology and to their use for the general welfare. Upon the authority of the charter granted to it by the Congress in 1863, the Academy has a mandate that requires it to advise the federal government on scientific and technical matters. Dr. Bruce M. Alberts is president of the National Academy of Sciences.

The **National Academy of Engineering** was established in 1964, under the charter of the National Academy of Sciences, as a parallel organization of outstanding engineers. It is autonomous in its administration and in the selection of its members, sharing with the National Academy of Sciences the responsibility for advising the federal government. The National Academy of Engineering also sponsors engineering programs aimed at meeting national needs, encourages education and research, and recognizes the superior achievements of engineers. Dr. Wm. A. Wulf is president of the National Academy of Engineering.

The **Institute of Medicine** was established in 1970 by the National Academy of Sciences to secure the services of eminent members of appropriate professions in the examination of policy matters pertaining to the health of the public. The Institute acts under the responsibility given to the National Academy of Sciences by its congressional charter to be an adviser to the federal government and, upon its own initiative, to identify issues of medical care, research, and education. Dr. Harvey V. Fineberg is president of the Institute of Medicine.

The **National Research Council** was organized by the National Academy of Sciences in 1916 to associate the broad community of science and technology with the Academy's purposes of furthering knowledge and advising the federal government. Functioning in accordance with general policies determined by the Academy, the Council has become the principal operating agency of both the National Academy of Sciences and the National Academy of Engineering in providing services to the government, the public, and the scientific and engineering communities. The Council is administered jointly by both Academies and the Institute of Medicine. Dr. Bruce M. Alberts and Dr. Wm. A. Wulf are chair and vice chair, respectively, of the National Research Council.

www.national-academies.org

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Appendixes

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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

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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

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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.)

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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.

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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

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[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.

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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

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[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.

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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

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[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.

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[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 $\delta_A - \delta_B$.

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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 SE	Revised 1992[b]	1992	–4.91 (0.98)**	–7.30 (1.57)**	–5.27 (1.22)**
3. Committee replication SE	Revised 2000[c]	1992	–1.76 (1.07)	–9.01 (1.70)**	–5.38 (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 SE	Revised 1992[b]	1992	–0.50 (0.41)	–4.25 (0.65)**	–1.37 (0.51)**
3. Committee replication SE	Revised 2000[c]	1992	–2.15 (0.39)**	–3.41 (0.62)**	–3.37 (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

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[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

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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

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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]

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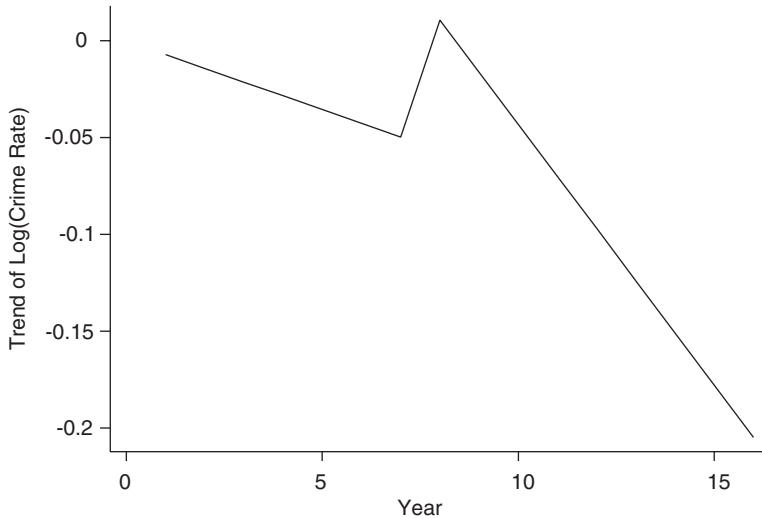
FIGURE 6-1 Trend in the logarithm of the violent crime rate.

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

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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.

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[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

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[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-

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[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.

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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

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[13]That is, we subtract the year 0 coefficient from each year's coefficient.
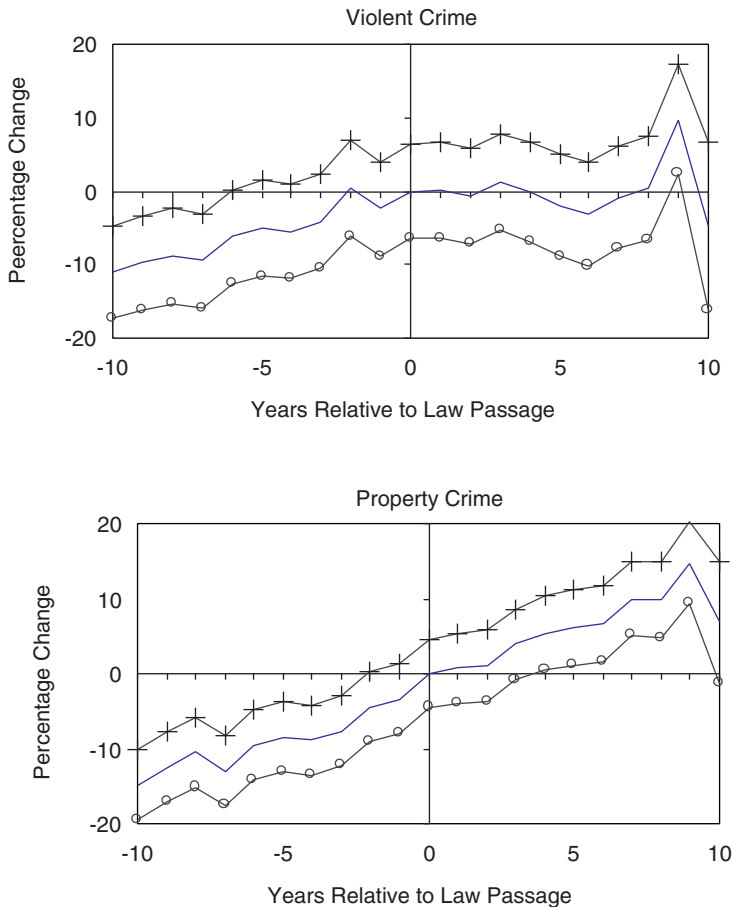
FIGURE 6-2 Year-by-year estimates of the percentage change in aggregate crime (normalized to adoption date of right-to-carry law, year 0).
—— Estimate, —o— bottom of 95% confidence interval (CI), —+—Top of 95% CI

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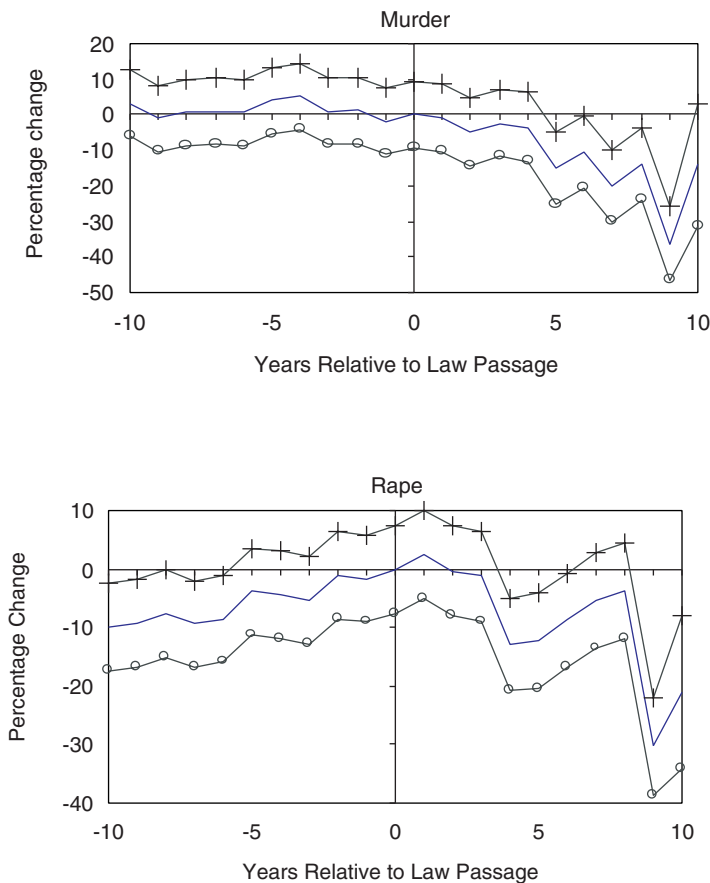
FIGURE 6-3 Year-by-year estimates of the percentage change in disaggregate violent crimes (normalized to adoption date of right-to-carry law, year 0).
—— Estimate, —o— bottom of 95% confidence interval (CI), —+—Top of 95% CI

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

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Aggravated Assault

Robbery
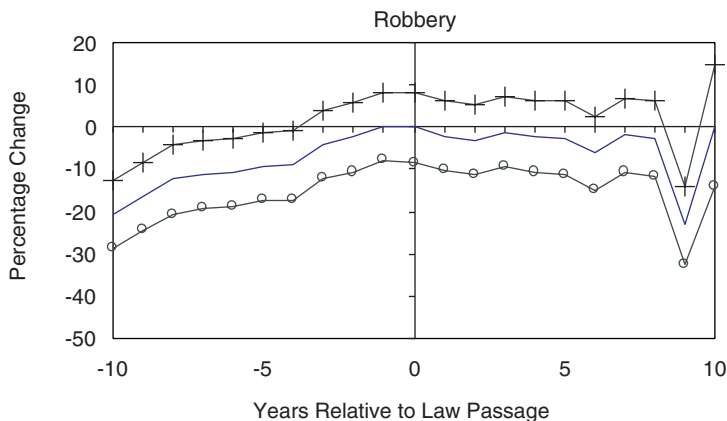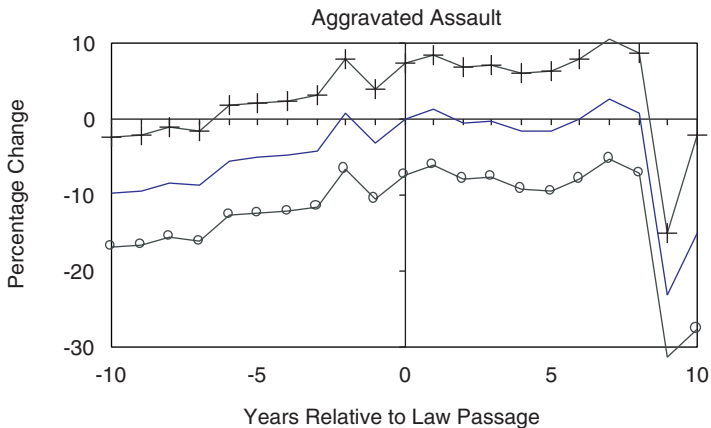
requires controlling for appropriate confounding variables and thereby re-
liance on a model such as those used by Lott and others.

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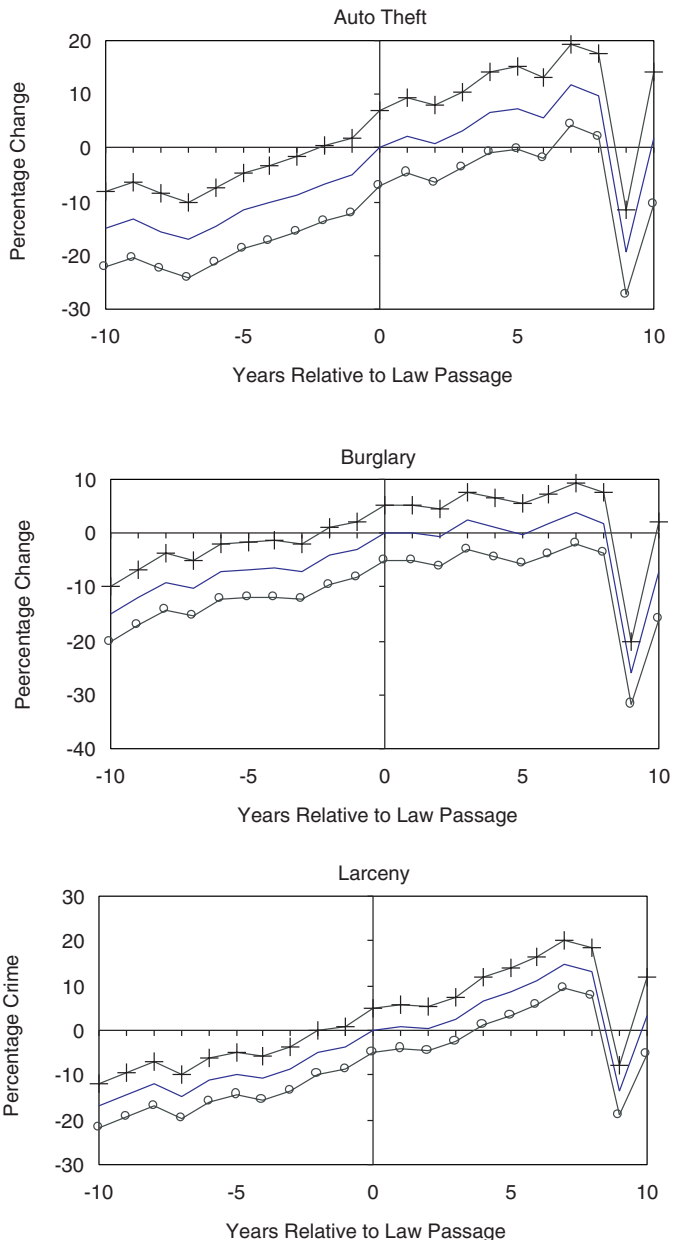
FIGURE 6-4 Year-by-year estimates of the percentage change in disaggregate property crimes (normalized to adoption date of right-to-carry law, year 0).
—— Estimate, —o— bottom of 95% confidence interval (CI), —+—Top of 95% CI

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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2   Myths about Defensive Gun Use and Permissive Gun Carry Laws
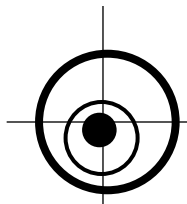
Myths about Defensive Gun Use and Permissive Gun Carry Laws

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

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[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-

[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.

[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

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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

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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

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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).

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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
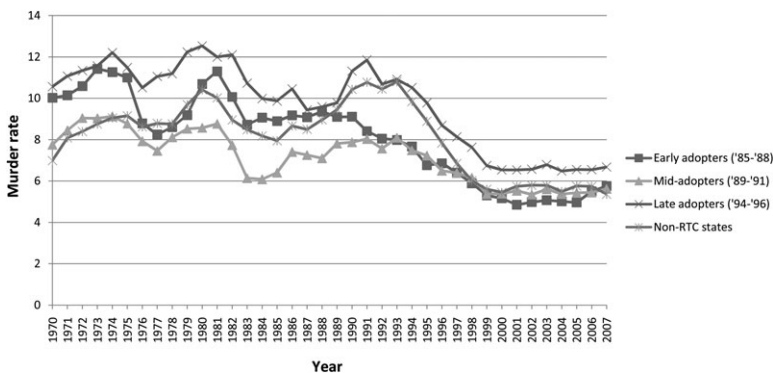
Figure 1. Murder Trends in RTC versus Non-RTC States—Weighted Average of Murder Rates per 100,000 Residents (1970–2007).
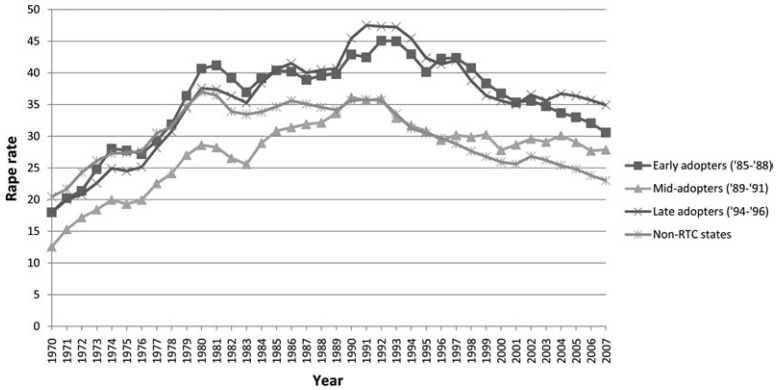
Figure 2.   Rape Trends in RTC versus Non-RTC States—Weighted Average of Rape Rates per 100,000 Residents (1970–2007).

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
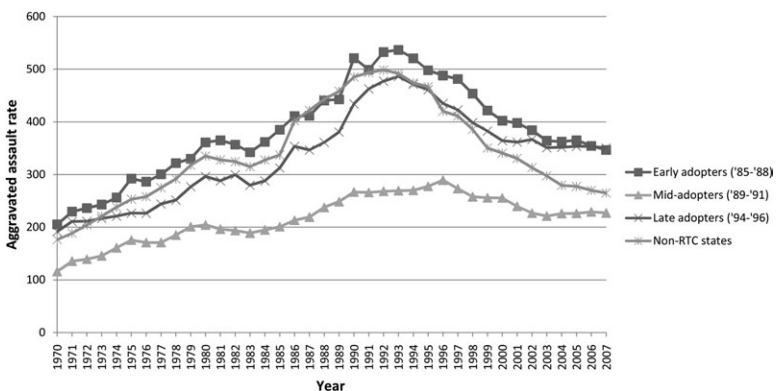
Figure 3.   Assault Trends in RTC versus Non-RTC States—Weighted Average of Assault Rates per 100,000 Residents (1970–2007).
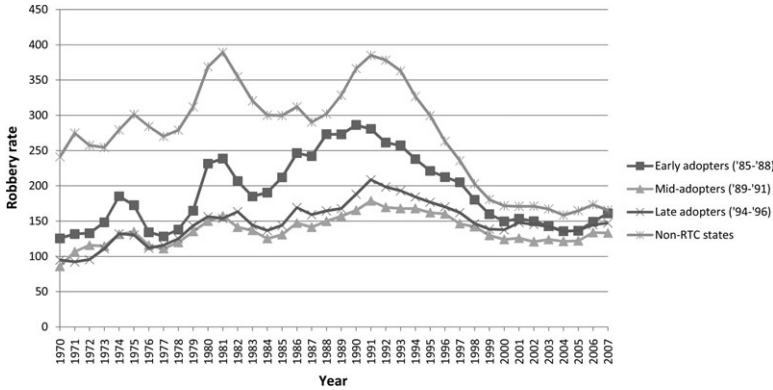
Figure 4. Robbery Trends in RTC versus Non-RTC States—Weighted Average of Robbery Rates per 100,000 Residents (1970–2007).

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
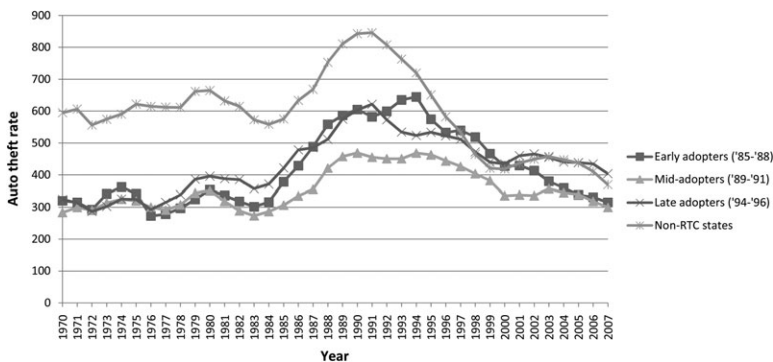
Figure 5. Auto Theft Trends in RTC versus Non-RTC States—Weighted Average of Auto Theft Rates per 100,000 Residents (1970–2007).
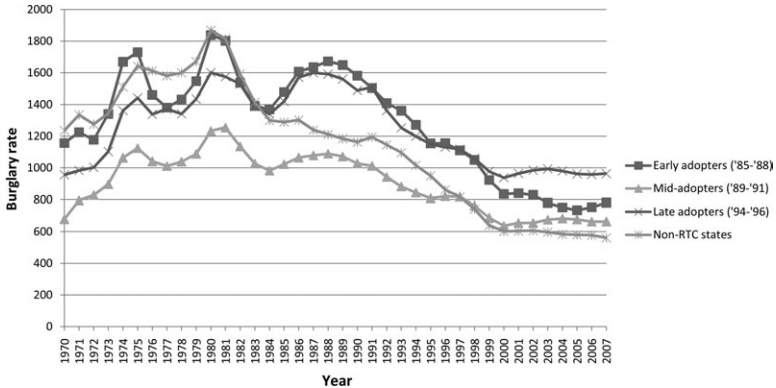
Figure 6. Burglary Trends in RTC versus Non-RTC States—Weighted Average of Burglary Rates per 100,000 Residents (1970–2007).

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

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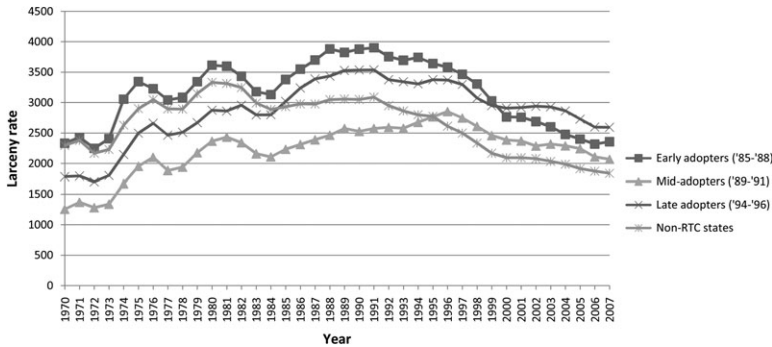
Figure 7. Larceny Trends in RTC versus Non-RTC States—Weighted Average of Larceny Rates per 100,000 Residents (1970–2007).

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

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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, popula-
tion, 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 exten-
sion also gives us an opportunity to explore how the NRC's results are

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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 re-
gression). 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

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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:

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

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

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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*** 1.05***	−0.16 0.83	3.05*** 0.80***	3.59*** 0.90***	12.74*** 0.78***	6.19*** 0.57***	12.40*** 0.59***
2. Spline model	−2.03*** 0.26***	−2.81*** 0.20***	−1.92*** 0.20***	−2.58*** 0.22***	−0.49*** 0.13***	−2.13*** 0.19***	−0.73*** 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%.

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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%.

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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

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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

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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	−6.17	−10.80	3.00	−5.31	0.21	−5.19	−0.40
model	5.31	8.27	3.60	5.66	5.85	3.55	3.04
2. Spline model	−1.21	−2.64	3.02**	−0.06	0.82	0.00	1.18
	1.46	3.48	1.23**	2.26	1.27	1.29	1.12
3. Hybrid model							
Post-passage	−5.14	−8.28	−0.64	−5.69	−0.83	−5.63	−1.95
dummy	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).

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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

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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

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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 re-searcher Michael Maltz, the FBI's county-level data are highly problematic.

The major problem with county data stems from the fact that law enforcement agencies voluntarily submit crime data to the FBI. As a result, the FBI has little control over the accuracy, consistency, timeliness, and completeness of the data it uses to compile the UCR reports. In a study published in the *Journal of Quantitative Criminology*, Maltz and Targonski (2002) carefully analyzed the short-comings in the UCR data set and concluded that UCR county-level data are unacceptable for evaluating the impact of RTC laws. For example, in Connecticut, Indiana, and Mississippi, over 50% of the county-level data points are missing crime data for more than 30% of their populations (Maltz and Targonski, 2002). In another thirteen states, more than 20% of the data points have gaps of similar magnitude. Based on their analysis, Maltz and Targonski (2002) concluded that:

County-level crime data cannot be used with any degree of confidence . . .. The crime rates of a great many counties have been underestimated, due to the exclusion of large fractions of their populations from contributing to the crime counts. Moreover, counties in those states with the most coverage gaps have laws permitting the carrying of con-cealed weapons. How these shortcomings can be compensated for is still an open ques-tion . . . 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

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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

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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	−4.94	−5.04**	1.44	−6.96**	0.31	−4.97**	2.32
model	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	−5.62	−3.77	−1.69	−7.41**	4.00	−3.92*	1.03
dummy	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	−3.32	−3.33	−1.12	−3.36	2.64	−1.93	1.21
model	3.47	2.20	2.78	3.04	2.71	1.37	1.07
2. Spline model	0.42	0.34	2.49***	0.46	−1.95***	0.35	0.39
	0.82	0.88	0.61***	1.00	0.72***	0.79	0.60
3. Hybrid model							
Post-passage	−3.83	−3.78	−3.33	−3.90	4.51	−2.33	0.92
dummy	3.58	2.42	2.84	3.10	2.85	1.62	1.28
Trend effect	0.61	0.54	2.67***	0.66	−2.19***	0.47	0.35
	0.81	0.92	0.63***	1.00	0.77***	0.83	0.64

[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]

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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 co-caine in the late 1980s and the early 1990s was likely one of the major influ-ences 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 through-out most of the country peaked at some point in the early 1990s. Coinciden-tally, 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 aggra-vated 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 find-ing 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%.

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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%.

Top 5 High & Low Crack States:
Average Crack Index, 1980-2000

Pop-weighted average crack index of high crack states (MD, MA, NJ, NY, NM)
Pop-weighted average crack index of low crack states (MT, SD, ND, NE, WY)

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.

Top 5 High & Low Crack States:
Average Murder Rate, 1980-2003

Pop-weighted average murder rate of high crack states (MD, MA, NJ, NY, NM)
Pop-weighted average murder rate of low crack states (MT, SD, ND, NE, WY)

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.)

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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

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29. In Appendix C, we further analyze the issue of misspecification and model fit by analyzing residuals from the regression analysis.
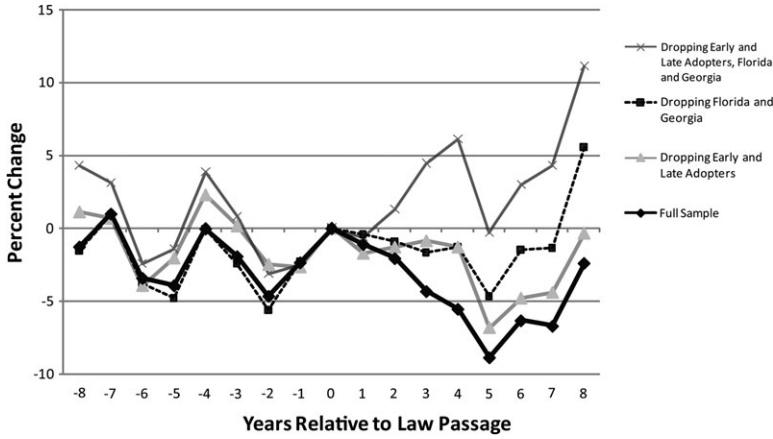
Figure 10.   Normalized Year-by-Year Estimates of the Impact of RTC Laws on Murder.
Notes: Estimations include year and county fixed effects, state trends, and are weighted by county population. The control variables include incarceration and police rates, unemployment rate, poverty rate, county population, 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
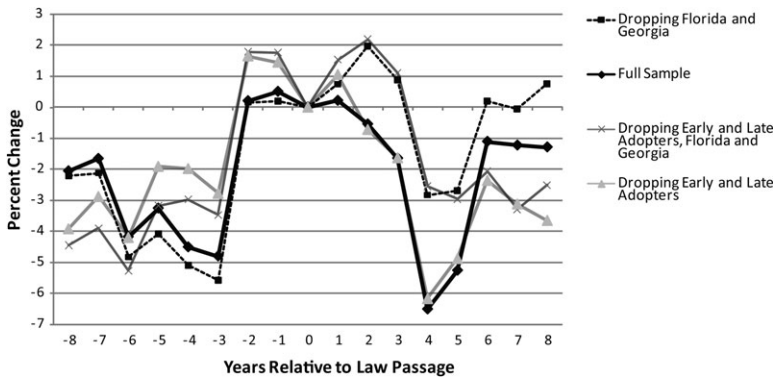
Figure 11.   Normalized Year-by-Year Estimates of the Impact of RTC Laws on Rape.
Notes: Estimations include year and county fixed effects, state trends, and are weighted by county population. The control variables include incarceration and police rates, unemployment rate, poverty rate, county population, population density, per capita income, and six demographic composition measures.
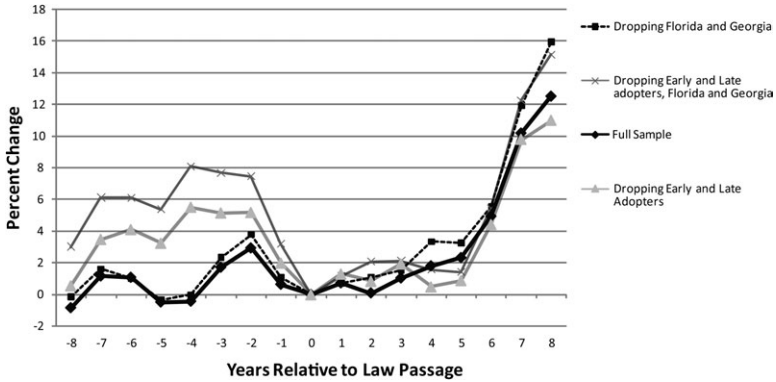
Figure 12. Normalized Year-by-Year Estimates of the Impact of RTC Laws on Assault.
Notes: Estimations include year and county fixed effects, state trends, and are weighted by county population. The control variables include incarceration and police rates, unemployment rate, poverty rate, county population, 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
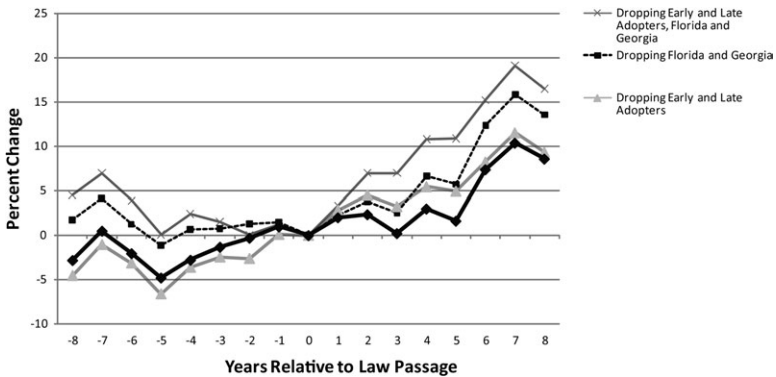
Figure 13. Normalized Year-by-Year Estimates of the Percent Change in Robbery.
Notes: Estimations include year and county fixed effects, state trends, and are weighted by county population. The control variables include incarceration and police rates, unemployment rate, poverty rate, county population, population density, per capita income, and six demographic composition measures.

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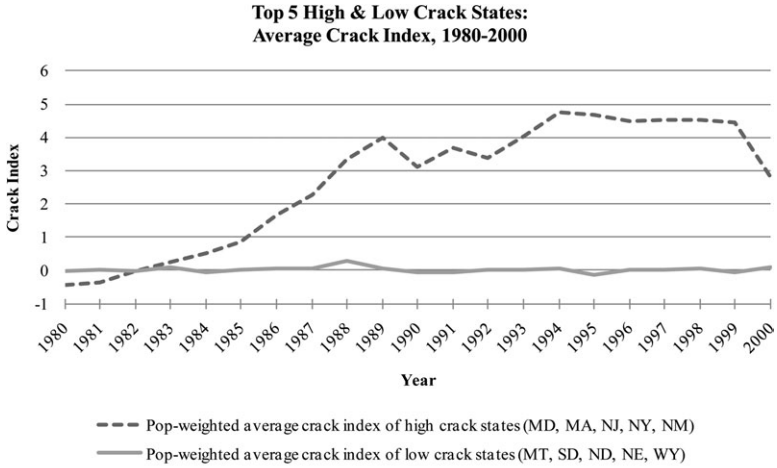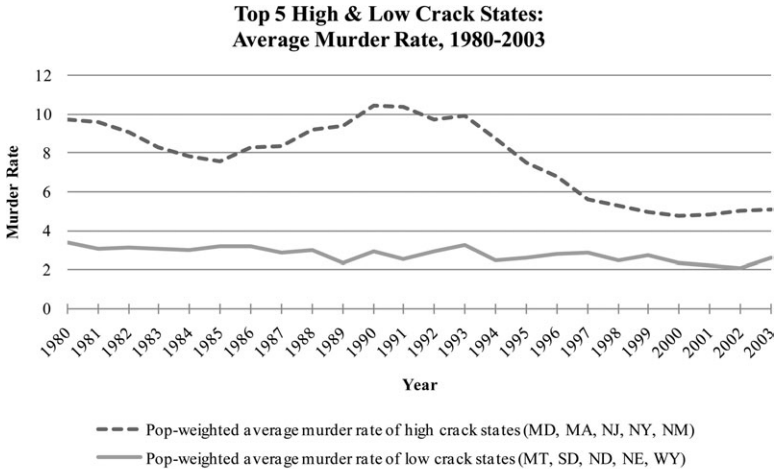
the earliest (1980) and latest (post-2000) law-adopting states. In this separate series of regressions, our estimates of the full set of lead and lag variables are based on the set of all twenty-five adopters between 1985 and 1996.[30]

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

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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

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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

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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 de-termine 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 judg-ments 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 aggra-vated assault. Further research will be needed to see if this conclusion sur-vives 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 stan-dard 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

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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	−1.79	8.33	11.70**	20.00**	24.70**	18.30***	16.60***
model	7.54	8.22	4.62**	7.90**	11.60**	6.69***	4.04***
2. Spline model	0.08	0.78	1.47**	1.98**	2.03*	1.73**	1.63***
	0.88	0.90	0.64**	0.96**	1.17*	0.72**	0.46***
3. Hybrid model							
Post-passage	−3.22	5.90	5.36	13.30*	19.60**	12.70**	11.00***
dummy	6.96	5.81	3.82	7.36*	9.00**	4.96**	3.69***
Trend effect	0.26	0.45	1.17*	1.24	0.90	0.99*	1.00**
	0.89	0.71	0.63*	0.96	0.86	0.56*	0.42**

[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	−0.31	−4.66**	0.62	3.43	8.38	1.10	0.92
model	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	−0.80	−4.39**	−0.90	3.30	8.63	1.25	1.24
dummy	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.

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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

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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	1.17	−3.56	−0.13	2.28	7.82**	1.31	1.77
model	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	0.73	−3.71	−1.77	2.14	9.13***	1.51	1.93
dummy	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	2.09	−2.88	−1.35	4.63	8.94***	1.42	2.41
model	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	1.69	−3.03	−2.50	4.39	10.00***	1.63	2.50
dummy	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

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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.

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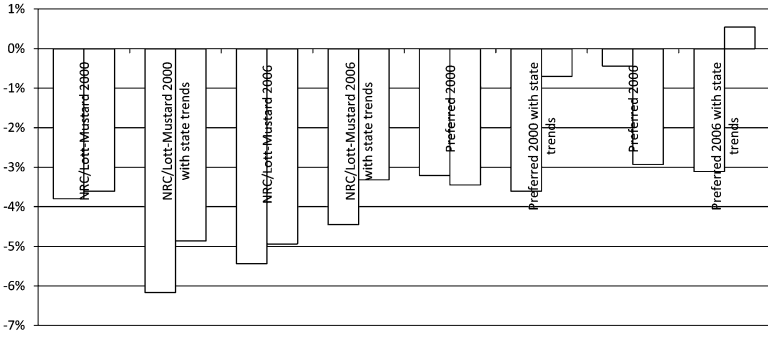
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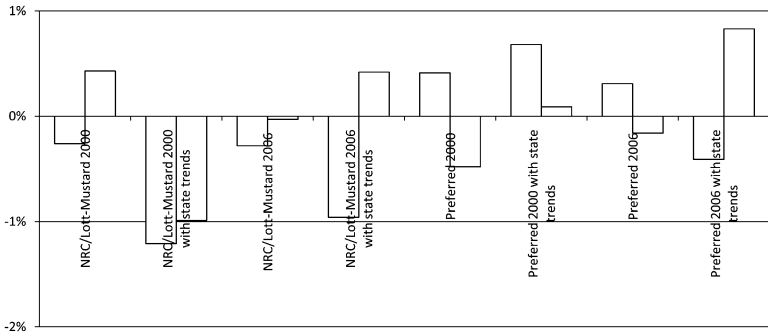
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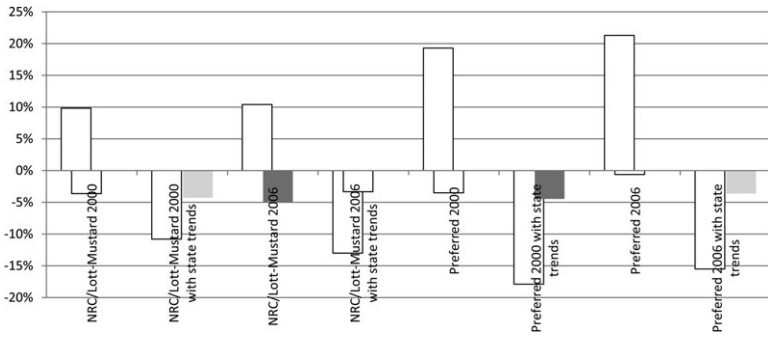
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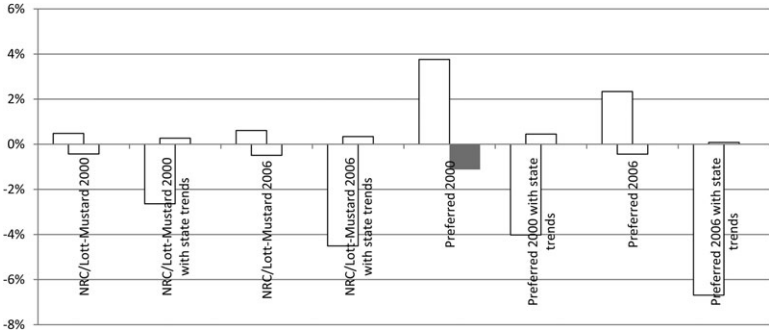
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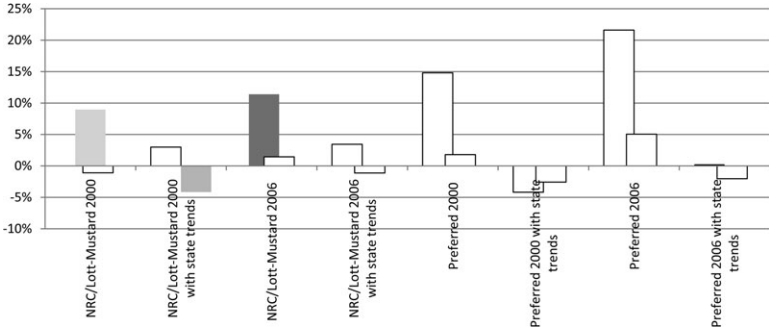
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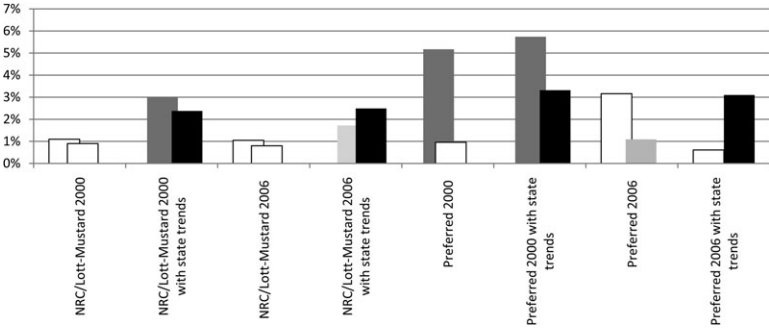
Figure D6. Various Assault Estimates (Spline Model).

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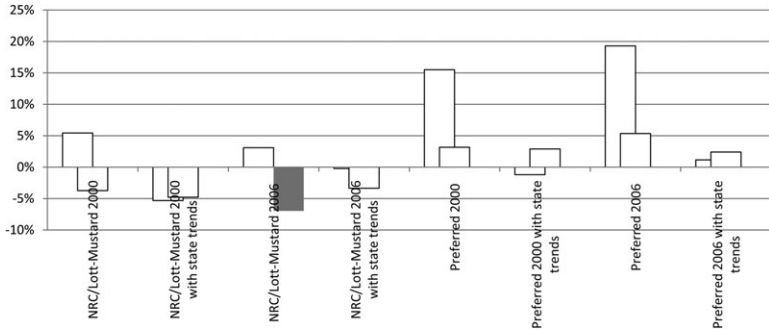
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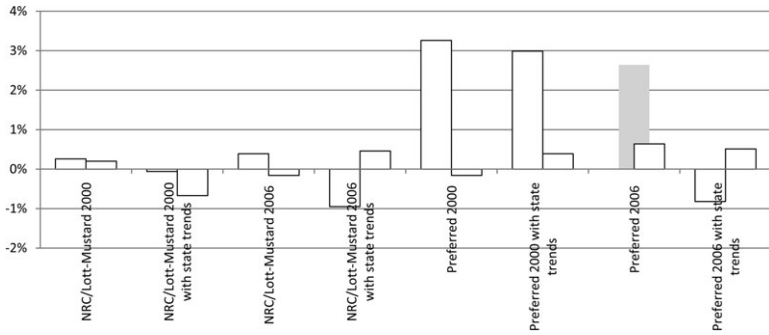
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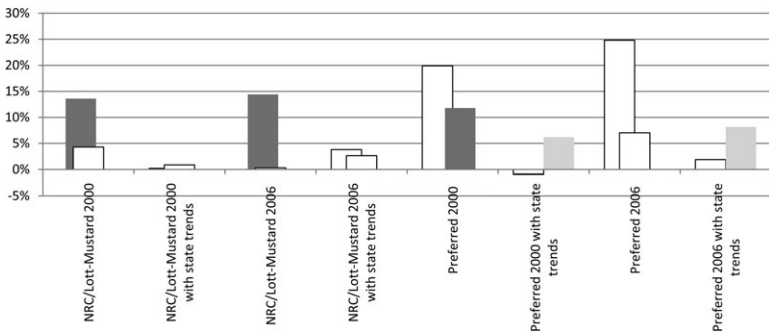
Figure D9. Various Auto Theft Estimates (Dummy Model).

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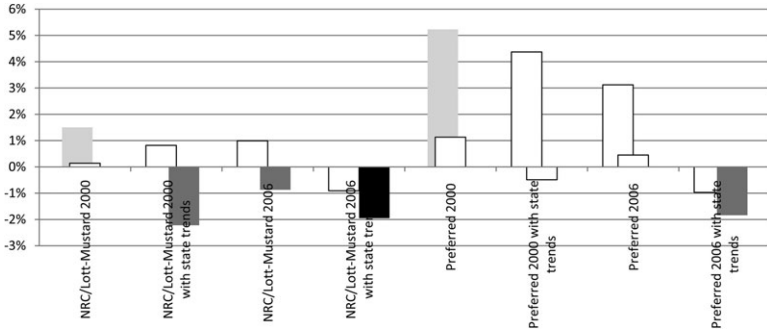
Figure D10. Various Auto Theft Estimates (Spline Model).
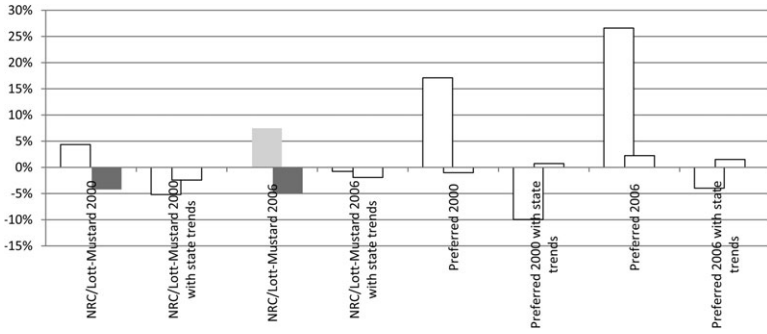
Figure D11. Various Burglary Estimates (Dummy Model).
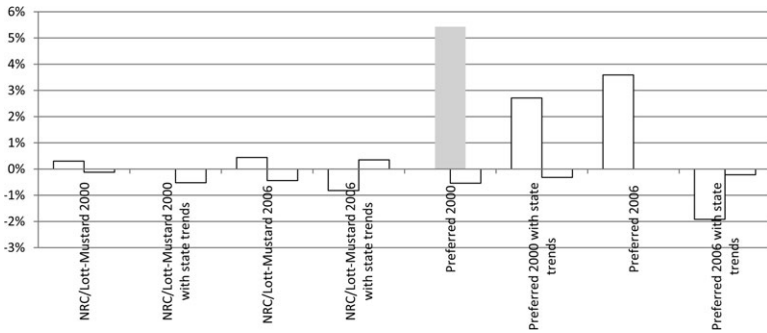
Figure D12. Various Burglary Estimates (Spline Model).
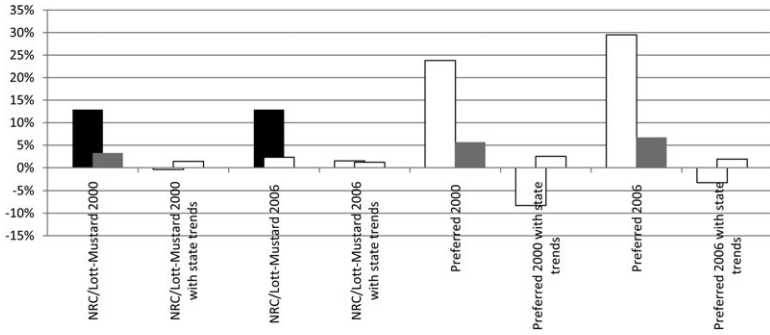
Figure D13. Various Larceny Estimates (Dummy Model).
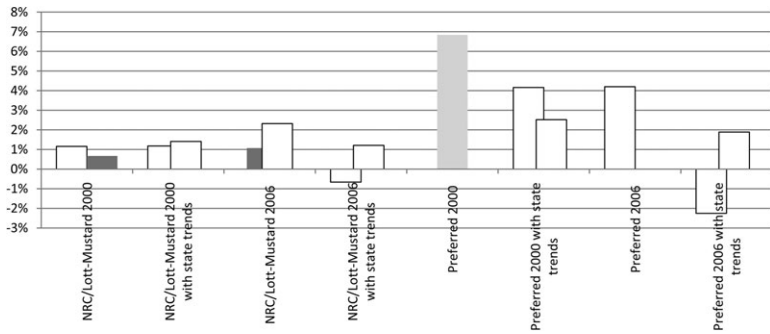
Figure D14. Various Larceny Estimates (Spline Model).

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

The New York Times   Reprints

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.

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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

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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.

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EXHIBIT F:

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

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January 28, 2007

Section: LOCAL

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."

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"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 them-selves," 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

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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.

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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

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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.

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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)

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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

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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.

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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."

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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-

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NEYS COUNCIL; PINELLAS; STATE RULES; STOP GUN VIOLENCE; SUN SENTINEL; TEXAS DE-PARTMENT 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; To-morrow: 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.

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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.

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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.

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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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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

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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

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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

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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))

Language: EN

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)

KEYWORDS: FLORIDA WEAPON LICENSE SS SERIES INVESTIGATION

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IN THE UNITED STATES DISTRICT COURT
FOR THE DISTRICT OF MARYLAND

RAYMOND WOOLLARD, et al.,   )   Case No. 1:10-cv-02068-BEL
)
            Plaintiffs,   )   SUPPLEMENTAL BRIEF IN
)   OPPOSITION TO DEFENDANTS'
      v.   )   MOTION FOR STAY PENDING
)   APPEAL
MARCUS BROWN, et al.,   )
)
            Defendants.   )
_____

      COME NOW the Plaintiffs, Raymond Woollard and Second Amendment Foundation, Inc.,

by and through undersigned counsel, and submit their Supplemental Brief in Opposition to

Defendants' Motion for Stay Pending Appeal.

      Dated: May 9, 2012   Respectfully submitted,

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                                            Attorneys for Plaintiffs

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SUPPLEMENTAL BRIEF IN OPPOSITION TO DEFENDANTS' MOTION FOR STAY PENDING APPEAL

PRELIMINARY STATEMENT

Defendants fail to carry their heavy burden of justifying the stay of an injunction, especially as the injunction addresses the widespread denial of a fundamental constitutional right. The facts are simple: (1) Maryland has higher crime than many "shall issue" states; (2) regardless of one's view of criminological theory—a belief that guns lead to crime or reduce crime—*handgun carry permit holders* do not commit much crime, and even less crime theoretically enabled by the permit; and (3) were this Court's decision reversed, the logistical impact upon Defendants would be negligible.

Defendants open by conceding that a stay is unavailable. They submit inconclusive, contradictory, and seriously controvertible studies. Defendant Brown testifies that the police are fully capable of flagging permit applications that do not meet their "good and substantial reason" ("GSR") test, for revocation upon any appellate reversal. Defendants also cite what they claim to be a too-high revocation rate in shall-issue states, states that nonetheless see negligible crime from permit holders and which generally enjoy lower overall crime rates than may-issue states. Defendants claim some states poorly administer their handgun carrying permit systems, but that can be said of any sort of government licensing scheme. And to the extent that Defendants offer further argument with respect to their prospects on appeal, those prospects have grown dimmer still with yet another court in this circuit striking down state laws that infringe upon the right to carry handguns for self-defense.

ARGUMENT

I.   DEFENDANTS CONCEDE THAT A STAY IS UNWARRANTED.

Surprisingly, Defendants open with a significant concession:

It is within the Court's discretion to grant a stay of the [injunction's] first paragraph [relating to general application of "good and substantial reason,"], but not the second [relating to Woollard]. In that case, MSP would promptly process Mr. Woollard's application.

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ECF 68, at 1 n.1. The Court should immediately lift the stay with respect to the injunction's second

paragraph.

Yet that is not all that should occur. If the Court lacks discretion to stay the injunction in

Woollard's favor, it lacks discretion to stay the injunction in the public's favor, and in favor of

Plaintiff SAF's members.[1] After all, Defendants have never claimed that Woollard is differently-

situated than anyone else, or that they erred in applying the GSR requirement against him. Were

Woollard the only plaintiff, Defendants could not self-moot the case after losing it, *United States*

Bancorp Mortg. Co. v. *Bonner Mall P'ship*, 513 U.S. 18 (1994), and the case would have

precedential value barring further enforcement of the GSR. Certainly there is no explanation given

for why the Court lacks discretion to stay enforcement of Woollard's constitutional rights, but not

anyone else's under identical circumstances.

II.   THE COSTS OF OBEYING THE CONSTITUTION ARE IRRELEVANT.

Defendant Brown's arguments with respect to the cost and bureaucratic burdens associated

with the issuance of handgun carry permits are not only unpersuasive, they are not relevant to the

issues before the Court. The Court did not ask whether the injunction would inconvenience the state.

Any injunction forces the subject party to do something it would otherwise not do, but the real harm

here is the continued deprivation of a fundamental right securing "intangible and unquantifiable

interests" in self-defense. *Ezell* v. *City of Chicago*, 651 F.3d 684, 699 (7th Cir. 2011).

As Plaintiffs noted previously, most states administer a "shall issue" system for handgun

carry permits. ECF 59, at 8-9 & n.3-5. Because states are allowed to (and do) charge nominal fees

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[1]The Court previously found, per Judge Motz, that SAF has representational standing to
assert its members' interests—an order Defendants have not appealed. *See also Ezell*, 651 F.3d at
696 (SAF members' interest in Second Amendment case).

for operating their handgun licensing programs, scaling up the applications also brings additional

funds to operate those licensing programs. There is no *evidence* that any state finds the operation of

a shall-issue system unduly burdensome, and in fact, the Court should reject the implicit suggestion

that individuals only enjoy those constitutional rights that the state does not find too burdensome to

regulate. Regulation can take various forms, and is always optional in a way that rights are not.

Twenty-seven states, including Maryland's geographic and circuit neighbors Pennsylvania, West

Virginia, Delaware, Virginia and North Carolina, generally allow unlicensed open handgun carry.

Maryland has the option of licensing all handgun carrying, and it can reasonably pass some

of the cost of that program to applicants. Md. Public Safety Code § 5-304(b). Of course neither

Plaintiffs nor this Court have sought to micromanage the logistics of *how* the state implements

permit issuance, but it is generally accepted that once a court rules on an issue, the ruling is to be

implemented, and there would be limited room for bureaucratic "delays."

As for the claim that "[d]uring the Interim Period . . . delays will most impact individuals

with GSR, those who, by definition, are most in need of a permit," Brown Decl., ¶ 17, this appears

to largely miss the point of the Court's opinion. "[T]he regulation at issue is a rationing system,"

slip op., 18, but "[t]he right's existence is all the reason [a citizen] needs" to carry a gun. *Id.*, 20.

The Court has not made any value judgments endorsing Defendants' belief that the people *they*

deem to have a "good and substantial reason" for a permit are, in fact, more urgently deserving of

one. Under the Court's injunction, a woman who wants to carry a handgun to guard against rape is

not less deserving of a permit than a business owner who handles large amounts of cash.

III.   DEFENDANTS COULD EASILY REVOKE PERMITS IN EVENT OF REVERSAL.

Defendants' assertion that it would be difficult, upon a reversal, to revoke only those permits

issued pursuant to the injunction, is belied by Defendant Brown's admission that the Maryland State

Police ("MSP") would "giv[e] applicants the option of identifying a GSR, and cooperating with

MSP's investigation of that GSR, voluntarily . . . MSP . . . would keep records of individuals who

do demonstrate GSR . . ." Brown Decl. ¶ 6.

Indeed, the injunction bars only the GSR requirement's "enforcement." Defendants are not

barred from thinking about the matter, or from taking good notes. While "the MSP would not

necessarily know whether a recipient of a [permit] during the Interim Period had GSR," Brown

Decl., ¶ 4, it could easily—instantly—return to the status quo ante, by revoking the permits of

anyone who did not establish "GSR." Brown's concern for possibly revoking the permits of people

who would have established GSR, but would have opted not to do so in reliance on the Court's

injunction, is appreciated; however, the possibility has always existed that someone might truly have

a GSR, but would be denied a permit because they could not articulate it or for whatever reason

declined to actually make the showing. If an applicant "decline[s] to provide [actual GSR] during

the application process as a matter of principle," id. ¶ 6, that's his or her calculated risk.[2]

Plaintiffs do note, however, that Defendants have means beyond voluntary compliance to

parse various carry permit applications. For example, Maryland Public Safety Code § 5-304(d) bars

Defendants from collecting application fees from "(1) a State, county, or municipal public safety

employee who is required to carry, wear, or transport a handgun as a condition of governmental

employment; or (2) a retired law enforcement officer of the State or a county or municipal

corporation of the State." This pile of applications could be reviewed last, as anyone who is fee

exempt—by definition, current or former law enforcement officers—would have presumably been

allowed to carry a firearm in any event. Additionally, the handgun carry permit application forms

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[2]People who feel strongly about the right to carry a gun and could establish a GSR likely
would have already obtained a permit.

request applicants list their occupation, employer, and title. It would be very easy to discern which applications came from security guards, armored car drivers, and the like.

The claim that some revoked licensees might not return the permit, and thus carry a gun on a revoked permit that appears to be facially valid, is unpersuasive. Defendants have doubtless revoked permits before, and the law merely requires permit holders to return the permit within ten days. Md. Public Safety Code § 5-310. Police officers can summon an astonishing amount of information about any individual or vehicle they encounter within fairly short order, including driver license information, outstanding warrants, and in many states, whether the individual is licensed to carry a handgun. It should not be difficult for the state to establish a means of verifying the current validity of handgun carry licenses much the way it does for driver's licenses or vehicle registrations.[3] In any event, those who would carry a handgun regardless of whether they had a valid permit to do so will not be impacted by any decision in this case.

IV.   MARYLAND IS DANGEROUS RELATIVE TO STATES, INCLUDING NEIGHBORING STATES, THAT ALLOW INDIVIDUALS TO CARRY HANDGUNS FOR SELF-DEFENSE.

The data on this point speaks for itself. The Court should take judicial notice of information contained on government websites. *Denius* v. *Dunlap*, 330 F.3d 919, 926 (7th Cir. 2003). Plaintiffs' Exhibit A summarizes and cites the latest available FBI Uniform Crime Reports statistics. Notably, Maryland's firearm murder rate (per 100,000 population) is 5.51, relative to the national average of 2.84. Firearm robberies in Maryland are also higher than average, 57 per 100,000 versus 42 nationally, albeit firearm assaults are lower, at 31 versus 45 per 100,000. Indeed, Maryland is hardly safer than its circuit and geographical neighbor states (Virginia, West Virginia, Pennsylvania, Delaware, North Carolina, South Carolina), all of whom enjoy lower firearm murder rates, and most

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[3]Brown does not explicitly deny the existence of this capability, which would be surprising.

of whom enjoy lower firearm robbery rates. Maryland is third of these seven neighboring states in firearm assault rates. Moreover, jurisdictions with may- or no-issue laws, which do not otherwise allow the carrying of handguns for self-defense,[4] averaged 4.41 firearm murders, 56.62 firearm robberies, and 32.62 firearm assaults per 100,000 people, with incomplete data from Illinois, against national averages of 2.84, 42, and 45 per 100,000 people, respectively.

Correlation may not be causation, but it is impossible to claim that shall-issue and permissive handgun carry regimes correlate to higher gun crime; the opposite is established fact. Certainly, the hard data does not support a claim that Maryland becoming a shall-issue state would lead to increased firearm crime.

V.   DEFENDANTS CANNOT PROVE THAT SHALL-ISSUE LAWS CAUSE CRIME.

The Court's question regarding the purported effects of shall-issue versus may-issue laws appears somewhat subsumed by the Court's related question regarding the behavior of carry permit holders. Whether Defendants meet their burden to obtain a stay relates more to the harm potentially caused by licensees rather than to the public benefit of allowing the exercise of constitutional rights, which at law must be presumed.

Plaintiffs have previously explored this ground, citing some of the voluminous research in this very contentious public policy debate. ECF 34, at 4-5. Space does not permit a full exposition of the literature on this debate. Two points bear specific mention. Though Defendants and their allies refuse to acknowledge any positive social value to carrying guns, "there seems little legitimate scholarly reason to doubt that defensive gun use is very common in the U.S., and that it probably is substantially more common than criminal gun use." Gary Kleck & Marc Gertz, *Armed Resistance*

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[4]California, D.C., Hawaii, Illinois, Maryland, Massachusetts, New York, New Jersey.

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to Crime: The Prevalence and Nature of Self- Defense with a Gun, 86 J. CRIM. L. &
CRIMINOLOGY 150, 180 (1995). And while the precise amount of positive and negative effects will
be debated forever, the Second Amendment does not "require judges to assess the costs and benefits
of firearms restrictions and thus to make difficult empirical judgments in an area in which they lack
expertise." *McDonald* v. *City of Chicago*, 130 S. Ct. 3020, 3050 (2010). Defendants' studies on
the topic are unpersuasive to many scientists in the field, and cannot satisfy the very severe burden
required to justify a stay of the Court's judgment.

It is unclear why Defendants cite to the National Research Council study, their Exhibit B, as
they admit that "[t]he panel concluded that the then-existing data was not sufficient to identify an
impact of 'shall issue' laws on crime to a scientific certainty." ECF 68, at 6 (citation omitted). But
that is not to say that the NRC study is useless. It refutes Defendants' Exhibit C, a paper suggesting
there would be a negative impact. Prof. Ludwig's other cited study concedes that data linking shall
issue laws to increased homicide is "not statistically significant." Jens Ludwig, *Concealed-Gun-
Carrying Laws and Violent Crime: Evidence from State Panel Data*, 18 INT'L REV. L. & ECON.
239, 248-49 (1998). Defendants rely very heavily on the Abhay Aneja, et al., paper submitted as
their Exhibit D, for the proposition that one of the leading "more guns, less crime" papers is flawed.
But the Aneja paper has itself been debunked. *See* Plaintiffs' Exh. B, Moody, Carlisle E., Lott, John
R., Marvell, Thomas B. and Zimmerman, Paul R., *Trust But Verify: Lessons for the Empirical
Evaluation of Law and Policy* (Jamuary 25, 2012), available at http://ssrn.com/abstract=2026957
(last visited May 8, 2012).[5] Defendants cite to one study in the Stanford Law Review purporting to
disprove the positive impact of gun carrying. Alas, the same issue relates a different perspective as

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[5]Notably, Prof. Donohue, an author of the Abeja paper, is also one of two co-editors of the
journal in which it appeared.

well. Florenz Plassman & John Whitley, *Comment: Confirming "More Guns, Less Crime,"* 55 STANFORD L. REV. 1313 (2003).

The Court has found that Defendants' GSR requirement violates a fundamental constitutional right. Injunctive relief cannot be stayed—an extraordinary request—by asserting one side of the most intractable of academic debates, of the species which the Supreme Court specifically advised is beyond the federal courts' power to resolve.

VI.   HANDGUN CARRY LICENSE HOLDERS ARE HIGHLY LAW-ABIDING.

This much has previously been explored by Plaintiffs, ECF 34, at 6, but there is now more data. Michigan issued 87,637 permits for the year ending June 30, 2011. In that time frame, it revoked only 466 permits. http://www.michigan.gov/documents/msp/2011_CPL_Report_ 376632_7.pdf (last visited May 9, 2012). North Carolina has revoked only 1,203 permits out of 228,072, half of one percent, in over 15 years. http://www.ncdoj.gov/CHPStats.aspx (last visited May 9, 2012). In 2011, Tennessee issued 94,975 handgun carry permits, and revoked just 97. http://www.tn.gov/safety/stats/DL_Handgun/Handgun/HandgunReport2011Full.pdf (last visited May 9, 2012). Texas compiles detailed information tracking the proclivity of handgun carry license permit holders to commit crimes. In 2009, of 65,561 serious criminal convictions in Texas, only 101— 0.1541%—could be attributed to individuals licensed to carry handguns, though not all such crimes necessarily utilized guns, or used them in public settings. http://www.txdps.state.tx.us/ administration/crime_records/chl/ConvictionRatesReport2009.pdf (last visited May 9, 2012). Perhaps the most comprehensive data comes from Florida, which reports having issued 2,186,010 handgun carry licenses since 1987. http://licgweb.doacs.state.fl.us/stats/ cw_monthly.pdf (last visited May 9, 2012). To date, Florida has only revoked 168 licenses—.00768%—for crimes utilizing firearms. *Id*.; *see also* David B. Kopel, *Pretend "Gun-Free" School Zones: A Deadly*

Legal Fiction, 42 CONN. L. REV. 515, 564-70 (2009) (surveying data).

Against this hard data, Defendants invoke a non-credible "study" by an anti-gun rights group, and the lamentable Zimmerman-Martin affair. ECF 68, at 8. Plaintiffs have already responded to the VPC website. ECF 34, at 5-6. As to the other matter, Plaintiffs have nothing useful to add to that public controversy, as *just like Defendants*, they know only what they see in the media: a tragedy under unclear yet hotly-disputed facts. Given the widespread prevalence of handgun carrying in the United States, *some* handgun license permit holders will commit crime, just like some licensed drivers will drive drunk and in an otherwise reckless manner. But that is hardly a reason to draw inferences about licensed drivers. Anecdotes do not refute the *data* that whatever else leads to the rare license revocation, crime utilizing publicly carried handguns is quite rare. Defendants complain that the number of revocations indicate the licensing is dangerous, but what matters is not the (valid) precautions taken in revoking licenses, but the virtually non-existent crime by licensees. Defendants alternatively complain that some states are too lax in revoking licenses, and perhaps that is true. But that is an indictment of the authorities, not of the right peaceably enjoyed by millions of Americans.

VII.   DEFENDANTS ARE UNLIKELY TO SUCCEED ON THE MERITS.

Defendants persist in attacking the case on its merits, arguing that the carrying of handguns is not a "core" right and that the right to bear arms for self-defense in everyday public life, presumably including on the streets of Baltimore, is satisfied by the carrying of long arms. ECF 68, 1-2. The argument is styled as an equitable one, but it is premised upon an assumption that the Court erred in its substantive constitutional analysis.

Defendants plainly fail to make a "*strong* showing that [they are] *likely* to succeed on the merits." *Hilton* v. *Braunskill*, 481 U.S. 770, 776 (1987) (emphasis added). The right to carry arms

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outside the home for self-defense is well-within the Second Amendment's historical understanding.

ECF 59, at 15-19. Moreover, *Heller* emphatically rejected the argument that handguns may be

banned because long arms are an available substitute. Likewise here, the question is not whether

people may carry shotguns (or sabers) instead of handguns. The question is whether traditionally,

practically, the right to bear arms encompasses carrying handguns outside the home. Plainly, it does,

and Defendants offer no evidence for the specious proposition that individuals can and do carry long

arms for personal self-defense under normal circumstances in public life. That the practice is

virtually unknown in this country is within judicial notice.

Indeed, since the conference call, Defendants' prospects on appeal are yet-more remote.

Another district court of this circuit has struck down laws restricting the carrying of handguns.

> [T]he Second Amendment right to keep and bear arms 'is not strictly limited to the home environment but extends in some form to wherever those activities or needs occur'. . . .
> Although considerable uncertainty exists regarding the scope of the Second Amendment right to keep and bear arms, it undoubtedly is not limited to the confines of the home.

Bateman v. *Perdue*, No. 5:10-CV-265-H, 2012 U.S. Dist. LEXIS 47336, at *10-*11 (E.D.N.C.

Mar. 29, 2012) (citation omitted).

CONCLUSION

The application for a stay must be denied.

Dated: May 9, 2012   Respectfully submitted,

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Gun Crime in the US, 2010

http://www.fbi.gov/about-us/cjis/ucr/crime-in-the-u.s/2010/crime-in-the-u.s.-2010

State	Code	Total murders	Total firearms murders, 2010	Total firearms murders, 2009	% change, 2009-10	Handguns murders	Firearms murders as % of all murders	Firearms murders per 100,000 population	Firearms robberies per 100,000 population	Firearms assaults per 100,000 population
UNITED STATES		**12,996**	**8,775**	**9,146**	**-4**	**6,009**	**68**	**2.84**	**42**	**45**
Alabama	AL	199	135	229	-41	112	68	2.85	17	32
Alaska	AK	31	19	13	46	3	61	2.68	22	77
Arizona	AZ	352	232	197	18	152	66	3.47	45	54
Arkansas	AR	130	93	107	-13	49	72	3.20	37	88
California	CA	1,811	1,257	1,360	-8	953	69	3.37	48	45
Colorado	CO	117	65	94	-31	34	56	1.28	22	38
Connecticut	CT	131	97	70	39	72	74	2.75	33	22
Delaware	DE	48	38	31	23	25	79	4.26	94	92
District of Columbia	DC	131	99	113	-12	32	76	16.21	256	99
Florida	FL								59	71
Georgia	GA	527	376	378	-1	315	71	3.79	62	52
Hawaii	HI	24	7	8	-13	6	29	0.54	7	13
Idaho	ID	21	12	5	140	12	57	0.77	4	23
Illinois (1)	IL	453	364	386	-6	355	80	2.81	2	6
Indiana	IN	198	142	209	-32	83	72	2.20	17	8
Iowa	IA	38	21	11	91	9	55	0.69	9	19
Kansas	KS	100	63	85	-26	30	63	2.22	24	71
Kentucky	KY	180	116	112	4	76	64	2.67	40	24
Louisiana	LA	437	351	402	-13	263	80	7.75	47	77
Maine	ME	24	11	11	0	4	46	0.84	6	4
Maryland	MD	424	293	305	-4	272	69	5.11	57	31
Massachusetts	MA	209	118	93	27	52	56	1.78	25	31
Michigan	MI	558	413	437	-5	239	74	4.16	56	83
Minnesota	MN	91	53	38	39	43	58	1.00	19	20
Mississippi	MS	165	120	105	14	98	73	4.05	48	30
Missouri	MO	419	321	276	16	189	77	5.34	53	89

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State	Abbr								
Montana	MT	21	12	19	-37	6	57	1.22	2	28
Nebraska	NE	51	32	23	39	29	63	1.77	24	29
Nevada	NV	158	84	91	-8	57	53	3.16	65	59
New Hampshire	NH	13	5	4	25	2	38	0.38	7	15
New Jersey	NJ	363	246	220	12	216	68	2.82	45	24
New Mexico	NM	118	67	78	-14	36	57	3.29	31	82
New York	NY	860	517	481	7	135	60	2.64	13	12
North Carolina	NC	445	286	335	-15	188	64	3.02	47	60
North Dakota	ND	9	4	3	33	3	44	0.61	2	3
Ohio	OH	460	310	311	0	176	67	2.69	56	30
Oklahoma	OK	188	111	125	-11	86	59	2.98	40	63
Oregon	OR	78	36	41	-12	20	46	0.93	15	16
Pennsylvania	PA	646	457	468	-2	367	71	3.62	52	39
Rhode Island	RI	29	16	18	-11	2	55	1.51	19	29
South Carolina	SC	280	207	197	5	136	74	4.50	58	115
South Dakota	SD	14	8	4	100	3	57	0.98	2	18
Tennessee	TN	356	219	295	-26	146	62	3.46	74	130
Texas	TX	1,246	805	862	-7	581	65	3.19	65	62
Utah	UT	52	22	25	-12	16	42	0.78	12	21
Vermont	VT	7	2	0		1	29	0.32	2	8
Virginia	VA	369	250	229	9	137	68	3.14	37	24
Washington	WA	151	93	101	-8	73	62	1.38	21	25
West Virginia	WV	55	27	38	-29	16	49	1.48	4	19
Wisconsin	WI	151	97	95	2	63	64	1.71	41	31
Wyoming	WY	8	5	8	-38	0	63	0.91	5	14

• Florida figures not provided
(1) Illinois supplied limited supplemental data in homicide, robbery and assault crimes.

TOTAL US RATE PER 100,000 IS OFFICIAL FBI RATE
• Rates for robbery and assault are FBI official rates, others are calculated using FBI data
SOURCE: FBI

Table 20: Murder by State , Type of Weapons, 2010

http://www.fbi.gov/about-us/cjis/ucr/crime-in-the-u.s/2010/crime-in-the-u.s.-2010/tables/10tbl20.xls

State	Code	Total murders (1)	Total firearms	Handguns	Rifles	Shotguns	Firearms (type unknown)	Knives or cutting instruments	Other weapons	Hands, fists, feet, etc. (2)	Population (census bureau)	Firearms murders as % of all murders	Firearms murders per 100,000 population
UNITED STATES		**12,996**	**8,775**	**6,009**	**358**	**373**	**2,035**	**1,704**	**1,772**	**745**	**309,050,816**	**67.52**	**2.84**
Alabama	AL	199	135	112	0	23	0	23	24	17	4,729,656	67.84	**2.85**
Alaska	AK	31	19	3	5	1	10	4	4	4	708,862	61.29	**2.68**
Arizona	AZ	352	232	152	14	10	56	51	62	7	6,676,627	65.91	**3.47**
Arkansas	AR	130	93	49	7	4	33	12	21	4	2,910,236	71.54	**3.20**
California	CA	1,811	1,257	953	59	44	201	250	201	103	37,266,600	69.41	**3.37**
Colorado	CO	117	65	34	0	4	27	20	21	11	5,095,309	55.56	**1.28**
Connecticut	CT	131	97	72	0	1	24	20	8	6	3,526,937	74.05	**2.75**
Delaware	DE	48	38	25	0	2	11	8	0	2	891,464	79.17	**4.26**
District of Columbia	DC	131	99	32	0	0	67	20	7	5	610,589	75.57	**16.21**
Georgia	GA	527	376	315	19	21	21	64	85	2	9,908,357	71.35	**3.79**
Hawaii	HI	24	7	6	0	0	1	6	5	6	1,300,086	29.17	**0.54**
Idaho	ID	21	12	12	0	0	0	4	2	3	1,559,796	57.14	**0.77**
Illinois (3)	IL	453	364	355	3	1	5	30	43	16	12,944,410	80.35	**2.81**
Indiana	IN	198	142	83	11	7	41	19	25	12	6,445,295	71.72	**2.20**
Iowa	IA	38	21	9	1	4	7	4	9	4	3,023,081	55.26	**0.69**
Kansas	KS	100	63	30	4	1	28	13	18	6	2,841,121	63.00	**2.22**
Kentucky	KY	180	116	76	6	16	18	19	25	20	4,339,435	64.44	**2.67**
Louisiana	LA	437	351	263	19	11	58	42	31	13	4,529,426	80.32	**7.75**
Maine	ME	24	11	4	2	1	4	6	1	6	1,312,939	45.83	**0.84**
Maryland	MD	424	293	272	3	12	6	59	53	19	5,737,274	69.10	**5.11**
Massachusetts	MA	209	118	52	0	1	65	50	31	10	6631280	56.46	**1.78**
Michigan	MI	558	413	239	25	14	135	43	71	31	9,931,235	74.01	**4.16**
Minnesota	MN	91	53	43	2	8	0	14	16	8	5,290,447	58.24	**1.00**
Mississippi	MS	165	120	98	3	12	7	21	19	5	2,960,467	72.73	**4.05**
Missouri	MO	419	321	189	26	4	102	35	49	14	6,011,741	76.61	**5.34**
Montana	MT	21	12	6	2	4	0	3	5	1	980,152	57.14	**1.22**

State												
Nebraska	NE	51	32	29	1	2	0	8	5	6	1,811,072	62.75	**1.77**
Nevada	NV	158	84	57	5	6	16	22	34	18	2,654,751	53.16	**3.16**
New Hampshire	NH	13	5	2	0	2	1	5	3	0	1,323,531	38.46	**0.38**
New Jersey	NJ	363	246	216	7	2	21	50	39	28	8,732,811	67.77	**2.82**
New Mexico	NM	118	67	36	6	2	23	29	15	7	2,033,875	56.78	**3.29**
New York	NY	860	517	135	6	12	364	173	148	22	19,577,730	60.12	**2.64**
North Carolina	NC	445	286	188	21	25	52	56	72	31	9,458,888	64.27	**3.02**
North Dakota	ND	9	4	3	0	1	0	0	4	1	653,778	44.44	**0.61**
Ohio	OH	460	310	176	7	2	125	40	93	17	11,532,111	67.39	**2.69**
Oklahoma	OK	188	111	86	8	7	10	24	32	21	3,724,447	59.04	**2.98**
Oregon	OR	78	36	20	1	2	13	16	17	9	3,855,536	46.15	**0.93**
Pennsylvania	PA	646	457	367	8	11	71	67	94	28	12,632,780	70.74	**3.62**
Rhode Island	RI	29	16	2	1	1	12	5	6	2	1,056,870	55.17	**1.51**
South Carolina	SC	280	207	136	8	7	56	22	34	17	4,596,958	73.93	**4.50**
South Dakota	SD	14	8	3	0	1	4	2	4	0	820,077	57.14	**0.98**
Tennessee	TN	356	219	146	12	11	50	35	83	19	6,338,112	61.52	**3.46**
Texas	TX	1,246	805	581	34	48	142	202	130	109	25,213,445	64.61	**3.19**
Utah	UT	52	22	16	0	1	5	7	12	11	2,830,753	42.31	**0.78**
Vermont	VT	7	2	1	1	0	0	1	2	2	622,433	28.57	**0.32**
Virginia	VA	369	250	137	9	12	92	47	50	22	7,952,119	67.75	**3.14**
Washington	WA	151	93	73	4	2	14	24	22	12	6,746,199	61.59	**1.38**
West Virginia	WV	55	27	16	0	4	7	11	9	8	1,825,513	49.09	**1.48**
Wisconsin	WI	151	97	63	5	6	23	13	22	19	5,668,519	64.24	**1.71**
Wyoming	WY	8	5	0	2	0	3	1	1	1	547,637	62.50	**0.91**

[1] Total number of murders for which supplemental homicide data were received.

[2] Pushed is included in hands, fists, feet, etc.

[3] Limited supplemental homicide data were received.

TOTAL US RATE PER 100,000 IS OFFICIAL FBI RATE

Table 20: Murder by State, Types of Weapons, 2009

http://www2.fbi.gov/ucr/cius2009/data/table_20.html

State	State code	Total murders (1)	Total firearms	Handguns	Rifles	Shotguns	Firearms (type unknown)	Knives or cutting instruments	Other weapons	Hands, fists, feet, etc. (2)	Firearms murders as % of all murders	Population, 2009	Firearms murders per 100,000 pop
US		13,636	9,146	6,452	348	418	94	1,825	969	801	67.1	307,006,550	2.98
Alabama	AL	318	229	196	1	32	0	29	40	20	72.0	4,708,708	4.86
Alaska	AK	22	13	1	0	0	12	4	3	2	59.1	698,473	1.86
Arizona	AZ	328	197	164	10	10	13	61	53	17	60.1	6,595,778	2.99
Arkansas	AR	171	107	54	5	5	43	21	38	5	62.6	2,889,450	3.70
California	CA	1,972	1,360	1,022	45	49	244	291	214	107	69.0	36,961,664	3.68
Colorado	CO	167	94	55	6	6	27	23	30	20	56.3	5,024,748	1.87
Connecticut	CT	107	70	51	0	2	17	17	14	6	65.4	3,518,288	1.99
Delaware	DE	41	31	20	2	0	9	6	1	3	75.6	885,122	3.50
District of Columbia	DC	144	113	80	1	1	31	17	9	5	78.5	599,657	18.84
Georgia	GA	543	378	323	17	19	19	56	97	12	69.6	9,829,211	3.85
Hawaii	HI	21	8	4	2	1	1	3	4	6	38.1	1,295,178	0.62
Idaho	ID	22	5	3	0	0	2	3	9	5	22.7	1,545,801	0.32
Illinois3	IL	479	386	360	5	8	13	39	48	6	80.6	12,910,409	2.99
Indiana	IN	293	209	136	8	14	51	34	40	10	71.3	6,423,113	3.25
Iowa	IA	34	11	3	1	3	4	8	6	9	32.4	3,007,856	0.37
Kansas	KS	118	85	38	9	0	38	14	11	8	72.0	2,818,747	3.02
Kentucky	KY	170	112	90	5	6	11	22	27	9	65.9	4,314,113	2.60
Louisiana	LA	486	402	330	20	11	41	32	37	15	82.7	4,492,076	8.95
Maine	ME	26	11	4	0	0	7	6	6	3	42.3	1,318,301	0.83
Maryland	MD	438	305	297	2	6	0	58	57	18	69.6	5,699,478	5.35
Massachusetts	MA	169	93	47	2	1	43	40	29	7	55.0	6,593,587	1.41
Michigan	MI	625	437	239	25	19	154	47	112	29	69.9	9,969,727	4.38
Minnesota	MN	72	38	35	1	1	1	14	8	12	52.8	5,266,214	0.72
Mississippi	MS	151	105	83	9	6	7	22	15	9	69.5	2,951,996	3.56
Missouri	MO	381	276	170	8	11	87	40	50	15	72.4	5,987,580	4.61

Montana	MT	28	19	9	2	5	3	4	2	3	67.9	974,989	**1.95**
Nebraska	NE	40	23	22	1	0	0	8	4	5	57.5	1,796,619	**1.28**
Nevada	NV	156	91	66	1	3	21	25	27	13	58.3	2,643,085	**3.44**
New Hampshire	NH	10	4	1	0	0	3	3	2	1	40.0	1,324,575	**0.30**
New Jersey	NJ	319	220	189	3	6	22	44	36	19	69.0	8,707,739	**2.53**
New Mexico	NM	144	78	54	2	3	19	24	29	13	54.2	2,009,671	**3.88**
New York	NY	779	481	117	8	13	343	166	109	23	61.7	19,541,453	**2.46**
North Carolina	NC	480	335	243	17	20	55	49	64	32	69.8	9,380,884	**3.57**
North Dakota	ND	9	3	1	1	1	0	0	3	3	33.3	646,844	**0.46**
Ohio	OH	502	311	193	2	9	107	52	95	44	62.0	11,542,645	**2.69**
Oklahoma	OK	225	125	104	10	4	7	45	25	30	55.6	3,687,050	**3.39**
Oregon	OR	83	41	9	2	10	20	21	19	2	49.4	3,825,657	**1.07**
Pennsylvania	PA	658	468	373	13	11	71	66	100	24	71.1	12,604,767	**3.71**
Rhode Island	RI	31	18	0	0	0	18	6	5	2	58.1	1,053,209	**1.71**
South Carolina	SC	286	197	115	4	12	66	28	41	20	68.9	4,561,242	**4.32**
South Dakota	SD	11	4	0	1	2	1	5	1	1	36.4	812,383	**0.49**
Tennessee	TN	461	295	200	13	22	60	45	92	29	64.0	6,296,254	**4.69**
Texas	TX	1,325	862	661	55	58	88	197	153	113	65.1	24,782,302	**3.48**
Utah	UT	37	25	15	0	5	5	8	2	2	67.6	2,784,572	**0.90**
Vermont	VT	7	0	0	0	0	0	4	1	2	0.0	621,760	**0.00**
Virginia	VA	347	229	108	8	7	106	41	55	22	66.0	7,882,590	**2.91**
Washington	WA	169	101	75	16	4	6	35	14	19	59.8	6,664,195	**1.52**
West Virginia	WV	76	38	20	2	3	13	19	13	6	50.0	1,819,777	**2.09**
Wisconsin	WI	144	95	65	3	9	18	22	13	14	66.0	5,654,774	**1.68**
Wyoming	WY	11	8	7	0	0	1	1	1	1	72.7	544,270	**1.47**

[1] Total number of murders for which supplemental homicide data were received.

[2] Pushed is included in hands, fists, feet, etc.

[3] Limited supplemental homicide data were received.

TOTAL US RATE PER 100,000 IS OFFICIAL FBI RATE

Table 21: Robbery by State, Types of Weapons, 2010

http://www.fbi.gov/about-us/cjis/ucr/crime-in-the-u.s/2010/crime-in-the-u.s.-2010/tables/10tbl21.xls

State	Total robberies (1)	Firearms	Knives or cutting instruments	Other weapons	Strong-arm	Agency count	Population (census bureau)	Firearms robberies per 100,000 pop)
UNITED STATES	**367832**	**128793**	**24388**	**27170**	**130839**		**309050816**	**41.67**
Alabama	1511	817	91	113	490	303	4729656	**17.27**
Alaska	584	153	53	55	323	35	708862	**21.58**
Arizona	6864	3036	645	599	2584	96	6676627	**45.47**
Arkansas	2283	1067	137	227	852	199	2910236	**36.66**
California	58035	18053	5044	5357	29581	729	37266600	**48.44**
Colorado	3068	1119	308	383	1258	171	5095309	**21.96**
Connecticut	3483	1164	377	297	1645	99	3526937	**33.00**
Delaware	1829	839	146	136	708	53	891464	**94.11**
District of Columbia	3914.00	1563.00	246.00	209.00	1896.00	1.00	610589.00	**255.98**
Florida	26071	11105	1730	2206	11030	663	18678049	**59.45**
Georgia	10551	6192	474	876	3009	458	9908357	**62.49**
Hawaii	988	97	80	92	719	3	1300086	**7.46**
Idaho	213	61	26	25	101	102	1559796	**3.91**
Illinois (2)	495	240	20	60	175	1	#N/A	**#N/A**
Indiana	2665	1122	199	311	1033	286	6445295	**17.41**
Iowa	995	274	95	104	522	188	3023081	**9.06**
Kansas	1511	695	120	161	535	236	2841121	**24.46**
Kentucky	3673	1716	301	323	1333	330	4339435	**39.54**
Louisiana	4067	2121	248	300	1398	172	4529426	**46.83**
Maine	414	75	70	52	217	167	1312939	**5.71**
Maryland	6809	3266	552	342	2649	153	5737274	**56.93**
Massachusetts	6712	1626	1298	880	2908	320	6631280	**24.52**
Michigan	11238	5523	524	909	4282	515	9931235	**55.61**
Minnesota	3088	1011	234	399	1444	305	5290447	**19.11**
Mississippi	2278	1422	97	190	569	121	2960467	**48.03**
Missouri	6029	3180	322	382	2145	578	6011741	**52.90**
Montana	122	20	16	41	45	96	980152	**2.04**
Nebraska	1018	439	70	76	433	211	1811072	**24.24**

Nevada	4844	1722	450	427	2245	40	2654751	**64.86**
New Hampshire	427	94	50	71	212	150	1323531	**7.10**
New Jersey	11720	3944	964	777	6035	578	8732811	**45.16**
New Mexico	1581	626	229	140	586	88	2033875	**30.78**
New York	8770	2540	1004	970	4256	533	19577730	**12.97**
North Carolina	8540	4419	551	769	2801	308	9458888	**46.72**
North Dakota	89	12	5	10	62	87	653778	**1.84**
Ohio	15644	6479	738	1491	6936	444	11532111	**56.18**
Oklahoma	3293	1503	268	262	1260	302	3724447	**40.35**
Oregon	2237	580	236	213	1208	140	3855536	**15.04**
Pennsylvania	16194	6574	1111	1022	7487	1264	12632780	**52.04**
Rhode Island	780	198	78	106	398	49	1056870	**18.73**
South Carolina	4780	2656	313	388	1423	407	4596958	**57.78**
South Dakota	147	18	24	27	78	107	820077	**2.19**
Tennessee	8309	4682	598	711	2318	457	6338112	**73.87**
Texas	32809	16280	2716	2745	11068	1020	25213445	**64.57**
Utah	1262	349	173	132	608	119	2830753	**12.33**
Vermont	54	13	14	7	20	67	622433	**2.09**
Virginia	5651	2955	365	552	1779	354	7952119	**37.16**
Washington	5906	1446	537	583	3340	254	6746199	**21.43**
West Virginia	235	66	32	34	103	127	1825513	**3.62**
Wisconsin	4453	2344	215	428	1466	342	5668519	**41.35**
Wyoming	76	25	10	8	33	65	547637	**4.57**

[1] The number of robberies from agencies that submitted 12 months of data in 2010 for which breakdowns by type of weapon were included.

[2] Limited data were received.

TOTAL US RATE PER 100,000 IS OFFICIAL FBI RATE

Table 21: Robbery by State, Types of Weapons, 2009

http://www2.fbi.gov/ucr/cius2009/data/table_21.html

State	Total robberies (1)	Firearms	Knives or cutting instruments	Other weapons	Strong-arm	Agency count	Population	Firearms robberies per 100,000 pop
TOTAL	**350,669**	**149,335**	**26,831**	**30,388**	**144,115**		**307,006,550**	**55.9**
Alabama	2,462	1,384	156	172	750	299	4,708,708	**29.4**
Alaska	652	169	45	56	382	34	698,473	**24.2**
Arizona	8,060	3,671	787	816	2,786	94	6,595,778	**55.7**
Arkansas	2,508	1,211	146	260	891	235	2,889,450	**41.9**
California	63,867	19,820	5,647	5,810	32,590	724	36,961,664	**53.6**
Colorado	3,317	1,190	332	459	1,336	216	5,024,748	**23.7**
Connecticut	3,990	1,445	393	299	1,853	103	3,518,288	**41.1**
Delaware	1,669	755	127	136	651	54	885,122	**85.3**
District of Columbia	4,389	1,860	221	218	2,090	2	599,657	**310.2**
Florida	30,881	13,668	1,938	2,732	12,543	593	18,537,969	**73.7**
Georgia	12,333	7,582	506	1,032	3,213	409	9,829,211	**77.1**
Hawaii	959	110	91	84	674	3	1,295,178	**8.5**
Idaho	243	101	15	33	94	105	1,545,801	**6.5**
Illinois (2)	596	262	40	57	237	1	#N/A	**#N/A**
Indiana	7,101	3,434	425	567	2,675	285	6,423,113	**53.5**
Iowa	1,178	322	114	107	635	193	3,007,856	**10.7**
Kansas	1,454	763	111	108	472	135	2,818,747	**27.1**
Kentucky	3,517	1,523	276	393	1,325	331	4,314,113	**35.3**
Louisiana	5,579	3,217	287	365	1,710	156	4,492,076	**71.6**
Maine	399	77	50	39	233	167	1,318,301	**5.8**
Maryland	8,257	3,810	746	420	3,281	152	5,699,478	**66.8**
Massachusetts	7,038	1,756	1,411	755	3,116	323	6,593,587	**26.6**
Michigan	12,280	6,148	607	1,046	4,479	583	9,969,727	**61.7**
Minnesota	3,574	1,120	237	574	1,643	304	5,266,214	**21.3**
Mississippi	2,303	1,329	129	238	607	114	2,951,996	**45.0**
Missouri	7,291	3,859	389	587	2,456	569	5,987,580	**64.5**
Montana	215	52	24	53	86	99	974,989	**5.3**
Nebraska	1,209	588	91	78	452	214	1,796,619	**32.7**

Nevada	5,996	2,286	518	501	2,691	38	2,643,085	**86.5**
New Hampshire	431	85	72	66	208	151	1,324,575	**6.4**
New Jersey	11,573	3,598	942	760	6,273	567	8,707,739	**41.3**
New Mexico	1,746	756	186	145	659	77	2,009,671	**37.6**
New York	9,410	2,797	1,062	1,075	4,476	542	19,541,453	**14.3**
North Carolina	11,165	6,130	735	916	3,384	360	9,380,884	**65.3**
North Dakota	104	24	15	17	48	84	646,844	**3.7**
Ohio	16,905	6,963	796	1,586	7,560	457	11,542,645	**60.3**
Oklahoma	3,320	1,580	248	242	1,250	305	3,687,050	**42.9**
Oregon	2,413	554	269	217	1,373	158	3,825,657	**14.5**
Pennsylvania	17,133	7,243	1,102	1,033	7,755	1,121	12,604,767	**57.5**
Rhode Island	786	229	107	99	351	49	1,053,209	**21.7**
South Carolina	5,482	3,058	382	476	1,566	427	4,561,242	**67.0**
South Dakota	107	24	17	8	58	116	812,383	**3.0**
Tennessee	9,594	5,692	609	738	2,555	450	6,296,254	**90.4**
Texas	37,955	19,036	3,020	3,106	12,793	1,008	24,782,302	**76.8**
Utah	1,295	413	142	128	612	122	2,784,572	**14.8**
Vermont	109	36	21	14	38	77	621,760	**5.8**
Virginia	5,848	3,107	407	613	1,721	396	7,882,590	**39.4**
Washington	6,423	1,713	504	591	3,615	237	6,664,195	**25.7**
West Virginia	644	188	75	95	286	222	1,819,777	**10.3**
Wisconsin	4,833	2,565	250	464	1,554	371	5,654,774	**45.4**
Wyoming	76	32	11	4	29	64	544,270	**5.9**

[1] The number of robberies from agencies that submitted 12 months of data in 2010 for which breakdowns by type of weapon were included.

[2] Limited data were received.

TOTAL US RATE PER 100,000 IS OFFICIAL FBI RATE

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Table 22: Aggravated Assault by State, Types of Weapons, 2010

http://www.fbi.gov/about-us/cjis/ucr/crime-in-the-u.s/2010/crime-in-the-u.s.-2010/tables/10tbl22.xls

State	Total aggravated assaults (1)	Firearms	Knives or cutting instruments	Other weapons	Personal weapons	Agency count	Population (census bureau)	Firearms assaults per 100,000 population
UNITED STATES	778901	138403	127857	222892	185029		309050816	44.78
Alabama	5700	1529	912	1273	1986	303	4729656	32.33
Alaska	3309	543	780	795	1191	35	708862	76.60
Arizona	15337	3618	2604	4577	4538	96	6676627	54.19
Arkansas	10044	2548	1522	2163	3811	199	2910236	87.55
California	95678	16937	15178	33074	30489	729	37266600	45.45
Colorado	9535	1936	2323	2659	2617	171	5095309	38.00
Connecticut	5703	792	1250	2026	1635	99	3526937	22.46
Delaware	3376	824	763	1400	389	53	891464	92.43
District of Columbia	3238	606	944	1233	455	1	610589	99.25
Florida	69482	13295	12385	25994	17808	663	18678049	71.18
Georgia	20287	5160	3580	5553	5994	458	9908357	52.08
Hawaii	1953	170	421	642	720	3	1300086	13.08
Idaho	2556	361	472	847	876	102	1559796	23.14
Illinois (2)	1646	805	219	311	311	1	#N/A	#N/A
Indiana	5496	514	662	1171	3149	286	6445295	7.97
Iowa	6010	566	1143	1369	2932	188	3023081	18.72
Kansas	7354	2016	1545	2274	1519	236	2841121	70.96
Kentucky	5056	1060	869	1976	1151	330	4339435	24.43
Louisiana	14895	3501	2409	3742	5243	172	4529426	77.29
Maine	794	48	157	235	354	167	1312939	3.66
Maryland	12754	1761	2872	4610	3511	153	5737274	30.69
Massachusetts	20904	2043	4770	10749	3342	320	6631280	30.81
Michigan	30673	8231	6005	10926	5511	515	9931235	82.88
Minnesota	6606	1058	1307	1876	2365	305	5290447	20.00
Mississippi	2910	888	521	781	720	121	2960467	30.00
Missouri	18676	5368	2407	4750	6151	578	6011741	89.29
Montana	1781	278	256	532	715	96	980152	28.36
Nebraska	3230	531	505	1378	816	211	1811072	29.32

Nevada	10336	1554	1893	5192	1697	40	2654751	**58.54**
New Hampshire	1220	202	401	328	289	150	1323531	**15.26**
New Jersey	13764	2101	3113	4320	4230	578	8732811	**24.06**
New Mexico	8578	1669	1474	2779	2656	88	2033875	**82.06**
New York	16331	2311	5182	4675	4163	533	19577730	**11.80**
North Carolina	19087	5677	3911	5270	4229	308	9458888	**60.02**
North Dakota	1134	21	105	228	780	87	653778	**3.21**
Ohio	14061	3511	2687	4463	3400	444	11532111	**30.45**
Oklahoma	12194	2347	2016	4416	3415	302	3724447	**63.02**
Oregon	5499	614	1033	1894	1958	140	3855536	**15.93**
Pennsylvania	25145	4984	3841	6324	9996	1264	12632780	**39.45**
Rhode Island	1596	302	426	676	192	49	1056870	**28.57**
South Carolina	20187	5274	3663	5614	5636	407	4596958	**114.73**
South Dakota	1098	144	378	353	223	107	820077	**17.56**
Tennessee	27640	8231	6137	10644	2628	457	6338112	**129.87**
Texas	71380	15544	15836	25244	14756	1020	25213445	**61.65**
Utah	3530	603	954	1148	825	119	2830753	**21.30**
Vermont	439	49	84	72	234	67	622433	**7.87**
Virginia	9472	1872	2189	3011	2400	354	7952119	**23.54**
Washington	12248	1678	2180	3824	4566	254	6746199	**24.87**
West Virginia	1712	339	269	402	702	127	1825513	**18.57**
Wisconsin	7962	1764	800	1564	3834	342	5668519	**31.12**
Wyoming	847	79	156	263	349	65	547637	**14.43**

1 The number of aggravated assaults from agencies that submitted 12 months of data in 2010
for which breakdowns by type of weapon were included.
2 Illinois: Limited data were received.
TOTAL US RATE PER 100,000 IS OFFICIAL FBI RATE

Table 22: Aggravated Assault by State, Types of Weapons, 2009

http://www2.fbi.gov/ucr/cius2009/data/table_22.html

State	Total aggravated assaults (1)	Firearms	Knives or cutting instruments	Other weapons	Personal weapons	Agency count	Population	Firearms rate per 100,000 pop
TOTAL US	**701,961**	**146,773**	**131,547**	**234,973**	**188,668**		**307,006,550**	**55.0**
Alabama	6,769	1,609	924	1,635	2,601	299	4,708,708	**34.2**
Alaska	3,194	540	704	855	1,095	34	698,473	**77.3**
Arizona	15,967	4,053	2,737	5,054	4,123	94	6,595,778	**61.4**
Arkansas	10,166	2,515	1,597	2,169	3,885	235	2,889,450	**87.0**
California	99,204	17,297	16,058	35,325	30,524	724	36,961,664	**46.8**
Colorado	10,857	2,059	2,326	3,011	3,461	216	5,024,748	**41.0**
Connecticut	5,760	772	1,215	2,079	1,694	103	3,518,288	**21.9**
Delaware	3,580	843	798	1,564	375	54	885,122	**95.2**
District of Columbia	3,388	728	953	1,256	451	2	599,657	**121.4**
Florida	76,023	15,015	13,439	29,167	18,402	593	18,537,969	**81.0**
Georgia	20,726	5,186	3,714	5,578	6,248	409	9,829,211	**52.8**
Hawaii	1,897	156	426	648	667	3	1,295,178	**12.0**
Idaho	2,695	401	469	1,032	793	105	1,545,801	**25.9**
Illinois (2)	1,332	624	249	319	140	1	#N/A	**#N/A**
Indiana	11,027	1,723	1,544	3,448	4,312	285	6,423,113	**26.8**
Iowa	5,978	508	1,044	1,306	3,120	193	3,007,856	**16.9**
Kansas	5,236	1,820	1,051	1,393	972	135	2,818,747	**64.6**
Kentucky	5,641	1,017	881	2,350	1,393	331	4,314,113	**23.6**
Louisiana	16,963	4,308	2,634	4,409	5,612	156	4,492,076	**95.9**
Maine	778	32	146	240	360	167	1,318,301	**2.4**
Maryland	14,343	1,838	3,178	4,986	4,341	152	5,699,478	**32.2**
Massachusetts	18,895	1,940	4,408	9,715	2,832	323	6,593,587	**29.4**
Michigan	31,748	8,251	5,964	11,390	6,143	583	9,969,727	**82.8**
Minnesota	7,138	1,175	1,420	2,037	2,506	304	5,266,214	**22.3**
Mississippi	2,873	822	520	840	691	114	2,951,996	**27.8**
Missouri	19,092	5,789	2,526	5,020	5,757	569	5,987,580	**96.7**
Montana	1,915	297	260	580	778	99	974,989	**30.5**
Nebraska	3,054	490	493	1,298	773	214	1,796,619	**27.3**

Nevada	11,255	1,707	2,009	5,680	1,859	38	2,643,085	**64.6**
New Hampshire	1,151	191	392	306	262	151	1,324,575	**14.4**
New Jersey	14,020	1,969	3,095	4,476	4,480	567	8,707,739	**22.6**
New Mexico	8,168	1,596	1,373	2,347	2,852	77	2,009,671	**79.4**
New York	16,801	2,276	4,995	4,859	4,671	542	19,541,453	**11.6**
North Carolina	21,025	6,110	4,288	5,816	4,811	360	9,380,884	**65.1**
North Dakota	931	12	79	151	689	84	646,844	**1.9**
Ohio	14,592	3,510	2,934	4,525	3,623	457	11,542,645	**30.4**
Oklahoma	12,744	2,449	2,098	4,583	3,614	305	3,687,050	**66.4**
Oregon	5,290	613	976	1,925	1,776	158	3,825,657	**16.0**
Pennsylvania	24,662	4,851	3,689	6,181	9,941	1,121	12,604,767	**38.5**
Rhode Island	1,556	320	418	605	213	49	1,053,209	**30.4**
South Carolina	21,682	5,685	3,908	5,988	6,101	427	4,561,242	**124.6**
South Dakota	865	109	307	264	185	116	812,383	**13.4**
Tennessee	29,390	9,154	6,018	11,015	3,203	450	6,296,254	**145.4**
Texas	73,823	17,516	16,393	26,622	13,292	1,008	24,782,302	**70.7**
Utah	3,648	537	1,039	1,199	873	122	2,784,572	**19.3**
Vermont	560	62	116	120	262	77	621,760	**10.0**
Virginia	9,187	1,819	2,128	3,127	2,113	396	7,882,590	**23.1**
Washington	11,971	1,719	2,023	3,843	4,386	237	6,664,195	**25.8**
West Virginia	3,239	762	598	849	1,030	222	1,819,777	**41.9**
Wisconsin	8,206	1,900	814	1,518	3,974	371	5,654,774	**33.6**
Wyoming	956	98	179	270	409	64	544,270	**18.0**

(1) The number of aggravated assaults from agencies that submitted 12 months of data in 2009 for which breakdowns by type of weapon included.
TOTAL US RATE PER 100,000 IS OFFICIAL FBI RATE

Murder Victims by Weapon, 2005–2010

For Years 2006-2010: http://www.fbi.gov/about-us/cjis/ucr/crime-in-the-u.s/2010/crime-in-the-u.s.-2010/tables/10shrtbl08.xls
For Year 2005: http://www2.fbi.gov/ucr/cius2009/offenses/expanded_information/data/shrtable_08.html

Weapons	2005	2006	2007	2008	2009	2010
Total	**14,965**	**15,087**	**14,916**	**14,224**	**13,752**	**12,996**
Total firearms:	10,158	10,225	10,129	9,528	9,199	8,775
Handguns	7,565	7,836	7,398	6,800	6,501	6,009
Rifles	445	438	453	380	351	358
Shotguns	522	490	457	442	423	373
Other guns	138	107	116	81	96	96
Firearms, type not stated	1,488	1,354	1,705	1,825	1,828	1,939
Knives or cutting instruments	1,920	1,830	1,817	1,888	1,836	1,704
Blunt objects (clubs, hammers, etc.)	608	618	647	603	623	540
Personal weapons (hands, fists, feet, etc.) (1)	905	841	869	875	817	745
Poison	9	12	10	9	7	11
Explosives	2	1	1	11	2	4
Fire	125	117	131	85	98	74
Narcotics	46	48	52	34	52	39
Drowning	20	12	12	16	8	10
Strangulation	118	137	134	89	122	122
Asphyxiation	96	106	109	87	84	98
Other weapons or weapons not stated	958	1,140	1,005	999	904	874

[1] Pushed is included in personal weapons.

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Weapons	2005	2006	2007	2008	2009	2010
Firearms	10,158	10,225	10,129	9,528	9,199	8,775
Knives or cutting instruments	1,920	1,830	1,817	1,888	1,836	1,704
Blunt objects (clubs, hammers, etc.)	608	618	647	603	623	540
Personal weapons (hands, fists, feet, etc.)1	905	841	869	875	817	745
Other	1,374	1,573	1,454	1,330	1,277	1,232

Geographic Area	Code	Population Estimates 7/1/2010	7/1/2009	Population Estimates 7/1/2008	7/1/2007	7/1/2006	7/1/2005	7/1/2004	7/1/2003	7/1/2002	7/1/2001	7/1/2000	4/1/2000 Estimates Base	Census
United States		309050816	307006550	304374846	301579895	298593212	295753151	293045739	290326418	287803914	285081556	282171957	281424602	281421906
Northeast		55417311	55283679	55060196	54879379	54710026	54598185	54514298	54364452	54167735	53930017	53667506	53594797	53594378
Midwest		66972887	66836911	66595597	66359247	66082058	65806421	65587713	65319024	65074729	64815413	64493956	64395207	64392776
South		114404435	113317879	112021022	110573419	108930843	107411036	105874018	104431612	103185017	101868637	100559939	100235848	100236820
West		72256183	71568081	70698031	69767850	68870285	67937509	67069710	66211330	65376433	64467489	63450556	63198750	63197932
Alabama	AL	4729656	4708708	4677464	4637904	4597688	4545049	4512190	4490591	4472420	4464034	4451849	4447355	4447100
Alaska	AK	708862	698473	688125	682297	677325	669488	661569	650884	642691	633316	627499	626931	626932
Arizona	AZ	6676627	6595778	6499377	6362241	6192100	5974834	5759425	5591206	5452108	5304417	5166697	5130607	5130632
Arkansas	AR	2910236	2889450	2867764	2842194	2815097	2776221	2746161	2722291	2704732	2691068	2678288	2673386	2673400
California	CA	37266600	36961664	36580371	36226122	35979208	35795255	35558419	35251107	34876194	34485623	33994571	33871650	33871648
Colorado	CO	5095309	5024748	4935213	4842259	4753044	4660780	4599681	4548775	4504265	4433068	4328070	4302015	4301261
Connecticut	CT	3526937	3518288	3502932	3488633	3485162	3477416	3474610	3467673	3448382	3428433	3411726	3405604	3405565
Delaware	DE	891464	885122	876211	864896	853022	839906	826639	814905	804131	794620	786411	783595	783600
District of Columbia	DC	610589	599657	590074	586409	583978	582049	579796	577777	579585	578042	571744	572053	572059
Florida	FL	18678049	18537969	18423878	18277888	18088505	17783868	17375259	16981183	16680309	16353869	16047118	15982813	15982378
Georgia	GA	9908357	9829211	9697838	9533761	9330086	9097428	8913676	8735259	8585535	8419594	8230161	8186812	8186453
Hawaii	HI	1300086	1295178	1287481	1276832	1275599	1266117	1252782	1239298	1228069	1218305	1211566	1211538	1211537
Idaho	ID	1559796	1545801	1527506	1499245	1464413	1425862	1391718	1364109	1342149	1321170	1299551	1293955	1293953
Illinois	IL	12944410	12910409	12842954	12779417	12718011	12674452	12645295	12597981	12558229	12507833	12437645	12419660	12419293
Indiana	IN	6445295	6423113	6388309	6346113	6301700	6253120	6214454	6181789	6149007	6124967	6091649	6080522	6080485
Iowa	IA	3023081	3007856	2993987	2978719	2964391	2949450	2941358	2932799	2929264	2929424	2928184	2926381	2926324
Kansas	KS	2841121	2818747	2797375	2775586	2755700	2741771	2730765	2721955	2712598	2701456	2692810	2688816	2688418
Kentucky	KY	4339435	4314113	4287931	4256278	4219374	4182293	4147970	4118627	4091330	4069191	4048903	4042284	4041769
Louisiana	LA	4529426	4492076	4451513	4376122	4240327	4497691	4489327	4474726	4466068	4460816	4468979	4468968	4468976
Maine	ME	1312939	1318301	1319691	1317308	1314963	1311631	1308253	1303102	1293938	1284791	1277211	1274922	1274923
Maryland	MD	5737274	5699478	5658655	5634242	5612196	5582520	5542659	5496708	5439913	5375033	5310579	5296516	5296486
Massachusetts	MA	6631280	6593587	6543595	6499275	6466399	6453031	6451279	6451637	6440978	6411730	6363015	6349113	6349097
Michigan	MI	9931235	9969727	10002486	10050847	10082438	10090554	10089305	10066351	10038767	10006093	9955308	9938492	9938444

Minnesota	MN	5290447	5266214	5230567	5191206	5148346	5106560	5079344	5047862	5017458	4982813	4933958	4919492	4919479
Mississippi	MS	2960467	2951996	2940212	2921723	2897150	2900116	2886006	2867678	2858643	2853313	2848310	2844666	2844658
Missouri	MO	6011741	5987580	5956335	5909824	5861572	5806639	5758444	5714847	5680852	5643986	5606065	5596678	5595211
Montana	MT	980152	974989	968035	957225	946230	934801	925887	916750	909868	905873	903293	902190	902195
Nebraska	NE	1811072	1796619	1781949	1769912	1760435	1751721	1742184	1733680	1725083	1717948	1713345	1711266	1711263
Nevada	NV	2654751	2643085	2615772	2567752	2493405	2408804	2328703	2236949	2166214	2094509	2018211	1998257	1998257
New Hampshire	NH	1323531	1324575	1321872	1317343	1311894	1301415	1292766	1281871	1271163	1256879	1240446	1235785	1235786
New Jersey	NJ	8732811	8707739	8663398	8636043	8623721	8621837	8611530	8583481	8544115	8489469	8430921	8414360	8414350
New Mexico	NM	2033875	2009671	1986763	1968731	1942608	1916538	1891829	1869683	1850035	1828809	1820813	1819041	1819046
New York	NY	19577730	19541453	19467789	19422777	19356564	19330891	19297933	19231101	19161873	19088978	18998044	18976816	18976457
North Carolin	NC	9458888	9380884	9247134	9064074	8866977	8669452	8531283	8416451	8316617	8203451	8079383	8046500	8049313
North Dakota	ND	653778	646844	641421	638202	636771	635365	636303	632809	633617	636267	641200	642195	642200
Ohio	OH	11532111	11542645	11528072	11520815	11492495	11475262	11464593	11445180	11420981	11396874	11363844	11353160	11353140
Oklahoma	OK	3724447	3687050	3644025	3612186	3574334	3532769	3514449	3498687	3484754	3464729	3453943	3450640	3450654
Oregon	OR	3855536	3825657	3782991	3732957	3677545	3617869	3573505	3550180	3517111	3470382	3430891	3421437	3421399
Pennsylvania	PA	12632780	12604767	12566368	12522531	12471142	12418161	12388368	12357524	12326302	12299533	12285504	12281052	12281054
Rhode Island	RI	1056870	1053209	1053502	1055009	1060196	1064989	1071414	1071504	1066034	1058051	1050736	1048319	1048319
South Carolin	SC	4596958	4561242	4503280	4424232	4339399	4256199	4201306	4146474	4103934	4062701	4023570	4011809	4012012
South Dakota	SD	820077	812383	804532	797035	788519	780084	774283	766975	762107	758983	755694	754837	754844
Tennessee	TN	6338112	6296254	6240456	6172862	6089453	5995748	5916762	5856522	5803306	5755443	5703243	5689270	5689283
Texas	TX	25213445	24782302	24304290	23837701	23369024	22801920	22418319	22057801	21710788	21332847	20945963	20851811	20851820
Utah	UT	2830753	2784572	2727343	2663796	2583724	2499637	2438915	2379938	2334473	2291250	2244314	2233204	2233169
Vermont	VT	622433	621760	621049	620460	619985	618814	618145	616559	614950	612153	609903	608826	608827
Virginia	VA	7952119	7882590	7795424	7719749	7646996	7563887	7468914	7373694	7283541	7191304	7104533	7079025	7078515
Washington	WA	6746199	6664195	6566073	6464979	6372243	6261282	6184289	6113262	6056187	5987785	5911122	5894143	5894121
West Virginia	WV	1825513	1819777	1814873	1811198	1807237	1803920	1803302	1802238	1799411	1798582	1806962	1808345	1808344
Wisconsin	WI	5668519	5654774	5627610	5601571	5571680	5541443	5511385	5476796	5446766	5408769	5374254	5363708	5363675
Wyoming	WY	547637	544270	532981	523414	512841	506242	502988	499189	497069	492982	493958	493782	493782
Puerto Rico		3791913	3967288	3954553	3941235	3926744	3910722	3893931	3876637	3858272	3837768	3814413	3808603	3808610

Source: Population Division, U.S. Census Bureau
http://www.census.gov/popest/data/intercensal/national/nat2010.html

Supp. App. 429

Trust But Verify:

Lessons for the Empirical Evaluation of Law and Policy

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Introduction

Economics and criminology cannot be a legitimate science unless published results can be easily replicated by others. The data and programs must be made readily available to interested parties. The data and programs should be checked before posting so that researchers who download the material can conveniently estimate the published regressions and generate the same results as those in the published article, including claims made in the text and footnotes. The data and programs should be publically available no later than the date of acceptance of the article in a journal, or the date of posting of the article on a working paper site such as Research Paper in Economics (repec.org), NBER (www.nber.org), Social Science Research Network (www.ssrn.com) or departmental working paper site.

As a case in point, in a recent article, Aneja, Donohue, and Zhang (2011), hereafter ADZ, reviewed the National Research Council (2005), hereafter NRC, review of the effect of right-to-carry laws, also known as shall-issue laws, on crime. These laws require that permits be issued to all persons who meet certain legislated requirements. Aside from Illinois, the one state that still bans concealed carry, states that have not passed right-to-carry laws leave it up to the issuing authorities, typically local police or sheriff departments, to determine whether or not to grant the applicant a concealed weapons permit. Such states are known as "may issue" states. It is the usual case that may-issue states, especially in urban cities and counties, issue very few concealed carry permits, typically limited to celebrities, wealthy individuals, and politicians. An interesting policy question is whether shall-issue laws, which increase the number of concealed carry permits, increase or decrease crime. The theory is that criminals, knowing that ordinary citizens

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might be carrying firearms, but being unable to discern who is and who isn't, will tend to

forswear a violent attack for fear of being shot. Under this theory, crime should go down.

The original article in this area is Lott and Mustard (1997) which found that states with shall-

issue laws had significantly lower violent crime rates than may issue states or states that ban

concealed carry. The publication of the Lott and Mustard article generated a controversy that, as

illustrated by the publication of the ADZ article, continues to this day. The Lott and Mustard

results have been tested many times. There have been a total of 29 peer reviewed studies by

economists and criminologists, 18 supporting the hypothesis that shall-issue laws reduce crime,

10 not finding any significant effect on crime, including the NRC report[1], and ADZ's paper,

using a different model and different data, finding that right-to-carry laws increase one type of

violent crime, aggravated assault.

In their article, ADZ make a strong point that data and methodological flaws can produce

inaccurate conclusions. They attempt to replicate the results of the NRC report with a data set

purportedly used by the NRC. Their attempts at replication fail. "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….Once again, our … estimates diverge wildly from the …

estimates which appeared in the NRC report." (ADZ p. 583) Their conclusion is unambiguous.

> 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

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[1] Although one member of the Council concluded that the NRC's own results indicated that shall-issue laws reduced murder.

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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… 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)…. 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. (ADZ, 613-4)

We enthusiastically agree with the thoughts expressed in the first sentence and in the last two

sentences. However, we have to point out that ADZ did not release their data and programs until

well after the article had been published. They also fail the basic requirement that data and

programs be easily downloadable because nowhere in their paper do they inform the reader

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where to acquire the data and programs. We also note that the NRC did not make their data and

programs readily available to other researchers.

 ADZ are claiming that the data set that John Lott provided to the NRC, which was the same data

set that he sent to over 120 researchers around the world, including one of the authors of this

paper, was flawed and therefore generated flawed results. The implication is that the 18 articles

that used that data to confirm the Lott and Mustard hypothesis that shall-issue laws reduce crime,

or the 10 articles that found no effect after altering the specification of the model, are irrelevant

because their results are based on bad data. If this is true, then it does call into question all

preceding studies based on the Lott data set. However, as we shall see, it is not true.

Failure to replicate published regression results can be due to data errors or program errors. ADZ

were able to get what they believed to be the NRC data but they were not able to get the

programs. This means that the fact that ADZ, "…cannot replicate the NRC results using the

NRC's own data set…. (p. 583) could be due to a mistake in the programming or the wrong data

set or errors in the data set. It is not obvious that it has to be a problem with the data.

Background

Lott and Mustard's study appeared in 1997. The results that became the focus of the ongoing

debate were based on a data set consisting of county data for the years 1977-1992 collected by

the authors from a variety of sources. This original data set was lost in a hard disk crash. It was

reconstructed from the original sources. The resulting "reconstructed" data set was the one

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provided to researchers by Lott. It is also the data set provided to the NRC by Lott. The NRC

refers to this data set as the "reconstructed Lott 1992" data set, though it more accurately should

have been referred to as the Lott-Mustard 1992 data set, since David Mustard did most of the

data collection. Lott then published a book entitled *More Guns, Less Crime* (Lott 1998) using

county level data from 1997 to 1994. In the second edition of the book (Lott 2000), Lott

expanded the county data through 1996 and provided city level data. David Mustard again

helped put these additional years of data together. The NRC also used a revised and extended

county data set for the years 1977-2000, again provided to the NRC by Lott, which is referred to

in the NRC report as the "revised Lott 2000" data set. John Whitley and Florenz Plassmann

supplied the data to Lott for the additional years from 1997 to 2000 and updated some data after

the census department corrected its earlier estimates following the 2000 census.

When the original Lott and Mustard (1997) article appeared, two of the authors of this paper

(Moody and Marvell) were skeptical of the results and sent Lott an email requesting the data set

for replication purposes. Lott sent both the data set and the STATA programs ("do files") used to

generate the results in the 1997 article. Moody was able to replicate the Lott and Mustard results.

It turns out that Moody still has the data set and do files from 1997 on the server. We used the

data set that Lott sent Moody in 1997 to generate the MLMZ (Moody, Lott, Marvell,

Zimmerman) results in Tables 1 and 2. Lott also made the "revised Lott 2000" data set available

to Moody and Marvell and many other researchers. This data set is also still on Moody's server

with the name Lott6.dta and a 2003 date.

Lott had also provided these data sets and "do files" to both the NRC and Donohue. ADZ make

much of the fact that the NRC's "…do files" for their tables had not been retained" (p. 580).

However, ADZ could have had the correct NRC data set and the correct do-files, if they had

simply asked John Pepper, one of the editors. Also, since the NRC was using Lott's data and "do

files" the very same data and programs that had been shared with Donohue, and the model

specification was published in Lott and Mustard (1997) and Lott (1998, 2000), it is strange that

ADZ claim not to be able to figure out which regressions the NRC estimated. It is also true that

these problems could have been avoided if the NRC had made its data and do files more readily

available to other researchers.

Replication achieved[2]

NRC Table 6-1: the dummy variable model

In the first column of Table 1 we show the original results from the Lott and Mustard (1997)

article (Table 3, top line, p. 20). These results were based on the original data set before the hard

disk crash.[3] The entries present the estimated coefficients on the shall-issue dummy variable. In

the second column we reproduce the NRC committee's replication of the same model based on

the reconstructed Lott 1992 data set. The estimates are exactly the same except for the coefficient

on murder which was reported as -.076 by Lott and Mustard and as -.073 by the NRC. Thus, it

would appear that the data set used by the NRC was virtually identical to that originally used by

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[2] All of the data and programs used to generate the results presented in this paper can be downloaded from
http://cemood.people.wm.edu/NRC.replicate.zip.
[3] Lott suffered a hard disk crash on July 3, 1997 where he lost the original data used in the paper with David
Mustard. Lott and Mustard then reconstructed the data set. They had given out the original data set to critics such as
Dan Black, Dan Nagin, and Jens Ludwig. These critics would not return a copy of the data set to Lott after the crash.
This forced Lott and Mustard to put the data together again from scratch so that it could be supplied to other
researchers such as the NRC.

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Lott and Mustard, even after being reconstructed. In the third column ("MLMZ") we replicate

the Lott and Mustard results using the reconstructed Lott 1992 data set sent by Lott to Moody in

1997. The results are identical to those of the NRC, indicating that the data set sent to Moody in

1997 is the same as the data set provided to the NRC.

Table 1: Replications of NRC Table 6-1
Coefficients on the Shall-Issue Dummy Variable

	Lott & Mustard	NRC	MLMZ	ADZ
	1	2	3	4
Violent	**-.049**	**-.049**	**-.049**	**-.044**
Murder	**-.076**	**-.073**	**-.073**	**-.064**
Rape	**-.053**	**-.053**	**-.053**	**-.061**
Robbery	-.022	-.022	-.022	-.007
Assault	**-.070**	**-.070**	**-.070**	**-.057**
Property	**.027**	**.027**	**.027**	**.032**
Burglary	.0005	.0005	-.0005	-.001
Larceny	**.033**	**.033**	**.033**	**.050**
Auto Theft	**.071**	**.071**	**.071**	**.098**

Notes: Column 1 reports the original results from Lott and Mustard (1997, Table 3, p. 20). Column 2 reports the NRC results from Table 6-1, line 2, pp. 128-9, "Committee Replication Revised 1992"). Column 3 reports the MLMZ results estimating the original Lott and Mustard specification on the reconstructed Lott 1992 data set sent to Moody in 1997. Column 4 reports the results of estimating the model on the new ADZ data for the years 1977-1992, using the original LM specification including arrest rates for each crime and the LM shall issue dummy, both merged into the ADZ data set from the reconstructed Lott 1992 data set. Bold indicates significance at the .05 level, two-tailed. Data and programs used to generate all tables are available at http://cemood.people.wm.edu/NRC.replicate.zip.

In the fourth column ("ADZ") we estimate the dummy variable model on the new ADZ data set,

using the Lott-Mustard specification including the Lott-Mustard shall-issue dummy, the arrest

rate for the crime in question, and all 36 demographic variables. Since the ADZ data set included

only the arrest rate for violent and property crimes, we merged the arrest rates for the individual

crimes from the Lott 1992 data set. We also merged the Lott-Mustard shall-issue law dummy.

The results are very similar, indicating that the new ADZ data set is not much different from the

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original Lott-Mustard data set for the years 1977-92. It would be very unusual if the data sets were identical because government agencies frequently revise previously published data.

NRC Table 6-2: Before and after trend model

Lott hypothesizes that the dummy variable model could be misleading if the crime rate is increasing before the law and decreasing after, sometimes referred to as the "inverted-V" hypothesis. In this case, if the "inverted-V" is symmetrical a dummy variable, estimated the difference in average crime rates before and after the law, would be zero. Similarly, if crime rates were rising quickly before the law and falling afterwards, a simple dummy variable could be positive or negative since the average crime rate after the law could still be higher than the average before the law, despite the fact that the crime rates are falling. The dummy variable can be misleading in other ways. To examine this point, Lott estimated trend models for each of the FBI index crimes and published the results (after coefficient minus before coefficient) in Table 4.8 of *More Guns, Less Crime* (2000, p 76). Table 2 reports one replication of the before-and-after trend model. This model simply replaces the shall-issue dummy variable with the before and after trend variables. The model includes the arrest rate for the crime in question and all 36 demographic variables. The NRC committee attempted to replicate the trend model in two ways: using the Lott 1992 data set and the Lott 2000 data set, restricted to 1977-1992. The results from Lott's Table 4.8 are reproduced in the first column of Table 2 ("Lott"). The NRC's attempt to replicate the results is presented in the second column ("NRC-1": from NRC 2004, Table 6-2, line 2). The results are slightly different from Lott's, but generally in agreement. In the third column ("MLMZ-1") we present the results of our replication attempt using the Lott 1992 data

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set. The results are identical, except for a slight discrepancy in the burglary estimate. This

confirms that the data set sent to the committee was indeed the same as that Lott sent to other

researchers.

Table 2: Replications of NRC Table 6-2
Difference Between the After and Before Trends

	Lott (2000)	NRC-1	MLMZ-1	NRC-2	MLMZ-2	ADZ
	1	2	3	4	5	6
Violent	**-.009**	-.005	-.005	**-.0215**	**-.0215**	**-.0067**
Murder	**-.030**	**-.0425**	**-.0425**	**-.0341**	**-.0341**	**-.0203**
Rape	**-.014**	**-.0137**	**-.0137**	**-.0337**	**-.0337**	**-.0365**
Robbery	**-.027**	**-.0272**	**-.0272**	**-.0302**	**-.0302**	**-.0267**
Assault	**-.0046**	.0046	.0046	**-.0263**	**-.0263**	.0059
Property	**-.006**	**-.0069**	**-.0069**	**-.0113**	**-.0113**	**-.0142**
Burglary	**-.003**	**-.0158**	**-.0163**	**-.0180**	**-.0180**	**-.0180**
Larceny	**-.015**	-.0011	-.0011	**-.0084**	**-.0084**	.0136
Auto Theft	-.001	-.0031	-.0031	.0025	.0042	**-.0167**

Notes: Column 1 reports the Lott results from NRC (2004, Table 6-2, line 0, pp.128-9) which
reproduce the results reported in Table 4.8 in Lott (2000, p. 76). Column 2 is from NRC (2004,
Table 6-2, line 2, pp.128-9, "Committee Replication Revised 1992"). The committee used the
original Lott and Mustard specification (including all 36 population-age-gender variables and the
arrest rate for the crime in question) on the reconstructed Lott 1992 data set. Column 3 reports
the MLMZ results from estimating the original Lott and Mustard specification on the
reconstructed data set sent to Moody in 1997. Column 4 reports the results from NRC (2004,
Table 6-2, line 3, pp. 128-9) where the trend model is estimated on Lott's "revised 2000" data
set, constrained to the years 1977-1992. It uses the original Lott and Mustard specification.
Column 5 reports our replication using the "revised 2000" data set. Column 6 reports the results
of our estimation of the Lott and Mustard trend model using the new ADZ data set. Bold
indicates significant at the .05 level, two-tailed. Data and programs used to generate all tables are
available http://cemood.people.wm.edu/NRC.replicate.zip.

Correcting the ADZ replications

ADZ did not report any attempts to replicate the NRC tables 6-1 and 6-2. However, they do report attempts to replicate parts of NRC tables 6-5 and 6-6. In ADZ Table 1b they report that they cannot replicate row 3 from NRC Table 6-5 (dummy variable model with no covariates) and Table 6-6 (trend model with no covariates). Using data and programs provided by John Pepper, we were able to replicate those results exactly, as demonstrated in Tables 3 and 4.

Table 3: Re-estimation of ADZ Table 1b

Crime	NRC Dummy Model No covariates	MLMZ Replication	NRC Trend Model No Covariates	MLMZ Replication
	1	2	3	4
Violent	**0.1292**	**0.1292**	**-0.0062**	**-0.0062**
Murder	-0.0195	-0.0012	-0.0122	-0.0122
Rape	**0.1791**	**0.1791**	**-0.0217**	**-0.0217**
Robbery	**0.1999**	**0.1999**	**-0.0088**	**-0.0088**
Assault	**0.1234**	**0.1234**	**-0.0065**	**-0.0065**
Property	**0.2124**	**0.2124**	**-0.0081**	**-0.0081**
Burglary	**0.1906**	**0.1906**	**-0.0199**	**-0.0199**
Larceny	**0.2258**	**0.2258**	**-0.0071**	**-0.0071**
Auto	**0.2333**	**0.2333**	**0.0057**	**0.0057**

Notes: Columns 1 and 2 report estimates on the shall-issue dummy variable. Column 1 reports NRC Table 6-5 row 3. Columns 3 and 4 report estimates on the post-law trend variable. Column 3 reports NRC Table 6-6 row 3. Data from 1977-2000. Bold indicates significant at the .05 level, two-tailed. Data and programs used to generate all tables are available at http://cemood.people.wm.edu/NRC.replicate.zip.

Table 4: Re-estimation of ADZ Table 2b

Difference Between After and Before Trends

Crime	NRC Dummy Model with Covariates	MLMZ Replication	NRC Trend Model with Covariates	MLMZ Replication
	1	2	3	4
Violent	**0.0412**	**0.0412**	**-0.0095**	**-0.0095**
Murder	**-0.0833**	**-0.0833**	**-0.0203**	**-0.0203**
Rape	-0.0016	-0.0016	**-0.0281**	**-0.0281**
Robbery	**0.0359**	**0.0359**	**-0.0258**	**-0.0258**
Assault	**0.0305**	**0.0305**	**-0.0192**	**-0.0192**
Property	**0.1148**	**0.1148**	-0.0001	-0.0001
Burglary	**0.0619**	**0.0619**	**-0.0213**	**-0.0213**
Larceny	**0.1240**	**0.1240**	**-0.0073**	**-0.0073**
Auto	**0.1274**	**0.1274**	**-0.0049**	**-0.0049**

Notes: Columns 1 and 2 report estimates of the coefficient on the shall-issue dummy variable. Column 1 reports NRC Table 6-5 row 1. Columns 3 and 4 report estimates of the coefficient on the post-law trend variable. Column 3 reports NRC Table 6-6 row 1. Bold indicates significant at the .05 level two-tailed. Data and programs used to generate all tables are available at http://cemood.people.wm.edu/NRC.replicate.zip.

The difference between the ADZ and NRC results is not a data problem. The problem is that

ADZ altered the regression model. As a result, their specifications are different from all those

who have attempted to replicate Lott's work and different from the NRC. For the murder, rape,

robbery, and assault regressions, ADZ did not use the arrest rate for the crime specific arrest rate,

but instead used the arrest rate for all violent crime. For those specifications involving burglary,

larceny, and auto theft, ADZ used the arrest rate for all property crime. ADZ ignored the

"truncation bias" that they are introducing into the estimates by making this seemingly innocent

change in the model specification. The truncation bias occurs in county-level data because in

some years many counties do not experience certain types of crimes at all—80 percent have no

murders for instance. If the murder rate in a county is zero before the law goes into effect, simple

randomness means that sometimes the crime rate will go up, but no matter how effective the law

is, the reverse cannot happen because crime rates cannot fall below zero. Using the arrest rate for

murder, which is usually missing or zero in counties with zero murders, drops those counties out

of the regression and allows murder to both increase and decrease in the remaining counties.

However, when the arrest rate for violent crime is used, counties with zero murders are kept in

the murder regression, truncating the dependent variable at zero. An entire literature has emerged

within the debate on right-to-carry laws that has dealt with this issue using Tobit, negative

binomial, or other limited dependent variable methods (e.g. Lott 1998 pp. 390 n. 8, 399 n. 19;

Lott 2000 pp. 285, 288; Plassmann and Tideman 2001; Plassmann and Whitley 2003 pp. 1354-

57). Unfortunately, ADZ ignore this literature and they write as if they are the first to discuss the

possible simultaneity between crime rates and arrest rates (see e.g., Bronars and Lott, 1998 and

Lott, 2000).

Not all the differences in the results were due to ADZ changing the specification. The ADZ data

set includes 100-140 more counties each year than the 3120-3140 counties in the Lott 1992 data

set. Some of these counties were omitted from the Lott 1992 data set because of data problems

documented by the FBI. However, when we dropped these problematic counties from the ADZ

data set and re-estimated, the results were virtually unchanged.

There are some errors in the data set that ADZ produced. For example,

- The observations for county 2060 in Alaska are repeated 73 times for 1996. .

- The first full year of the shall-issue law for Kansas is coded as 1996 when in fact the law
  was not passed until 2006.

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- The first full year of the shall-issue law for Florida is coded as 1989 when in fact the law was passed in 1987.

- The first full year of the shall-issue law for South Dakota is coded as 1987, however the law was passed in 1985.[4]

As can be seen from the last three columns in Table 1, it is the change in the specification, not these errors in the ADZ data set that prevented them from replicating the NRC estimates. The puzzle here is why ADZ chose to alter the specification without stating they had been that. In previous work Donohue used the correct specifications and was able to replicate Lott's results.

The NRC and ADZ were selective over what regressions to replicate. Many of the points ADZ raise have already been dealt with extensively with in other specifications that were not replicated by the NRC. Such specifications included the impact of cocaine in four different ways; tens of thousands of regressions using different combinations of control variables (including removing demographic variables); data on the number of police, per capita expenditures on police and various policing strategies; state level unemployment and poverty as well as county level measures; the large differences in state right-to-carry laws; and city, county and state level crime data (Bartley and Cohen, 1998; Lott, 2000, 2010; Moody, 2001).

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[4] These coding errors assume that ADZ are correct about when other states adopted right-to-carry laws. For example Lott and Mustard (1997) followed Cramer and Kopel (1995) and assumed that North and South Dakota adopted right-to-carry laws prior to 1977.

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Conclusion

ADZ couldn't replicate the NRC results with the NRC data. They jumped to the conclusion that

it was due to bad data from Lott. We now know that the data that Lott provided to the NRC was

the same as that provided to hundreds of other researchers. Using Lott's data, we could we

replicate the NRC results for both the dummy variable and trend model, corresponding to the

NRC Tables 6-1 and 6-2 and, since the NRC was also able to replicate the original Lott and

Mustard results, the NRC must have been using the same data. We were also able to replicate

the NRC "no covariate" model for the 1977-2000 sample. We find it hard to believe that ADZ

couldn't replicate those results with the NRC data. Researchers cannot be held responsible for

errors committed by others who request their data.

The fact that ADZ did not try to contact Lott in an attempt to understand the source of their

replication problems is curious. These issues could have been resolve with an email or phone

call. ADZ are practicing one-upmanship when they should be practicing science.

We now know that the data provided to the NRC was not tainted with errors. Therefore James Q.

Wilson was perfectly justified to conclude that right-to-carry laws reduced murders, since the

NRC's own regressions, based on good data provided by Lott, showed a significant negative

effect on crime. Also, all the peer-reviewed studies that are based on Lott's data that find that

shall-issue laws reduce violent crime, or at least do not increase violent crime, are not tainted by

errors. The record still stands at 18-1-10.

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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.   *

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

**REPLY IN SUPPORT OF DEFENDANTS' MOTION
FOR STAY PENDING APPEAL**

INTRODUCTION

This case presents a question of first impression about the constitutionality of a provision of Maryland law that the General Assembly believed necessary to protect public safety and that, according to Maryland law enforcement officials, does protect public safety. Not quite four years ago, the Supreme Court reset the Second Amendment landscape in a decision that left many open questions without offering much guidance to help lower courts resolve them. The Fourth Circuit, recognizing the potentially devastating consequences of miscalculating as to Second Amendment rights, has cautioned lower courts to tread lightly until questions about those rights are resolved by higher courts. This caution is particularly warranted here, where this Court has reached a different conclusion than the majority of courts to have addressed the questions presented in this case. As detailed in the defendants' prior briefing, these factors favor granting a

stay to preserve the status quo pending resolution by the Fourth Circuit.[1] The plaintiffs'

Supplemental Brief in Opposition to Defendants' Motion for Stay Pending Appeal (ECF

No. 69) fails to undermine any of these fundamental reasons why a stay is appropriate.

I.   THE COURT SHOULD STAY THE ENTIRE INJUNCTION.

The plaintiffs misread the first footnote in the defendants' opening supplemental

brief (ECF No. 68) to be a concession that the Court lacks discretion to stay its injunctive

relief as to Mr. Woollard. ECF No. 69, at 1-2. To the contrary, the Court clearly has the

discretion to, and should, stay the effect of both paragraphs of the injunctive relief it

awarded. *See* Fed. R. Civ. P. 62(c). The sole point made in the first footnote of the

defendants' opening supplemental brief is that the Court's options in resolving the motion

for stay pending appeal are not limited to staying either (i) both paragraphs of its

injunction, or (ii) neither paragraph. In footnote 1, the defendants were responding to a

question posed by the Court about the range of relief that could be entered by noting that

if the Court were to conclude that a stay is merited as to the broader relief awarded but

not as to Mr. Woollard himself, the Court could enter a stay consistent with that

conclusion. *Id.* ("[T]he court may suspend, modify, restore, or grant an injunction . . .").

II.   THE PLAINTIFFS FAIL TO APPRECIATE THE IMPACT OF AN ABSENCE OF A STAY.

In arguing that the Maryland State Police ("MSP") could "easily" revoke permits

in the event of a reversal by the Fourth Circuit, the plaintiffs demonstrate a lack of

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[1] The defendants' opening brief is due on June 15, 2012, and the current schedule will have all briefing completed by August 2, 2012, more than six weeks before the Fourth Circuit's next scheduled sitting for oral argument, which is September 18-21, 2012.

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understanding of the realities of law enforcement.   In their supplemental brief, the

defendants discussed the process that the MSP would follow in an attempt to minimize,

to the greatest extent possible, the negative impact of the absence of a stay. ECF No. 68,

at 2-5 & Ex. A, Declaration of Marcus Brown ("Brown Decl.").   The plaintiffs

erroneously infer from this discussion that the MSP "could easily—instantly—return to

the status quo ante, by revoking the permits of anyone who did not establish 'GSR.'"

ECF No. 69, at 4.   The plaintiffs' conclusion is entirely unwarranted.   Attempting to

revoke a large number of permits would be a messy, difficult process with negative

effects, especially for individuals who have good and substantial reason under the

existing law. *See generally*, ECF No. 68, at 2-5; ECF No. 68-1, Brown Decl. ¶¶ 5-15.

Moreover, the plaintiffs' attempt to play down the effect of the absence of a stay

on the administration of Maryland's handgun permit law by reference to the experience

of states that have legislatively adopted "shall issue" laws is misplaced.   It would be

expected that in adopting such "shall issue" laws the legislatures in those states provided

sufficient time and resources to manage any transition. A judicially-imposed transition

would not be accompanied by such advanced planning or provision of resources to

manage any increased influx of applications. As described in the opening supplemental

memorandum, that would likely have an adverse effect on those applicants who, under

existing law, have a demonstrable need to wear and carry a handgun. ECF No. 68, at 4-5.

III.   **CRIME STATISTICS SUPPORT A STAY.**

The plaintiffs make three points about crime rates that they contend counsel against entry of a stay. Two of these points actually favor granting a stay, and the third is simply inaccurate.

A.   Maryland's Violent Crime Problem Favors Granting a Stay.

The plaintiffs note that Maryland has a crime problem that is worse than crime problems in some states with more lenient handgun carry laws. Plaintiffs' Supplemental Brief (ECF No. 69) at 5-6. Maryland's crime problem is not new information in this case, of course, as it was highlighted by the defendants on summary judgment. *See* ECF No. 26, at 10-14. In fact, a critical purpose of Maryland's handgun wear and carry permit statute, including the good and substantial reason requirement, is to address Maryland's crime problem. *Id.* at 10-16.

Although the plaintiffs acknowledge that "[c]orrelation may not be causation," they nonetheless cite correlation statistics purportedly showing that jurisdictions with more permissive handgun carry laws have, in the aggregate, fewer firearm murders and robberies—but more firearm assaults—than states with less permissive carry laws. ECF No. 69, at 5-6. The plaintiffs' correlation evidence is severely flawed.

First, the plaintiffs' calculations accord equal weight to the crime rates in California, with its 37 million people, and Washington, D.C., with its 600,000 people. The impact of doing so is greatly exacerbated by the fact that Washington, D.C. is a statistical anomaly, with more than double the rate of firearms murders and robberies of

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any state. *See* Plaintiffs' Ex. A (ECF No. 69-1), at 1-2. Just removing the anomalous

Washington, D.C. from the plaintiffs' calculations paints a much different picture:

2.72 firearm murders, 28.14 firearm robberies, and 23.14 firearm assaults in the seven

"may- or no-issue" states, versus the United States averages of 2.84 firearm murders,

42 firearm robberies, and 45 firearm assaults per 100,000 people. *See id.*

Second, even if the plaintiffs' statistics were not so heavily skewed by a single,

anomalous jurisdiction, their simplistic comparison ignores the many variables that affect

crime statistics, including the fact that the states with less permissive gun laws are

generally much more concentrated with urban, densely-populated areas.[2]   Such areas

present public safety challenges that affect both crime rates and the public safety risks

associated with different types of handgun permit legislation. The plaintiffs' simplistic

comparison of these groups of states without recognizing these or any other variables is

inappropriate.

As of 2010, Maryland's level of violent crime was the lowest ever recorded for

both overall violent crime rate and homicide rate. *See* Maryland Governor's Office of

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[2] The divide between "shall issue" and "may-issue" states based on the number of
densely populated areas is stark. Of the 87 geographical areas within the 50 United
States and the District of Columbia the U.S. Census Bureau identifies as having a
population density exceeding 7,500 people per square mile, 72 of those areas—more than
82%—are in one of the eight jurisdictions the plaintiffs identify as having "may- or no-
issue laws." *See* ECF No. 68, at 6 n.4 (identifying California, D.C., Hawaii, Illinois,
Maryland, Massachusetts, New York, and New Jersey); U.S. Census Bureau, *Population,
Housing Units, Area, and Density: 2010 – United States -- Places with 50,000 or More
Population by State; and for Puerto Rico (2010)*, U.S. Census Bureau (available at
http://factfinder2.census.gov/faces/tableservices/jsf/pages/productview.xhtml?pid=DEC_10_SF1_GCTPH1.US14PR
&prodType=table) (last visited May 23, 2012). By comparison, the remaining 43 states have
only 15 such areas among them. *Id.* That is the same number as New Jersey has by
itself, and fewer than half the number California has. *Id.*

Crime   Control   &   Prevention,   Maryland   2010   Crime   Totals,   *available   at*
http://www.goccp.maryland.gov/msac/crime-statistics.php (last   visited   May   23,   2012).
The role of the good and substantial reason requirement in Maryland's ongoing effort to
reduce that level of violent crime, as attested by Maryland law enforcement officials,
favors granting a stay.

B.   The Plaintiffs' Claims About Defensive Gun Use Have Been Disproven.

The plaintiffs rely on a 1995 article by Gary Kleck and Marc Gertz to support their
claim that defensive gun use is very common, and likely even more common than
criminal gun use.   ECF No. 69, at 6-7.   However, the basis for Kleck and Gertz's
conclusion has long since been refuted.   *See* David Hemenway, *Policy & Perspective:
Survey Research & Self-Defense Gun Use: An Explanation of Extreme Overestimates*, 87
J. CRIM. L. & CRIMINOLOGY 1430 (1997) (the extreme overestimate of the frequency of
defensive gun use comes from the study's reliance on self-reporting of a rare event).
Studies using a consistent methodology to estimate both instances of criminal gun use
and of defensive gun use demonstrate that "guns in the United States are used far more in
crime than in self-defense."   Ex. 1, David Hemenway and Mary Vriniotis, *Comparing the
Incidence of Self-Defense Gun Use and Criminal Gun Use*, Bulletins, at 2-3 (Spring
2009) (claim that defensive gun use is more common than criminal gun use is based on
comparing results of "two radically different survey methodologies").

C.   The Parties Agree That It Is Not the Court's Job to Evaluate the Costs and Benefits of Firearms Restrictions.

The plaintiffs correctly observe that there have been numerous published studies reaching different conclusions as to the impact of shall-issue carry laws on crime rates. All studies of the impact of "shall-issue" carry laws on crime rates are not created equal,[3] but the defendants agree with the plaintiffs that it is not the responsibility of the courts to resolve a dispute about the benefits and costs of different types of firearms restrictions. *See* ECF No. 69, at 7. The legislative, not the judicial, branch is generally entrusted with such judgments. *See, e.g., Turner Broad. Sys., Inc. v. FCC*, 520 U.S. 180, 195 (1997) (legislative branch is "far better equipped than the judiciary to 'amass and evaluate the vast amounts of data' bearing upon legislative questions") (internal quotation omitted).

The question currently before the Court, however, is whether to enter a stay. The importance of the challenged provision to maintaining public safety is confirmed in legislative findings and by the testimony of Maryland law enforcement officials, and the best available evidence indicates that "shall issue" carry laws likely have an adverse impact on at least some crime, ECF No. 68, at 7. Other legislatures, law enforcement officials, and authors have reached different conclusions, but the existence of a debate

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[3] Contrary to their claim, the unpublished document the plaintiffs attach as Exhibit B to their supplemental brief (ECF No. 69-2) hardly "debunk[s]" the article from Aneja, Donohue, and Zhang ("ADZ") attached as Exhibit D to the defendants' opening supplemental brief (ECF No. 68-4). ECF No. 69, at 7. Although the plaintiffs' Exhibit B attempts to defend against some of the criticisms made in the ADZ article, it fails even to address most of the criticisms and fails to undermine the fundamental conclusions of the ADZ article about the flaws in studies purporting to show that "shall issue" laws decrease crime and the results of the ADZ analysis after correcting and updating the data. *See* ECF No. 68, at 5-7.

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does not require all states to adopt shall-issue laws. *Cf. McDonald v. City of Chicago*, 130 S. Ct. 3020, 3046 (2010) ("'[s]tate and local experimentation with reasonable firearms regulations will continue under the Second Amendment.'") (quoting with approval Brief of State of Texas, *et al.* as *Amici Curiae*, at 23). In light of the novel legal issues involved, the substantial implications for public safety if Maryland's legislature and officials are correct, and the limited burden on the plaintiffs pending appeal, *see, e.g.*, ECF No. 68, at 1-2, the evidence supports a stay.

IV.   THE PLAINTIFFS' ARGUMENTS ABOUT PERMIT HOLDER STATISTICS ARE UNPERSUASIVE.

In an attempt to move the focus off of public safety more generally, the plaintiffs continue to argue that the only relevant consideration is the conduct of permit holders themselves. The defendants do not agree with the plaintiffs' premise, which ignores broader public safety concerns. But even if that premise were accepted, the plaintiffs' claims are based almost entirely on inapposite statistics regarding permit revocation. Revocation statistics are a poor proxy for identifying actual criminal activity because they depend on states having adequately-staffed, efficient, appropriate mechanisms for their licensing authorities to receive information about, and then act on, criminal activities of permit holders. There is no evidence that the states whose revocation statistics the plaintiffs cite—or Texas, the only state for which the plaintiffs purport to offer actual crime statistics—have any such mechanisms. To the contrary, investigations have found the opposite. *See* ECF No. 68 at 9-10; *see also* ECF Nos. 68-5, 68-6, & 68-7.

Moreover, although the plaintiffs dismiss the Violence Policy Center's ("VPC") cataloguing of killings by concealed carry permit holders since 2007 as something they have addressed previously, ECF No. 69, at 9, that information is significant.   The plaintiffs previously claimed that the full list compiled by the VPC, which currently identifies 440 killings by concealed carry permit holders since May 2007, *available at* http://www.vpc.org/fact_sht/ccwtotalkilled.pdf (last visited May 23, 2012), is over-inclusive, primarily because of its inclusion of suicides and certain unintentional killings. *See* ECF No. 34 at 5-6. However, even excluding all suicides and unintentional killings, the list still shows 268 killings by permit holders since May 2007.

V.   DEFENDANTS ARE LIKELY TO SUCCEED ON THE MERITS.

Finally, the plaintiffs argue erroneously that the "Defendants' prospects on appeal are yet-more remote" as a result of a decision by the United States District Court for the Eastern District of North Carolina holding that the scope of the Second Amendment extends outside the home. ECF No. 69 at 10 (quoting *Bateman v. Purdue*, 5:10-CV-265-H, 2012 U.S. Dist. LEXIS 47336 (E.D.N.C. Mar. 29, 2012)). *Bateman*, which did not address the constitutionality of a permit statute, became only the third court in the country to hold that the scope of the Second Amendment right identified in *Heller* extends beyond the home. If the plaintiffs want to make this a counting exercise, the overall balance is still very much in favor of the defendants. *See, e.g.*, *Moore v. Madigan*, No. 11-cv-03134, 2012 U.S. Dist. LEXIS 12967, *19-*22 (Feb. 3, 2012) (listing cases "concluding that the Second Amendment right in Heller is limited to the

right to bear arms in the home for self-defense"); ECF No. 26, at 22, 24 & n.8; ECF No. 54, at 14-17. However, this is not a counting exercise, and the decision in *Bateman* highlights the fact that the scope of the Second Amendment right recognized in *Heller* and *McDonald* is still an unresolved question. *United States v. Masciandaro*, 638 F.3d. 458, 467 (4th Cir. 2011). Accordingly, and this Court should enter a stay to maintain the status quo until the Fourth Circuit can resolve this case.

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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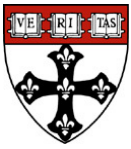
Bullet*ins*

An update on firearms research provided by the Harvard Injury Control Research Center

Comparing the Incidence of Self-Defense Gun Use and Criminal Gun Use

I. Introduction

It has been claimed that in the United States, "guns are used for defensive purposes about five times as often as they are used for crimes".[1,2] This claim is made even though the criminal chooses both the time and place of the crime, most households don't have guns, and very few of the individuals who own guns are carrying at any time. So what data support the claim?

The common method to determine the number of both self-defense and criminal gun uses has been to survey potential victims of crime. There have been two main approaches taken: Approach 1: ask everyone about gun use; Approach 2: ask only those who first report that someone tried to commit a crime against them--and assume that a negative response to this screener question means that the respondent could not have experienced either a genuine self-defense or criminal gun use. For example, a preemptive strike would not be considered a self-defense gun use. Approach 2, which obtains gun information from only a subset of those asked in Approach 1, yields much lower estimates of both types of gun uses (see McDowall et al. 2000 for the differences with respect to self-defense gun use).[3]

Many other aspects of surveys can affect the estimated number of self-defense and criminal gun uses, such as whether to include military uses or the experiences of police and security guards; whether self-defense against animals is included; if the survey corrects for possible telescoping (the reporting of events that occurred outside the time period in question), and the time period asked about (e.g., past 6 months, past 5 years, ever). However, the differences in results from these aspects are usually dwarfed by the differences from using Approach 1 rather than Approach 2.

II. Findings from the different approaches

Most private surveys have used Approach 1. After controlling for other aspects of the surveys, these surveys indicate that criminal gun use is far more common that self-defense gun use. For example, in May 2000, a Washington Post national random-digit dial survey asked "Not counting military service, have you ever been threatened with a gun or shot at?" Twenty-three percent (23%) said yes. In that same month (May 2000), a Gallup national random-digit dial survey asked "Not including military combat, have you ever used a gun to defend yourself either by firing it or threatening to fire it?" Seven percent (7%) of respondents said yes.

Some surveys have used Approach 1 to ask the same respondents about both self-defense and criminal gun use. Again, these surveys find that criminal gun use is far more common than self-defense gun use.[4][5][6] For example, data from the 2001 California Health Interview Surveys of 5,800 adolescents found that the typical California teen was 13 times more likely to be the victim of a gun threat than to have used a gun in self defense.[7]

Approach 2 has been used by the large semi-annual National Crime Victimization Surveys (NCVS). The NCVS obtains information from respondents about criminal and self-defense gun use only if they first

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report an attempted crime (i.e., assault, sexual assault, robbery, burglary, non-business larceny or motor vehicle theft) against them. Reports of gun use on the NCVS are also lower than on most private surveys because the NCVS effectively eliminates the problem of telescoping. Results from NCVS data find that criminal gun uses are roughly six to twelve times higher than self-defense gun uses.

Table 1 provides crude ballpark estimates for annual criminal and self-defense gun uses from these two types of surveys for the 1990s.

Table 1: Estimates of Annual Criminal and Self-Defense Gun Use (1990s)

	Criminal Gun Use	**Self-defense Gun Use**
Approach 1: ask everyone directly about gun use	10 million (Box A)	2.5 million (Box B)
Approach 2: ask only those who report an attempted crime against them (Used by NCVS)	800,000 (Box C)	80,000 (Box D)

For both type of surveys, the number of estimated criminal gun uses are far higher than the number of self-defense gun uses. Indeed, no survey that has used the same methodology for estimating both criminal and self-defense (i.e., comparing Box A with Box B or Box C with Box D) has found anywhere near the number of self-defense gun uses compared to criminal gun uses.

So how can anyone claim that there are more self-defense gun uses than criminal gun uses? They do so by comparing different types of surveys. They compare the results of Box B with Box C! However, it is completely inappropriate to compare estimates which come from two radically different survey methodologies. An appropriate assessment of the data is that *the overwhelming evidence from both types of surveys is that guns in the United States are used far more in crime than in self-defense.*

III. The difficulty of estimating gun use

There are two key problems which make it difficult to determine with any accuracy the actual number of criminal and self-defense gun uses using survey methodology. First, the estimates rely on self-report with all the known biases of that approach (e.g., self-presentation bias). This is a particular problem with determining gun use, since the information usually concerns a quick, dangerous event that involves ego, emotions, and potentially severe consequences. In addition, the information is provided from only one side of what is a hostile interaction. The other side might tell a very different story. Second, there is no accepted understanding of what constitutes a criminal or a self-defense gun use. For example, criminals often believe that their gun use during a crime was in self-defense. The National Research Council reviewed the scientific literature on self-defense gun use in 2005 and concluded that "self-defense is an ambiguous term" (p.106) and that whether one is a defender or a perpetrator may depend on perspective.[8]

For simplicity, let us require that socially undesirable gun use be classified as criminal gun uses, and socially desirable gun use be classified as self-defense gun use. Surveys that ask open-ended questions about gun use (allowing the respondent to describe the event in their own words) (e.g., Hemenway & Azrael 2000; Hemenway, Miller & Azrael 2000) provide evidence about the proper categorization of responses using this classification. From such surveys, it appears that all of the criminal gun uses

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reported using Approach 1 are probably socially undesirable, and many might inappropriately be missed by Approach 2 (e.g., many gun intimidations may not be reported as crimes).

By contrast it appears that the large majority of the self-defense gun uses reported using Approach 1 are socially undesirable; they are largely escalating arguments, or preemptive gun use out of fear rather than a response to an attempted crime. Most would appropriately be missed by Approach 2, and should not be considered genuine self-defense gun uses.[5] They are actually reports of inappropriate or criminal gun use.

Researchers analyzing one Approach 1 survey concluded: "Most commentators have assumed that the [defensive gun uses] reported by survey respondents are actions that would be endorsed by an impartial observer who knew all the facts. Yet the sketchy and unverified accounts available from surveys leave considerable uncertainty about what actually happened, whether the respondent was the victim or the perpetrator, and whether the respondent's actions were otherwise legal, reasonable, and in the public interest".[9] An analysis of another Approach 1 survey concluded that many of the incidents "relied heavily on respondent judgments about the motives of possible offenders, and motives may be murky if the respondents acted quickly...the gun use may follow mistaken perceptions of innocuous actions by the supposed criminal. These cases of armed resistance would then legally amount to aggravated assaults".[3]

IV. Conclusion

The opportunity for a law-abiding gun owner to use a gun in a socially desirable manner--against a criminal during the commission of a crime--will occur, for the average gun owner, perhaps once or never in a lifetime. It is a rare event. Other than self-defense, the use of a gun against another human is socially undesirable. Regular citizens with guns, who are sometimes tired, angry, drunk, or afraid, and who are not trained in dispute resolution, have lots of opportunities for inappropriate gun uses. People engage in innumerable annoying and somewhat hostile interactions with each other in the course of a lifetime. It should not be surprising that inappropriate, socially undesirable "self-defense" gun uses by people who believe they are law-abiding citizens outnumber the appropriate and socially beneficial use of guns.[6]

Although most of the reported self-defense gun uses from Approach 1 surveys seem more like criminal uses, even if one believed they were all genuine socially beneficial uses, the number of criminal gun uses would still vastly exceeds the number of self-defense gun uses in the United States. No survey using similar methodology to determine both criminal and self-defense use has ever found otherwise.

Contributors: David Hemenway, PhD; Mary Vriniotis, MS

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**IN THE UNITED STATES DISTRICT COURT
FOR THE DISTRICT OF MARYLAND**

RAYMOND WOOLLARD, et al.

        Plaintiffs

    v.

MARCUS BROWN, et al.

        Defendants

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Civil Case No. L-10-2068

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MEMORANDUM

Presently pending before the Court is the Motion of Defendants for a Stay Pending

Appeal. Docket No. 67. The issues have been comprehensively briefed, and the Court finds oral

hearing unnecessary. See Local Rule 105.6 (D. Md. 2011). For the reasons set forth herein, the

Court will, by separate Order, DENY the Motion.

I.   PROCEDURAL BACKGROUND

This case involves a challenge to the constitutionality of the State of Maryland's handgun

permitting scheme. In July of 2010, Plaintiff Robert Woollard filed suit contending that § 5-

306(a)(5)(ii) of the Public Safety Article of the Maryland Code violates the Second Amendment

to the United States Constitution. The provision in question requires that, prior to issuing a

permit to wear or carry a handgun in the state of Maryland, the Secretary of the State Police must

make a finding that the applicant "has good and substantial reason to wear, carry, or transport a

handgun, such as a finding that the permit is necessary as a reasonable precaution against

apprehended danger."

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The Court agreed with Woollard, and entered an Order permanently enjoining

Defendants, their officers, agents, and employees, from enforcing § 5-306(a)(5)(ii). See Docket

Nos. 52 and 63. The Court further ordered Defendants to promptly process Woollard's 2009

application for a permit renewal, the denial of which gave rise to the instant suit, without

consideration of the "good and substantial reason" requirement. Id.

Defendants timely filed an application for stay and a notice of appeal to the Fourth

Circuit Court of Appeals. Implementation of the Court's ruling was preliminarily stayed while

the parties briefed the issue of whether a more permanent stay should be entered pending the

Fourth Circuit's decision. Following expedited initial briefing, the Court convened a

teleconference with counsel for all parties and ordered supplemental briefing, which has now

been completed.

II.   LEGAL STANDARD

Federal Rule of Civil Procedure 62(c) permits a District Court to stay pending appeal a

final judgment that grants, dissolves, or denies an injunction. In determining whether a stay is

warranted, the Court must consider: "(1) whether the stay applicant has made a strong showing

that he is likely to succeed on the merits; (2) whether the applicant will be irreparably injured

absent a stay; (3) whether issuance of the stay will substantially injure the other parties interested

in the proceeding; and (4) where the public interest lies." Hilton v. Braunskill, 481 U.S. 770,

776 (1987).

III.   DISCUSSION

In this case, Defendants have failed to meet their burden of establishing that the aforementioned factors weigh in favor of a stay. The Court will briefly address each of these factors.

a. Likelihood of Success

Just as every party to appeal a trial court's judgment does so with the expectation (or at least the hope) of vindication, every court that renders a judgment does so in the belief that its judgment is the correct one. Accordingly, the "likelihood-of-success standard does not mean that the trial court needs to change its mind or develop serious doubts concerning the correctness of its decision in order to grant a stay pending appeal." Goldstein v. Miller, 488 F. Supp. 156, 172 (D. Md. 1980). A party seeking a stay, however, must nevertheless make a "strong showing" that he is likely to succeed. Hilton, 481 U.S. at 776.

Defendants rest their argument largely on the fact that this case involved difficult and novel issues in a largely undeveloped area of law. As this Court has long noted, however, a stay is not required "every time a case presents difficult questions of law." St. Agnes Hosp., Inc. v. Riddick, 751 F. Supp. 75, 76 (D. Md. 1990) (quoting Miller, 488 F. Supp. at 173). While the result in the case at bar was not ineluctably dictated by controlling precedent, it did flow naturally from the Supreme Court's decisions in District of Columbia v. Heller, 554 U.S. 570 (2008) and McDonald v. City of Chicago, 130 S. Ct. 3020 (2010), and, perhaps more importantly, from the Fourth Circuit's decision in United States v. Masciandaro, 638 F.3d 458 (4th Cir. 2011). These decisions, as the Court noted, left useful "signposts" and provided "a ready guide." Mem. Op. at 7, 6, Docket No. 52.

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While Defendants have cited post-<u>Heller</u> decisions in which courts have upheld similar (though not identical) permitting regulations, they cite none from the Fourth Circuit. By contrast, as Woollard points out, subsequent to this Court's award of summary judgment another district court in the Fourth Circuit has also held, as this Court did, that "[a]lthough considerable uncertainty exists regarding the scope of the Second Amendment right to keep and bear arms, it undoubtedly is not limited to the confines of the home." <u>Bateman v. Perdue</u>, No. 5:10-CV-265-H, 2012 U.S. Dist. LEXIS 47336, at *10–*11 (E.D.N.C. Mar. 29, 2012). In so doing, the United States District Court for the Eastern District of North Carolina, like this Court, placed considerable reliance on Judge Niemeyer's non-controlling opinion in <u>Masciandaro</u>.

Defendants have beyond question shown that considerable difference of opinion exists throughout the country as to the proper scope and application of the Second Amendment following the Supreme Court's watershed decisions in <u>Heller</u> and <u>McDonald</u>. In the case at bar, the Fourth Circuit could certainly find reasonable grounds to reverse this Court's decision. Such an outcome does not appear so probable, however, as to outweigh the remaining considerations discussed below.

b. Irreparable Injury

Defendants point to little in the way of truly irreparable injury that is likely to result should their request for a stay be denied. First, Defendants urge that "their ability to protect public safety will be curtailed" because of their "inability to enforce an important component of the handgun permit regulations . . . ." Defs.' Mot for Stay 17, Docket No. 54. The problem with this line of argument is that it begs a question that has already been answered. To accept Defendants' contention would be to ignore the Court's determination that the "good and substantial reason" requirement is insufficiently tailored to serving the State's admittedly

legitimate interest in public safety. This and other such arguments that seek to relitigate the merits of the case must fail.

Next, Defendants advert to what they characterize as "significant, immediate administrative burdens" that would be involved in implementing the Court's Order. Id. at 20. While the Court is not unsympathetic to the very real and often costly considerations involved in revamping a regulatory scheme, administrative hardship does not rise to the level of irreparable harm. As to the more concrete costs of compliance, "[m]ere economic injury is rarely, if ever, sufficient to warrant entry of a stay of judgment to protect a party against it . . . ." Miller, 488 F. Supp. at 175.

Nor does it seem likely that the attendant burdens would be as onerous as Defendants would have the Court believe. The Court's main concern involved the difficulty Defendants might have in revoking permits that will have already been issued should they succeed on appeal. On this point, the parties appear to agree that the Court's decision does not stop Defendants from tracking whether applicants have a "good and substantial reason," only from denying permits on this basis. While Defendants concede this point, they contend that "in light of the strong feelings surrounding this issue, [the Maryland State Police ('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." Defs.' Supp. Brief 4, Docket No. 68. Notably, however, Defendants offer no factual support for such an expectation. Moreover, applicants with good and substantial reason who decline to provide it would do so with the understanding that, as a consequence, they might have their permits revoked and be forced to repeat the application process.

As to those new applicants without good and substantial reason, Defendants admit that "MSP expects that many such individuals would comply with its directions and return their permits." Id. While they assert that it would be "impractical" to track down and recover the remainder, id., MSP is doubtless called upon to recover revoked permits from time to time. Furthermore, any permit holder who refused to voluntarily return a permit would be in knowing violation of MD Code, Public Safety, § 5-310, which requires the holder to "return the permit to the Secretary [of State Police] within 10 days after receipt of written notice of the revocation." Defendants have given the Court no basis from which to infer that a significant number of those applicants who have waited patiently for the outcome of this litigation and complied with the permit application process in full would, upon revocation, suddenly decline to adhere to the law.

c. Interest of Other Parties and the Public

Against costs to Defendants of complying immediately with the Court's ruling, the Court must balance the harm to Woollard and those like him. If a stay is granted, a sizeable number of people will be precluded from exercising, while the case is argued on appeal, what this Court has recognized as a valid aspect of their Second Amendment right. In the First Amendment context, the Supreme Court has stated that "loss of First Amendment freedoms, for even minimal periods of time, unquestionably constitutes irreparable injury." Elrod v. Burns, 427 U.S. 347, 373 (1976). As the Court discussed in its summary judgment opinion, there are substantial similarities between the First and Second Amendments, and the analogy is appropriate here as well.

The question of public interest is somewhat more involved. It is self-evident, as the Fourth Circuit has noted, that "[s]urely, upholding constitutional rights serves the public interest." Newsom v. Albemarle Cnty. Sch. Bd., 354 F.3d 249, 261 (4th Cir. 2003). At the same

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time, however, the Court would not forget another admonition from the Fourth Circuit: that "[t]his is serious business. We do not wish to be even minutely responsible for some unspeakably tragic act of mayhem because in the peace of our judicial chambers we miscalculated as to Second Amendment rights." Masciandaro, 638 F.3d at 475.

For this reason, the Court directed the parties to file supplemental briefs addressing how Maryland's permitting scheme, without the "good and substantial reason" requirement, compares to the systems in force in other states and how Maryland's rate of handgun violence compares to that of other states with more liberal regulations. The Second Amendment does not "require judges to assess the costs and benefits of firearms restrictions and thus to make difficult empirical judgments in an area in which they lack expertise." McDonald, 130 S. Ct. at 3050. Nevertheless, persuasive evidence that states with more permissive regulatory schemes suffer from more handgun crime, or that states experience an increase in handgun violence when moving from a "may issue" to a "shall issue" framework, would certainly militate in favor of a stay.

The parties have conducted commendably thorough research on the subject, and each has dedicated considerable time and energy to debating the relative merits of the studies and statistics offered by the other. The inescapable conclusion, however, is that the evidence does not point strongly in any one direction. As Defendants aptly state, "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." Defs.' Supp. Brief 5, Docket No. 68. On this dimension, then, the Court cannot say that a stay would demonstrably serve or disserve the State's goal of preventing a potential increase in

handgun violence pending appeal. Defendants have not established that the public interest

weighs in favor of a stay.

IV.   CONCLUSION

Having given due weight to the four <u>Hilton</u> factors, the Court determines that a stay

pending appeal is not warranted. The Court will, by separate Order, lift the temporary stay now

in effect.

Dated this 23rd day of July, 2012

                                        /s/
                                        _____

                                        Benson Everett Legg
                                        United States District Judge

**IN THE UNITED STATES DISTRICT COURT
FOR THE DISTRICT OF MARYLAND**

RAYMOND WOOLLARD, et al.

        Plaintiffs

    v.

MARCUS BROWN, et al.

        Defendants

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Civil Case No. L-10-2068

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ORDER

Presently pending is Defendants' Renewed Motion for a Stay Pending Appeal. Docket

No. 67. It is, this 23rd day of July, 2012, hereby ORDERED that:

1. The Motion is DENIED; and

2. The temporary stay entered by Order of March 30, 2012 is DISSOLVED effective 14

    days following the issuance of this Order.

/s/

_____

Benson Everett Legg
United States District Judge

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Supp. App. 469