FOR PUBLICATION

UNITED STATES COURT OF APPEALS
FOR THE NINTH CIRCUIT

EDWARD PERUTA; MICHELLE LAXSON; JAMES DODD; LESLIE BUNCHER, DR.; MARK CLEARY; CALIFORNIA RIFLE AND PISTOL ASSOCIATION FOUNDATION, *Plaintiffs-Appellants*, v. COUNTY OF SAN DIEGO; WILLIAM D. GORE, individually and in his capacity as Sheriff, *Defendants-Appellees*, STATE OF CALIFORNIA, *Intervenor.*	No. 10-56971 D.C. No. 3:09-cv-02371-IEG-BGS

Appeal from the United States District Court
for the Southern District of California
Irma E. Gonzalez, Senior District Judge, Presiding

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ADAM RICHARDS; SECOND AMENDMENT FOUNDATION; CALGUNS FOUNDATION, INC.; BRETT STEWART,	No. 11-16255
Plaintiffs-Appellants,	D.C. No. 2:09-cv-01235-MCE-DAD
v.	
ED PRIETO; COUNTY OF YOLO, *Defendants-Appellees*.	OPINION

Appeal from the United States District Court
for the Eastern District of California
Morrison C. England, Chief District Judge, Presiding

Argued and Submitted En Banc June 16, 2015
San Francisco, California

Filed June 9, 2016

Before: Sidney R. Thomas, Chief Judge and Harry
Pregerson, Barry G. Silverman, Susan P. Graber,
M. Margaret McKeown, William A. Fletcher, Richard A.
Paez, Consuelo M. Callahan, Carlos T. Bea, N. Randy
Smith and John B. Owens, Circuit Judges.

Opinion by Judge W. Fletcher;
Concurrence by Judge Graber;
Dissent by Judge Callahan;
Dissent by Judge Silverman;
Dissent by Judge N.R. Smith

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SUMMARY[*]

Civil Rights

The en banc court affirmed the district courts' judgments and held that there is no Second Amendment right for members of the general public to carry concealed firearms in public.

Appellants, who live in San Diego and Yolo Counties, sought to carry concealed firearms in public for self-defense, but alleged they were denied licenses to do so because they did not satisfy the good cause requirements in their counties. Under California law, an applicant for a license must show, among other things, "good cause" to carry a concealed firearm. California law authorizes county sheriffs to establish and publish policies defining good cause. Appellants contend that San Diego and Yolo Counties' published policies defining good cause violate their Second Amendment right to keep and bear arms.

The en banc court held that the history relevant to both the Second Amendment and its incorporation by the Fourteenth Amendment lead to the same conclusion: The right of a member of the general public to carry a concealed firearm in public is not, and never has been, protected by the Second Amendment. Therefore, because the Second Amendment does not protect in any degree the right to carry concealed firearms in public, any prohibition or restriction a state may choose to impose on concealed carry — including

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[*] This summary constitutes no part of the opinion of the court. It has been prepared by court staff for the convenience of the reader.

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a requirement of "good cause," however defined — is
necessarily allowed by the Amendment. The en banc court
stated that there may or may not be a Second Amendment
right for a member of the general public to carry a firearm
openly in public, but the Supreme Court has not answered
that question.

The en banc court granted the motion to intervene by the
State of California, which sought intervention after the San
Diego Sheriff declined to petition for rehearing en banc
following the panel's decision. The en banc court held that
under the circumstances presented here, California's motion
to intervene was timely.

Concurring, Judge Graber, joined by Chief Judge Thomas
and Judge McKeown, wrote separately only to state that, even
if the Second Amendment applied to the carrying of
concealed weapons in public, the provisions at issue would be
constitutional.

Dissenting, Judge Callahan, joined by Judge Silverman as
to all parts except section IV, by Judge Bea, and by Judge
N.R. Smith as to all parts except section II.B, stated that in
the context of present-day California law, the defendant
counties' limited licensing of the right to carry concealed
firearms is tantamount to a total ban on the right of an
ordinary citizen to carry a firearm in public for self-defense.
Thus, plaintiffs' Second Amendment rights have been
violated.

Dissenting, Judge Silverman, joined by Judge Bea, would
hold that the challenged laws are unconstitutional under the
Second Amendment because they do not survive any form of
heightened scrutiny analysis.

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Dissenting, Judge N.R. Smith stated that he joined the dissent of Judge Callahan but wrote separately only to express his opinion that the appropriate remedy is to remand this case to the district courts to allow them to initially determine and apply an appropriate level of scrutiny.

COUNSEL

In No. 10-56971:

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OPINION

W. FLETCHER, Circuit Judge:

Under California law, a member of the general public may not carry a concealed weapon in public unless he or she has been issued a license. An applicant for a license must satisfy a number of conditions. Among other things, the applicant must show "good cause" to carry a concealed firearm. California law authorizes county sheriffs to establish and publish policies defining good cause. The sheriffs of San Diego and Yolo Counties published policies defining good cause as requiring a particularized reason why an applicant needs a concealed firearm for self-defense.

Appellants, who live in San Diego and Yolo Counties, allege that they wish to carry concealed firearms in public for self-defense, but that they do not satisfy the good cause requirements in their counties. They contend that their counties' definitions of good cause violate their Second Amendment right to keep and bear arms. They particularly rely on the Supreme Court's decisions in *District of Columbia v. Heller*, 554 U.S. 570 (2008), and *McDonald v. City of Chicago*, 561 U.S. 742 (2010).

We hold that the Second Amendment does not preserve or protect a right of a member of the general public to carry concealed firearms in public.

I. Procedural History

Plaintiff Edward Peruta lives in San Diego County. He applied for a license to carry a concealed firearm in February 2009, but his application was denied because he had not

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shown good cause under the policy published in his county.
Plaintiff Adam Richards lives in Yolo County. He sought a
license to carry a concealed firearm in May 2009, but was
told he could not establish good cause under his county's
policy. Peruta, Richards, and the other plaintiffs — five
residents of San Diego and Yolo Counties, as well as several
gun-rights organizations — brought two separate suits
challenging under the Second Amendment the two counties'
interpretation and application of the statutory good cause
requirement under California law.

The district courts granted summary judgment in each
case, holding that the counties' policies do not violate the
Second Amendment. *Peruta v. Cty. of San Diego*, 758 F.
Supp. 2d 1106 (S.D. Cal. 2010); *Richards v. Cty. of Yolo*,
821 F. Supp. 2d 1169 (E.D. Cal. 2011). A divided three-
judge panel of this court reversed both decisions. The panel
majority held in a published opinion in *Peruta* that San
Diego's policy violated the Second Amendment. *See Peruta
v. Cty. of San Diego*, 742 F.3d 1144 (9th Cir. 2014); *see also
id.* at 1179 (Thomas, J., dissenting). Although Plaintiffs
challenged only the county's concealed firearms policy, the
panel held that their challenge should not be "viewed in
isolation." Rather, in the view of the panel majority,
Plaintiffs' suit should be viewed as a challenge to "the
constitutionality of [California's] entire [statutory] scheme."
Id. at 1171. In the majority's view, the Second Amendment
required that "the states permit *some form* of carry for self-
defense outside the home." *Id.* at 1172 (emphasis in original).
Because California's statutory scheme permits concealed
carry only upon a showing of good cause and because open
carry is also restricted, the panel held that the county's
definition of good cause for a concealed carry license violates
the Second Amendment. *Id* at 1179. The panel held in

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Richards that, in light of its holding in *Peruta*, the Yolo County policy also violated the Second Amendment. *See Richards v. Prieto*, 560 F. App'x 681 (9th Cir. 2014); *see also id.* at 682 (Thomas, J., concurring in the judgment).

Yolo County and its sheriff, Ed Prieto, filed a petition for rehearing en banc in *Richards*. San Diego County's sheriff, William Gore, announced that he would not petition for rehearing en banc in *Peruta*. After Sheriff Gore declined to file a petition, the State of California moved to intervene in *Peruta* in order to seek rehearing en banc. The same divided three-judge panel denied California's motion to intervene. *See Peruta v. Cty. of San Diego*, 771 F.3d 570 (9th Cir. 2014); *see also id.* at 576 (Thomas, J., dissenting).

We granted rehearing en banc in both cases. *Peruta v. Cty. of San Diego*, 781 F.3d 1106 (9th Cir. 2015); *Richards v. Prieto*, 782 F.3d 417 (9th Cir. 2015).

II. Standard of Review

We review a district court's grant of summary judgment de novo. *Sanchez v. Cty. of San Diego*, 464 F.3d 916, 920 (9th Cir. 2006). We review constitutional questions de novo. *Am. Acad. of Pain Mgmt. v. Joseph*, 353 F.3d 1099, 1103 (9th Cir. 2004).

III. California Firearms Regulation

California has a multifaceted statutory scheme regulating firearms. State law generally prohibits carrying concealed firearms in public, whether loaded or unloaded. Cal. Penal Code § 25400. State law also generally prohibits carrying loaded firearms on the person or in a vehicle in any public

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place or on any public street, in either an incorporated city or
a "prohibited area" of "unincorporated territory." *Id.* §
25850. Finally, state law generally prohibits carrying
unloaded handguns openly on the person in a public place or
on a public street, in either an incorporated city or a
"prohibited area" of an "unincorporated area of a county." *Id.*
§ 26350.

However, there are numerous exceptions to these general
prohibitions. For example, the prohibitions of §§ 25400 and
25850 do not apply to active and retired "peace officers." *Id.*
§§ 25450, 25900. The prohibition of § 25400 does not apply
to guards or messengers of common carriers of banks or
financial institutions while employed in the shipping of things
of value. *Id*. § 25630. The prohibition of § 25850 does not
apply to armored vehicle guards, guards or messengers of
common carriers, banks or financial institutions, security
guards, animal control officers, or zookeepers, provided they
have completed an approved course in firearms training. *Id.*
§§ 26015, 26025, 26030.

Further, the prohibition of § 25400 does not apply to
licensed hunters or fishermen while engaged in hunting or
fishing, to members of target shooting clubs while on target
ranges, or to the transportation of unloaded firearms while
going to or returning from hunting or fishing expeditions or
target ranges. *Id.* §§ 25640, 25635. Nor does it apply to the
transportation of a firearm to and from a safety or hunting
class or a recognized sporting event involving a firearm, to
transportation between a person's residence and business or
private property owned or possessed by the person, or to
transportation between a business or private residence for the
purpose of lawful repair, sale, loan, or transfer of the firearm.
Id. §§ 25520, 25525, 25530. The prohibition of § 25850 does

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not apply to a person having a loaded firearm at his or her
residence, including a temporary residence or campsite. *Id.*
§ 26055. Nor does it apply to a person having a loaded
firearm at his or her place of business or on his or her private
property. *Id.* § 26035. It also does not apply to a person "who
reasonably believes that any person or the property of any
person is in immediate, grave danger and that the carrying of
the weapon is necessary for the preservation of that person or
property." *Id.* § 26045. Nor does it apply to persons using
target ranges for practice, or to members of shooting clubs
while hunting on the premises of the clubs. *Id.* § 26005.
Finally, the prohibitions of §§ 25400, 25850 and 23650 do
not apply to transportation of firearms between authorized
locations, provided that the firearm is unloaded and in a
locked container, and that the course of travel has no
unreasonable deviations. *Id.* § 25505.

The case before us concerns the general prohibition of
§ 25400 against carrying loaded or unloaded concealed
weapons, and a license-based exception to that prohibition.
The prohibition of § 25400 does not apply to those who have
been issued licenses to carry concealed weapons. *Id.*
§ 25655. The sheriff of a county may issue a concealed carry
license to a person upon proof of all of the following:

> (1) The applicant is of good moral character.
>
> (2) Good cause exists for issuance of the
> license.
>
> (3) The applicant is a resident of the county or
> a city within the county, or the applicant's
> principal place of employment or business is
> in the county or a city within the county and

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> the applicant spends a substantial period of
> time in that place of employment or business.
>
> (4) The applicant has completed a course of
> training as described in Section 26165.

Id. § 26150(a). The chief of a municipal police department may issue a concealed carry license under comparable criteria; the only difference is that the applicant must be a "resident of that city." *Id.* §§ 26155(a), 26155(a)(3) (residence). Sheriffs and municipal police chiefs are required to "publish and make available a written policy summarizing the provisions" of §§ 26150(a) and 26155(a). *Id.* § 26160.

An affidavit of Blanca Pelowitz, Manager of the San Diego Sheriff's Department License Division, describes the definition of good cause in San Diego County:

> Good Cause . . . is defined by this County
> to be a set of circumstances that distinguish
> the applicant from the mainstream and causes
> him or her to be placed in harm's way.
> Simply fearing for one's personal safety alone
> is not considered good cause. This criterion
> can be applied to situations related to personal
> protection as well as those related to
> individual businesses or occupations.
>
> Good cause is also evaluated on an
> individual basis. Reasons applicants request
> a license will fall into one of . . . four general
> categories[.]

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The only two general categories potentially relevant to this appeal are:

> Category 2 = Personal Protection Only includes: documented threats, restraining orders and other related situations where an applicant can demonstrate they are a specific target at risk.
>
> Category 4 = Business owners/employees includes a diversity of businesses & occupations, such as doctors, attorneys, CEO's, managers, employees and volunteers whose occupation or business places them at high risk of harm.

The published policy of Yolo County does not define "good cause" but gives examples of where good cause does, or does not, exist. The policy provides as follows:

> Examples of valid reasons to request a permit include, but are not limited to:
>
>> Victims of violent crime and/or documented threats of violence.
>>
>> Business owners who carry large sums of cash or valuable items.
>>
>> Business owners who work all hours in remote areas and are likely to encounter dangerous people and situations.

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Examples o[f] invalid reasons to request a permit include, but are not limited to:

Recreation in remote areas.

Hunting or fishing.

Self protection and protection of family (without credible threats of violence).

Employment in the security field, i.e., security guard, body guard, VIP protection.

Personal safety due to job conditions or duties placed on the applicant by their employer.

IV. Second Amendment and Concealed Carry

Plaintiffs contend that the good cause requirement for concealed carry, as interpreted in the policies of the sheriffs of San Diego and Yolo Counties, violates the Second Amendment. Plaintiffs' arguments in the two cases differ in some particulars, but they essentially proceed as follows. First, they contend that the Second Amendment guarantees at least some ability of a member of the general public to carry firearms in public. Second, they contend that California's restrictions on concealed and open carry of firearms, taken together, violate the Amendment. Third, they contend that there would be sufficient opportunity for public carry of firearms to satisfy the Amendment if the good cause requirement for concealed carry, as interpreted by the sheriffs of San Diego and Yolo Counties, were eliminated. Therefore,

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they contend, the counties' good cause requirements for
concealed carry violate the Amendment. While Plaintiffs
base their argument on the entirety of California's statutory
scheme, they allege only that they have sought permits to
carry concealed weapons, and they seek relief only against
the policies requiring good cause for such permits. Notably,
Plaintiffs do *not* contend that there is a free-standing Second
Amendment right to carry concealed firearms.

We do not reach the question whether the Second
Amendment protects some ability to carry firearms in public,
such as open carry. That question was left open by the
Supreme Court in *Heller*, and we have no need to answer it
here. Because Plaintiffs challenge only policies governing
concealed carry, we reach only the question whether the
Second Amendment protects, in any degree, the ability to
carry concealed firearms in public. Based on the
overwhelming consensus of historical sources, we conclude
that the protection of the Second Amendment — whatever the
scope of that protection may be — simply does not extend to
the carrying of concealed firearms in public by members of
the general public.

The Second Amendment may or may not protect, to some
degree, a right of a member of the general public to carry
firearms in public. But the existence *vel non* of such a right,
and the scope of such a right, are separate from and
independent of the question presented here. We hold only
that there is no Second Amendment right for members of the
general public to carry concealed firearms in public.

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A. *Heller* and *McDonald*

The Second Amendment provides: "A well regulated Militia, being necessary to the security of a free State, the right of the people to keep and bear Arms, shall not be infringed." U.S. Const. amend. II. The watershed case interpreting the Amendment is *District of Columbia v. Heller*, 554 U.S. 570 (2008). The plaintiff in *Heller* challenged a District of Columbia statute that entirely banned the possession of handguns in the home, and required that any lawful firearm in the home be "disassembled or bound by a trigger lock at all times, rendering it inoperable." *Id.* at 628.

Relying on the phrase "shall not be infringed," the Court in *Heller* viewed the Amendment as having "codified a *pre-existing* right." *Id.* at 592 (emphasis in original). The Court focused on the history leading to the adoption of the Amendment, and on the common understanding of the Amendment in the years following its adoption. The Court concluded that the "pre-existing right" to keep and bear arms preserved by the Second Amendment was in part an individual right to personal self-defense, not confined to the purpose of maintaining a well-regulated militia. The Court struck down the challenged statute, concluding that the Amendment preserves the right of members of the general public to keep and bear arms in their homes for the purpose of self-defense: "[W]e hold that the District's ban on handgun possession in the home violates the Second Amendment, as does its prohibition against rendering any lawful firearm in the home operable for the purpose of immediate self-defense." *Id.* at 635.

The Court in *Heller* was careful to limit the scope of its holding. Of particular interest here, the Court noted that the

Second Amendment has not been generally understood to protect the right to carry concealed firearms. The Court wrote:

> *Like most rights, the right secured by the Second Amendment is not unlimited.* From Blackstone through the 19th-century cases, commentators and courts routinely explained that the right was not a right to keep and carry any weapon whatsoever in any manner whatsoever and for whatever purpose. *For example, the majority of the 19th-century courts to consider the question held that prohibitions on carrying concealed weapons were lawful under the Second Amendment or state analogues. See, e.g.*, *State v. Chandler*, 5 La. Ann., at 489–90 [(1850)]; *Nunn v. State*, 1 Ga., at 251 [(1846)]; see generally 2 Kent, [Commentaries on American Law (O. Holmes ed., 12th ed. 1873)] *340, n.2; The American Students' Blackstone 84, n.11 (G. Chase ed. 1884).

Id. at 626–27 (emphasis added) (some citations omitted).

At the end of its opinion, the Court again emphasized the limited scope of its holding, and underscored the tools that remained available to the District of Columbia to regulate firearms. Referring the reader back to the passage just quoted, the Court wrote:

> The Constitution leaves the District of Columbia a variety of tools for combating th[e] problem [of handgun violence],

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including some measures regulating handguns, see *supra* at 626–627, and n.26.

Id. at 636.

Heller left open the question whether the Second Amendment applies to regulation of firearms by states and localities. The Court answered the question two years later, in *McDonald v. City of Chicago*, 561 U.S. 742 (2010), holding that the Due Process Clause of the Fourteenth Amendment incorporates the Second Amendment. In substantial part, the Court based its holding on the understanding of a "clear majority" of the states when the Fourteenth Amendment was adopted. The Court wrote:

> A clear majority of the States in 1868 . . . recognized the right to keep and bear arms as being among the foundational rights necessary to our system of Government.
>
> In sum, it is clear that the Framers and ratifiers of the Fourteenth Amendment counted the right to keep and bear arms among those fundamental rights necessary to our system of ordered liberty.

Id. at 777–78.

B. Second Amendment Right to Keep and Bear Concealed Arms

In analyzing the meaning of the Second Amendment, the Supreme Court in *Heller* and *McDonald* treated its historical analysis as determinative. The Court in *Heller* held that the

Second Amendment, as originally adopted, "codified a pre-existing right," 554 U.S. at 592 (emphasis omitted), a "right inherited from our English ancestors," *id.* at 599 (internal quotation marks omitted). The Court in *McDonald* held, further, that this "pre-existing right" was incorporated into the Fourteenth Amendment, based in substantial part on the importance attached to the right by a "clear majority" of the states. In determining whether the Second Amendment protects the right to carry a concealed weapon in public, we engage in the same historical inquiry as *Heller* and *McDonald*. As will be seen, the history relevant to both the Second Amendment and its incorporation by the Fourteenth Amendment lead to the same conclusion: The right of a member of the general public to carry a concealed firearm in public is not, and never has been, protected by the Second Amendment.

1. History Relevant to the Second Amendment

a. Right to Bear Arms in England

The right to bear arms in England has long been subject to substantial regulation. In 1299, Edward I directed the sheriffs of Safford and Shalop to prohibit anyone from "going armed within the realm without the king's special licence." 4 Calendar Of The Close Rolls, Edward I, 1296–1302, at 318 (Sept. 15, 1299, Canterbury) (H.C. Maxwell-Lyte ed., 1906). Five years later, in 1304, Edward I ordered the sheriff of Leicester to enforce his prohibition on "any knight, esquire or other person from . . . going armed in any way without the king's licence." 5 Calendar Of The Close Rolls, Edward I, 1302–1307, at 210 (June 10, 1304, Stirling) (H.C. Maxwell-Lyte ed., 1908).

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In 1308, Edward II ordered the town of Dover to ensure that "no knight, esquire, or other shall . . . go armed at Croydon or elsewhere before the king's coronation." 1 Calendar Of The Close Rolls, Edward II, 1307–1313, at 52 (Feb. 9, 1308, Dover) (H.C. Maxwell-Lyte ed., 1892). In 1310, he issued a similar order to the sheriff of York, demanding that the sheriff prohibit any "earl, baron, knight, or other" from going armed. *Id.* at 257 (Mar. 20, 1310, Berwick-on-Tweed). Two years later, in 1312, Edward II ordered the sheriffs in Warwick and Leicester to seize the weapons of any that "go armed" without special permission from the king. *Id.* at 553 (Oct. 12, 1312, Windsor). These early prohibitions, targeting particular towns and counties, and particular actors, foreshadowed a more general proclamation nearly two decades later, in which Edward II prohibited "throughout [the King's] realm" "any one going armed without [the King's] licence." 4 Calendar Of The Close Rolls, Edward II, 1323–1327, at 560 (Apr. 28, 1326, Kenilworth) (H.C. Maxwell-Lyte ed., 1892).

In 1328, under Edward III, Parliament enacted the Statute of Northampton, an expanded version of Edward II's earlier prohibition. The Statute provided that

> no Man great nor small, of what Condition soever he be, except the King's Servants in his presence, and his Ministers in executing of the King's Precepts, or of their Office, and such as be in their Company assisting them . . . be so hardy to come before the King's Justices, or other of the King's Ministers doing their office, with force and arms, nor bring no force in affray of the peace, nor to go nor ride armed by night nor by day, in Fairs,

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> Markets, nor in the presence of the Justices or other Ministers, nor in no part elsewhere, upon pain to forfeit their Armour to the King, and their Bodies to Prison at the King's pleasure.

2 Edw. 3, c. 3 (1328). The Statute of Northampton would become the foundation for firearms regulation in England for the next several centuries. *See* Patrick J. Charles, *The Faces of the Second Amendment Outside the Home: History Versus Ahistorical Standards of Review*, 60 Clev. St. L. Rev. 1, 7–36 (2012) (describing the Statute of Northampton, as well as English firearms regulation before and after the adoption of the statute).

The Statute of Northampton was widely enforced. In 1388, for example, Richard II issued to the bailiffs of Scardburgh an

> [o]rder to arrest and imprison until further order for their deliverance all those who shall be found going armed within the town, leading an armed power, making unlawful assemblies, or doing aught else whereby the peace may be broken and the people put in fear . . . as in the statute lately published at Northampton among other things it is contained that no man of whatsoever estate or condition shall be bold to appear armed before justices or king's ministers in performance of their office, lead an armed force in breach of the peace, ride or go armed by day or night in fairs and markets or elsewhere in presence of justices etc. . . .

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3 Calendar Of The Close Rolls, Richard II, 1385–1389, at 399–400 (May 16, 1388, Westminster) (H.C. Maxwell-Lyte ed., 1914). A half-century later, Henry VI issued a similar order, reminding his subjects of the

> statute published in the parliament holden at Norhampton [*sic*] in 2 Edward III, wherein it is contained that no man of whatsoever estate or condition shall go armed, lead an armed power in breach of the peace, or ride or pass armed by day or night in fairs, markets or elsewhere in the presence of the justices, the king's ministers or others under pain of losing his arms and of imprisonment at the king's will . . . .

4 Calendar Of The Close Rolls, Henry VI, 1441–1447, at 224 (May 12, 1444, Westminster) (A.E. Stamp ed., 1937).

John Carpenter's *The White Book of the City of London* published in 1419 — England's first common law treatise — documented the continuing authority of the Statute of Northampton. With narrow exceptions, Carpenter wrote, the law mandated that "no one, of whatever condition he be, go armed in the said city or in the suburbs, or carry arms, by day or by night." *Liber Albus: The White Book Of The City Of London* 335 (Henry Thomas Riley ed., 1861).

In 1541, under the second Tudor king, Henry VIII, Parliament enacted a statute to stop "shamefull murthers roberies felonyes ryotts and routs." 33 Hen. 8, c. 6, § 1 (1541–1542) (Eng.). The statute limited gun ownership to the wealthy — those who "have lands tents rents fees annuityes or Offices, to the yearley value of one hundred Pounds."

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33 Hen. 8, c. 6, § 2 (1541–1542) (Eng.). Of particular
importance to the case now before us, the statute expressly
forbade everyone, including the wealthy, from owning or
carrying concealable (not merely concealed) weapons, such
as "little shorte handguns and little hagbutts," and guns "not
of the lengthe of one whole Yarde or hagbutt or demyhake
beinge not of the lenghe of thre quarters of a Yarde." *Id.*; *see*
Lois G. Schwoerer, *To Hold and Bear Arms: The English
Perspective*, 76 Chi.-Kent L. Rev. 27, 35–37 (2000–2001)
(discussing the 1541 statute of Henry VIII and related laws).

A half-century later, Elizabeth I continued her father's
prohibition against concealed weapons. She issued a
proclamation in 1594 emphasizing that the Statute of
Northampton prohibited not just the "open carrying" of
weapons, but also the carrying of "a device to have secretly
small Dagges, commonly called pocket Dags." By The
Quenne Elizabeth I: A Proclamation Against the Carriage of
Dags, and for Reformation of Some Other Great Disorders
(London, Christopher Barker, 1594). Six years later, she
ordered "all Justices of the Peace" to enforce the Statute
"according to the true intent and meaning of the same," which
meant a prohibition on the "car[r]ying and use of Gunnes . . .
and especially of Pistols, Birding pieces, and other short
pieces and small shot" that could be easily concealed. By
The Quenne Elizabeth I: A Proclamation Prohibiting The Use
And Cariage Of Dagges, Birding Pieces, And Other Gunnes,
Contrary To Law 1 (London, Christopher Barker 1600).

The first Stuart king, James I, issued a proclamation in
1613, forbidding concealed weapons and reciting that the
"bearing of Weapons covertly . . . hath ever beene . . . straitly
forbidden":

> Whereas the bearing of Weapons *covertly*, and
> specially of short Dagges, and Pistols, (truly termed
> of their use, pocket Dagges, that are apparently made
> to be carried *close*, and *secret*) *hath ever beene*, *and
> yet is* by the Lawes and policie of this Realme *straitly
> forbidden* as carying with it inevitable danger in the
> hands of desperate persons . . . .

By The King James I: A Proclamation Against The Use Of
Pocket-Dags 1 (London, Robert Barker, 1613) (emphases
added).   Three years later, James I issued another
proclamation similar to Elizabeth I's, banning the sale,
wearing, and carrying of "Steelets, pocket Daggers, pocket
Dags and Pistols, which are weapons utterly unserviceable for
defence, Militarie practise, or other lawfull use, but odious,
and noted Instruments of murther, and mischiefe." By The
King James I: A Proclamation Against Steelets, Pocket
Daggers, Pocket Dagges and Pistols, *reprinted in* 1 Stuart
Royal Proclamations 359–60 (James F. Larkin & Paul L.
Hughes eds., 1973).

In the late 1600s, in *Sir John Knight's Case*, England's
Attorney General charged John Knight with violating the
Statute of Northampton by "walk[ing] about the streets armed
with guns." 87 Eng. Rep. 75 (K.B. 1686). After clarifying
that "the meaning of [the Statute of Northampton] was to
punish people who go armed to terrify the King's subjects,"
id., the Chief Justice acquitted Knight, but only because, as a
government official, he was exempt from the statute's
prohibition.

In 1694, Lord Coke described the Statute of Northampton
as providing that a man may neither "goe nor ride armed by
night nor by day . . . in any place whatsoever." *The Third*

Part of the Institutes of the Laws of England 160, ch. 73 (London, R. Brooke, 1797). Coke recounted the case of Sir Thomas Figett, who was arrested when he "went armed under his garments" before a justice of the King's bench. *Id*. at 161–62. William Hawkins wrote in 1716 that, under the Statute of Northampton, "a Man cannot excuse the wearing [of] such Armour in Publick, by alledging that such a one threatened him, and that he wears it for the Safety of his Person from his Assault." 1 William Hawkins, *A Treatise Of The Pleas Of The Crown* 489, ch. 28, § 8 (London, J. Curwood, 8th ed. 1824). Blackstone, writing in the 1760s, compared the Statute of Northampton to "the laws of Solon, [under which] every Athenian was finable who walked about the city in armour." 5 William Blackstone, *Commentaries on the Laws of England*, edited by St. George Tucker, 149 § 9 (Phila. 1803).

James II, the last of the Stuart kings and England's last Catholic monarch, sought to disarm his Protestant subjects. James II was driven from the throne in 1688 in the Glorious Revolution. In 1689, under his Protestant successors, William of Orange (William III) and Mary II, Parliament enacted the English Bill of Rights, which, as the Court in *Heller* recognized, has "long been understood to be the predecessor to our Second Amendment." *Heller*, 554 U.S. at 593. The Bill of Rights provided, with respect to the right to bear arms, "[t]hat the subjects which are Protestants may have arms for their defence suitable to their conditions and *as allowed by law*." 1 W. & M., c. 2, § 7, in 3 Eng. Stat. at Large 441 (1689) (emphasis added).

To the degree that the English Bill of Rights is an interpretive guide to our Second Amendment, the critical question is the meaning of the phrase "as allowed by law."

More narrowly, with respect to the case now before us, the specific question is whether the arms that are "allowed by law" — that is, the arms Protestants had the right to bear — included concealed firearms. The history just recounted demonstrates that carrying concealed firearms in public was not "allowed by law." Not only was it generally prohibited by the Statute of Northampton, but it was specifically forbidden by the statute enacted under Henry VIII, and by the later proclamations of Elizabeth I and James I.

The English writer, Granville Sharp, addressed this precise point in 1782. Sharp is a particularly important source, given that the Court in *Heller* cited his treatise as an authority supporting its understanding of the English Bill of Rights. *See Heller*, 554 U.S. at 594. According to Sharp, the phrase "as allowed by law" referred to pre-existing restrictions, including the statute passed under Henry VIII prohibiting concealed arms. Sharp wrote:

> [The] latter expression, "*as allowed by law*," respects the limitations in the above-mentioned act of 33 Hen. VIII c. 6, which restrain the use of *some particular sort of arms*, meaning only such arms as were liable to be *concealed*, or otherwise favour the designs of murderers, as "*cross-bows, little short hand-guns, and little hagbuts*," and all guns UNDER CERTAIN LENGTHS, specified in the act . . . .

Tracts, Concerning the Ancient and Only True Legal Means of National Defence, by a Free Militia 17–18 (3d ed. 1782) (emphasis in original).

Thus, by the end of the eighteenth century, when our Second Amendment was adopted, English law had for centuries consistently prohibited carrying concealed (and occasionally the even broader category of concealable) arms in public. The prohibition may be traced back generally to the Statute of Northampton in 1328, and specifically to the Act of Parliament under Henry VIII in 1541. The prohibition was continued in the English Bill of Rights, adopted in 1689, and was clearly explained by Granville Sharp in 1782, less than a decade before the adoption of the Second Amendment.

b. Right to Bear Arms in Colonial America

We have found nothing in the historical record suggesting that the law in the American colonies with respect to concealed weapons differed significantly from the law in England. In 1686, the New Jersey legislature, concerned about the "great abuses" suffered by "several people in the Province" from persons carrying weapons in public, passed a statute providing that "no person or persons . . . shall presume privately to wear any pocket pistol, skeins, stilladers, daggers or dirks, or other unusual or unlawful weapons within this Province." *An Act Against Wearing Swords, &c.*, N.J. Laws Chap. IX (1689). Other colonies adopted verbatim, or almost verbatim, English law. For example, in 1692, the colony and province of Massachusetts Bay authorized the Justice of the Peace to arrest those who "shall ride or go armed Offensively before any of Their Majesties Justices . . . or elsewhere, by Night or by Day, in Fear or Affray of Their Majesties Liege People." *An Act for the Punishing of Criminal Offenders*, Mass. Laws Chap. XI § 6 (1692).

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c. Right to Bear Arms in the States

The Supreme Court in *Heller* discussed state court decisions after the adoption of the Second Amendment on the ground that they showed how the Amendment — and the right to bear arms generally — was commonly understood in the years following its adoption. We recognize that these decisions are helpful in providing an understanding of what the adopters of the Second Amendment intended, but we postpone our discussion to the next section, for they are even more helpful in providing an understanding what the adopters of the Fourteenth Amendment intended.

2. History Relevant to the Fourteenth Amendment

a. Pre-amendment History

Following the lead of the Supreme Court in both *Heller* and *McDonald*, we look to decisions of state courts to determine the scope of the right to keep and bear arms as that right was understood by the adopters of the Fourteenth Amendment. With only one exception — and a short-lived exception at that — state courts before the Civil War unanimously concluded that members of the general public could be prohibited from carrying concealed weapons.

In *State v. Mitchell*, 3 Blackf. 229 (Ind. 1833), the Supreme Court of Indiana, in a one-sentence opinion, upheld a state statute prohibiting the general public from carrying concealed weapons: "It was *held* in this case, that the statute of 1831, prohibiting all persons, except travelers, from wearing or carrying concealed weapons, is not unconstitutional." *Id.* at 229 (emphasis in original).

In *State v. Reid*, 1 Ala. 612 (1840), the defendant had been convicted of violating a statute prohibiting any person from "carry[ing] concealed about his person, any species of fire arms, or any Bowie knife, Arkansaw tooth pick, or any other knife of the like kind, dirk, or any other deadly weapon." *Id.* at 614. The Supreme Court of Alabama upheld the statute against a challenge under the state constitution. It based its analysis in substantial part on its conclusion that the English Bill of Rights did not protect a right to carry concealed weapons. The court wrote:

> The evil which was intended to be remedied [by the English Bill of Rights] was a denial of the right of Protestants to have arms for their defence, and *not an inhibition to wear them secretly*.

Id. at 615 (emphasis added). The court defended the Alabama statute on practical as well as legal grounds:

> [A] law which is intended merely to promote personal security, and to put down lawless aggression and violence, and to that end inhibits the wearing of certain weapons, in such a manner as is calculated to exert an unhappy influence upon the moral feelings of the wearer, by making him less regardful of the personal security of others, does not come in collision with the constitution.

Id. at 617.

In *Aymette v. State*, 21 Tenn. 154 (1840), a jury convicted the defendant of wearing a bowie knife concealed under his

clothes. The defendant contended that the conviction violated a Tennessee constitutional provision stating that "free white men of this State have a right to keep and bear arms for their common defence." *Id.* at 156. The Tennessee Supreme Court interpreted the English Bill of Rights, as well as the Tennessee constitution, as protecting a group right to engage in military action rather than an individual right to self-defense:

> When, therefore, Parliament says that "subjects which are Protestants may have arms for their defence, suitable to their condition, as allowed by law," it does not mean for private defence, but, being armed, they may as a body rise up to defend their just rights, and compel their rulers to respect the laws.

Id. at 157. In the view of the court, concealable weapons did not come within the scope of either the English Bill of Rights or the state constitution:

> The Legislature, therefore, have a right to prohibit the wearing or keeping [of] weapons dangerous to the peace and safety of the citizens, and which are not usual in civilized warfare, or would not contribute to the common defence.

Id. at 159.

In *State v. Buzzard*, 4 Ark. 18, 19 (1842), the trial court quashed an indictment alleging violation of a state statute providing that "every person who shall wear any pistol, dirk,

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butcher or large knife, or a sword in a cane, concealed as a weapon, unless upon a journey, shall be adjudged guilty of a misdemeanor." The Arkansas Supreme Court reversed. Even though the Fourteenth Amendment had not yet been adopted, the court believed that the statute was properly challenged under both the Second Amendment and the state constitution. The court held that the statute violated neither the federal nor the state constitution.   In upholding the statute, Justice Dickinson wrote that the purpose of the two constitutional provisions was to provide "adequate means for the preservation and defense of the State and her republican institutions." *Id.* at 27.

> The act in question does not, in my judgment,
> detract anything from the power of the people
> to defend their free state and the established
> institutions of the country. It inhibits only the
> wearing of certain arms concealed.

Id.

In *Nunn v. State*, 1 Ga. 243 (1846), the defendant was charged with carrying a pistol, but the indictment did not specify that he carried it "secretly." An 1837 state statute criminalized carrying concealed weapons, but allowed open carry.   Like the Arkansas Supreme Court in *Buzzard*, the Georgia Supreme Court addressed the statute under both the Second Amendment and the state constitution.   The court discussed extensively the right to bear arms, writing that the Second Amendment

> assigns as a reason why this right [to keep and
> bear arms] shall not be interfered with, or in
> any manner abridged, that the free enjoyment

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> of it will prepare and qualify *a well-regulated militia*, which are necessary to the security of a free State.

Id. at 250 (emphasis in original). The court concluded that insofar as it prohibited the carrying of concealed weapons, the statute was constitutional:

> We are of the opinion . . . that so far as the act of 1837 seeks to suppress the practice of carrying certain weapons *secretly*, that it is valid, inasmuch as it does not deprive the citizen of his *natural* right of self-defense, or of his constitutional right to keep and bear arms.

Id. at 251 (emphasis in original). However, because the indictment failed to allege that the defendant had carried his pistol in a concealed manner, the court dismissed it. *See* Saul Cornell, *The Right to Carry Firearms Outside of the Home: Separating Historical Myths from Historical Realities*, 39 Fordham Urb. L.J. 1695, 1716–26 (2011–2012) (discussing *Nunn* and the emergence of public carry regulation outside the south).

In *State v. Chandler*, 5 La. Ann. 489 (1850), the defendant argued that the trial judge should have instructed the jury that it was not a crime in Louisiana to carry a concealed weapon because the Second Amendment guaranteed to citizens the right to bear arms. The Louisiana Supreme Court rejected the argument, holding that a law prohibiting concealed weapons did not violate the Amendment:

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> The [Louisiana statute] makes it a misdemeanor to be "found with a concealed weapon, such as a dirk, dagger, knife, pistol, or any other deadly weapon concealed in his bosom, coat, or any other place about him, that does not appear in full open view." This law became absolutely necessary to counteract a vicious state of society, growing out of the habit of carrying concealed weapons, and to prevent bloodshed and assassinations committed upon unsuspecting persons. It interfered with no man's right to carry arms . . . "in full open view," which places men upon an equality. This is the right guaranteed by the Constitution of the United States, and which is calculated to incite men to a manly and noble defence of themselves, if necessary, and of their country, without any tendency to secret advantages and unmanly assassinations.

Id. at 489–90.

The only exception to this otherwise uniform line of cases is *Bliss v. Commonwealth*, 12 Ky. 90 (1822), in which the Kentucky Court of Appeals, by a vote of two to one, struck down a state statute prohibiting the wearing of "a pocket pistol, dirk, large knife, or sword in a cane, concealed as a weapon." *Id.* at 90. The court held that the statute violated Article 10, § 23 of Kentucky's constitution, which provided that "the right of the citizens to bear arms in defense of themselves and the state, shall not be questioned." *Id.* The court wrote:

> [I]n principle, there is no difference between
> a law prohibiting the wearing concealed arms,
> and a law forbidding the wearing such as
> are exposed; and if the former be
> unconstitutional, the latter must be so
> likewise.

Id. at 92.

The court's decision in *Bliss* was soon attacked, and was overruled over a decade before the Civil War. In 1837, Governor James Clark, deeply concerned about the "bloodshed and violence" caused by concealed weapons in the wake of *Bliss*, called on the Kentucky legislature to pass a new statute banning the practice. The Kentucky legislative committee that received the Governor's message criticized the court for reading the state constitution too literally. *See* Robert M. Ireland, *The Problem of Concealed Weapons in Nineteenth-Century Kentucky*, 91 Reg. Ky. Hist. Soc'y 370, 373 (1993). In 1849, a Kentucky constitutional convention adopted without debate a provision authorizing the legislature to "pass laws to prevent persons from carrying concealed arms." Ky. Const. art. XIII, § 25. Then, in 1854, the Kentucky legislature passed a new statute prohibiting the concealed carry of "any deadly weapons other than an ordinary pocket knife." *An Act to prohibit the carrying of concealed weapons*, Mar. 10, 1853, Ky. Acts, Chap. 1020 (1854).

The Supreme Court stated in *Heller* that "*the majority* of the 19th-century courts to consider the question held that prohibitions on carrying concealed weapons were lawful under the Second Amendment or state analogues." 554 U.S. at 626 (emphasis added). The Court substantially understated

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the matter. As just noted, with the exception of *Bliss*, those pre-Civil War state courts that considered the question *all* upheld prohibitions against concealed weapons. Four of the six courts upholding prohibitions specifically discussed, and disagreed with, *Bliss*. *See Reid*, 1 Ala. at 617–20; *Aymette*, 21 Tenn. at 160–61; *Buzzard*, 4 Ark. at 25–26; *Nunn*, 1 Ga. at 247–48. Moreover, the two-to-one *Bliss* decision did not last. *Bliss* was decided in 1822; a state constitutional amendment was adopted in 1849 to overturn *Bliss*; the legislature then passed a statute in 1854 outlawing concealed weapons.

The Supreme Court wrote in *McDonald* that a "*clear majority* of the States in 1868 . . . recognized the right to keep and bear arms as being among the foundational rights necessary to our system of Government." 561 U.S. at 777 (emphasis added).   Based in substantial part on its understanding of the "clear majority" of states, the Court held that the adopters of the Fourteenth Amendment intended to incorporate the right to bear arms preserved by the Second Amendment. As just seen, an *overwhelming majority* of the states to address the question — indeed, after 1849, *all* of the states to do so — understood the right to bear arms, under both the Second Amendment and their state constitutions, as not including a right to carry concealed weapons in public.

b. Post-Amendment History

The Supreme Court in *Heller* discussed the decisions of early 19th century courts after the adoption of the Second Amendment, on the ground that they were relevant to understanding the intent of the eighteenth century adopters of the Amendment. 554 U.S. at 605–14. We follow the Court's lead with respect to the Fourteenth Amendment and discuss decisions after its adoption.

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The pre-Civil War consensus about the meaning of the right to keep and bear arms continued after the war and the adoption of the Fourteenth Amendment. The post-war constitutions of five states explicitly stated that the right to carry concealed weapons could be prohibited by the legislature. N.C. Const. of 1868, art. I, § 24 (1875) ("A well-regulated militia being necessary to the security of a free State, the right of the people to keep and bear arms shall not be infringed; . . . Nothing herein contained shall justify the practice of carrying concealed weapons, or prevent the Legislature from enacting penal statutes against said practice."); Colo. Const. art. II, § 13 (1876) ("The right of no person to keep and bear arms in defense of his home, person and property, or in aid of the civil power when thereto legally summoned, shall be called in question; but nothing herein contained shall be construed to justify the practice of carrying concealed weapons."); La. Const. of 1879, art. III ("A well regulated militia being necessary to the security of a free State, the right of the people to keep and bear arms shall not be abridged. This shall not prevent the passage of laws to punish those who carry weapons concealed."); Mont. Const. of 1889, art. II, § 12 ("The right of any person to keep or bear arms in defense of his own home, person, and property, or in aid of the civil power when thereto legally summoned, shall not be called in question, but nothing herein contained shall be held to permit the carrying of concealed weapons."); Miss. Const. art. III, § 12 (1890) ("The right of every citizen to keep and bear arms in defense of his home, person, or property, or in aid of the civil power when thereto legally summoned, shall not be called in question, but the legislature may regulate or forbid carrying concealed weapons."). *See generally* David B. Kopel, *The Second Amendment in the Nineteenth Century*, 1998 BYU L. Rev. 1359, 1410 n. 190 (1998).

The post-war constitutions of another six states, while not explicitly granting to the legislatures the authority to prohibit concealed weapons, gave state legislatures broad power to regulate the manner in which arms could be carried. *See* Ga. Const. of 1868, art. I, § 14 ("A well-regulated militia being necessary to the security of a free people, the right of the people to keep and bear arms shall not be infringed; but the general assembly shall have power to prescribe by law the manner in which arms may be borne."); Tex. Const. of 1868, art. I, § 13 ("Every person shall have the right to keep and bear arms in the lawful defence of himself or the State, under such regulations as the legislature may prescribe."); Tenn. Const. art. I, § 26 (1870) ("That the citizens of this State have a right to keep and to bear arms for their common defense; but the Legislature shall have power, by law, to regulate the wearing of arms with a view to prevent crime."); Fla. Const. of 1885, art. I, § 20 ("The right of the people to bear arms in defence of themselves and the lawful authority of the State, shall not be infringed, but the Legislature may prescribe the manner in which they may be borne."); Idaho Const. of 1889, art. I, § 11 ("The people have the right to bear arms for their security and defense; but the Legislature shall regulate the exercise of this right by law."); Utah Const. of 1896, art. I, § 6 ("The people have the right to bear arms for their security and defense, but the legislature may regulate the exercise of this right by law."). *See generally* Eugene Volokh, *State Constitutional Rights to Keep and Bear Arms*, 11 Tex. Rev. of L. & Pol. 191, 192–217 (2006) (collecting state constitutional provisions). In these states, the legislatures of Georgia and Tennessee had already passed statutes prohibiting concealed weapons, and the supreme courts of those states, in *Nunn* and *Aymette*, had already upheld the statutes against constitutional challenges. *Aymette*, 21 Tenn. 154; *Nunn*, 1 Ga. 243. As will be seen in a moment, the

Texas legislature would soon pass a statute prohibiting concealed weapons, and that statute, too, would be upheld.

Two state courts and one territorial court upheld prohibitions against carrying concealable (not just concealed) weapons in the years following the adoption of the Fourteenth Amendment. In *English v. State*, 35 Tex. 473 (1871), a Texas statute "regulating, and in certain cases prohibiting, the carrying of deadly weapons," including "pistols, dirks, daggers, slungshots, swordcanes, spears, brass-knuckles and bowie knives," *id.* at 474, was challenged under the Second Amendment, as well as under an analogous provision of the Texas constitution. The Texas Supreme Court upheld the statute. The court construed "arms" in the Second Amendment and the Texas constitution as referring only to weapons "used for purposes of war." *Id.* at 475. The court wrote:

> To refer the deadly devices and instruments called in the statute "deadly weapons," to the proper or necessary arms of a "well-regulated militia," is simply ridiculous. No kind of travesty, however subtle or ingenious, could so misconstrue this provision of the constitution of the United States, as to make it cover and protect that pernicious vice, from which so many murders, assassinations, and deadly assaults have sprung, and which it was doubtless the intention of the legislature to punish and prohibit.

Id. at 476.

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In *State v. Workman*, 14 S.E. 9, 11 (W. Va. 1891), a West Virginia statute prohibited carrying, whether openly or concealed, "any revolver or other pistol, dirk, bowie-knife, razor, slung-shot, billy, metallic or other false knuckles, or any other dangerous or deadly weapon of like kind or character." The statute exempted from the prohibition a person who is "a quiet and peaceable citizen, of good character and standing in the community in which he lives," and who had "good cause to believe, and did believe, that he was in danger of death or great bodily harm at the hands of another person." *Id.* at 9. Defendant was convicted under the statute because he failed to prove that he was of good character, despite the fact that he had been in clear and immediate danger of death from a particular individual. *Id.* at 10. Defendant contended that the statute violated the Second Amendment. *Id.* at 11. The West Virginia Supreme Court upheld the statute on the ground that the Amendment protected only the right to carry weapons of war:

> [I]n regard to the kind of arms referred to in the amendment, it must be held to refer to the weapons of warfare to be used by the militia, such as swords, guns, rifles, and muskets, — arms to be used in defending the state and civil liberty, — and not to pistols, bowie-knife, brass knuckles, billies, and such other weapons as are usually employed in brawls, street fights, duels, and affrays, and are only habitually carried by bullies, blackguards, and desperadoes, to the terror of the community and the injury of the state.

Id.

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In *Walburn v. Territory*, 59 P. 972 (Okla. 1899), the defendant was convicted of "carrying a revolver on his person." The Supreme Court of the Territory of Oklahoma sustained the law under which the defendant had been convicted. "[W]e are of the opinion that the statute violates none of the inhibitions of the constitution of the United States, and that its provisions are within the police power of the territory." *Id.* at 973.

Finally, and perhaps most importantly, in *Robertson v. Baldwin*, 165 U.S. 275 (1897), the United States Supreme Court made clear that it, too, understood the Second Amendment as not protecting the right to carry a concealed weapon. The Court wrote:

> [T]he first 10 amendments to the constitution, commonly known as the "Bill of Rights," were not intended to lay down any novel principles of government, but simply to embody certain guaranties and immunities which we had inherited from our English ancestors, and which had, from time immemorial, been subject to certain well-recognized exceptions, arising from the necessities of the case. In incorporating these principles into the fundamental law, there was no intention of disregarding the exceptions, which continued to be recognized as if they had been formally expressed. Thus . . . the right of the people to keep and bear arms (article 2) is not infringed by laws prohibiting the carrying of concealed weapons[.]

Id. at 281–82.

3. No Second Amendment Right to Carry Concealed Weapons

The historical materials bearing on the adoption of the Second and Fourteenth Amendments are remarkably consistent. Under English law, the carrying of concealed weapons was specifically prohibited since at least 1541. The acknowledged predecessor to the Second Amendment, the 1689 English Bill of Rights, protected the rights of Protestants to have arms, but only those arms that were "allowed by law." Concealed weapons were not "allowed by law," but were, instead, flatly prohibited. In the years after the adoption of the Second Amendment and before the adoption of the Fourteenth Amendment, the state courts that considered the question nearly universally concluded that laws forbidding concealed weapons were consistent with both the Second Amendment and their state constitutions. The only exception was Kentucky, whose court of appeals held to the contrary in a two-to-one decision based on its state constitution. Kentucky thereafter amended its constitution to overturn that result. In the decades immediately after the adoption of the Fourteenth Amendment, all of the state courts that addressed the question upheld the ability of their state legislatures to prohibit concealed weapons. Finally, the United States Supreme Court unambiguously stated in 1897 that the protection of the Second Amendment does not extend to "the carrying of concealed weapons." *Baldwin*, 165 U.S. at 282.

We therefore conclude that the Second Amendment right to keep and bear arms does not include, in any degree, the right of a member of the general public to carry concealed firearms in public. In so holding, we join several of our sister circuits that have upheld the authority of states to prohibit

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entirely or to limit substantially the carrying of concealed or
concealable firearms. *See Peterson v. Martinez*, 707 F.3d
1197 (10th Cir. 2013) (right to carry concealed weapons does
not fall within the Second Amendment's scope)*; Woollard v.
Gallagher*, 712 F.3d 865 (4th Cir. 2013) (Maryland
requirement that handgun permits be issued only to
individuals with "good and substantial reason" to wear, carry,
or transport a handgun does not violate Second Amendment);
Drake v. Filko, 724 F.3d 426, 429–30 (3d Cir. 2013) (New
Jersey "justifiable need" restriction on carrying handguns in
public "does not burden conduct within the scope of the
Second Amendment's guarantee"); *Kachalsky v. Cty. of
Westchester*, 701 F.3d 81 (2d Cir. 2012) (New York "proper
cause" restriction on concealed carry does not violate Second
Amendment).

Our holding that the Second Amendment does not protect
the right of a member of the general public to carry concealed
firearms in public fully answers the question before us.
Because the Second Amendment does not protect in any
degree the right to carry concealed firearms in public, any
prohibition or restriction a state may choose to impose on
concealed carry — including a requirement of "good cause,"
however defined — is necessarily allowed by the
Amendment. There may or may not be a Second Amendment
right for a member of the general public to carry a firearm
openly in public. The Supreme Court has not answered that
question, and we do not answer it here.

V. Intervention by the State of California

The State of California moved to intervene in *Peruta* after
Sheriff Gore of San Diego County declined to petition for
rehearing en banc. Plaintiffs did not oppose intervention by

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the State. As recounted at the beginning of this opinion, however, a divided panel denied the State's motion. We disagree and grant the motion.

Under Federal Rule of Civil Procedure 24(a)(2), a party may intervene as of right if

> (1) it has a significant protectable interest relating to the subject of the action; (2) the disposition of the action may, as a practical matter, impair or impede its ability to protect its interest; (3) the application is timely; and (4) the existing parties may not adequately represent its interest.

Day v. Apoliona, 505 F.3d 963, 965 (9th Cir. 2007) (citation, internal quotation marks, and alterations omitted); *see also* Fed. R. Civ. P. 24(a)(2).

There is no question that California has a significant interest in *Peruta* (and, indeed, in *Richards*). As the panel majority noted, Plaintiffs "focuse[d] [their] challenge on [the counties'] licensing scheme for concealed carry." *Peruta*, 742 F. 3d at 1171. But the panel majority construed the challenge as an attack on "the constitutionality of [California's] entire [statutory] scheme." *Id.* at 1171; *see also id.* at 1169 (assessing whether "*the California scheme* deprives any individual of his constitutional rights" (emphasis added)). While Plaintiffs' original challenge to the county policies did not appear to implicate the entirety of California's statutory scheme, the panel opinion unmistakably did.

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The panel opinion in *Peruta*, if left intact, would have substantially impaired California's ability to regulate firearms. A key premise of the opinion was that the Second Amendment requires the states to "permit *some form* of carry for self-defense outside the home." *Id.* at 1172 (emphasis in original). Though California's statutory scheme permits many residents, in many contexts, to carry a firearm outside the home, it does not permit law-abiding residents of sound mind to do so without a particularized interest in self-defense.

Under the circumstances presented here, we conclude that California's motion to intervene was timely. To determine whether a motion to intervene is timely, we consider "(1) the stage of the proceeding at which an applicant seeks to intervene; (2) the prejudice to other parties; and (3) the reason for and length of the delay." *United States v. Alisal Water Corp.*, 370 F.3d 915, 921 (9th Cir. 2004) (internal quotation marks omitted). We recognize that California sought to intervene at a relatively late stage in the proceeding. But the timing of California's motion to intervene did not prejudice Plaintiffs; indeed, Plaintiffs did not, and do not, oppose the State's intervention. Equally important, California had no strong incentive to seek intervention in *Peruta* at an earlier stage, for it had little reason to anticipate either the breadth of the panel's holding or the decision of Sheriff Gore not to seek panel rehearing or rehearing en banc.

Our conclusion that California's motion to intervene was timely is consistent with our decision in *Apoliona*, in which the State of Hawai'i had made an argument, as *amicus curiae* before the district court, that the defendants had chosen not to make. 505 F.3d at 964. The district court agreed with Hawai'i's argument, but we reversed, holding that the argument was foreclosed by circuit precedent. *Id.* Hawai'i

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moved to intervene as a party in order to file a petition for
rehearing en banc. *Id.* Notwithstanding the fact that "the
state was aware of the litigation and that the litigation had the
potential to affect its interests," we granted the motion. *Id.* at
966. Permitting Hawai'i to intervene, we wrote, "will not
create delay by injecting new issues into the litigation, but
instead will ensure that our determination of an already
existing issue is not insulated from review simply due to the
posture of the parties." *Id.* at 965 (citation, internal quotation
marks, and alterations omitted).

If we do not permit California to intervene as a party in
Peruta, there is no party in that case that can fully represent
its interests. At trial and on appeal, attorneys representing
Sheriff Gore ably defended San Diego County's
interpretation of the good cause requirement. But after the
panel decision was issued, Sheriff Gore informed the court
that he would neither petition for rehearing en banc nor
defend the county's position in en banc proceedings.
California then appropriately sought to intervene in order to
fill the void created by the late and unexpected departure of
Sheriff Gore from the litigation.

VI. Response to Dissents

Our colleagues Judges Callahan, Silverman and N.R.
Smith have each written dissenting opinions. We consider
Judge Callahan's opinion to be the principal dissent because
its argument provides an essential premise for the other two.

None of the dissents contends that there is a free-standing
Second Amendment right for a member of the general public
to carry a concealed weapon in public. Nor do they make any
effort to contradict or undermine any of the historical

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evidence showing that the carrying of concealed weapons was consistently forbidden in England beginning in 1541; was consistently forbidden in the American colonies; and was consistently forbidden by the states (with the sole and short-lived exception of Kentucky) both before and after the Civil War. Nor do they dispute that the United States Supreme Court in 1897 clearly stated that carrying concealed weapons was not protected by the Second Amendment. *Robertson v. Baldwin*, 165 U.S. 275, 281–82 (1897) ("[T]he right of the people to keep and bear arms (article 2) is not infringed by laws prohibiting the carrying of concealed weapons[.]").

The argument of the principal dissent begins with the premise that Plaintiffs, as members of the general public, have a Second Amendment right to carry firearms in public as a means of self-defense. Principal Diss. at 66. The principal dissent characterizes California's restrictions on open carry as effectively prohibiting open carry. It concludes that when California's restrictions on open and concealed carry are considered together, they violate the Second Amendment: "In the context of California's choice to prohibit open carry, the counties' policies regarding the licensing of concealed carry are tantamount to complete bans on the Second Amendment right to bear arms outside the home for self-defense, and are therefore unconstitutional." *Id.* at 69–70. Therefore, according to the principal dissent, California's restrictions on concealed carry violate the Second Amendment. Judge N.R. Smith's dissent agrees with this argument, emphasizing the "context" of Plaintiffs' challenge to California's restrictions on concealed carry.

The argument of the principal dissent is based on a logical fallacy. Even construing the Second Amendment as protecting the right of a member of the general public to carry

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a firearm in public (an issue we do not decide), and even assuming that California's restrictions on public open carry violate the Second Amendment so construed (an issue we also do not decide), it does not follow that California's restrictions on public concealed carry violate the Amendment.

As the uncontradicted historical evidence overwhelmingly shows, the Second Amendment does not protect, in any degree, the right of a member of the general public to carry a concealed weapon in public. The Second Amendment may or may not protect to some degree a right of a member of the general public to carry a firearm in public. If there is such a right, it is only a right to carry a firearm openly. But Plaintiffs do not challenge California's restrictions on open carry; they challenge only restrictions on concealed carry.

If there is a Second Amendment right of a member of the general public to carry a firearm openly in public, and if that right is violated, the cure is to apply the Second Amendment to protect that right. The cure is not to apply the Second Amendment to protect a right that does not exist under the Amendment.

VII. Agreement with the Concurrence

Our colleague Judge Graber concurs fully in our opinion, but writes separately "to state that, even if we assume that the Second Amendment applied to the carrying of concealed weapons in public, the provisions at issue would be constitutional." Graber, J., concurrence at 52. Even if we assume that the Second Amendment applies, California's regulation of the carrying of concealed weapons in public survives intermediate scrutiny because it "promotes a substantial government interest that would be achieved less

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effectively absent the regulation." *Id.* at 58 (internal quotation marks omitted). For the reasons given in our opinion, we do not need to reach the question addressed by the concurrence. But if we were to reach that question, we would entirely agree with the answer the concurrence provides.

Conclusion

We hold that the Second Amendment does not protect, in any degree, the carrying of concealed firearms by members of the general public. This holding resolves the Second Amendment question presented in this case. It also necessarily resolves, adversely to Plaintiffs, their derivative claims of prior restraint, equal protection, privileges and immunities, and due process. In light of our holding, we need not, and do not, answer the question of whether or to what degree the Second Amendment might or might not protect a right of a member of the general public to carry firearms openly in public.

We **AFFIRM** the judgments of the district courts in both cases.

GRABER, Circuit Judge, with whom THOMAS, Chief Judge, and MCKEOWN, Circuit Judge, join, concurring:

I concur fully in the majority opinion. I write separately only to state that, even if we assume that the Second Amendment applied to the carrying of concealed weapons in public, the provisions at issue would be constitutional. Three of our sister circuits have upheld similar restrictions under

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intermediate scrutiny. Such restrictions strike a permissible balance between "granting handgun permits to those persons known to be in need of self-protection and precluding a dangerous proliferation of handguns on the streets." *Woollard v. Gallagher*, 712 F.3d 865, 881 (4th Cir. 2013); *see also Drake v. Filko*, 724 F.3d 426, 431–32 (3d Cir. 2013) (assuming that the Second Amendment applies and upholding New Jersey's "justifiable need" restriction on carrying handguns in public); *Kachalsky v. County of Westchester*, 701 F.3d 81, 89, 97 (2d Cir. 2012) (assuming that the Second Amendment applies and upholding New York's "proper cause" restriction on the concealed carrying of firearms). If restrictions on concealed carry of weapons in public are subject to Second Amendment analysis, we should follow the approach adopted by our sister circuits.

Judge Silverman's dissent acknowledges the "significant, substantial, and important interests in promoting public safety and reducing gun violence." (Dissent at 81–82.)   He contends, though, that Defendants have failed to demonstrate "a reasonable fit" between the challenged licensing criteria and the government's objectives. (Dissent at 82.) I disagree.

Judge Silverman points to evidence cited by two amici "showing that concealed-carry license holders are disproportionately *less* likely to commit crimes . . . than the general population." (Dissent at 83–84 (citing Amicus Brief for the Governors of Texas, Louisiana, Maine, Mississippi, Oklahoma, and South Dakota at pp. 10–15; and Amicus Brief for International Law Enforcement Educators and Trainers Association, et al., at pp. 22–26.)) There are, however, at least two reasons to question the relevance of those studies.

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First, even accepting Judge Silverman's premise, lawmakers are entitled to weigh the severity of the risk as well as the likelihood of its occurrence. Indeed, examples abound of "law-abiding citizens" in the seven states studied who place the public safety in jeopardy. In Florida, a state touted in the second of the cited amicus briefs, a "law-abiding" holder of a concealed-weapons permit shot and killed another person in 2014 in a movie theater after an argument over texting and popcorn. Amicus Brief for the Law Center to Prevent Gun Violence and Marin County Sheriff Robert Doyle In Support of Appellees' Petition for Rehearing En Banc at 13. Two years earlier, another concealed-carry permit holder in Florida fatally shot someone after an argument over loud music in a gas station's parking lot. *Id.* In Arizona, a qualified handgun carrier shot 19 people, including a congresswoman and a federal judge, outside a supermarket in 2011. *Id.* Those shooters all were legally entitled to carry their concealed firearms, which they used to kill others. Sadly, those incidents are not anomalies. Nationwide, since May 2007, concealed-carry permit holders have shot and killed at least 17 law enforcement officers and more than 800 private citizens—including 52 suicides. *Concealed Carry Killers*, Violence Policy Center, www.concealedcarrykillers.org (last visited Apr. 6, 2016). Thus, even if we assume that each and every one of those tragedies was less likely to occur because of the shooter's prior status as a "law-abiding citizen," that does not mean that a state legislature's regulation of concealed carry fails to address the problem in a reasonable way.

Second, to the extent that concealed-carry license holders are, in fact, less likely to commit crimes, their relative peacefulness may result from (and not exist in spite of) the restrictions that are disputed in this case. For example, in

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Delaware, five upstanding citizens must swear that carrying a concealed deadly weapon is necessary for the protection of the applicant, the applicant's property, or both. 11 Del. Code Ann. § 1441(a)(2). In Maryland, the applicant must show that he or she has a "good and substantial reason to wear, carry, or transport a handgun." Md. Code Ann., Pub. Safety § 5-306(a)(6)(i). In Hawaii, a concealed-carry permit may be issued only "[i]n an exceptional case, when an applicant shows reason to fear injury to the applicant's person or property." Haw. Rev. Stat. § 134-9(a). In New York, a person seeking a license to carry a concealed handgun must show "proper cause," N.Y. Penal Law § 400.00(2)(f); and, in New Jersey, the applicant must demonstrate "that he has a justifiable need to carry a handgun," N.J. Stat. Ann. § 2C:58-4. Rhode Island and the District of Columbia require the applicant to show that he or she is a "suitable person" and has a "reason," such as "fear[ing] an injury to his or her person or property," for carrying a firearm. Mass. Gen. Laws ch. 140, § 131(d); 1956 R.I. Gen. Laws § 11-47-11(a); D.C. Code § 22-4506(a). In other words, it may be the heightened restrictions on concealed-carry permits in many jurisdictions—the very provisions challenged in this case—that cause statistically reduced violence by permit holders.

Of equal importance, the studies to which Judge Silverman alludes are not the only side of the story. Much respected evidence is to the contrary. Several studies suggest that "the clear majority of states" that enact laws broadly allowing concealed carrying of firearms in public "experience *increases* in violent crime, murder, and robbery when [those] laws are adopted." John J. Donohue, *The Impact of Concealed-Carry Laws*, *in* Evaluating Gun Policy Effects on Crime and Violence 287, 320 (2003), *available at*

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http://www.brookings.edu/~/media/press/books/2003/evalu
atinggunpolicy/evaluatinggunpolicy_chapter.pdf; *see also*
Jens Ludwig, *Concealed-Gun-Carrying Laws and Violent
Crime: Evidence from State Panel Data*, 18 Int'l Rev. L. &
Econ. 239, 239 (1998) (noting that laws broadly allowing
concealed carrying of weapons "have resulted, if anything, in
an *increase* in adult homicide rates"), *available at*
http://home.uchicago.edu/ludwigj/papers/IJLE-
ConcealedGunLaws-1998.pdf; David McDowall et al.,
*Easing Concealed Firearms Laws: Effects on Homicide in
Three States*, 86 J. Crim. L. & Criminology 193, 202–03
(1995) (noting that, in the aftermath of relaxed concealed-
carry laws, "firearms homicides increased" while "homicides
without guns remained steady," and concluding that weaker
firearms regulation may "raise levels of firearms murders"),
available at http://scholarlycommons.law.northwestern.edu/
cgi/viewcontent.cgi?article=6855&context=jclc.

Similarly, some studies suggest that "policies to
discourage firearms in public may help prevent violence."
McDowall et al., *Easing Concealed Firearms Laws* at 203.
A study of prisoners incarcerated for gun offenses, for
example, found that two-thirds of those prisoners "reported
that the chance of running into an armed victim was very or
somewhat important in their own choice to use a gun." Philip
Cook et al., *Gun Control After Heller: Threats and
Sideshows From a Social Welfare Perspective*, 56 U.C.L.A.
L. Rev. 1041, 1081 (2009). The study continues:

> Currently, criminals use guns in only about 25
> percent of noncommercial robberies and 4
> percent of assaults. If increased gun carrying
> among potential victims causes criminals to
> carry guns more often themselves, or become

> quicker to use guns to avert armed self-defense, the end result could be that street crime becomes more lethal.

Id. (footnote omitted).

Clearly, social scientists disagree about the practical effect of modest restrictions on concealed carry of firearms. In the face of that disagreement, and in the face of inconclusive evidence, we must allow the government to select among reasonable alternatives in its policy decisions. As the Second Circuit explained, in upholding a requirement that an applicant show an objective threat to personal safety, or a special need for self-protection, to obtain a concealed-carry license for a handgun:

> To be sure, we recognize the existence of studies and data challenging the relationship between handgun ownership by lawful citizens and violent crime. We also recognize that many violent crimes occur without any warning to the victims. But New York also submitted studies and data demonstrating that widespread access to handguns in public increases the likelihood that felonies will result in death and fundamentally alters the safety and character of public spaces. It is the legislature's job, not ours, to weigh conflicting evidence and make policy judgments.

Kachalsky, 701 F.3d at 99; *see also Woollard*, 712 F.3d at 876–82 (detailing the reasons why Maryland's law, requiring a good and substantial reason to carry a concealed firearm in

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public, advances the government's important public safety objectives); *Drake*,724 F.3d at 439 (noting that "conflicting empirical evidence . . . does not suggest, let alone compel, a conclusion that the 'fit' between [a state's] individualized, tailored approach and public safety is not 'reasonable'").

Defendants must show only that the regulation "promotes a 'substantial government interest that would be achieved less effectively absent the regulation,'" not that the chosen regulation is the "least restrictive means" of achieving the government's important interest. *Fyock v. City of Sunnyvale*, 779 F.3d 991, 1000 (9th Cir. 2015) (quoting *Colacurcio v. City of Kent*, 163 F.3d 545, 553 (9th Cir. 1998)); *see also United States v. Marzzarella*, 614 F.3d 85, 98 (3d Cir. 2010) (stating that the fit need only "be reasonable, not perfect"). In examining reasonableness, we "must accord substantial deference to the predictive judgments" of legislative bodies, *Turner Broad. Sys., Inc. v. FCC*, 512 U.S. 622, 665 (1994), and the government must be allowed to experiment with solutions to serious problems, *Jackson v. City of San Francisco*, 746 F.3d 953, 966 (9th Cir. 2014) (citing *City of Renton v. Playtime Theatres, Inc.*, 475 U.S. 41, 52 (1986)).

Finally, despite Judge Silverman's argument to the contrary, California's decision to confer permit discretion on its counties is not an arbitrary one. Localizing the decision allows closer scrutiny of the interests and needs of each community, increasing the "reasonable fit" between the level of restriction and local conditions and decreasing the extent of the restriction that otherwise would apply, statewide, in places that do not require it. Similarly, localizing the decision allows more careful and accurate consideration of each individual's license application. California entrusts the decision-making responsibility to local law enforcement

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officials because they are best positioned to evaluate the potential dangers that increasing or decreasing concealed carry would have in their communities. This structure allows for a nuanced assessment of the needs of each locality in processing applications for concealed carry. In short, California's decision to place licensing in local hands is itself reasonable.

In sum, even if the Second Amendment applied to concealed carry of firearms in public, the challenged laws and actions by Defendants survive heightened scrutiny. No constitutional violation occurred.

CALLAHAN, Circuit Judge, dissenting, in which SILVERMAN, Circuit Judge, joins as to all parts except section IV, BEA, Circuit Judge, joins, and N.R. SMITH, Circuit Judge, joins as to all parts except section II.B:

The Second Amendment is not a "second-class" constitutional guarantee. *See McDonald v. City of Chicago*, 561 U.S. 742, 780 (2010). In the watershed case *District of Columbia v. Heller*, 554 U.S. 570 (2008), the Supreme Court held that the Second Amendment codified an existing individual right to keep and bear arms for self-defense. Two years later, the Court reaffirmed *Heller* in *McDonald*, 561 U.S. at 742, and held that the individual right to bear arms for self-defense under the Second Amendment was fundamental and applied to the states. Although these opinions specifically address firearms in the home, any fair reading of *Heller* and *McDonald* compels the conclusion that the right to keep and bear arms extends beyond one's front door. Like the rest of the Bill of Rights, this right is

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indisputably constitutional in stature and part of this
country's bedrock.

Plaintiffs assert that the counties' concealed weapons
licensing schemes, in the context of California's regulations
on firearms, obliterate their right to bear arms for self-defense
in public. The Supreme Court in *Heller* addressed concealed-
carry restrictions and instructed that those restrictions be
evaluated in context with open-carry laws to ensure that the
government does not deprive citizens of a constitutional right
by imposing incremental burdens. *Heller*, 554 U.S. at 629.
In the context of present-day California law, the Defendant
counties' limited licensing of the right to carry concealed
firearms is tantamount to a total ban on the right of an
ordinary citizen to carry a firearm in public for self-defense.
Thus, Plaintiffs' Second Amendment rights have been
violated. While states may choose between different manners
of bearing arms for self-defense, the right must be
accommodated.

The majority sets up and knocks down an elaborate straw
argument by answering only a narrow question—whether the
Second Amendment protects a right to carry concealed
firearms in public. But this approach is contrary to *Heller*,
and contrary to the prescribed method for evaluating and
protecting broad constitutional guarantees. Indeed, the
majority's lengthy historical analysis fails to appreciate that
many of its cited cases either presumed a right to openly carry
a firearm in public or relied on a pre-*Heller* interpretation of
the Second Amendment. Because the majority eviscerates
the Second Amendment right of individuals to keep and bear
arms as defined by *Heller* and reaffirmed in *McDonald*, I
respectfully dissent.

I. The Individual Right to Bear Arms Extends Beyond the Home

A. *Under Heller and McDonald, the individual right to bear arms for self-defense extends beyond the home*

Our analysis begins with the text of the Second Amendment and the Supreme Court's opinions in *Heller* and *McDonald*, which instruct that the right to bear arms extends beyond the home.

The Second Amendment guarantees "the right of the people to keep and bear Arms." U.S. Const. amend. II. *Heller* held that the Second Amendment conferred an individual right to keep and bear arms for self-defense. 554 U.S. at 595. Indeed, *Heller* adopted Justice Ginsburg's definition of "carries a firearm" to mean "wear, bear, or carry . . . upon the person or in the clothing or in a pocket, for the purpose . . . of being armed and ready for offensive or defensive action in a case of conflict with another person." *Id.* at 584 (quoting *Muscarello v. United States*, 524 U.S. 125, 143 (1998) (Ginsburg, J., dissenting)). *McDonald* affirmed that the constitutional right to keep and bear arms applies to the states. *McDonald*, 561 U.S. at 778 ("[T]he Framers and ratifiers of the Fourteenth Amendment counted the right to keep and bear arms among those fundamental rights necessary to our system of ordered liberty.").

Heller and *McDonald* also instruct that the right to bear arms exists outside the home. Under these cases, the Second Amendment secures "an individual right protecting against both public and private violence," indicating that the right extends in some form to locations where a person might become exposed to public or private violence. *See Heller*,

554 U.S. at 594. The Court reinforced this view by noting that the need for the right is "most acute" in the home, *id.* at 628, thus implying that the right exists outside the home. *See also McDonald*, 561 U.S. at 780 ("[T]he Second Amendment protects a personal right to keep and bear arms for lawful purposes, most notably for self-defense within the home."). *Heller* also identifies "laws forbidding the carrying of firearms in sensitive places such as schools and government buildings" as presumptively lawful. 554 U.S. at 626. Were the right to self-defense confined to the home, the validity of such laws would be self-evident.

The history of the Second Amendment also indicates that the right to bear arms applies outside the home. The common-law "right of having and using arms for self-preservation and defence," according to Blackstone, protected "the natural right of resistance and self-preservation." 1 William Blackstone, *Commentaries* *144. Blackstone's Commentaries also made clear that Congress would exceed its authority were it to "pass a law prohibiting any person from bearing arms." 1 William Blackstone & St. George Tucker, *Blackstone's Commentaries: With Notes of Reference, to the Constitution and Laws, of the Federal Government of the United States; and of the Commonwealth of Virginia* 289 (St. George Tucker ed., 1803). Furthermore, the majority of Nineteenth Century courts agreed that the Second Amendment right extended outside the home and included, at minimum, the right to carry an operable weapon in public for the purpose of lawful self-defense.[1] Although

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[1] See Judge O'Scannlain's comprehensive analysis of the historical underpinnings of the Second Amendment's right to some form of carry for self-defense outside the home set forth in *Peruta v. Cty. of San Diego*, 742 F.3d 1144 (9th Cir. 2014), *vacated* 781 F.3d 1106 (2015).

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some courts approved limitations on the manner of carry outside the home, none approved a total destruction of the right to carry in public.

Our sister circuits either have agreed that the Second Amendment right to bear arms extends outside the home or have assumed that the right exists. *See Drake v. Filko*, 724 F.3d 426, 431 (3d Cir. 2013) (recognizing that "the Second Amendment's individual right to bear arms *may* have some application beyond the home"); *Woollard v. Gallagher*, 712 F.3d 865, 876 (4th Cir. 2013) (assuming without deciding "that the *Heller* right exists outside the home"); *Moore v. Madigan*, 702 F.3d 933, 937 (7th Cir. 2012) ("To confine the right to be armed to the home is to divorce the Second Amendment from the right of self-defense described in *Heller* and *McDonald*."); *Kachalsky v. Cty. of Westchester*, 701 F.3d 81, 89 & n.10 (2d Cir. 2012) (noting that "[t]he plain text of the Second Amendment does not limit the right to bear arms to the home," and assuming that the Amendment has "some application" in the context of public possession of firearms (emphasis omitted)). Notably, the majority does not refute this analysis, hedging that "[t]he Second Amendment may or may not protect, to some degree, a right of a member of the general public to carry firearms in public." Maj. Op. 19. Thus, pursuant to *Heller* and *McDonald*, an individual's right to self-defense extends outside the home and includes a right to bear arms in public in *some* manner.

B. *States may choose between different manners of bearing arms for self-defense so long as the right to bear arms for self-defense is accommodated*

Heller balances the Second Amendment right to bear arms in public with a state's ability to choose between

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regulating open carry or concealed carry. *Heller* first noted that laws prohibiting concealed carry were examples of how the right secured by the Second Amendment was not a right to keep and carry any weapon whatsoever in any manner whatsoever and for whatever purpose:

> Like most rights, the right secured by the Second Amendment is not unlimited. From Blackstone through the 19th-century cases, commentators and courts routinely explained that the right was not a right to keep and carry any weapon whatsoever in any manner whatsoever and for whatever purpose. See, *e.g.*, *Sheldon*, in 5 Blume 346; Rawle 123; Pomeroy 152–153; Abbott 333. For example, the majority of the 19th-century courts to consider the question held that prohibitions on carrying concealed weapons were lawful under the Second Amendment or state analogues. See, *e.g.*, *State v. Chandler*, 5 La. Ann., at 489–490; *Nunn v. State*, 1 Ga., at 251; see generally 2 Kent *340, n. 2; The American Students' Blackstone 84, n. 11 (G. Chase ed. 1884). Although we do not undertake an exhaustive historical analysis today of the full scope of the Second Amendment, nothing in our opinion should be taken to cast doubt on longstanding prohibitions on the possession of firearms by felons and the mentally ill, or laws forbidding the carrying of firearms in sensitive places

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> such as schools and government buildings, or
> laws imposing conditions and qualifications
> on the commercial sale of arms.

554 U.S. at 626–27.

Importantly, while the Court enumerated four presumptively lawful "longstanding prohibitions," it did not list prohibitions of concealed weapons as one of them. Instead, the Court identified concealed weapons prohibitions as an example of regulating the *manner* in which individuals can exercise their right to keep and carry a firearm for self-defense. The Court further noted that a prohibition on carrying concealed handguns in conjunction with a prohibition of open carry of handguns would destroy the right to bear and carry arms:

> Few laws in the history of our Nation have
> come close to the severe restriction of the
> District's handgun ban. And some of those
> few have been struck down. In *Nunn v. State*,
> the Georgia Supreme Court struck down a
> prohibition on carrying pistols openly (even
> though it upheld a prohibition on carrying
> concealed weapons). See 1 Ga., at 251. In
> *Andrews v. State*, the Tennessee Supreme
> Court likewise held that a statute that forbade
> openly carrying a pistol "publicly or privately,
> without regard to time or place, or
> circumstances," 50 Tenn., at 187, violated the
> state constitutional provision (which the court
> equated with the Second Amendment). That
> was so even though the statute did not restrict
> the carrying of long guns. *Ibid.* See also

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> *State v. Reid*, 1 Ala. 612, 616–617 (1840) ("A statute which, under the pretence of regulating, amounts to a destruction of the right, or which requires arms to be so borne as to render them wholly useless for the purpose of defence, would be clearly unconstitutional").

Id. at 629.

In sum, *Heller* indicates that concealed-weapons prohibitions may be proper as long as individuals retain other means to exercise their Second Amendment right to bear arms for self-defense. However, where other ways of exercising one's Second Amendment right are foreclosed, a prohibition on carrying concealed handguns constitutes a "severe restriction" on the Second Amendment right, just like the District of Columbia's unconstitutional handgun ban in *Heller*.

II. Given California's Choice to Prohibit Open Carry, the Counties' Policies of Not Allowing for Concealed Carry for Self-Defense are Unconstitutional

As the Plaintiffs have some right to carry a firearm in public for self-defense, the next task is to determine whether the counties' policies, in light of the state's open-carry restrictions, are constitutional. We have held (and the majority does not hold otherwise) that when a law burdens conduct falling within the scope of the Second Amendment's guarantee, a two-step inquiry is appropriate. *Jackson v. City & Cty. of San Francisco*, 746 F.3d 953, 963 (9th Cir. 2014). "The two-step inquiry we have adopted '(1) asks whether the challenged law burdens conduct protected by the Second

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Amendment and (2) if so, directs courts to apply an appropriate level of scrutiny.'" *Id.* at 960 (quoting *United States v. Chovan*, 735 F.3d 1127, 1136 (9th Cir. 2013)).

A. *Procedural posture and California's gun control regime*

First, we consider the posture of this case in the context of California's concealed- and open-carry laws. The *Richards* Plaintiffs filed suit in May 2009, and the *Peruta* Plaintiffs filed suit in October 2009. Both plaintiff groups challenged their respective counties' concealed weapons licensing policies under the Second Amendment.

California prohibits an individual from carrying a concealed handgun in public. Cal. Penal Code § 25400 (prohibiting concealed carry of a loaded firearm in public). There are exceptions to this prohibition on concealed carry, including for peace officers, military personnel, and persons in private security. *Id.* §§ 25450, 25620, 25630, 25650. There are also exceptions for persons engaged in particular activities, such as hunting. *Id.* § 25640.

A member of the general public, however, cannot carry a concealed handgun without a concealed-weapons license. The sheriff of a county may issue an applicant a license to lawfully carry a concealed handgun in the city or county in which that applicant works or resides. *Id.* §§ 26150, 26155. However, the applicant must be a resident of (or spend substantial time in) the county in which he or she applies, pass a background check, take a firearms course, demonstrate good moral character, and demonstrate "good cause." *Id.* §§ 26150, 26155, 26165.

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The counties' interpretation of "good cause" is a focal point in this case. Both counties define "good cause" as requiring a particular need. San Diego County defines "good cause" as "a set of circumstances that distinguish[es] the applicant from the mainstream and causes him or her to be placed in harm's way." Similarly, Yolo County's written policy requires "valid" reasons for requesting a license. Importantly, under both policies a general desire for self-protection and protection of family does not constitute "good cause."

In upholding the counties' restrictions, the district courts relied on the fact that, at that time, California permitted unloaded open carry of handguns under then Penal Code § 12031(g). Thus, the district courts found that the counties' licensing schemes did not substantially burden the right to bear arms for self-defense. *Peruta v. Cty. of San Diego*, 758 F. Supp. 2d 1106, 1114 (S.D. Cal. 2010) ("As a practical matter, should the need for self-defense arise, nothing in section 12031 restricts the open carry of unloaded firearms and ammunition ready for instant loading."); *Richards v. Cty. of Yolo*, 821 F. Supp. 2d 1169, 1175 (E.D. Cal. 2011) ("Under the statutory scheme, even if Plaintiffs are denied a concealed weapon license for self-defense purposes from Yolo County, they are still more than free to keep an unloaded weapon nearby their person, load it, and use it for self-defense in circumstances that may occur in a public setting.").

However, during the pendency of these appeals, California repealed its open-carry law, and enacted broad legislation prohibiting open carry of handguns in public

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places. AB 144, 2011–12 Leg., 2011–12 Sess. (Cal. 2011).**²**
Thus, California now generally prohibits individuals from
openly carrying a handgun—whether loaded or unloaded—in
public locations. *See* Cal. Penal Code § 25850 (prohibiting
carry of a loaded firearm); *id.* § 26350 (prohibiting open carry
of an unloaded firearm).**³**

    B. *In the context of California's ban on open carry, the
       counties' ban on concealed carry for self-defense is
       unconstitutional*

In the context of California's choice to prohibit open
carry, the counties' policies regarding the licensing of

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² AB 144 provided, among other things, that "[a] person is guilty of
openly carrying an unloaded handgun when that person carries upon his
or her person an exposed and unloaded handgun outside a vehicle while
in or on any of the following: (A) A public place or public street in an
incorporated city or city and county. (B) A public street in a prohibited
area of an unincorporated area of a county or city and county. (C) A public
place in a prohibited area of a county or city and county." Cal. Penal
Code § 26350(a)(1).

³ There are exceptions. California law permits (1) possession of a
loaded or unloaded firearm at a person's place of residence, temporary
residence, campsite, on private property owned or lawfully possessed by
the person, or within the person's place of business, Cal. Penal Code
§§ 25605, 26035, 26055; (2) the transportation or carrying of any pistol,
revolver, or other firearm capable of being concealed upon the person
within a motor vehicle, unloaded and locked in the vehicle's trunk or in
a locked container in the vehicle, and carrying the firearm directly to or
from any motor vehicle within a locked container, *id.* §§ 25505, 25610,
25850; (3) carrying a loaded or unloaded firearm in some unincorporated
areas, *id.* §§ 25850(a), 26350(a); and (4) carrying a loaded firearm where
the person reasonably believes that any person or the property of any
person is in immediate, grave danger and that the carrying of the weapon
is necessary for the preservation of that person or property, *id.* § 26045.

concealed carry are tantamount to complete bans on the Second Amendment right to bear arms outside the home for self-defense, and are therefore unconstitutional.

Heller defined the right to bear arms as the right to be "armed and ready for offensive or defensive action in a case of conflict with another person." *Heller*, 554 U.S. at 584 (quoting *Muscarello*, 524 U.S. at 143 (Ginsburg, J., dissenting)). Here, California has chosen to ban open carry but grants its citizens the ability to carry firearms in public through county-issued concealed weapons licenses. Thus, in California, the only way that the average law-abiding citizen can carry a firearm in public for the lawful, constitutionally protected purpose of self-defense is with a concealed-carry license. And in San Diego and Yolo Counties that option has been taken off the table. Both policies specify that concern for one's personal safety alone does not satisfy the "good cause" requirement for issuance of a license.

California's exceptions to the general prohibition against public carry do little to protect an individual's right to bear arms in public for self-defense. The exceptions for particular groups of law enforcement officers and military personnel do not protect the average citizen. Bearing arms on private property and at places of business does not allow citizens to protect themselves by bearing arms in public. And the exceptions for "making or attempting to make a lawful arrest" or for situations of "immediate, grave danger" offer no solace to an individual concerned about protecting self and family from unforeseen threats in public.

Here, as in *Heller*, the exceptions are limited and do not adequately allow the ordinary citizen to exercise his or her right to keep and bear arms for self-defense within the

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meaning of the Second Amendment, as defined by the Supreme Court. Thus, the counties' concealed-carry policies in the context of California's open-carry ban obliterate the Second Amendment's right to bear a firearm in some manner in public for self-defense. *See also Moore v. Madigan*, 702 F.3d 933, 936–42 (7th Cir. 2012) (striking down the open-and-concealed-carry regulatory regime in Illinois because the state failed to justify "so substantial a curtailment of the right of armed self-defense").

C. *If the counties' policies were not a ban, remand to the district courts would be appropriate*

Even if the counties' policies in light of the California laws prohibiting open carry were not tantamount to complete bans, the proper remedy would be to remand to the district courts. The district courts did not have the benefit of our recent case law applying our Second Amendment framework. *See Jackson*, 746 F.3d at 963; *Chovan*, 735 F.3d at 1130. Additionally, the underlying statutory scheme has changed dramatically since the district courts' decisions. At the time the district courts rendered their decisions, California permitted unloaded open carry, a fact that both district courts relied upon to find that the counties' policies did not substantially burden any Second Amendment rights. *See Peruta*, 758 F. Supp. 2d at 1114–15; *Richards*, 821 F. Supp. 2d at 1175. However, open carry is now effectively prohibited.

Furthermore, reasonable jurists might find triable issues of material fact as to whether the policies substantially burden the right to carry a firearm in public for self-defense, whether there are open alternative channels to bear arms for self-defense, whether there are sufficient governmental interests

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that justify some of the restrictions, and whether the restrictions are sufficiently tailored to those interests. *See Jackson*, 746 F.3d at 963; *Chovan*, 735 F.3d at 1127. Thus, if the counties' policies are to be upheld, in whole or in part, the parties ought to have the opportunity to present evidence as to these issues, and the district court ought to have the opportunity to consider this evidence under the correct framework.[4]

Instead of remanding, the concurrence would hold that the concealed-weapons restrictions here survive intermediate scrutiny. The concurrence follows the approach of the Second, Third, and Fourth Circuits, which have held that states may limit the right to bear arms to persons who show good cause or meet a similar elevated standard. But the analyses in these cases are questionable as they rely on pre-*Heller* interpretations of the Second Amendment.[5] Even if

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[4] On a remand, I would apply heightened scrutiny. *Jackson*, 746 F.3d at 964 (noting that a "severe burden" on the Second Amendment right "requires [a] higher level of scrutiny"); *see also Ezell v. City of Chicago*, 651 F.3d 684, 691–92, 708 (7th Cir. 2011) (applying "rigorous" review "if not quite 'strict scrutiny'" to law that required firing range training prior to gun ownership but then banned all firing ranges).

[5] For example, in *Drake*, the Third Circuit upheld New Jersey's requirement that prior to receiving a license to carry a gun, either openly or concealed, an applicant had to show a "justifiable need." 724 F.3d at 428. The court held that restrictions on concealed weapons are "longstanding regulation[s] that enjoy[] presumptive constitutionality," and thus "regulate[] conduct falling outside the scope of the Second Amendment's guarantee." *Id.* at 434. *Drake* noted that New Jersey courts had upheld the restriction of gun permits in *Siccardi v. State*, 284 A.2d 533, 538 (N.J. 1971), and *Siccardi*, in turn, relied on *Burton v. Sills*, 248 A.2d 521, 525–26 (N.J. 1968). *Drake*, 724 F.3d at 432. *Burton*, however, erroneously held that the Second Amendment referred only to the collective right of the people to keep and bear arms, and not an

Heller and *McDonald* are seen as a departure from any prior understanding of the Second Amendment, they are law and remain binding upon us.

III.   The Majority Errs By Ignoring California's Choice to Ban Open Carry and Focusing Myopically on the Counties' Bans on Concealed Carry

The majority's opinion is not in accord with our usual approach to broadly defined constitutional rights, and fails to appreciate the context in which the Plaintiffs' challenges to the counties' policies arise. Moreover, its historical analysis is largely irrelevant because it again fails to appreciate the contexts in which the cited cases arose.

A. *Courts review a law's constitutionality in that law's larger context, just as the Supreme Court did in Heller*

A holistic approach to evaluating concealed weapons laws in context of the open-carry laws comports with how courts have evaluated other laws that allegedly infringed on

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individual right to self-defense. *Burton*, 248 A.2d at 526 ("As the language of the [Second] [A]mendment itself indicates it was not framed with individual rights in mind. Thus it refers to the collective right 'of the people' to keep and bear arms in connection with 'a well-regulated militia.'").

Similarly in *Kachalsky*, the Second Circuit noted that New York had long regulated the possession and use of firearms. 701 F.3d at 84–85. However, the Second Circuit acknowledged that "the law was upheld, in part, on what is now the erroneous belief that the Second Amendment does not apply to the states." *Id.* at 85.

constitutional rights. In the First Amendment context, for
example, our precedents inform us that we should not cabin
our inquiry to the challenged law before us. Rather, the
preferred course is to examine other, related laws to
determine the nature of the asserted constitutional right and
the extent of the burden on that right. *See, e.g.*, *Doe v. Reed*,
561 U.S. 186 (2010) (examining other disclosure laws to
determine the constitutionality of a requirement to disclose
petition signatories); *Chula Vista Citizens for Jobs & Fair
Competition v. Norris*, 782 F.3d 520 (9th Cir. 2015) (en banc)
(examining other disclosure laws to determine the
constitutionality of a requirement to disclose the identity of
a petition proponent). Similarly here, we must examine the
applicable open-carry restrictions to determine the nature of
Plaintiffs' asserted right to some carry in public and the
extent of the burden of the policies on that right.

    B. *Defining the constitutional right to bear arms
       narrowly is inconsistent with judicial protection of
       other fundamental freedoms*

Regardless of how a jurist feels about the Second
Amendment, there can be no doubt that *Heller* construed the
words "keep and bear arms" broadly to encompass an
individual's right to self-defense, as opposed to a collective
right to keep and bear arms for maintaining a militia. The
Court has defined other constitutional rights broadly as well.
See, e.g., *Obergefell v. Hodges*, 135 S. Ct. 2584, 2598 (2015)
(defining constitutional right as right to marry, not right to
same-sex marriage); *Lawrence v. Texas*, 539 U.S. 558,
566–70 (2003) (right to privacy, not right to engage in
sodomy); *Griswold v. Connecticut*, 381 U.S. 479, 485–86
(1965) (right to marital privacy, not the right to use birth
control devices). Thus, the question in *Obergefell* was not

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whether the plaintiffs have a right to same-sex marriage, the question was whether the states' limitation of marriage to a man and woman violated the right to marry. The question in *Griswold* was not whether there was a constitutional right to use birth control, but rather whether the state's prohibition on birth control violated a person's right to marital privacy.

So too here. The individual constitutional right that Plaintiffs seek to protect is not the right to concealed carry per se, but their individual right to self-defense guaranteed by *Heller*. States may choose how to accommodate this right but they must accommodate it. This distinction may be subtle, but it is critical. Narrowly defining the right may disguise a law's substantive impact on a constitutional freedoms. *See, e.g.*, *Bowers v. Hardwick*, 478 U.S. 186, 190 (1986) (upholding sodomy law, holding that the Constitution does not "confer[] a fundamental right upon homosexuals to engage in sodomy"), *overruled by Lawrence*, 539 U.S. at 569 (striking down sodomy law, holding that "criminal convictions for adult consensual sexual intimacy in the home violate[d] [plaintiffs'] vital interests in liberty and privacy protected by the Due Process Clause of the Fourteenth Amendment").

The majority reasons, however, that "if that right is violated, the cure is to apply the Second Amendment to protect that right. The cure is not to apply the Second Amendment to protect a right that does not exist under the Amendment." Maj. Op. 51. This is an over-simplistic analysis. The counties and California have chipped away at the Plaintiffs' right to bear arms by enacting first a concealed weapons licensing scheme that is tantamount to a complete ban on concealed weapons, and then by enacting an open carry ban. Constitutional rights would become meaningless

if states could obliterate them by enacting incrementally more burdensome restrictions while arguing that a reviewing court must evaluate each restriction by itself when determining its constitutionality. *See Heller*, 554 U.S. at 629 ("A statute which, under the pretence of regulating, amounts to a destruction of the right, or which requires arms to be so borne as to render them wholly useless for the purpose of defence, would be clearly unconstitutional" (quoting *State v. Reid*, 1 Ala. 612, 616–17 (1840))). Indeed, such an approach was rejected by *Heller* which discussed concealed-carry laws in the context of open-carry prohibitions. *Id.*[6]

By narrowly defining the asserted right as a right to concealed carry, the majority fails to recognize the real impact of the counties' policies on the Second Amendment right to keep and bear arms.

C. *Given the right to bear arms for self-defense extends beyond the home, states must accommodate that right to self-defense*

As explained above, given the right to bear arms for self-defense exists outside the home, it follows then that states must accommodate that right. While *Heller* prohibits states from completely banning carrying a firearm in public for self-defense, it leaves states room to choose what manner of carry is allowed. States may choose how to accommodate the right by allowing only open carry, only concealed carry, or some combination of both. However, states may not disallow both

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[6] Under the majority's approach, a court reviewing a challenge to California's regulation of the open carrying of firearms could not consider the fact that in some counties an ordinary citizen also cannot carry a concealed weapon.

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manners of carry as the counties and California have done here.

The majority concedes that "[t]he Second Amendment may or may not protect to some degree a right of a member of the general public to carry a firearm in public." Maj. Op. 51. However, it claims that "[i]f there is such a right, it is only a right to carry a firearm openly." Maj. Op. 51. The majority's holding—that California *must* accommodate the right to bear arms in public through open carry—is unsupported by Supreme Court precedent and contrary to federalism principles. The Supreme Court has *never* dictated how states must accommodate a right to bear arms. The majority's cited cases, also cited in *Heller*, make this point clear. *See, e.g.*, *Reid*, 1 Ala. at 616–17 ("We do not desire to be understood as maintaining, that in regulating the manner of bearing arms, the authority of the Legislature has no other limit than its own discretion. A statute which, under the pretence of regulating, amounts to a destruction of the right, or which requires arms to be so borne as to render them wholly useless for the purpose of defence, would be clearly unconstitutional."); *Nunn v. State*, 1 Ga. 243, 243 (1846) ("A law which merely inhibits the wearing of certain weapons in a concealed manner is valid. But so far as it cuts off the exercise of the right of the citizen altogether to bear arms, or, under the color of prescribing the mode, renders the right itself useless-it is in conflict with the Constitution, and

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void.").**7**   Thus, the majority errs by suggesting that states
must accommodate the right to bear arms through open carry.

Moreover, the majority's requirement that states
accommodate the right to bear arms through open carry is
unwise.   States may have good reasons for allowing
concealed carry but banning open carry. *See* Eugene Volokh,
*Implementing the Right to Keep and Bear Arms for Self-
Defense: An Analytical Framework and A Research Agenda*,
56 UCLA L. Rev. 1443, 1521 (2009) ("In many places,
carrying openly is likely to frighten many people, and to lead
to social ostracism as well as confrontations with the

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7 Because the majority miscasts the issue in these appeals, its historical
analysis is largely irrelevant. But there are also substantive problems with
that analysis. Some authorities are unpersuasive as they rely on a pre-
Heller interpretation of the Second Amendment as being limited to a right
to bear arms for purposes of maintaining a "well-regulated" militia. *See,
e.g.*, *Aymette v. State*, 21 Tenn. 154, 158 (1840) (limiting "arms" to mean
those "such as are usually employed in civilized warfare, and that
constitute the ordinary military equipment"); *State v. Buzzard*, 4 Ark. 18,
19 (1842) (rejecting individual Second Amendment right to self-defense;
holding that right was tied to well-regulated militia); *English v. State*,
35 Tex. 473, 476 (1871) ("The word 'arms' in the connection we find it
in the constitution of the United States, refers to the arms of a militiaman
or soldier, and the word is used in its military sense."); *State v. Workman*,
14 S.E. 9, 11 (W. Va. 1891) (limiting "arms" to mean those "weapons of
warfare to be used by the militia, such as swords, guns, rifles, and
muskets,—arms to be used in defending the state and civil liberty").

Still other authorities, such as *Robertson v. Baldwin*, 165 U.S. 275
(1897), are of limited value because they fail to disclose whether the
concealed-weapon law existed in conjunction with laws permitting open
carry, or do not indicate whether the court interpreted the Second
Amendment to be limited to a collective right related to the militia, instead
of an individual right to self-defense. *See also Walburn v. Territory*, 59
P. 972 (Okla. 1899).

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police."). Different states may have different opinions about whether concealed carry or open carry is preferable. The point is that, under *Heller*, states cannot prohibit both open and concealed carry, thus eviscerating the right to bear arms in public for self-defense.[8]

IV.   The Counties' Unfettered Discretion to Grant or Deny Concealed Weapons Licenses is Troubling

Finally, while the majority and I would decide this case on Second Amendment grounds, Plaintiffs have raised non-frivolous concerns as to whether the counties' discretion as to who obtains a license violates the Equal Protection Clause and constitutes an unlawful prior restraint. The issues are not ripe for review, but I note that a discretionary licensing scheme that grants concealed weapons permits to only privileged individuals would be troubling.[9] Such discretionary schemes might lead to licenses for a privileged class including high-ranking government officials (like judges), business owners, and former military and police officers, and to the denial of licenses to the vast majority of citizens. *See, e.g.*, *McDonald*, 561 U.S. at 771 ("After the Civil War, many of the over 180,000 African Americans who served in the Union Army returned to the States of the old

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[8] Despite California's belated appreciation of the importance of these appeals, the majority grants its motion to intervene. Hence, now that California is a party, there is no reason to confine our inquiry to the counties' policies. Rather, California's intervention supports examining Plaintiffs' challenges to the counties' policies in the context of the California open carry ban.

[9] Indeed, a declaration submitted by the County of San Diego indicates that the point of the concealed weapons licensing policy was to make concealed carry "a very rare privilege."

Confederacy, where systematic efforts were made to disarm them and other blacks. The laws of some States formally prohibited African-Americans from possessing firearms." (citations omitted)); Br. for Congress of Racial Equality, Inc. as Amicus Curiae Supporting Appellants 15, 20, 24, ECF No. 249 (arguing that California's gun control history evidences attempts to disarm ethnic minorities including persons of Mexican Heritage, Asian-Americans, and African-Americans); *cf.* Br. for Pink Pistols et al. as Amici Curiae Supporting Appellants 3, ECF No. 240 ("[W]ithout self-defense, there are no gay rights." (alteration and emphasis omitted)). Whatever licensing scheme remains in place in California or in other states, the right to keep and bear arms must not become a right only for a privileged class of individuals.

* * *

The Second Amendment is not a "second-class" Amendment. *See McDonald*, 561 U.S. at 780.

> Undoubtedly some think that the Second Amendment is outmoded in a society where our standing army is the pride of our Nation, where well-trained police forces provide personal security, and where gun violence is a serious problem. That is perhaps debatable, but what is not debatable is that it is not the role of th[e] [Supreme] Court to pronounce the Second Amendment extinct.

Heller, 554 U.S. at 636. Today the majority takes a step toward extinguishing the Second Amendment right recognized by the Supreme Court in *Heller* and *McDonald*.

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With no clear guidance from the Court regarding how to evaluate laws that restrict and obliterate the right to keep and bear arms for self-defense, the Second Amendment is becoming "[a] constitutional guarantee subject to future judges' assessments" which is "no constitutional guarantee at all." *Id.* at 634.

Accordingly, I dissent.

SILVERMAN, Circuit Judge, with whom BEA, Circuit Judge joins, dissenting:

I dissent from the majority's opinion because the challenged laws do not survive any form of heightened scrutiny – strict or intermediate scrutiny. *See D.C. v. Heller*, 554 U.S. 570, 629 n.27 (2008) (explaining that "rational-basis scrutiny" is inappropriate for reviewing Second Amendment challenges); *see also United States v. Chovan*, 735 F.3d 1127, 1137 (9th Cir. 2013), *cert. denied*, 135 S. Ct. 187 (2014) ("In *Heller*, the Supreme Court did not specify what level of scrutiny courts must apply to a statute challenged under the Second Amendment. The *Heller* Court did, however, indicate that rational basis review is not appropriate."). The more lenient of the two standards – intermediate scrutiny – requires "(1) the government's stated objective to be significant, substantial, or important; and (2) a reasonable fit between the challenged regulation and the asserted objective." *Chovan*, 735 F.3d at 1139.

No one disputes that the County Defendants and California have significant, substantial, and important interests in promoting public safety and reducing gun

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violence. *See Fyock v. Sunnyvale*, 779 F.3d 991, 1000 (9th Cir. 2015) ("Sunnyvale's interests in promoting public safety and reducing violent crime are substantial and important government interests."). However, the County Defendants and California have failed to provide sufficient evidence showing that there is a reasonable fit between the challenged laws and these two objectives. *See Chovan*, 735 F.3d at 1140–41 (stating that it is the government's burden to establish that the challenged law survives intermediate scrutiny); *Celotex Corp. v. Catrett*, 477 U.S. 317, 323 (1986) (explaining that summary judgment is appropriate if "the nonmoving party has failed to make a sufficient showing on an essential element of her case with respect to which she has the burden of proof").

In evaluating the constitutionality of a law under intermediate scrutiny, a reviewing court must assure that, in formulating their judgments, lawmakers have "'drawn reasonable inferences based on *substantial evidence*.'" *Turner Broad. Sys., Inc. v. F.C.C.*, 520 U.S. 180, 195 (1997) (emphasis added) (quoting *Turner Broad. Sys., Inc. v. F.C.C.*, 512 U.S. 622, 666 (1994)); *see also City of Renton v. Playtime Theatres, Inc.*, 475 U.S. 41, 51–52 (1986) (explaining that the evidence that the lawmakers relied on must be "reasonably believed to be relevant to the problem" the government is addressing). In evaluating whether California lawmakers have drawn reasonable inferences based on substantial evidence, it is important to note that the constitutional claims at issue in this case do not seek to provide all California citizens with the unrestricted ability to carry concealed firearms in public. To the contrary, Plaintiffs do not challenge California Penal Code §§ 25655 and 26150's requirements: (1) that a person desiring to carry a concealed firearm in public first obtain a concealed carry

license; and (2) that in order to obtain that license the person must be a law-abiding citizen of good moral character and complete the necessary course of firearms training.

Thus, Plaintiffs only challenge California's concealed carry licensing scheme as interpreted and implemented by San Diego County and Yolo County to the extent it prohibits certain law-abiding citizens, who have completed the necessary training and applied for the necessary license, from carrying a concealed firearm in public because they cannot satisfy San Diego County and Yolo County's required heightened showing of a particular need to carry a firearm in public for self-defense purposes. This distinction is important because the County Defendants and California have not provided any evidence, let alone substantial evidence, specifically showing that preventing law-abiding citizens, trained in the use of firearms, from carrying concealed firearms helps increase public safety and reduces gun violence. The County Defendants have merely provided evidence detailing the general dangers of gun violence and concealed firearms. This evidence is of questionable relevance to the issues in this case because it does not distinguish between firearm violence committed by people who are either concealed carry license holders or are qualified to obtain such a license and firearm violence committed by people who could not obtain a concealed carry license because of either their criminal record or because they have not completed the necessary course of firearms training.

There is simply no evidence in the record showing that establishing a licensing regime that allows trained law-abiding citizens to carry concealed firearms in public results in an increase in gun violence. Indeed, the only evidence in the record shows the exact opposite. Amici have provided

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evidence showing that concealed-carry license holders are disproportionately *less* likely to commit crimes – including violent crimes such as aggravated assault with a deadly weapon – than the general population, and that the adoption of a concealed carry licensing regime such as the one proposed by Plaintiffs in other areas of the country has either had no effect on violent crime or has helped reduce violent crime. *See* Amicus Brief for the Governors of Texas, Louisiana, Maine, Mississippi, Oklahoma, and South Dakota at 10–15; Amicus Brief for International Law Enforcement Educators and Trainers Association, et al. at 22–26. Accordingly, the evidence in the record is insufficient to show that there is a reasonable fit between the challenged laws and the government's stated objectives.

Moreover, the undisputed facts in this case show that there is not a reasonable fit because California law arbitrarily allows its counties to set forth different standards for obtaining a concealed carry license without any reasonable or rational explanation for the differences. For example, in Sacramento County, Fresno County, Stanislaus County, and Ventura County, California Penal Code § 26150(a)'s "good cause" requirement is satisfied by the applicant simply stating that he wishes to carry a firearm in public for self-defense purposes. In contrast, in the two counties at issue in the present appeals – San Diego County and Yolo County – a desire to carry a firearm in public for self-protection purposes by itself is insufficient to satisfy § 26150(a)'s "good cause" requirement. California argues that local officials are best situated to determine what applicants should be required to show in order to satisfy the "good cause" requirement; and, therefore, it is reasonable to confer this discretion to its County sheriffs. However, it does not appear that California's sheriffs are exercising this discretion in a rational

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way. Neither California nor the County Defendants have provided any explanation for why it is reasonable and rational for a desire to carry a firearm in public for self-defense purposes to be insufficient to constitute "good cause" in Yolo County (population 213,016[1]) when right next door in Sacramento County (population 1,501,335[2]) it is sufficient to constitute "good cause." There cannot be a reasonable fit if the same standard – here, § 26150(a)'s "good cause" requirement – is arbitrarily applied in different ways from county to county without any explanation for the differences.

In sum, I would hold that the challenged laws are unconstitutional under the Second Amendment because they do not survive any form of heightened scrutiny analysis, and therefore, I would reverse.

N.R. SMITH, Circuit Judge, dissenting:

I join the dissent of Judge Callahan. I agree that the majority errs "by answering only a narrow question—whether the Second Amendment protects a right to carry concealed firearms in public." Dissent 60. I write separately only to express my opinion that the appropriate remedy is to remand this case to the district courts.

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[1] United States Census Bureau, State & County QuickFacts, Yolo County, California, http://www.census.gov/quickfacts/table/PST045215/06113,00 (last visited June 2, 2016).

[2] United States Census Bureau, State & County QuickFacts, Sacramento County, California, http://www.census.gov/quickfacts/table/PST045215/06067,00 (last visited June 2, 2016).

I.

This case turns on how the applicable issue is framed. The majority states the issue narrowly—whether the "Second Amendment . . . preserve[s] or protect[s] a right to carry concealed firearms in public." Maj. Op. 11. In contrast, the dissent[1] asks whether "[i]n the *context* of California's choice to prohibit open carry," the counties' restrictions on concealed carry violate the Second Amendment. Dissent 69 (emphasis added).

As a result of this difference in framing the applicable issue, the majority's arguments and the dissent's arguments are often like "two ships passing in the night." For example, the majority engages in a lengthy academic exercise to reach the conclusion that "the carrying of concealed weapons was consistently forbidden in England beginning in 1541; was consistently forbidden in the American colonies; and was consistently forbidden by the states." Maj. Op. 49–50. This historical analysis is relevant to the issue framed by the majority, but it is irrelevant to the issue framed by the dissent "because it again fails to appreciate the *contexts* in which the cited cases arose." Dissent 73 (emphasis added).

The majority's historical analysis is also unnecessary to resolve the issue as framed by the majority opinion. In *District of Columbia v. Heller*, the Supreme Court explicitly recognized that prohibitions on carrying concealed weapons were appropriate for regulating the manner in which

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[1] All references to the dissent refer to the dissent of Judge Callahan.

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individuals could exercise their Second Amendment rights.**²**
554 U.S. 570, 626 (2008). If the issue before us is truly
whether California can, in isolation, prohibit concealed carry,
a simple memorandum disposition citing to *Heller* would be
sufficient. A formal opinion, much less the gathering of our
en banc panel, would not be necessary to answer the issue
framed by the majority.

Accordingly, I agree with the dissent's articulation of the
relevant issue in this case. We should not review the
counties' concealed weapons licensing schemes in isolation.
Instead, we must review them in the context of the underlying
statutory scheme as a whole. That review is consistent with
the Supreme Court's approach in *Heller*.**³** It is also consistent
with our court's two-step Second Amendment inquiry. *See
Jackson v. City & Cty. of San Francisco*, 746 F.3d 953, 961
(9th Cir. 2014) (noting that, under the second step of the
inquiry, courts should consider whether firearm regulations
"leave open alternative channels for self-defense").

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² The Supreme Court also recognized that context was important when
reviewing a statute that regulates rights secured by the Second
Amendment. "A statute which, under the pretence of regulating, amounts
to a destruction of the right, or which requires arms to be so borne as to
render them wholly useless for the purpose of defence, would be clearly
unconstitutional." *Heller*, 554 U.S. at 629 (quoting *State v. Reid*, 1 Ala.
612, 616–17 (1840)).

³ *Heller* involved, in part, various prohibitions in the District of
Columbia that (i) made it a crime to carry an unregistered firearm,
(ii) prohibited the registration of handguns, and (iii) required a license to
carry a handgun. *Heller*, 554 U.S. at 574–75. The Supreme Court did not
review these prohibitions in isolation, but instead concluded that the
various prohibitions together "totally ban[ned] handgun possession in the
home." *Id.* at 628.

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Accordingly, we cannot ignore the context surrounding the counties' concealed carry prohibitions.

II

During the pendency of these appeals, California's underlying statutory scheme changed. At the time the district courts issued their decisions, California permitted unloaded open carry. However, under the current scheme, open carry (loaded and unloaded) is prohibited. *See* Dissent 68–69. Further, as noted by the dissent, the district courts did not have the benefit of our recent decisions in *Jackson*, 746 F.3d 953 and *United States v. Chovan*, 735 F.3d 1127 (9th Cir. 2013). *See* Dissent 71.

We have consistently concluded that, when confronted with an intervening change in law, the better approach would be to remand for the district court to consider the case under the new legal framework. *See, e.g.*, *Betz v. Trainer Wortham & Co.*, 610 F.3d 1169, 1171 (9th Cir. 2010) (discussing why "remand is the better procedure" when an intervening change in the law required further analysis of the facts of the case); *Baker v. Hazelwood (In re Exxon Valdez)*, 270 F.3d 1215, 1241 (9th Cir. 2001) (noting that, in cases where there is an intervening change in the law, it will often be "the better approach" to remand for the district court to "apply the appropriate standards"); *White Mountain Apache Tribe v. Ariz., Dep't of Game & Fish*, 649 F.2d 1274, 1285–86 (9th Cir. 1981) ("This court may remand a case to the district court for further consideration when new cases or laws that are likely to influence the decision have become effective after the initial consideration.").

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Of course, we have discretion to determine "what questions may be taken up and resolved for the first time on appeal." *Singleton v. Wulff*, 428 U.S. 106, 121 (1976). We typically feel most comfortable resolving such an issue when it has nonetheless been "extensively litigated in the district court" or "where the proper resolution is beyond any doubt." *Beck v. City of Upland*, 527 F.3d 853, 867 (9th Cir. 2008) (quoting *Golden Gate Hotel Ass'n v. City & Cty. of San Francisco*, 18 F.3d 1482, 1487 (9th Cir. 1994)). However, neither circumstance is present here.   The issue at hand—whether the counties' licensing scheme for concealed carry violates the Second Amendment in light of California's restrictions on open carry—was not litigated in the district courts. Further, as is apparent from the various opposing views of my colleagues, proper resolution of this issue is not beyond any doubt.
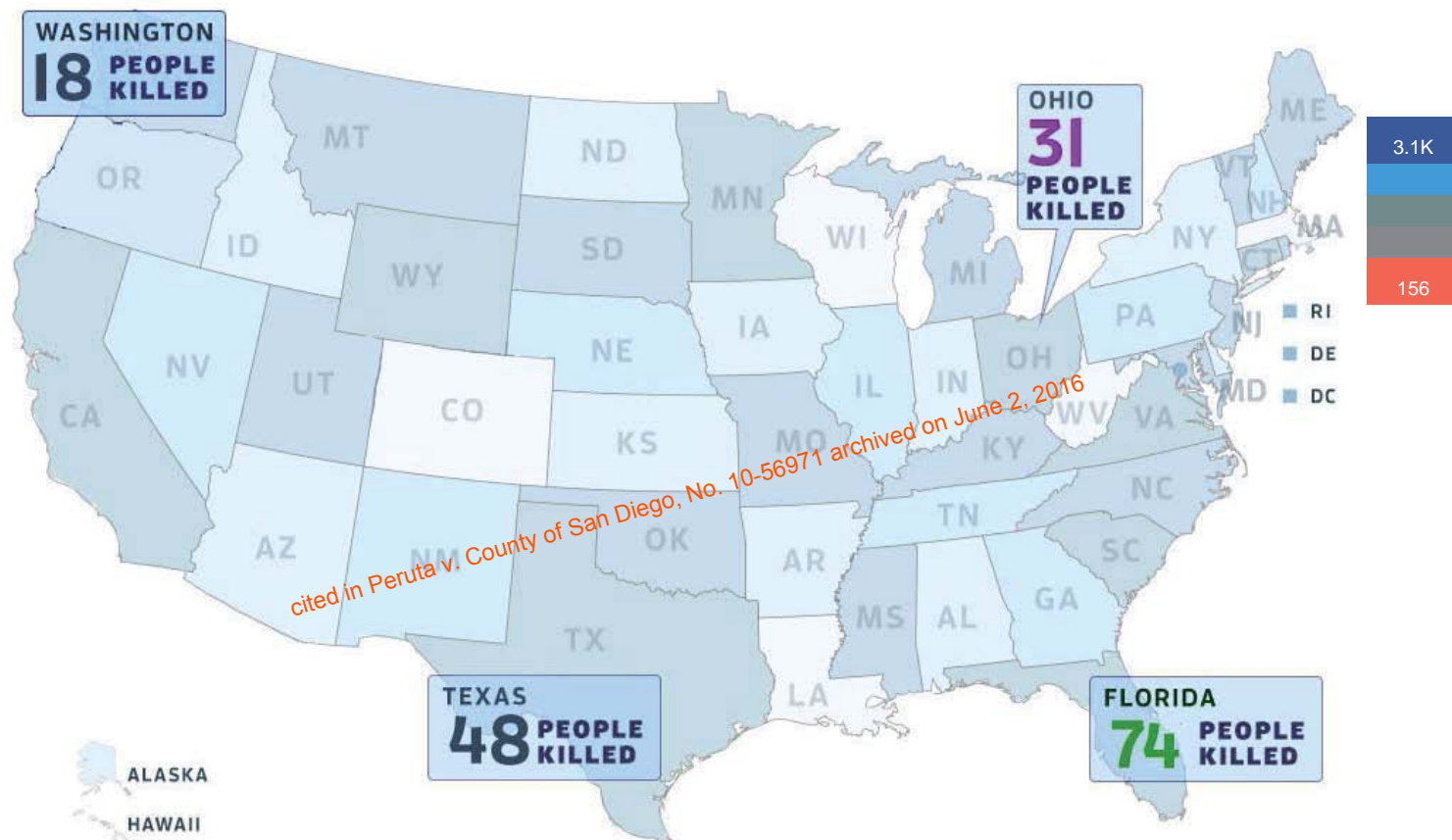
Indeed, we would benefit greatly from the district courts' expertise in developing the record and applying the appropriate standards in light of California's significant intervening change in its legal framework. I agree that the "challenged law burdens conduct protected by the Second Amendment." *Chovan*, 735 F.3d at 1136. I would therefore remand to allow the district courts to initially determine and "apply an appropriate level of scrutiny." *Id.*

Accordingly, I dissent.

Violence Policy Center: Concealed Carry Killers

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cited in Peruta v. County of San Diego, No. 10-56971 archived on June 2, 2016

Concealed Carry Killers

Concealed carry permit holders are supposed to be the "good guys" with guns. In reality, far too many permit holders are a direct threat to public safety.

Concealed Carry Killers is a resource maintained by the Violence Policy Center that includes hundreds of examples of non-self defense killings by private citizens with permits to carry concealed, loaded handguns in public that took place since May 2007. These incidents include homicides, suicides, mass shootings, murder-suicides, lethal attacks on law enforcement, and unintentional deaths. Only a tiny fraction of these cases are ever ruled to be in self-defense. Any

homicide that is legally determined to be in self-defense is documented and removed from the *Concealed Carry Killers* database and the ongoing tallies.

Spotlight: Workplace Murder-Suicide in Indiana

On March 10, 2016, concealed handgun permit holder Qing Chen, 37, shot and killed his supervisor Ward Edwards, 49, before turning the 9mm Glock pistol on himself in a meeting room at the Cummins Seymour Engine Plant where they worked. The motive was described as a personnel issue, with the Seymour Police Chief noting that the two had a "supervisor/employee relationship." Read more…

These incidents are only examples. There is no comprehensive federal database of concealed carry incidents, and some states even bar the release of such information by law. As a result, the examples in *Concealed Carry Killers* are taken primarily from news reports and from the reporting required in a few states. **These examples represent an unknown fraction of similar incidents that routinely occur across the nation.**

Currently, *Concealed Carry Killers* documents 684 incidents in 41 states and the District of Columbia resulting in 873 deaths. In 86 percent of the incidents (585) the concealed carry killer committed suicide (293), has already been convicted (222), perpetrated a murder-suicide (53), or was killed in the incident (17). Of the 74 cases still pending, the vast majority (64) of concealed carry killers have been charged with criminal homicide, four were deemed incompetent to stand trial, and six incidents are still under investigation. An additional 25 incidents were fatal unintentional shootings involving the gun of the concealed handgun permit holder. Seventeen of the victims were law enforcement officers. Twenty-nine of the incidents were mass shootings, resulting in the deaths of 139 victims.

More than just numbers, *Concealed Carry Killers* provides detailed accounts of lethal incidents involving concealed handgun permit holders. Whenever possible, this includes the names of the killers and victims, the legal status of the cases, and the circumstances of the incidents. To find out more, click on the numbers on the left-hand side to view nationwide information, and see the map above to view the incidents by state.

Last updated April 15, 2016

873
Total People Killed By Concealed Carry Killers

29
Number of Mass Shootings Committed By Concealed Carry Killers

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53
Number of Murder-Suicides Committed By Concealed Carry Killers

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17
Law Enforcement Officers Killed By Concealed Carry Killers

VPC PUBLICATIONS

Cash and Carry: How Concealed Carry Laws Drive Gun Industry Profits

Firearm Justifiable Homicides and Non-Fatal Self-Defense Gun Use

License to Kill IV: More Guns, More Crime

Concealing the Risk: Real-World Effects of Lax Concealed Weapons Laws

Concealed Carry: The Criminal's Companion

ADDITIONAL VPC PUBLICATIONS>>>

VPC FACT SHEETS

Mass Shootings Involving Concealed Handgun Permit Holders

Armed Citizens Are Not the Answer to Mass Shootings

Research Exposes Dangerous Flaws in State Concealed Handgun Permit Systems

Children and Youth Victims of Concealed Carry Killers

Federally Mandated Concealed Carry: The Impact on Your State

ADDITIONAL RESOURCES

Media Matters, "Who is Gun Advocate John Lott?"

Center for American Progress Fact Sheet On National Concealed Carry Reciprocity Legislation

Everytown for Gun Safety, *Federally Mandated Concealed Carry Reciprocity: How Congress Could Undercut State Laws on Guns in Public*

Right-to-carry gun laws linked to increase in violent crime, Stanford research shows

National Academy of Sciences, *Firearms and Violence: A Critical Review* (2004), Chapter 6

American Journal of Public Health, *When Concealed Handgun Licensees Break Bad: Criminal Convictions of Concealed Handgun Licensees in Texas, 2001–2009*

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The Violence Policy Center is a national tax-exempt educational organization working for a safer America through research, investigation, analysis, and advocacy.The VPC provides information to policymakers, journalists, organizations, advocates, and the general public. Click here to learn more.

cited in Peruta v. County of San Diego, No. 10-56971 archived on June 2, 2016

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8 | *The Impact of Concealed-Carry Laws*

Thirty-three states have "shall-issue" laws that require law-enforcement authorities to issue permits to carry concealed weapons to any qualified applicant who requests one—that is, to adults with no documented record of significant criminality or mental illness. A spirited academic debate has emerged over whether these laws are helpful or harmful. While it is fairly easy to list the possible consequences of the passage of these laws, it has not been easy to come to agreement about which effects dominate in practice. Many scholars fear that these laws will stimulate more ownership and carrying of guns, leading to adverse effects such as an increase in spur-of-the-moment shootings in the wake of arguments or opportunistic criminal acts, increased carrying and quicker use of guns by criminals, more opportunities for theft of guns, thereby moving more legally owned guns into the hands of criminals, and more accidental killings and gun suicides. However, a pathbreaking article by John Lott and David Mustard in 1997 and a subsequent book by Lott have made the case that opportunistic crime should fall for everyone as criminals ponder whether

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they will be shot or otherwise thwarted by a potential victim or bystander carry-ing a concealed weapon.[1]

Scholars have lined up on both sides of this debate. For example, Frank Zim-ring, Gordon Hawkins, Jens Ludwig, Dan Nagin, Mark Duggan, and others have been highly critical of the evidence marshaled by Lott and Mustard.

At the same time, criminologist James Q. Wilson calls Lott's book "the most scientific study ever done of these matters, using facts from 1977 through 1996 and controlling for just about every conceivable factor that might affect the crim-inal use of guns."[2] Wilson gives a ringing endorsement to Lott's thesis:

> Lott's work convinces me that the decrease in murder and robbery in states with shall-issue laws, even after controlling statistically for every other cause of crime re-duction, is real and significant. Of the many scholars who were given Lott's data and did their own analyses, most agree with his conclusions. States that passed these laws experienced sharp drops in murder, rape, robbery, and assault, even after allowing for the effects of poverty, unemployment, police arrest rates, and the like. States that did not pass these laws did not show comparable declines. And these declines were not trivial—he is writing about as many as 1,000 fewer murders and rapes and 10,000 fewer robberies. Carrying concealed guns reduces—it does not increase—the rate of serious crime, and that reduction is vastly greater than the generally trivial effect of gun-carrying on accidental shootings.[3]

Sorting out who is right in this debate is important for social science and for public policy. Indeed, the resolution of this academic controversy may also in-fluence the current dispute over the meaning of the Second Amendment, which states that "a well regulated Militia, being necessary to the security of a free State, the right of the people to keep and bear Arms, shall not be infringed." As Erwin Griswold, Nixon's solicitor general and former dean of Harvard Law School, noted a decade ago: "Never in history has a federal court invalidated a law reg-ulating the private ownership of firearms on Second Amendment grounds. In-deed, that the Second Amendment poses no barrier to strong gun laws is per-haps the most well settled proposition in American constitutional law."[4] Not

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1. Lott and Mustard (1997). Note the importance of the requirement that the weapon be con-cealed, thereby creating a possible protective shield for those not carrying weapons. Guns that are carried openly do not create this protective shield in that they may simply cause criminals to shift their attack to the unarmed. Thus concealed guns may protect unarmed citizens, while openly car-ried guns put unarmed citizens at greater risk (unless criminals believe the open carriers will fre-quently come to the aid of unarmed crime victims).

2. Wilson (2000).

3. Wilson (2000).

4. Erwin N. Griswold, "Phantom Second Amendment 'Rights,'" *Washington Post,* November 4, 1990, p. C7.

any more. Buoyed by the new research claiming a substantial life-saving bene-
fit from laws enabling citizens to carry concealed handguns and some revisionist
literature on the intent of the founders, the Fifth Circuit Court of Appeals has
recently contradicted Griswold's interpretation of the Second Amendment.[5]
The National Rifle Association and its supporters argue that the way is now
paved to make the right to carry concealed handguns a constitutional mandate
governing the fifty states rather than just a legislative initiative in thirty-three
predominantly small or southern and western states. But are Lott and Mustard
correct that laws facilitating the carrying of concealed handguns reduce crime?
With the benefit of more complete data than were available initially to Lott and
Mustard, I conclude that the best statistical evidence does not support the claim
that shall-issue laws reduce crime.

Although the discussion of the approach used by and problems with the work
of Lott and Mustard can get technical, the points can be summarized in a more
intuitive fashion. First, their initial analysis compares the changes in crime in ten
states that passed shall-issue laws between 1985 and 1991, including states like
Maine, West Virginia, Idaho, and Montana, with states that did not, such as New
York, California, Illinois, and New Jersey. However, I suspect the changes in crime
in the late 1980s were quite different in these two groups for reasons that had
nothing to do with the shall-issue laws, but rather with the criminogenic influence
of the new crack cocaine trade in more urban, poor inner city areas (most com-
monly found in states that did not adopt shall-issue laws). If this suspicion is true,
then the relatively smaller crime increases in adopting states over this period
would be incorrectly attributed to the law when wholly separate forces were really
the explanation.

Second, because the adoption of shall-issue laws does not occur randomly
across states and over time, it is harder to discern the impact of the law (just as
a randomized medical experiment to determine the effectiveness of a drug will
provide better guidance than merely observing who chooses to take the drug and
what happens to those who do and do not). Since there is evidence of a "treat-
ment effect" even before the laws are adopted, one needs to be cautious in draw-
ing conclusions about the actual effect of the shall-issue laws. This concern is
heightened by fears that spikes in crime encourage the adoption of shall-issue
laws, and then the accompanying drops in crime (representing a return to more
normal times or "regression to the mean") will be inaccurately attributed to the
passage of the law. When the Lott and Mustard statistical model is run for the pe-
riod in the 1990s when the spikes in crime reversed themselves, suddenly shall-

5. *U.S.* vs. *Emerson,* 281 F.3d 1281 (Fifth Circuit 2001).

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issue laws are associated with uniform *increases* in crime. Thus, with the benefit
of five more years of data, during which time thirteen states and the city of
Philadelphia adopted shall-issue laws, one sees very different patterns than what
Lott and Mustard observed in their initial study on ten adopting states with
dates ending in 1992.

With the expanded data set, there is much evidence that could be amassed
to support the view that shall-issue laws tend to increase crime, at least in most
states. But the third set of factors that undermines the more-guns, less-crime hy-
pothesis probably weakens that conclusion too: the results tend to be sensitive
to whether one uses county or state data, which time period one looks at, and what
statistical method one employs. While scholars may be able to sort out some of
the disputes about coding adoption dates for shall-issue laws, which when cor-
rected tend to modestly weaken the Lott and Mustard results, there are still un-
certainties about data quality and model specification that may not easily be re-
solved with the current aggregated crime data. In the end, the most that can be
said is that when adopted in the states that have so far adopted them, shall-issue
laws may not increase crime as much as many feared. But these laws still may
create social unease if citizens are apprehensive that even greater numbers of in-
dividuals walking through shopping malls, schools, and churches and sitting in
movie theatres are carrying lethal weapons.

Lott and Mustard emphasize that few holders of gun permits are found to have
committed murder, but they fail to recognize that the number of murders can
rise from the passage of shall-issue laws, even if no permit holder ever commits a
crime. First, knowing that members of the public are armed may encourage crim-
inals to carry guns and use them more quickly, resulting in more felony murders.
Second, as already mentioned, the massive theft of guns each year means that
anything that increases the number of guns in America will likely increase the
flow of guns into the hands of criminals, who may use them to commit murders.
Notably, the typical gun permit holder is a middle-aged Republican white male,
which is a group at relatively low risk of violent criminal victimization with or
without gun ownership, so it is not clear whether substantial crime reduction
benefits are likely to occur by arming this group further.

The Basic Methodology of Lott and Mustard

Lott and Mustard follow the basic contours of the current gold standard of
microeconometric evaluation—a panel data model with fixed effects. That is, Lott
and Mustard collect data over 1977–92 for individual states and counties across

the United States, and then use panel data regression techniques to estimate the effect of the adoption of shall-issue laws, controlling for an array of social, economic, and demographic factors.[6] Essentially this approach determines for the ten states that adopted the shall-issue laws over this period how crime looks different after passage than it was before passage. In a study of this magnitude, the researcher must make many choices about data issues, model specification, and control variables, each of which has the potential to influence the outcome of the analysis in ways that are not often predictable.[7]

The Use of County Data

Lott relies most heavily on county crime data rather than state crime data (although he presents some state data results), noting that the far greater number of counties than states can add precision to the estimates and that county fixed effects will explain a great deal of the fixed cross-sectional variation in crime across the country. The use of these county fixed effects diminishes the inevitable problem of omitting some appropriate, but possibly unavailable, time-invariant explanatory variables. The county data have some disadvantages, though: Mark Duggan notes the concern that using county data to assess the impact of a (generally) statewide intervention may artificially elevate statistical significance by exaggerating the amount of independent data available to the researcher.[8] Furthermore, county data on the arrest rate (the ratio of arrests to crime in a county) are often unavailable because they are missing or because the county experienced no crime in a particular category in a particular year (leaving the rate undefined owing to the zero denominator). Since Lott uses the arrest rate as an explanatory variable, many counties are thrown out of the Lott analysis by virtue of the realization of the dependent variable (if it is zero in a given year, that county is dropped from the analysis), which can potentially bias the results of the regression estimation. Finally, Michael Maltz and Joseph Targonski raise some serious questions about the quality of UCR county-level data (at least for data before 1994).[9]

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6. The "fixed effect" is a dummy variable that is included for each county or state that is designed to reflect any unvarying trait that influences crime in that county or state yet is not captured by any of the other explanatory variables. Lott and Mustard (1997).

7. As noted, the initial paper on this topic was by Lott and Mustard and the subsequent book (Lott [2000]) (now in its second edition) is by Lott. For ease of reference I henceforth refer to Lott as a shorthand for both Lott's work and that of Lott and Mustard.

8. One exception is Pennsylvania, which initially excluded Philadelphia from its 1989 shall-issue law. In 1995 the law was extended to include Philadelphia. Duggan (2001, p. 1109, note 20).

9. Maltz and Targonski (2001).

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Model Specification

Lott basically uses two models to test the impact of a shall-issue law, but there are advantages in employing a third—hybrid—model discussed in the following paragraphs.[10]

— The dummy variable model: After controlling for all of the included explanatory variables, this model essentially tests whether on-average crime in the prepassage period is different in a statistically significant way from crime in the postpassage era. Since the dependent variable is the natural log of the crime rate, the coefficient on the postpassage dummy variable can be interpreted as the percentage change in crime associated with the adoption of the law.

— The Lott spline model: Rather than simply measuring the average pre- and postpassage effect (net of the controls), this model attempts to measure whether the trend in crime is altered by the adoption of a shall-issue law. Lott stresses this model may be needed to capture a reversal in trend that a simple dummy variable model might miss (because the law reverses an upward trend, but the symmetry of a rise in the prepassage crime rate and a fall in the postpassage crime rate leaves the average pre- and postcrime level the same).

— The hybrid or main effect plus trend model: Ayres and Donohue have argued that the at times conflicting results of the two previous models suggest that a third more general model may be needed. This hybrid model allows a postpassage dummy to capture the main effect of the law but also allows the law to change the linear trend in crime for adopting states. This model could be important if an announcement effect initially scares some criminals into fearing possible victim or bystander retaliation, but the ultimate effect is that more guns lead to more serious criminal acts—perhaps as fistfights end with someone dead or seriously injured instead of with a bloodied nose. Under this scenario, one might even see an initial drop in crime followed by a subsequent turnaround as the number of concealed guns being carried and crime increase in tandem. Although Lott does not employ this model (except in a modified model in a paper by Stephen Bronars and John R. Lott discussed below), it can be used to test whether one or both of the first two models is appropriate.[11]

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10. Ayres and Donohue (forthcoming).
11. Ayres and Donohue (forthcoming); Bronars and Lott (1998). If the estimated coefficient on the postpassage dummy were virtually zero, one would reject the first model, and if the estimated coefficient on the time trend were virtually zero, one would reject the second model. If they were both virtually zero, one would conclude that the law had no effect on crime.

Note that the third model will generate two estimated effects that could be reinforcing (both the dummy and trend have the same sign) or in conflict in that one effect is positive and the other is negative. It is theoretically difficult to tell a story in which the main effect of the law would be pernicious while the trend effect is benign, so if we were to see such a pattern, it would probably be suggestive of some model mis-specification rather than evidence that the law actually generated this pattern.[12]

Lott and Mustard's Data

Lott begins his analysis by examining county-level data over 1977–92. Line 1 of table 8-1 shows the predicted effect on nine crime categories using the dummy variable model and his data (which he has generously supplied to numerous scholars interested in examining his work). A quick examination of the line 1 results reveals four of the five categories of violent crime (the exception is robbery) have negative and statistically significant coefficients, suggesting that shall-issue laws reduce these types of violent crime by 4 to 7 percent; and all four property crimes have positive and statistically significant coefficients, suggesting that the laws increase property crime by 2 to 9 percent. Lott accepts the regression results at face value and concludes that the passage of these laws causes criminals to shift from committing violent crime to committing property crime, where, he argues, they are less likely to be shot since the victim is frequently not present when the crime occurs. Thus we see violent crime decreasing by 3.5 percent and murders falling by more than twice that percentage, while property crime rises by more than 5 percent. As Ayres and Donohue stressed, however, the fact that robbery is not dampened by the adoption of a shall-issue law constitutes a major theoretical problem for Lott's interpretation of the results of the dummy variable model.[13] If there is to be the type of substitution away from violent crime that Lott predicts, one would expect that the new law would induce potential robbers to avoid confronting victims and shift to more stealthy property crime; yet in the first row of table 8-1, we see no evidence of any dampening

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12. Lott does suggest a way in which a pernicious main effect could be followed by a benign long-term trend effect, but this argument is unconvincing. In discussing his findings that public shootings increase for a few years after passage of nondiscretionary handgun laws, Lott suggests that people planning such shootings might "do them sooner than they otherwise would have, before too many citizens acquire concealed-handgun permits." Lott (2000, p. 102). This Procrustean explanation seems designed to make contrary evidence appear supportive of a preferred theory.

13. Ayres and Donohue (1999).

Table 8-1. *The Estimated Impact of Shall-Issue Laws on Crime, County Data*

Percent

Item	Violent crime	Murder	Rape	Aggravated assault	Robbery	Property crime	Auto theft	Burglary	Larceny
Lott's time period (1977–92)									
1. Dummy variable model	**-3.5**	**-7.3**	**-4.8**	**-5.3**	-0.1	**5.2**	**8.9**	**2.3**	**5.9**
Robust std. error	(1.2)	(2.5)	(1.5)	(1.6)	(1.9)	(1.1)	(2.0)	(1.1)	(1.9)
2. Lott-Spline model	-0.4	**-4.7**	**-1.7**	0.5	**-1.9**	0.1	0.1	-0.4	0.8
Robust std. error	(0.5)	(1.1)	(0.6)	(0.7)	(0.8)	(0.7)	(0.9)	(0.5)	(1.4)
3. Hybrid model									
Postpassage dummy	**6.7**	2.9	**6.5**	**9.6**	-2.9	0.2	0.3	-2.5	0.3
Robust std. error	(2.3)	(4.9)	(2.9)	(3.0)	(3.2)	(1.8)	(2.9)	(1.9)	(3.0)
Trend effect	**-2.0**	**-5.4**	**-3.2**	**-1.7**	-1.2	0.0	0.0	0.2	0.8
Robust std. error	(0.8)	(1.5)	(0.9)	(1.0)	(1.1)	(0.6)	(1.2)	(0.6)	(1.2)
Entire period (1977–97)									
4. Dummy variable model	0.2	**-7.8**	**-2.9**	-0.1	-0.4	**7.6**	**10.8**	**1.5**	**9.6**
Robust std. error	(1.1)	(1.7)	(1.1)	(1.3)	(1.3)	(0.8)	(1.5)	(0.9)	(1.2)
5. Lott-Spline model	**-1.6**	**-2.7**	**-2.7**	**-2.7**	**-3.6**	**-0.4**	**-0.8**	**-2.6**	**-1.1**
Robust std. error	(0.2)	(0.5)	(0.4)	(0.4)	(0.4)	(0.2)	(0.4)	(0.3)	(0.4)
6. Hybrid model									
Postpassage dummy	0.2	**6.8**	**6.1**	**6.1**	3.5	-0.7	**8.9**	**4.2**	**5.4**
Robust std. error	(1.4)	(2.9)	(2.1)	(2.3)	(2.3)	(1.1)	(2.4)	(1.7)	(2.1)
Trend effect	**-1.6**	**-3.5**	**-3.4**	**-3.2**	**-4.0**	-0.3	**-1.8**	**-3.0**	**-1.7**
Robust std. error	(0.3)	(0.7)	(0.5)	(0.6)	(0.6)	(0.2)	(0.6)	(0.4)	(0.5)

Note:: The dependent variable is the natural log of the crime rate named at the top of each column. The data set is composed of annual county-level observations (including the District of Columbia). The top panel uses data from the time period that Lott analyzes, 1977–92. The bottom panel uses the same data set but with appended entries for the years 1993–97. County- and year-fixed effects are included in all specifications. All regressions are weighted by county population. Standard errors (in parentheses) are computed using the Huber-White robust estimate of variance. Coefficients that are significant at the .10 level are underlined. Coefficients that are significant at the .05 level are displayed in bold. Coefficients that are significant at the .01 level are both underlined and displayed in bold.

effect on robbery. Hence the dummy variable model undermines a key pre-
diction that Lott offers to explain the line 1 regression results for the period
1977–92.[14]

Lott presents his version of the line 1 regression evidence in the first regres-
sion table in his book. Interestingly, this table shows that robbery reduces crime
by 2.2 percent, which is statistically significant at the .10 level (considered mar-
ginally significant). But Ayres and Donohue reveal that this −2.2 percent figure
is an error that results from a miscoding of the effective date of the shall-issue
laws.[15] The problem was that, instead of following his own strategy of assuming
that the effect of the law would emerge in the first year after passage, Lott coded
the shall-issue law in that fashion only for Florida and Georgia, with all other
states being coded so that the effect of the law begins in the year of passage. Cor-
recting this error to adhere consistently to the articulated Lott protocol wipes
out the size and significance of the estimated effect on robbery.[16] These same in-
correct results appeared in 2000 in the second edition of the book. Thus both edi-
tions incorrectly suggest that the dummy variable model shows that shall-issue
laws reduce the number of robberies.

14. Lott and Mustard respond that the implications of the passage of a shall-issue law are un-
certain since, for example, banks and businesses have always been protected by gun-toting person-
nel. Therefore, they contend, there may be substitution from highway robberies to robberies of banks
and convenience stores, with uncertain implications for the overall number of robberies. I am not
persuaded by this point. In 1999, 64.1 percent of robberies were either highway robberies (48.3 per-
cent of the total) or robberies that occurred in churches, schools, trains, etc. (15.8 percent of the
total)—the remainder being robberies in commercial firms including banks or in residences. FBI
(1999, table 2.20). Thus the substantial majority of robberies are exactly the sort of crimes that Lott
and Mustard argue should be deterred. In fact, the proportion of robberies that occur in public places
is greater than the proportion of aggravated assaults occurring in public places. In 1999 aggravated
assaults occurring in public places constituted 58.6 percent of the total. Bureau of Justice Statistics
(1999, table 61). Moreover, even in the 8.2 percent of robberies that occur in convenience stores or
gas stations, the armed citizenry are supposed to be protecting against crime (indeed, Mustard argues
they even protect armed police officers! See Mustard (2001)).
    15. Lott (2000, table 4-1); Ayres and Donohue (1999).
    16. Ayres and Donohue (1999) replicate Lott precisely with the coding error and then show how
the correction eliminates the robbery effect. The line 1 results in table 8-1 of this chapter are identi-
cal to the results in Lott's table 4-1 with three exceptions, which are maintained in all the regressions
presented here: the coding error is corrected; standard errors are corrected to adjust for heterogene-
ity; and one explanatory variable—a measure of the real per capita income maintenance, SSI and
other, for those over 65—was dropped. One can compare the results in table 1 of Ayres and Donohue
(1999) with those of table 8-1 here to see that the only change that influences the basic story is the
correction for the coding error. The explanatory variable of real per capita income maintenance for
the elderly was omitted because, in expanding the data set to include the period 1993–97, we were
unable to match the series for this variable with Lott's series through 1992. Since the omission had
little impact on the pre-1993 results, and the theoretical argument for inclusion is not strong, we
simply dropped the variable completely.

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Lott's Spline Model

The only numbers that Lott reports in his book concerning his trend analysis are found in a single row of figures representing the difference between the before-passage linear trend and after-passage linear trend for the states that passed shall-issue laws.[17] Lott's regressions include year effect dummies, so the pre- and post-passage trend coefficients would capture linear movements in crime in the ten passing states, apart from the general movements in crime for the nation as a whole (which would be captured by the general year dummies). Lott's message is that a trend analysis shows that shall-issue laws lower all crime categories—both violent and property—and in all cases but one (larceny) the reduction is statistically significant. But Lott's regressions incorrectly identify the passage date of four jurisdictions that adopted shall-issue laws, which make the laws look more effective than they are.[18] The corrected numbers are presented in line 2 of table 8-1, which shows that the shall-issue laws reduce crime in a statistically significant way in only three of the nine categories (murder, rape, and robbery).

Note that the story in line 2 is changed in several respects from that of line 1 (the dummy variable model). Instead of all violent crime (but robbery) falling and property crime rising, line 2 suggests that shall-issue laws have no effect on property crime (or overall violent crime and aggravated assault) but dampen murder, rape, and the heretofore unaffected robbery. Consequently, Lott's discussion of the impact of shall-issue laws causing criminals to shift from committing violent to committing property crime is no longer central if the Lott spline analysis (regression 2 in table 8-1) is the appropriate estimation approach.

The Hybrid Model Testing for Main and Trend Effects

The Lott spline results predict that shall-issue laws decrease murder, rape, and robbery, thereby eliminating the problem for Lott's theory posed by the dummy variable model's failure to show a dampening of robbery. To sort out the conflicts between the dummy and trend models, Ayres and Donohue suggest using the hybrid regression 3 in table 8-1, which is the generalized model of regressions 1 and 2.[19] Regression 3 confirms the prediction of regression 2 and contradicts that of regression 1 that the shall-issue laws have virtually no effect on property crime. Once again, robbery largely drops out of the picture (although

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17. Lott (2000, table 4-8).
18. Lott coded the enactment dates in Oregon, Pennsylvania, Virginia, and Philadelphia earlier than was proper. In his dummy variable analysis, Lott similarly miscoded these three states (and five others, but he correctly coded Philadelphia), as noted in Ayres and Donohue (1999, p. 449, note 21).
19. Ayres and Donohue (forthcoming).

it is negative in sign), thus reviving the theoretical problem that the shall-issue law does not reduce the one crime for which one would most expect a reduction if the Lott hypothesis were correct. For the other four violent crime categories, we see a pattern that is the exact opposite of what one might expect—the main effect of the shall-issue laws is positive, but over time this effect gets overwhelmed as the linear trend turns crime down. In other words, according to the hybrid model, in the year after passage the main effect of the shall-issue law is a 6.7 percent increase in violent crime, which is dampened by the 2 percent drop associated with the negative trend variable, for a net effect of 4.7 percent higher crime. After 3.5 years the conflicting effects cancel out, at which point crime begins to fall. This particular result of a positive main effect and a negative trend effect is inconsistent with any plausible theoretical prediction of the impact of a shall-issue law, since it is not clear why the law should initially accelerate crime and then dampen it.[20] The anomalous results suggest that even the most general form of the three crime models is still misspecified and hence that its results are unreliable.

Extending the County Data through 1997

Lott's initial analysis using 1977–92 data captured the period in which only ten states newly adopted shall-issue laws, and therefore Lott's regression results should be taken as the predicted effect of the adoption of the law in these ten states. Since 1992, however, thirteen more states and the city of Philadelphia have adopted the law, and therefore one might hope to gain more accurate results by extending the period over which the effect of the law is estimated. Before doing so, however, it is worth noting that Ayres and Donohue ran the precise table 8-1 and table 8-2 models on the period from 1991–97 during which fourteen jurisdictions adopted a shall-issue law. In both the county and state data and for all three models (dummy, spline, hybrid), shall-issue laws were uniformly associated with crime *increases*.[21] This sharply different finding from Lott's 1977–92 results

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20. As noted above, if the results had been flipped with the main effect dampening crime and the time trend suggesting a longer term increase, one could interpret those results in a straightforward manner: the announcement of the law scared potential criminals, thereby dampening crime initially, but as more guns got out on the street or as the fear subsided, crime ultimately turned up (or returned to its previous level).

21. Ayres and Donohue (forthcoming). For the county data, virtually all the dummy model estimates were statistically significant, as were many of the estimates in the spline model. For the state data, the individual coefficients were frequently statistically significant for the dummy model, while generally not for the spline model. In both data sets, the results tended to be jointly statistically significant for the hybrid models.

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should be kept in mind during my discussion of the aggregated results over the entire period 1977–97.

Regressions 4 through 6 in table 8-1 simply repeat the models of regressions 1 through 3, but now estimate them over the longer period 1977–97 (and thus measure the effect of adoption of the law in twenty-four states). Comparing lines 1 and 4 (the dummy variable model), we see that adding more years of data weakens Lott's story, which should not be surprising given the strong "more guns, more crime" finding for the 1991–97 period that was just discussed. Importantly, violent crime is no longer negative, so the basic story that the prospect of meeting armed resistance shifts criminals from violent crime to property crime is undermined. Lott might respond that murders fall by nearly 8 percent and rape by almost 3 percent, as murderers and rapists shift over to committing property crime, thereby raising its prevalence by 8 percent. But the suggestion that this pattern could be explained by the changed behavior of would-be murderers and rapists is not compelling.[22] Indeed, the idea that a thwarted rapist would decide to switch to property crime because rape had become more dangerous (to the perpetrator) seems rather fanciful. Again, the possibility of model misspecification seems to be a serious concern.

Interestingly, while the added five years of data weaken Lott's story based on the dummy variable model (line 1 versus line 4), the added data appear to strengthen the story using Lott's spline analysis (compare lines 2 and 5 in table 8–1). For the spline model in line 5, all the estimated coefficients are negative, and all are significant at the .05 level (except property, which is significant at the .10 level). Unlike in both dummy variable models, the Lott spline estimated effect for robbery for both time periods is negative and significant—an almost indispensable finding if the Lott deterrence story is true.

Finally, for the hybrid model, the added five years of data again repeats the unexpected conflicting effects of a positive main effect and a negative trend effect that was observed for the 1977–92 period for violent crime (line 3 of table 8-1) and extends it to property crime, as seen in line 6 of table 8-1. While this regression purports to show declines in overall violent crime and robbery, it suggests that crime initially rises before falling for murder, rape, aggravated assault, auto theft, larceny, and burglary. The absence of a plausible explanation for why a shall-issue law would first increase and then reduce crime again provides

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22. Consider Florida—one of the states that is most conducive to the Lott story in that murders fell after the passage of a shall-issue law in 1987. If the law caused the predicted drop in murders and rape and accompanying rise in property crime from the 1987 level, then one would expect to see 106 fewer murders and 176 fewer rapes in the state and an increase in property crime of 68,590. It seems unlikely that the shall-issue law could explain an increase in property crime of this magnitude, by virtue of declining murders and rapes.

a clear indication of model misspecification. Although I have previously criticized Lott's suggestion that the passage of the laws may cause violent criminals to speed up their attacks to successfully complete them before the effect of shall-issue laws can kick in, this argument becomes even more untenable because of the property crime effects seen in line 6 of table 8-1. Why would auto theft, burglary, and larceny be rising then falling because of the passage of a shall-issue law, apparently mimicking the effect on violent crime? The entire argument of substitutability from violent to property crime, which has ostensible support in lines 1 and 4 of table 8-1 (the dummy variable model), breaks down completely either because there is no effect on property crime (lines 2 and 3) or because the effect is virtually identical to that estimated for violent crime (lines 5 and 6). The instability in these models to changes in the five extra years of data or the inclusion of both a dummy variable and a time trend effect is striking in the table.

A State Data Analysis

As already noted, strong criticism has been leveled at the use of countywide data. Thus it is useful to explore whether the estimated effects of the passage of the shall-issue law hold up when the analysis uses statewide data for the three different models and the two different time periods.

Again, the striking finding is how sensitive the results are in the six different regressions presented. The state data results in table 8-2 are clearly stronger for the Lott argument than the county data results in table 8-1, but again there are anomalies. First, the strongest story one could probably find to support the Lott thesis would be to find violent crime dropping and no effect on property crime (since the latter will frequently not entail contact with the victim, unless by chance in the home, where guns are already prevalent without shall-issue laws). The dummy variable models (lines 1 and 4 of table 8-2) show this pattern and would thus be strongly corroborative of Lott's thesis but for one obstacle: the two hybrid models reject that specification because the postpassage dummy is virtually never significant.

Second, the spline and hybrid models for the full period (lines 5 and 6 of table 8-2) seem to suggest that crime fell for all categories by roughly 2 percent, which again raises the question of why property crime should be falling in just the same way that violent crime is falling. The supporters of shall-issue laws will probably be glad to jettison the previous argument that the laws cause shifts from violent to property crime, but the lack of any theory for the crime drop in property crime may well suggest that the regression is simply picking up unrelated trends in crime and incorrectly attributing them to the shall-issue law.

Table 8-2. *The Estimated Impact of Shall-Issue Laws on Crime, State Data*

Percent

Item	Violent crime	Murder	Rape	Aggravated assault	Robbery	Property crime	Auto theft	Burglary	Larceny
Lott's time period (1977–92)									
1. Dummy variable model	**-8.3**	**-9.4**	4.1	**-9.3**	**-11.4**	-2.2	-0.6	**-6.0**	-1.1
Robust std. error	(2.6)	(3.5)	(0.8)	(3.2)	(3.9)	(1.9)	(3.9)	(2.4)	(1.9)
2. Lott-Spline model	-1.6	**-5.4**	-0.8	-1.9	**-6.1**	-0.8	**-3.3**	**-2.0**	0.0
Robust std. error	(1.0)	(1.4)	(1.0)	(1.2)	(1.7)	(0.8)	(1.4)	(1.1)	(0.8)
3. Hybrid model									
Postpassage dummy	**6.4**	7.5	-2.6	7.6	0.7	1.4	**13.4**	1.6	-0.3
Robust std. error	(3.7)	(5.6)	(4.4)	(4.8)	(5.6)	(2.7)	(5.0)	(3.3)	(2.7)
Trend effect	**-3.2**	**-7.3**	-0.2	**-3.8**	**-6.2**	-1.2	**-6.7**	**-2.4**	0.1
Robust std. error	(1.3)	(2.0)	(1.5)	(1.6)	(1.8)	(0.9)	(1.8)	(1.3)	(0.9)
Entire period (1977–97)									
4. Dummy variable model	**-7.0**	-4.5	**-4.7**	**-5.9**	**-7.3**	0.3	**5.8**	**-4.2**	0.7
Robust std. error	(2.4)	(2.9)	(2.3)	(2.5)	(3.1)	(1.6)	(3.1)	(2.0)	(1.5)
5. Lott-Spline model	**-2.3**	**-2.3**	**-1.8**	**-1.9**	**-3.0**	**-1.1**	**-1.7**	**-2.3**	**-0.9**
Robust std. error	(0.6)	(0.8)	(0.6)	(0.6)	(0.8)	(0.4)	(0.6)	(0.5)	(0.4)
6. Hybrid model									
Postpassage dummy	-2.5	-0.7	-0.6	-0.4	0.0	1.8	**8.9**	0.5	1.5
Robust std. error	(2.7)	(3.8)	(2.9)	(3.2)	(3.7)	(1.7)	(3.6)	(2.3)	(1.6)
Trend effect	**-2.0**	-2.2	-1.8	-1.5	**-3.0**	**-1.3**	**-2.7**	**-2.3**	**-1.0**
Robust std. error	(0.7)	(0.9)	(0.7)	(0.8)	(0.9)	(0.4)	(0.7)	(0.5)	(0.4)

Note: The dependent variable is the natural log of the crime rate named at the top of each column. The data set is composed of annual state-level observations (including the District of Columbia). The top panel uses data from the time period that Lott analyzes, 1977–92. The bottom panel uses the same data set but with appended entries for the years 1993–97. State- and year-fixed effects are included in all specifications. All regressions are weighted by state population. Standard errors (in parentheses) are computed using the Huber-White robust estimate of variance. Coefficients that are significant at the .10 level are underlined. Coefficients that are significant at the .05 level are displayed in bold. Coefficients that are significant at the .01 level are both underlined and displayed in bold.

County and State Data Results from Tables 8-1 and 8-2

The foundation of the Lott thesis essentially is captured in regressions 1 and 2 in tables 8-1 and 8-2, with the greatest prominence in Lott's book going to the dummy variable model of table 8-1 but with greater emphasis now placed on the spline model of the same table. Although these results are not the same as those presented in Lott's book, these are the ones to look at because some coding errors have been corrected. The results are not as stable as one might like, but if one were to examine only those four regressions, the evidence would tend to support Lott's thesis. Obviously, the analyst's task would be easiest if the regressions generated by three different models (dummy, spline, hybrid), for three different time periods (1977–92, 1991–97, and 1977–97), on two different data sets (county and state) all conveyed essentially the same picture. Unfortunately, they do not. For the county data, we see that the hybrid model essentially rejects the dummy variable and trend analyses but yields only flawed results itself. The hybrid model's prediction of initial jumps in crime followed by subsequent declines in response to the adoption of a shall-issue law seems to conflict with any plausible story of how the laws might influence criminal conduct. This pattern again suggests the likelihood of model misspecification, perhaps resulting from some other omitted variable that is generating a drop in crime, which is being spuriously attributed to the shall-issue law. Accordingly, the county data set results of table 8-1 do not provide compelling support for Lott's thesis.

Perhaps surprisingly, though, the state results—which Lott has tended to argue against—seem generally more supportive (table 8-2). First, robbery is always negative in table 8-2, as are most of the violent crime categories—although not always significantly. Second, the strange results of the county data set in the hybrid model is not repeated, as we generally do not see uniform large and positive main effects offset by negative trend effects for the full time period. While in table 8-1 the hybrid model rejected both the county dummy variable and spline models, the table 8-2 hybrid model, if anything, seems to reject the dummy variable model and support the spline model, particularly in the full data set. The inconsistency in the hybrid model across time periods (regressions 3 versus 6) is somewhat unsettling. Still, if one took regressions 5 and 6 in table 8-2 as perhaps the "best" regressions from these two tables, one might argue that shall-issue laws seem to be associated with drops of roughly 2 percent across all crime categories. Although this is perhaps a weaker story than Lott initially ventured, it has the virtue of not having the theoretically problematic result of no effect on robbery, even though it does stumble on two other anomalies: first, the peculiar finding that the estimated effects are virtually identical for both violent and property crime, and second, the problem that shall-issue laws are associated with *higher*

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crime in the regressions (both county and state) run over the 1991–97 period. The anomalies suggest that further exploration is needed before any conclusions on the impact of shall-issue laws can be drawn.

Robustness and Endogeneity

The basic Lott regression using panel data with fixed state and year effects essentially acknowledged that the included explanatory variables do not fully capture all of the differences in crime across states or the changes in crime over time within states. Using fixed state and year effects corrects for a certain amount of omitted variable bias, and if the remaining excluded effects are random, then we should be able to determine the impact of shall-issue laws if we have the correct model.[23] If there are county or state trends in crime that are persistent and not explained by the included independent variables, though, the models of tables 8-1 and 8-2 can give misleading results. To address this issue we added state fixed trends to the regressions presented in tables 8-1 and 8-2. These new regressions, presented in tables 8-3 and 8-4, allow each state to have its own time trend and see whether shall-issue laws cause departures from these state trends.

Table 8-3 (county data) reveals the familiar but unsettling pattern of strong positive main effects and strong negative time trend results in regressions 2 and 4. This finding essentially rejects the appropriateness of the Lott spline model in this case, so those regressions are not presented (nor were they run). Once again, the county data results of table 8-3 seem as flawed and inconclusive as those of table 8-1.

While I suggested earlier that the table 8-2 state results were probably the strongest in favor of Lott's thesis, these results are largely undermined by the inclusion of state fixed trends in table 8-4. In other words, what might look to have been caused by the shall-issue law may have only been a trend over time that got improperly attributed to the shall-issue law. Adding fixed state trends may not always be appropriate, however, especially if it causes the standard errors on the estimated coefficient to rise sharply. But since that is not a problem in this case (compare tables 8-2 and 8-4), it would appear that the earlier results that might have tentatively supported the Lott thesis are greatly weakened with the inclusion of state fixed trends.

23. The fixed county or state effects essentially imply that crime rates are always higher by a fixed percentage in New York than in, say, Vermont unless some included explanatory variable explains the difference. Similarly, the fixed year effects imply that there are national influences that will operate proportionally on all states or counties.

Table 8-3. *The Estimated Impact of Shall-Issue Laws on Crime Controlling for State Trends in Crime, County Data*

Percent

Item	Violent crime	Murder	Rape	Aggravated assault	Robbery	Property crime	Auto theft	Burglary	Larceny
Lott's time period (1977–92)									
1. Dummy variable model	0.1	**−8.7**	−1.5	3.4	**−7.5**	−1.4	−1.2	**−3.6**	0.6
Robust std. error	(1.6)	(3.4)	(2.1)	(2.0)	(2.2)	(2.1)	(2.2)	(1.4)	(4.5)
2. Hybrid model									
Postpassage dummy	**6.9**	5.8	5.5	**6.0**	6.3	−0.1	5.2	1.1	−3.1
Robust std. error	(2.3)	(5.3)	(3.1)	(3.0)	(3.4)	(1.9)	(2.9)	(2.0)	(3.0)
Trend effect	**−3.2**	**−6.6**	**−3.2**	−1.2	**−6.3**	−0.6	**−3.0**	**−2.2**	1.7
Robust std. error	(0.8)	(1.8)	(1.1)	(1.0)	(1.3)	(1.1)	(1.2)	(0.8)	(2.5)
Entire period (1977–97)									
3. Dummy variable model	1.7	0.0	2.7	**7.3**	0.3	−0.6	**4.1**	0.4	**4.1**
Robust std. error	(1.4)	(2.3)	(1.6)	(1.8)	(1.9)	(1.3)	(2.0)	(1.3)	(2.2)
4. Hybrid model									
Postpassage dummy	0.9	**5.8**	**6.7**	**6.7**	**5.5**	−1.4	**7.1**	**4.3**	**4.6**
Robust std. error	(1.5)	(2.7)	(2.0)	(2.2)	(2.2)	(1.2)	(2.3)	(1.7)	(2.1)
Trend effect	0.5	**−3.9**	**−2.7**	0.4	**−3.5**	0.5	**−2.1**	**−2.7**	−0.3
Robust std. error	(0.4)	(0.8)	(0.6)	(0.8)	(0.7)	(0.4)	(0.7)	(0.5)	(0.7)

Note: The dependent variable is the natural log of the crime rate named at the top of each column. The data set is composed of annual county-level observations (including the District of Columbia). The top panel uses data from the time period that Lott analyzes, 1977–92. The bottom panel uses the same data set but with appended entries for the years 1993–97. County- and year-fixed effects are included in all specifications. All regressions are weighted by county population. Standard errors (in parentheses) are computed using the Huber-White robust estimate of variance. Coefficients that are significant at the .10 level are underlined. Coefficients that are significant at the .05 level are displayed in bold. Coefficients that are significant at the .01 level are both underlined and displayed in bold.

Table 8-4. *The Estimated Impact of Shall-Issue Laws on Crime Controlling for State Trends in Crime, State Data*

Percent

Item	Violent crime	Murder	Rape	Aggravated assault	Robbery	Property crime	Auto theft	Burglary	Larceny
Lott's time period (1977–92)									
1. Dummy variable model	0.2	−6.7	−3.2	−0.9	−7.1	−0.9	5.4	−2.8	−0.4
Robust std. error	(3.4)	(4.0)	(2.9)	(3.2)	(5.2)	(2.5)	(4.9)	(2.8)	(2.6)
2. Hybrid model									
Postpassage dummy	4.8	7.7	−3.0	1.9	7.9	1.1	**16.6**	3.5	−1.1
Robust std. error	(4.5)	(5.7)	(4.4)	(3.9)	(7.7)	(3.0)	(6.7)	(3.7)	(3.0)
Trend effect	−2.3	**−7.1**	−0.1	−1.4	**−7.4**	−1.0	**−5.5**	**−3.1**	0.4
Robust std. error	(1.6)	(2.1)	(1.6)	(1.5)	(3.1)	(1.0)	(2.4)	(1.5)	(1.1)
Entire period (1977–97)									
3. Dummy variable model	0.0	−1.9	−1.2	0.1	−0.7	1.9	4.6	0.7	2.0
Robust std. error	(2.6)	(3.0)	(2.2)	(2.8)	(3.2)	(1.7)	(3.0)	(2.1)	(1.7)
4. Hybrid model									
Postpassage dummy	0.2	2.7	1.2	1.0	**6.2**	**3.4**	**10.1**	3.1	**2.8**
Robust std. error	(2.9)	(2.9)	(2.5)	(3.3)	(3.1)	(1.7)	(2.8)	(2.2)	(1.7)
Trend effect	−0.2	**−3.5**	**−1.8**	−0.9	**−5.3**	**−1.2**	**−4.3**	**−1.9**	−0.6
Robust std. error	(0.8)	(1.0)	(0.7)	(1.0)	(1.2)	(0.6)	(0.9)	(0.7)	(0.6)

Note: The dependent variable is the natural log of the crime rate named at the top of each column. The data set is composed of annual state-level observations (including the District of Columbia). The top panel uses data from the time period that Lott analyzes, 1977–92. The bottom panel uses the same data set but with appended entries for the years 1993–97. State- and year-fixed effects are included in all specifications. All regressions are weighted by state population. Standard errors (in parentheses) are computed using the Huber-White robust estimate of variance. Coefficients that are significant at the .10 level are underlined. Coefficients that are significant at the .05 level are displayed in bold. Coefficients that are significant at the .01 level are both underlined and displayed in bold.

Dropping the Arrest Rate and Including the Incarceration Rate

Donohue and Steven Levitt did not use the arrest rate (that is, arrests divided by crimes) in estimating crime equations to test the impact of interventions unrelated to shall-issue laws.[24] Instead, they relied on state incarceration data because of the bias of having the crime rate on both the left-hand and right-hand side of the regression equation when the arrest rate is used as an explanatory variable.[25] As noted, the problems with the arrest rate are compounded when county data are used because a number of counties will be excluded from the analysis because of missing arrest rate data or the fact that when no observations of a crime are reported in a certain county in a certain year, the arrest rate for that county is undefined, which will disproportionately exclude low-crime areas from the analysis.[26] As Ayres and Donohue emphasized, the incarceration rate may be a useful proxy in its place, and I have repeated the analysis of tables 8-1 through 8-4 by replacing the arrest rate with the state incarceration rate as a control variable.[27] The bottom line is that in most ways the analysis changes little from this alteration, although if anything the Lott story is weaker still using the incarceration rate.

At the Brookings Conference on Gun Violence, Willard Manning suggested that it might be preferable simply to eliminate the arrest rate and incarceration rate since they are not truly exogenous variables but will be in part caused by the crime rate (which is the dependent variable in the various regressions). William Alan Bartley and Mark A. Cohen report that generally simply dropping the arrest rate tends to marginally weaken the Lott story across the board. Since both changes (replacing the arrest rate with the incarceration rate or simply dropping the arrest rate) tend to modestly hurt the more-guns, less-crime hypothesis, I will continue to present regressions with the arrest rate in order to be conservative and to promote greater comparability with the Lott results.[28]

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24. Donohue and Levitt (2001).

25. Measurement error in the crime variable will cause spurious negative correlation between the crime rate and the arrest rate (arrests/crime).

26. Excluding data by virtue of the realization of the value on the dependent variable is generally problematic. In the dummy variable model for violent crime for the 1977–92 period, the regression had 46,052 county-year observations when the incarceration rate was the explanatory variable but only 43,451 when the arrest rate data were used. Thus using the incarceration rate rather than the arrest rate increases the sample size by 6 percent.

27. Note the state incarceration rate is not perfect for the two county data analysis tables since we do not have incarceration rates by county.

28. Bartley and Cohen (1998). When I ran the hybrid model on a disaggregated basis for the county data set for 1977–97, the results overwhelmingly showed that more jurisdictions experienced increases than decreases in crime from shall-issue laws. Dropping arrest rates from this regression reduces (but not to one) the ratio of jurisdictions experiencing crime increases to those experiencing crime decreases.

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Introducing Lead and Lag Dummies

The dummy, spline, and hybrid models used in tables 8-1 through 8-4 to esti-
mate the effect of the adoption of a shall-issue law imposed a great deal of struc-
ture by limiting the response to an upward or downward shift in crime or a
changed linear time trend. Obviously, more complex responses are possible, and
by including a series of postpassage dummies, we can allow the data to reveal the
pattern in crime change (if any) that follows the adoption of the shall-issue laws,
rather than constraining the estimates to fit a prespecified structure.

Panel data analyses of the type that we have shown thus far implicitly assume
that the passage of the shall-issue law is an exogenous event. This assumption is
necessary if, for example, the estimated coefficient on a postpassage dummy is
to be interpreted as an unbiased measure of the impact of the law. Including
a series of prepassage dummies can tell us whether crime is changing in un-
expected ways before the shall-issue laws are passed.

As David Autor, John Donohue and Stewart Schwab have indicated in ana-
lyzing the impact of state laws involving exceptions to employment at will: "Ide-
ally, from the perspective of getting a clean estimate of the impact of the [rele-
vant state laws], the lead dummies would be close to zero and statistically
insignificant."[29] Conversely, if the coefficients on the lead dummies are statisti-
cally significant, then this reveals the presence of systematic differences between
adopting and nonadopting states that are not captured by the statistical model
and that are present even before the laws are implemented. Since the statistical
model cannot explain the differences between the two sets of states before pas-
sage, there is less reason for confidence that the model is able to explain the dif-
ferences between the two sets of states after passage. In other words, significant
lead dummies can be taken as another indicator of model misspecification.

Indeed, it is not hard to envision how such problems could exist in the shall-
issue law context. For example, Douglas Bice and David Hemley find that the
demand for handguns is sensitive to the lagged violent crime rate, which may
suggest the following causal sequence: increases in crime lead to increased de-
mand for guns, which in turn leads to increased pressure on legislatures to adopt
laws allowing citizens to carry concealed handguns.[30] In this event, crime would
be elevated from some extraneous event, the shall-issue law would be adopted,
and when crime returned to normal levels the regressions shown in tables 8-1
through 8-4 would erroneously attribute the crime drop to the shall-issue law.

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29. Autor, Donohue, and Schwab (2001).

30. Bice and Hemley (2001). We have recent evidence that one consequence of the terrorist at-
tacks of September 11 is that gun sales rose sharply.

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This phenomenon would then bias our estimates of the effect of shall-issue laws by making them seem to reduce crime even if they did not.

To explore the possibility of this endogeneity or other model misspecification, we estimated the impact of shall-issue laws while introducing three lead dummies, one estimating the crime rate five to six years before adoption, the second estimating the crime rate three to four years before adoption, and the third estimating the situation one to two years before adoption. Other time dummies are included to estimate the crime situation in the year of and after adoption, two to three years after adoption, four to five years after adoption, six to seven years after adoption, and eight or more years after adoption. Tables 8-5 and 8-6 show the results of this estimation of lead and lag dummies for the initial Lott and expanded time periods for both the county and state data sets.[31]

Table 8-5 tells a story that is about as far as possible from the ideal. Rather than the lead dummies being close to zero and statistically insignificant, they are often quite large and highly significant. For example, for the entire 1977–97 period, table 8-5 (estimated on county data) reveals that for every crime category except murder there are very large positive and statistically significant coefficients in the three dummies before passage occurred. This implies that in the years *before* adoption, crime was higher than average in the adopting states, controlling for national effects occurring each year, the average rate of crime in each county overall, and an array of explanatory variables. Of course, no one would make the mistake of attributing the large positive prepassage coefficients to a *subsequently* adopted shall-issue law, but their presence suggests that one must be very careful in attributing the negative coefficients in the postpassage period to the shall-issue law. At the very least, one must acknowledge the possibility that high crime levels induce passage of shall-issue laws, and that the subsequent return to more normal crime levels is now being incorrectly attributed to the laws.

How are the lead and lag results to be interpreted? Look at the table 8-5 results for 1977–97. A good place to start is to compare the estimated effects for one or two years before passage with the effects for two or three years after. This comparison has two advantages: all twenty-four states enter into the estimate of this prepassage period, and twenty-one of the twenty-four enter into this postpassage dummy.[32] (For the next two dummies, only the ten original states that Lott evaluated for the 1977–92 period are included in the estimation; and it

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31. In both tables 8-5 and 8-6, the dummies were chosen to reflect the information available as of 1992. Thus, even though we know, for example, that four states (Alaska, Arizona, Tennessee, and Wyoming) adopted shall-issue laws in 1994, these states do not appear in the lead dummies for three to six years before adoption.

32. The reason is that states that pass the law in 1996, say, will contribute data to the "year of or year after" dummy in both 1996 and 1997 but will never contribute to the successive dummies.

Table 8-5. *The Estimated Impact of Shall-Issue Laws on Crime—Leads and Lags to Adoption, County Data*

Percent

Item	Violent crime	Murder	Rape	Aggravated assault	Robbery	Property crime	Auto theft	Burglary	Larceny
Lott's time period (1977–92)									
5 or 6 years prior	–1.9	2.3	1.0	–2.4	0.5	2.3	2.5	1.6	2.6
Robust std. error	(1.5)	(3.3)	(1.9)	(2.2)	(2.8)	(1.3)	(2.4)	(1.5)	(1.6)
3 or 4 years prior	–7.0	5.2	–2.0	–10.0	0.8	2.7	3.6	0.4	2.6
Robust std. error	(1.6)	(3.2)	(1.9)	(2.0)	(2.4)	(1.3)	(2.5)	(1.3)	(2.3)
1 or 2 years prior	–2.8	3.0	–2.2	–9.7	8.9	6.5	10.1	4.8	7.7
Robust std. error	(1.5)	(3.0)	(1.9)	(1.8)	(2.2)	(1.5)	(2.3)	(1.3)	(2.5)
Year of or year after	–3.2	2.1	–1.2	–9.3	6.9	6.2	14.3	5.9	2.6
Robust std. error	(1.6)	(3.1)	(2.1)	(.9)	(2.4)	(2.5)	(2.3)	(1.5)	(6.0)
2 or 3 years after	–3.8	–0.1	–5.8	–8.5	5.4	9.8	14.0	6.0	10.1
Robust std. error	(1.8)	(3.3)	(2.3)	(2.0)	(2.6)	(1.4)	(2.8)	(1.6)	(1.7)
4 or 5 years after	–10.5	–17.3	–15.0	–15.4	1.8	7.1	17.5	2.6	8.9
Robust std. error	(2.5)	(4.3)	(3.0)	(2.9)	(3.8)	(1.7)	(4.8)	(1.9)	(3.0)
6 or 7 years after	–47.2	–26.8	7.1	–64.0	–22.3	9.7	–8.5	14.3	9.2
Robust std. error	(4.9)	(15.7)	(6.3)	(6.9)	(8.7)	(2.7)	(5.0)	(2.9)	(3.2)

Entire period (1977–97)

5 or 6 years prior	**3.7**	0.4	1.3	**3.9**	**5.6**	**6.4**	3.9	**6.7**	**7.9**
Robust std. error	(1.1)	(1.7)	(1.3)	(1.2)	(1.5)	(1.1)	(1.7)	(1.0)	(1.9)
3 or 4 years prior	**4.3**	1.0	1.6	**4.7**	**9.1**	**7.2**	**8.1**	**5.6**	**9.6**
Robust std. error	(1.2)	(2.0)	(.3)	(1.5)	(1.6)	(1.1)	(1.8)	(1.0)	(1.9)
1 or 2 years prior	**6.5**	−2.9	3.7	**6.7**	**13.2**	**10.3**	**11.0**	**6.5**	**13.9**
Robust std. error	(1.7)	(2.2)	( )	(1.7)	(1.9)	(1.2)	(1.9)	(1.2)	(2.0)
Year of or year after	**8.3**	−2.0	**5.7**	**9.1**	**14.0**	**12.8**	**17.8**	**9.6**	**14.5**
Robust std. error	(1.9)	(2.5)	(1.6)	(1.9)	(2.1)	(1.6)	(2.3)	(1.3)	(3.3)
2 or 3 years after	**6.1**	−4.2	2.6	**6.3**	**10.7**	**13.3**	**17.7**	**9.0**	**17.4**
Robust std. error	(2.0)	(2.8)	(1.8)	(2.1)	(2.4)	(1.4)	(2.6)	(1.5)	(2.1)
4 or 5 years after	1.6	**−11.2**	**−8.1**	2.4	**7.8**	**16.0**	**20.6**	**6.2**	**22.0**
Robust std. error	(2.1)	(3.2)	(2.1)	(2.3)	(2.5)	(1.4)	(2.6)	(1.6)	(2.1)
6 or 7 years after	2.6	**−14.4**	−3.0	**6.2**	**6.1**	**19.0**	**25.6**	**8.8**	**26.9**
Robust std. error	(2.1)	(3.2)	(2.3)	(2.3)	(2.7)	(1.5)	(3.1)	(1.8)	(2.2)
8 or more years after	2.6	**−34.1**	**−18.1**	**−15.5**	−10.6	**17.8**	6.3	**−11.3**	6.9
Robust std. error	(2.6)	(5.8)	(5.0)	(5.0)	(5.1)	(1.8)	(5.2)	(4.1)	(4.6)

Note: The dependent variable is the natural log of the crime rate named at the top of each column. The data set is composed of annual county-level observations (including the District of Columbia). The top panel uses data from the time period that each analyzes, 1977–92. The bottom panel uses the same data set but with appended entries for the years 1993–97. County- and year-fixed effects are included in all specifications. All regressions are weighted by county population. Standard errors (in parentheses) are computed using the Huber-White robust estimate of variance. Coefficients that are significant at the .10 level are underlined. Coefficients that are significant at the .05 level are displayed in bold. Coefficients that are both underlined and displayed in bold.

Table 8-6. *The Estimated Impact of Shall-Issue Laws on Crime—Leads and Lags to Adoption, State Data*

Percent

Item	Violent crime	Murder	Rape	Aggravated assault	Robbery	Property crime	Auto theft	Burglary	Larceny
Lott's time period (1977–92)									
5 or 6 years prior	−3.4	0.1	1.1	0.5	8.8	−0.7	−2.6	−0.9	−0.6
Robust std. error	(2.7)	(4.6)	(2.7)	(2.7)	(5.4)	(1.9)	(4.6)	(2.2)	(1.8)
3 or 4 years prior	−12.7	2.5	−1.6	−11.7	0.2	−4.2	−6.0	−6.5	−3.6
Robust std. error	(2.8)	(4.1)	(2.5)	(2.9)	(4.9)	(1.9)	(4.0)	(2.3)	(2.0)
1 or 2 years prior	−14.3	−0.9	−1.1	−15.6	−1.7	−3.2	−0.3	−5.6	−3.4
Robust std. error	(3.0)	(3.8)	(3.2)	(3.5)	(5.7)	(2.5)	(4.8)	(2.8)	(2.7)
Year of or year after	−16.5	−1.0	−3.8	−17.8	−4.0	−3.4	5.5	−5.7	−4.8
Robust std. error	(3.8)	(4.9)	(4.1)	(4.5)	(5.9)	(2.8)	(5.1)	(3.3)	(2.9)
2 or 3 years after	−20.2	−6.3	−7.6	−21.4	−10.2	−3.8	0.8	−10.1	−2.9
Robust std. error	(3.8)	(4.8)	(4.8)	(3.9)	(5.9)	(2.7)	(6.2)	(3.4)	(2.7)
4 or 5 years after	−31.5	−25.2	−3.3	−34.6	−25.3	−10.0	−18.0	−19.9	−6.0
Robust std. error	(6.0)	(9.4)	(5.5)	(6.0)	(8.6)	(3.8)	(7.8)	(5.1)	(3.6)
6 or 7 years after	−53.7	−62.2	−11.7	−65.9	−43.0	−9.0	−13.2	−9.0	−10.9
Robust std. error	(7.1)	(13.7)	(8.7)	(9.2)	(11.5)	(6.1)	(9.5)	(7.2)	(6.1)

Entire period (1977–97)									
5 or 6 years prior	**3.4**	3.9	1.3	**4.2**	3.4	1.8	4.4	2.0	2.3
Robust std. error	(1.7)	(2.7)	(1.9)	(2.0)	(3.1)	(1.7)	(3.0)	(2.1)	(1.8)
3 or 4 years prior	0.8	**6.8**	0.9	2.6	0.7	0.8	3.5	-0.8	1.7
Robust std. error	(2.3)	(3.1)	(2.2)	(2.6)	(3.3)	(1.8)	(3.1)	(2.5)	(1.8)
1 or 2 years prior	-2.0	_5.6_	-0.4	-1.5	2.4	1.2	**_9.5_**	-0.5	1.5
Robust std. error	(2.7)	(3.0)	(2.5)	(3.2)	(3.5)	(1.9)	(3.6)	(2.3)	(2.0)
Year of or year after	-4.1	**7.8**	-1.6	-3.6	1.1	2.7	**_16.9_**	0.3	2.0
Robust std. error	(3.4)	(3.8)	(3.0)	(4.1)	(4.0)	(2.2)	(4.2)	(2.7)	(2.1)
2 or 3 years after	-6.3	5.7	-4.2	-4.6	-2.8	3.4	**_17.4_**	-1.7	3.8
Robust std. error	(4.2)	(4.6)	(3.9)	(4.5)	(5.0)	(2.4)	(5.2)	(3.2)	(2.4)
4 or 5 years after	**_-15.9_**	-4.4	**_-11.7_**	**_-13.1_**	**_-14.9_**	-1.6	**12.0**	**_-11.1_**	0.1
Robust std. error	(4.6)	(5.7)	(4.9)	(4.8)	(5.5)	(2.8)	(5.8)	(3.8)	(2.8)
6 or 7 years after	**_-14.7_**	-1.9	**_-12.8_**	**-12.0**	**-14.8**	-0.8	**11.7**	**_-12.6_**	0.8
Robust std. error	(5.5)	(5.8)	(5.0)	(5.7)	(6.7)	(3.2)	(5.1)	(4.3)	(3.3)
8 or more years after	**_-17.1_**	-2.8	**_-12.4_**	-11.8	**-20.5**	-3.8	4.3	**_-13.8_**	-2.4
Robust std. error	(7.9)	(9.2)	(7.3)	(8.9)	(8.0)	(4.1)	(5.3)	(5.0)	(4.5)

Note: The dependent variable is the natural log of the crime rate named at the top of each column. The data set is composed of annual state-level observations (including the District of Columbia). The top panel uses data from the time period that Lott analyzes, 1977–92. The bottom panel uses the same data set but with appended entries for the years 1993–97. State- and year-fixed effects are included in all specifications. All regressions are weighted by state population. Standard errors (in parentheses) are computed using the Huber–White robust estimate of variance. Coefficients that are significant at the .10 level are underlined. Coefficients that are significant at the .05 level are displayed in bold. Coefficients that are significant at the .01 level are both underlined and displayed in bold.

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seems plausible that any effect of the law should show up by two or three years
after passage.)

For the 1977–97 period, the effect for the "two or three years after" dummy
is seen to be highly positive and statistically significant in seven of the nine cate-
gories. The other two categories are insignificant, with one negative (murder)
and one positive (rape). Importantly, in all cases the dummy just before passage
has virtually the same size and sign as the dummy after passage. Certainly, there
is no evidence of any statistically significant decline in the value of the estimated
effect across these two periods, which is not what one would expect if shall-issue
laws reduced crime. Lott mentions one danger in this particular pre- and post-
passage comparison—it may fail to capture a beneficial impact of the law if crime
is peaking at the time of passage and then the law reverses the upward trend—
the so-called inverted V hypothesis. Although there might be some hint of this
for violent crime, rape, aggravated assault, and robbery, the effects are not sta-
tistically significant (and, even if real, could reflect a regression to the mean effect
as opposed to a benign influence of the shall-issue law).

The comparable lead-lag regressions on the state data are shown in table 8-6.
The first difference to note in comparing the 1977–97 results for tables 8-5 and
8-6 is that while the lead dummies in table 8-5 were all positive (suggesting
crime was higher than expected just before passage), the lead dummies in table
8-6 are only positive and significant for murder and auto theft. Thus, if we be-
lieve the county data, it seems that shall-issue laws are adopted during unusually
high crime periods, but the state data results suggest this is not true for all crimes
(but may be true for murder and auto theft). The pre- and postpassage com-
parison with table 8-6 leads to a similar conclusion to that of table 8-5: there is
no evidence of a statistically significant drop in crime from the passage of the
shall-issue law, and the inverted V story does not appear to be a factor (the only
hint of the story is for murder, but again the effect is not statistically significant).[33]

Although the county and state results have some discrepancies, the general
pattern is that any result that is statistically significant for the "two and three years
after" dummy was similarly signed and significant in the period before adoption,
suggesting that the "effect" (the change in crime) preceded the alleged cause (the
shall-issue law). A supporter of the Lott thesis might note that the dummies for
the periods more than three years after passage tend to become negative and sta-

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33. The analysis was also repeated by adding state time trends to the county and state analyses
shown in tables 8-5 and 8-6. The county results again showed that crime was significantly higher
during the prepassage period and if anything tended to rise (though not significantly) in the second
and third year after the shall-issue law was adopted. The state pre- and postpassage comparisons show
a tendency for crime to fall after passage (except for aggravated assault), but none of the changes is
statistically significant.

tistically significant, but in my opinion the coefficient estimates for the dummies lagged beyond three years tend to weaken Lott's case rather than buttress it. First, drops in crime of 50 to 60 percent, which can be seen for certain crimes in the 1977–92 period in both the county and state data are simply too large to be believed. Second, the ostensibly growing effect on crime—see the increasingly larger negative numbers after passage in table 8-5—are taken by Lott as evidence that shall-issue laws become more beneficial over time, but something very different is at work. The observed pattern again shows that numerous states experiencing *increases* in crime after passage drop out of the analysis because these states' laws were adopted too close to 1997 to be included in the estimate for beyond three years. (Indeed, none of the fourteen shall-issue laws that were adopted after the period for inclusion in Lott's original work affect the estimates of these "after three years" dummies). Presumably, more complete data that would allow those states to remain in the estimation would weaken the observed negative effect for the period after three years, for as already noted, if one runs the dummy variable or Lott spline model for the period 1991–97, the results are striking: in every case the shall-issue law is associated with more crime, and these increases are always statistically significant for the dummy variable model and statistically significant at least at the .10 level for every crime but murder.

One comes away from the lead-lag discussion with a concern that endogeneity may be undermining the previous panel data estimates of the effect of shall-issue laws. Lott is aware of this problem and indeed confirms it in his book in noting that shall-issue laws "have so far been adopted by relatively low-crime states in which the crime rate is rising."[34] To his credit, he tries to use the appropriate two-state least squares (2SLS) technique to address the problem of endogenous adoption of shall-issue laws. However, it is well known that finding a suitable instrument that is correlated with the presence of a shall-issue law but uncorrelated with crime (except through the influence of the shall-issue law on crime) is notoriously difficult. Lott creates his instrumental variable by regressing the presence of a shall-issue law on rates for violent and property crime and the change in those rates; percent of state population in the National Rifle Association, percent of state population voting for Republican presidential candidate, percent of blacks and whites in state population, total state population; dummies for the South, Northeast, and Midwest; and year dummies.[35]

The effort is commendable, but the results prove unreliable. My immediate thought on seeing this list of instruments is that one should not be including the crime rates since they are not exogenous influences. The percent of the state

34. Lott (2000, p. 120).
35. Lott (2000, p. 118).

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population in the National Rifle Association might be a good instrument, but
I do not have that information (and have been unable to get it), so I am unable
to conduct my own 2SLS estimation. As Dan A. Black and Daniel S. Nagin and
Jens Ludwig have stressed, Lott's 2SLS regressions yield such implausibly high
estimates for the crime reduction generated by a shall-issue law—reductions in
homicides of 67 percent, in rapes of 65 percent, and in assaults of 73 percent—
that one is forced to conclude that Lott's instruments, and hence his 2SLS esti-
mates, are not valid.[36]

Disaggregating the Results by State

On the surface, the initial tables 8-1 and 8-2 created the impression that the
panel data regressions establish a prima facie case that shall-issue laws reduce
crime (or, at least in the dummy variable county model, reduce violent crime
while increasing property crime). The analysis done so far has always estimated
an aggregated effect for the laws across all adopting states. Since the previous dis-
cussion of the estimates on the 1991–97 period indicates that the later-passing
states experienced statistically significant *increases* in crime, there is reason for
concern that the aggregated estimates may be creating a misleading picture of
the effect of the shall-issue laws. This effect is buttressed by the fact that the
county-level data suggest a problem of endogeneity in the lead-lag analysis, and
the most focused inquiry on the comparison of pre- and postpassage effects
when most states are included in the analysis suggests that the aggregated analy-
ses are misleadingly affected by the changing composition of the states included
in the postpassage period beyond three years.

   One way to explore the factors that drive the overall results in these aggregated
analyses is to change the specification in both models to predict a state-specific
effect from the passage of the law. This approach—that is, having a separate post-
passage dummy in the dummy variable model for each adopting state and a sep-
arate postpassage trend in the linear model for each adopting state—can reveal
whether the patterns estimated in the aggregated regressions hold up in the more
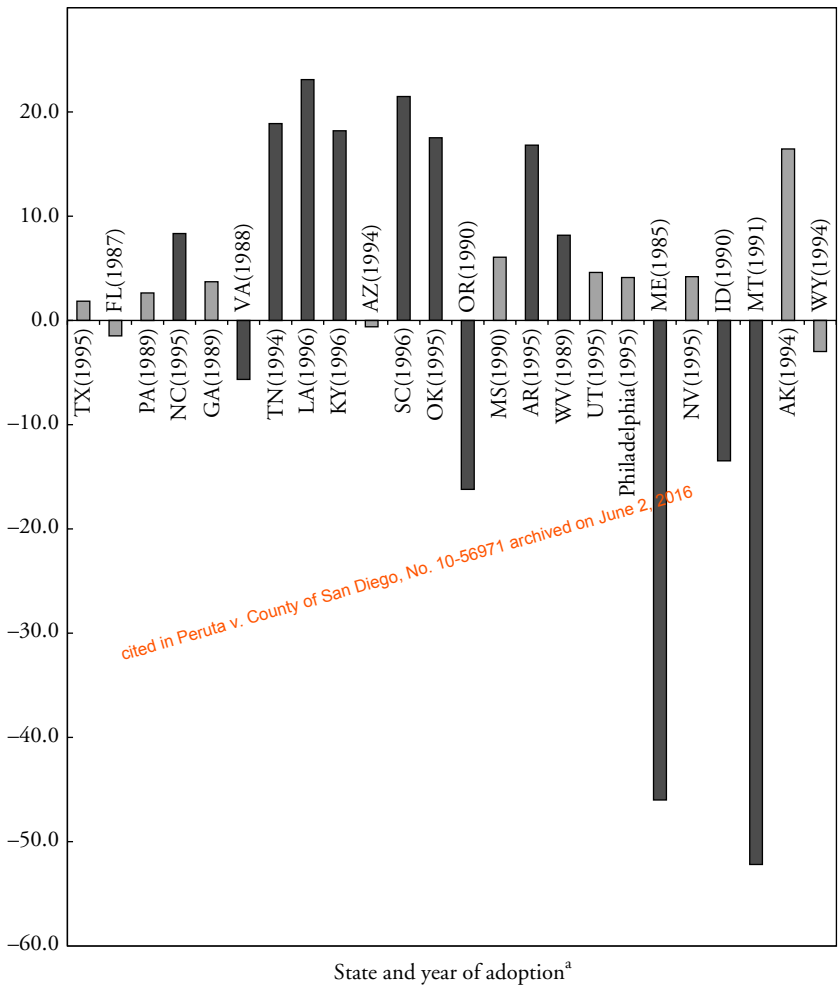disaggregated analysis.

Disaggregating the Dummy Variable Model

Figures 8-1 through 8-4 use a modified dummy variable model to depict the es-
timated effects on violent crime, murder, robbery, and property crime from pass-
ing a shall-issue law for each of the twenty-four states (or more precisely, twenty-

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36. Black and Nagin (1998, p. 211); Ludwig (1998, p. 242).

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Figure 8-1. *Estimated Effect of Shall-Issue Laws on Violent Crime, Dummy Variable Model*
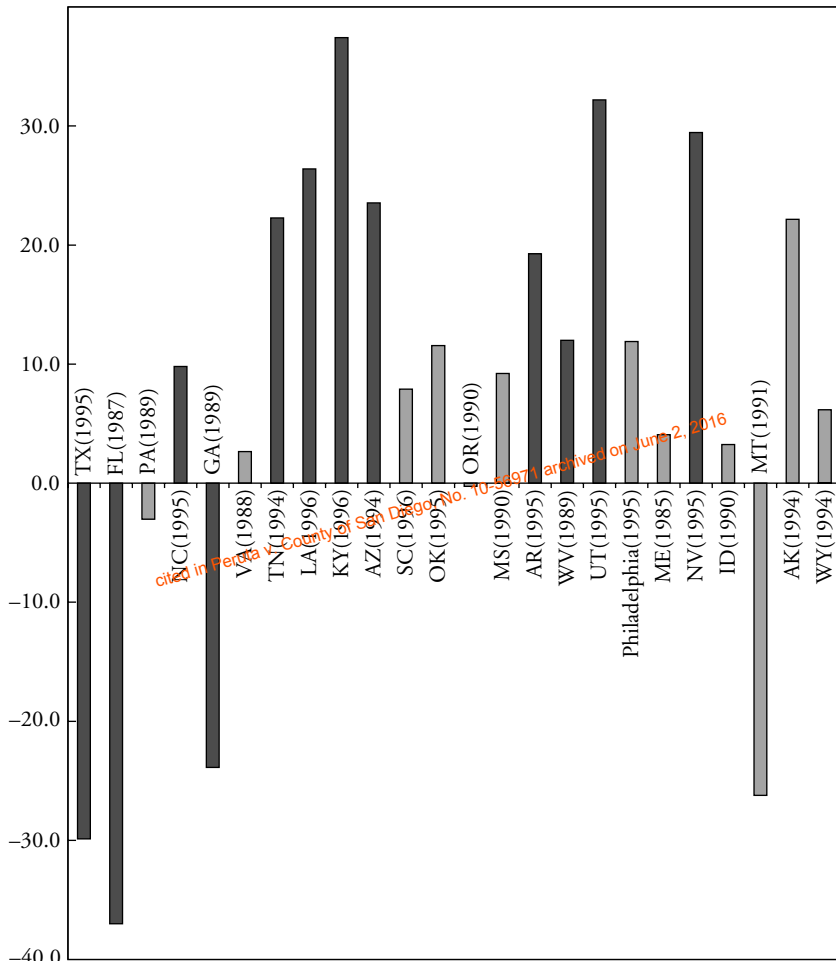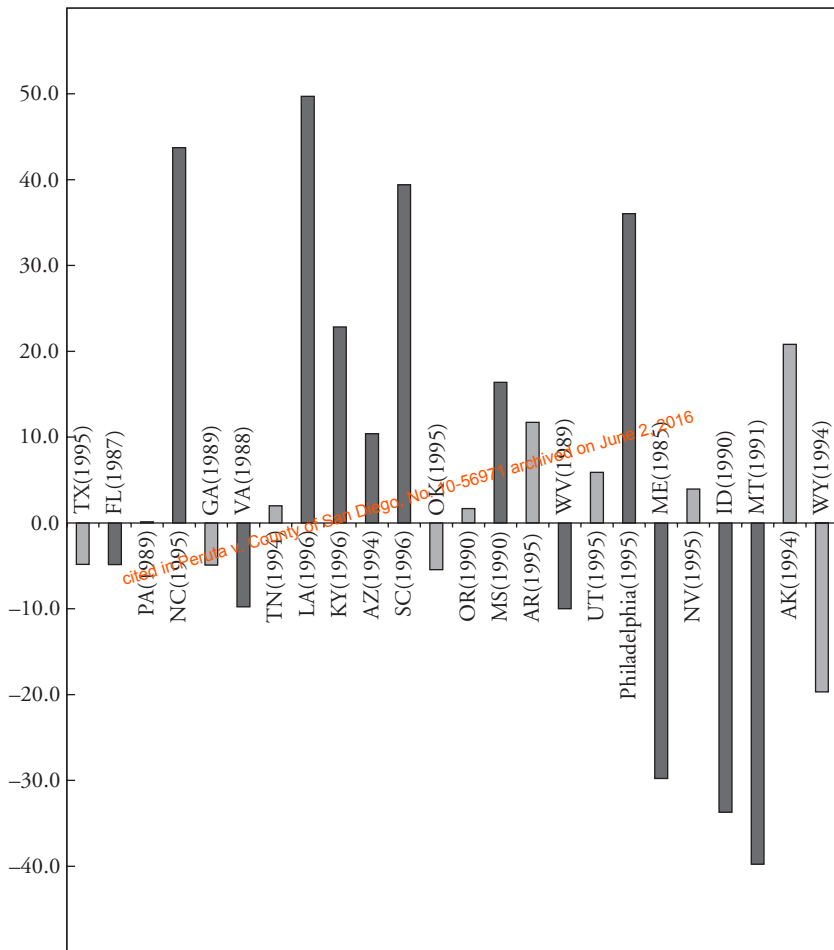
Shall-issue violent crime effect (percent)

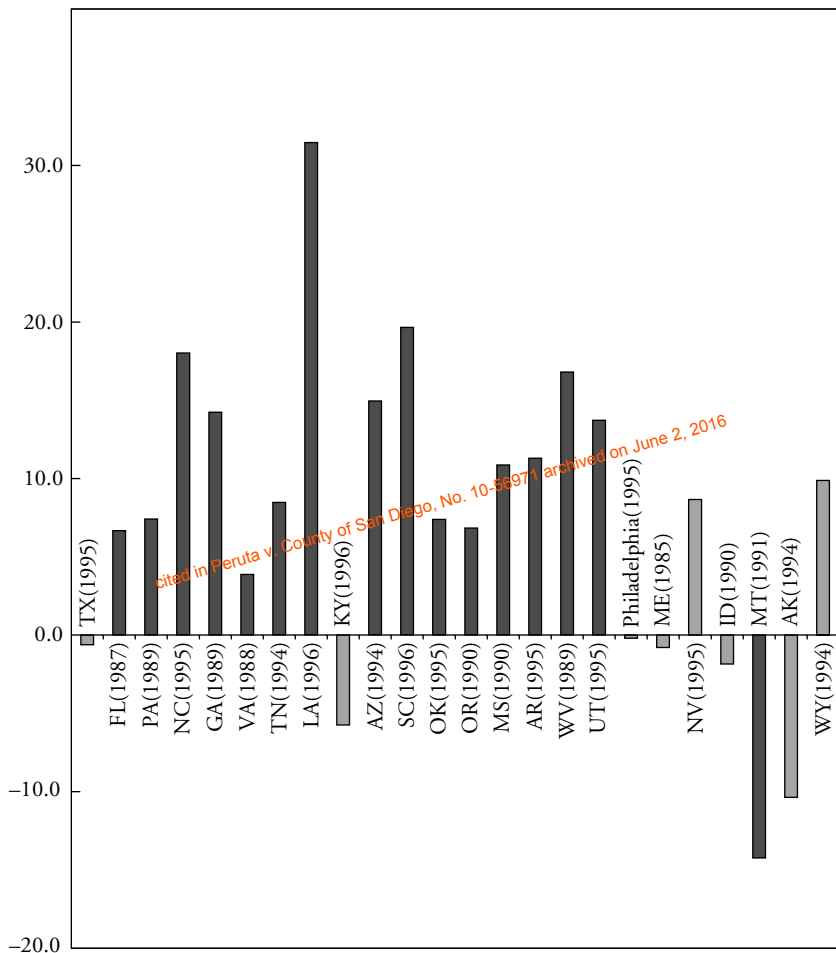

State and year of adoption[a]
Estimated effect for all jursidictions: .21% (t value: .19)

a. The dark shade means statistically significant.

three states and one city) that adopted such statutes between 1977 and 1996. These figures array the twenty-four jurisdictions in declining order of population size and indicate the year in which the shall-issue law was adopted, the estimated effect by state, and the estimated effect across all jurisdictions. Beginning with violent crime, one again sees that the aggregated effect (shown at the bottom of the

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Figure 8-2. *Estimated Effect of Shall-Issue Laws on Murder, Dummy Variable Model*

Shall-issue murder effect (percent)

State and year of adoption[a]
Estimated effect for all jursidictions: –7.77% (t value: –4.57)

a. The dark shade means statistically significant.

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Figure 8-3. *Estimated Effect of Shall-Issue Laws on Robbery, Dummy Variable Model*

Shall-issue robbery effect (percent)

State and year of adoption[a]
Estimated effect for all jursidictions: −.38% (t value: −.29)

a. The dark shade means statistically significant.

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Figure 8-4. *Estimated Effect of Shall-Issue Laws on Property Crime, Dummy Variable Model*

Shall-issue property effect (percent)

State and year of adoption[a]
Estimated effect for all jurisdictions: 7.6% (t value: 9.3)

a. The dark shade means statistically significant.

table) is small and statistically insignificant once Lott's data set is expanded to add the years 1993–97 (figure 8-1).[37] While the extension of the data set destroys Lott's claim that shall-issue laws reduce overall violent crime (that is, the move from regression 1 to regression 4 in table 8-1 eliminates the estimated negative effect for violent crime), and the aggregated results for robbery have never been sizable or statistically significant in the county dummy variable model (regressions 1 and 4 of table 8-1), the aggregated murder results remain large and statistically significant in support of Lott's claim that shall-issue laws lower crime (regressions 1 and 4 of table 8-1). But when we look at the disaggregated individual state results for murder in figure 8-2, we see a pattern that is contrary to what the Lott aggregated regression suggested. Instead of shall-issue laws broadly reducing murder in adopting states, we find that the estimated postpassage effect is negative in only six of the twenty-four jurisdictions, of which only four are sizable, and only three are statistically significant. Conversely, eighteen jurisdictions have an estimated increase in murders after passage, and nine of these are statistically significant and sizable. Thus, while the overall aggregated estimate from the dummy variable model suggests that shall-issue laws lower murder rates dramatically, the picture looks remarkably different in the disaggregated analysis—there are three times as many statistically significant increases in murder as decreases.

The reason for this apparent anomaly is worth exploring. First, note that weighting by population gives far greater influence in the regression to large states: Texas and Florida (the two largest states) and Georgia (the fifth largest) were the three states with large and statistically significant estimated drops in murder after they passed shall-issue laws. As figure 8-2 indicates, the estimated aggregated effect on murder in the dummy variable model is a drop in crime of 7.8 percent. Running the aggregated regression without weighting by population lowers the estimated effect on murder from −7.8 percent to −5.1 percent. Hence, weighting clearly increases the apparent murder-reducing capacity of shall-issue laws in the aggregated dummy variable model, but it is not the entire story.

Second, as already noted, the fact that a state adopts a shall-issue law earlier means that it will have a greater impact in the estimation of any postpassage dummy in the aggregated analysis. Thus imagine a scenario under which only two states (with equal populations) adopt shall-issue laws—one in 1987 and another in 1996. Assume the effect in the two states is exactly opposite. In the early adopter crime drops by 10 percent in the first year after passage and stays at that lower level through 1997, while in the late adopter crime increases by 10 percent and will stay that way for ten years. In the disaggregated analysis, one will see equal and opposite impacts, suggesting no overall net effect on crime. This is

37. The aggregated estimate comes from table 8-1, regression 4.

what the aggregated dummy variable analysis would show if the laws had been adopted at the same time. But because one state has adopted the shall-issue law nine years later, the aggregated analysis will generate a very different result than the disaggregated analysis of the type shown in figures 8-1 through 8-3. The later adoption in the second state means that its impact will be diminished when the aggregated dummy variable model is estimated. Indeed, the aggregated effect in this hypothetical will be a drop in crime of roughly 8 percent because the ten years of a crime drop of 10 percent will be averaged with the one year of the crime increase of 10 percent. Since we have seen that the fourteen late adopters had an aggregate effect of *increasing* crime, while Lott found a dampening effect for the previous ten adopters, we can see that the aggregated analysis will give much greater weight to the earlier adopters. This explains how a few early adopters can alter the analysis to show an aggregated predicted crime drop even though most individual states are showing crime increases when their laws are adopted.

What should be concluded from this analysis? If one accepts the regression output at face value, it suggests that the clear majority of states experience *increases* in violent crime, murder, and robbery when shall-issue laws are adopted. It is only the happenstance that some of the early adopters experienced crime drops, which are disproportionately weighted in the aggregated analysis, that has generated the impression of uniform crime reductions. Figure 8-4 shows the results of this disaggregated analysis for property crime. Since virtually every adopting state experiences an increase in property crime (fifteen statistically significant crime increases, one statistically significant crime drop), the disaggregated results conform to the aggregated prediction of substantial property crime increases. This result is stronger than any crime-reducing result associated with a shall-issue law that has been presented anywhere. Thus, if one accepts the panel data results, the strongest possible conclusion about the effect of shall-issue laws is that they increase property crime. But the only theory that would explain that result is the Lott substitution hypothesis from violent crime to property crime, which is not borne out in the disaggregated analysis of figures 8-1 through 8-3. Most of the states for which we see statistically significant increases in property crime do *not* experience any drops in violent crime. If the Lott substitution story were true, it would have to be the case that the states that experienced the property crime increases also experienced a violent crime drop, and this we do not see.[38] Reading the regression results at face value, shall-issue laws increase prop-

38. For the seventeen states that experienced an *increase* in property crime shown in figure 8-4, eleven experienced an *increase* in robbery shown in figure 8-3 (of which five were statistically significant increases). The other six states conformed to the Lott story of increased property crime coupled with decreased robbery, but only three of these were statistically significant drops in robbery.

erty crime, yet without a theoretical reason to believe this effect that has any empirical backing, one may be inclined to say the regression is not working properly (perhaps because of problems of misspecification or omitted variable bias).

Disaggregating the Linear Trend Model

The patterns revealed in figures 8-1 through 8-4 for the disaggregated analysis of the dummy variable model also emerge in the comparable figures based on the linear trend model (available from the author). Recall that in table 8-1, line 5, we saw that when the linear trend model was estimated on an aggregated basis, it showed that robbery fell by a statistically significant 3.6 percent and property crime remained virtually unchanged. Although the apparent drop in robbery might be taken as support for the more guns, less crime story for the 1977–97 county data, the story collapses if one disaggregates by state. In the disaggregated analysis, robberies increased in eighteen of the twenty-four jurisdictions (nine of them were significant). In the six jurisdictions where robberies fell, in only one case (Oregon) was there a statistically significant increase in property crime. Moreover, for the seventeen (of twenty-four) states that experienced an *increase* in property crime, fifteen also experienced an *increase* in violent crime, and ten of them were statistically significant increases in violent crime. Of the remaining two states, which experienced an increase in property crime but a decrease in violent crime, in only one was the decrease statistically significant. Indeed, for the clear majority of states for all four crimes in the disaggregated analysis, shall-issue laws are associated with increases in crime, which are generally statistically significant. Although the story of murder or robbery dropping can be found in the aggregated analysis with the linear trend model, it is purely an artifact of the happenstance of early adoption that weights a few large states most heavily.

Summary

Lott and Mustard have clearly launched an enormous amount of scholarly work on the effect of laws enabling citizens to carry concealed handguns. It is not hard to see why they and others may have believed that these laws reduce crime, because simple panel data regression models for the 1977–92 data period that they first analyzed provided support for the view that some or most violent crime rates fell for the ten states that adopted shall-issue laws over that period. Indeed, some superficially supportive work—for example, a paper by Bronars and Lott (1998) arguing that the passage of a shall-issue law pushed criminals across the border into

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non-shall-issue states[39] and a paper by Lott and William M. Landes indicating the multiple victim homicides fell when shall-issue laws were adopted—might have been thought to buttress the more-guns, less-crime hypothesis.[40] Moreover, if the statistical evidence backed up the more-guns, less-crime hypothesis, the anecdotal evidence of cases in which guns were used defensively to thwart attacks and the overall estimates of the number of incidents of defensive gun might seem to provide some plausibility to the initial Lott and Mustard findings.[41]

Right from the start, though, there have been concerns. Several analysts showed that disaggregating the 1977–92 data to estimate effects on ten individual states led to a more mixed picture with some states showing increases and others showing decreases in crime.[42] Others expressed concern that the Lott and Mustard result was vulnerable because the panel data model may not adequately control for "unobserved or difficult-to-measure factors that influence local crime rates but change over time."[43] Indeed, Ludwig noted that because all shall-issue laws have minimum age requirements, any deterrent effect related to these laws should be concentrated among adults, yet the evidence did not support this prediction.

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39. The Bronars and Lott piece seemed at first to be important buttressing evidence since it purported to show that for a given metropolitan area, crime fell on the side of the border that adopted the shall-issue law but rose on the other side of the border. Unfortunately, the disaggregated results depicted in figures 8-1 through 8-4 give every reason to be suspicious of the highly aggregated Bronars and Lott result. In essence, all that Bronars and Lott showed was that a highly aggregated dummy variable for non-passing jurisdictions bordering the ten adopting states seemed to show crime increases while crime was falling for the ten adopting states. But as shown, the disaggregated results typically reveal that crime rises for most jurisdictions, which almost certainly undermines the claimed substitution effect across state lines. Unless Bronars and Lott can show that the substitution across state lines is actually occurring by linking drops in crime to passing state X with increases in crime in neighboring nonpassing state Y (which I doubt will be the case), then the Bronars and Lott article really illustrates the unreliability of the aggregated analysis that is uniformly used in the papers endorsing the more-guns, less-crime hypothesis.

40. In the wake of a recent school shooting in Germany that killed fourteen, Lott summarized his finding from the Lott and Landes study: "multiple-victim public shootings fell on average by 78 percent in states that passed [right-to-carry] laws." John Lott, "Gun Control Misfires in Europe," *Wall Street Journal,* April 30, 2002, p. A16. Although the results may at first seem persuasive, there is a major problem with the Lott and Landes data. Lott and Landes (2001). The FBI Supplementary Homicide Report (SHR) reveals more than 800 such multiple-victim deaths a year, while Lott and Landes use a Lexis search that generates only about 20. FBI (2000). While it may be that not all 800 should be included (for example, Lott and Landes would eliminate some of the murders in the FBI data because they are not committed in public places), the true number of cases is vastly greater than the number that Lott and Landes employ. Indeed, Lott and Landes have now found that when they use the SHR data, their results "were rarely statistically significant." Consequently, if their story doesn't emerge when they use the best data, why should we believe their results using much less accurate data?

41. Ludwig (1998 provides an illuminating discussion of the prevalence of defensive gun use, and that paper and Duggan (2001) provide evidence that at least raises doubts about how much the actual carrying of guns increases in the wake of the adoption of shall-issue laws.

42. Black and Nagin (1998); Dezhbakhsh and Rubin (1998); and Plassmann and Tideman (2000).

43. Ludwig (1998, p. 244); Ayres and Donohue (1999); Zimring and Hawkins (1997).

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Using a difference-in-difference-in-differences model, Ludwig showed that the evidence refuted the view that shall-issue laws resulted in relative decreases in adult homicide rates.[44]

All of this work speculated that factors such as the enormous, but geographically nonuniform, stimulus to crime caused by the crack cocaine trade in the late 1980s and early 1990s could well be generating spurious results. Ayres and Donohue noted some coding errors that, when corrected, tended to weaken some Lott results, and Duggan offered interesting evidence that more guns generate more murders and that Lott's results were eliminated with proper adjustments to the standard errors.[45]

At the same time, Bartley and Cohen showed that a hybrid model (admittedly with the Lott data set and its coding errors and in the aggregated model that Ayres and Donohue have questioned) could withstand an extreme bounds analysis to reveal drops in murder and robbery after a shall-issue law was passed for the 1997–92 full and large-county data sets.[46] Using the 1977–92 data and aggregated dummy variable and spline models, David E. Olson and Michael D. Maltz presented some generally supportive findings that shall-issue laws reduced homicides, but their finding that firearm homicides fell by 20 percent while nonfirearm homicides rose by 10 percent did not seem to fit well with a story that shall-issue laws had a deterrent impact on crime. Again, the inconsistencies were troubling, but for some these problems and the array of skeptical voices were largely ignored, especially with other studies expressing apparent approval of the Lott findings.[47] Those studies, however, were based solely on analyses of the now discredited or superseded aggregated dummy variable models that use the 1977–92 data with coding errors identified by Ayres and Donohue.[48]

But whatever the number of articles embracing or rejecting the initial Lott and Mustard results—and it is not clear to me that more articles supported Lott and Mustard or that counting the number of articles is the best measure of resolving

44. Ludwig (1998).
45. Ayres and Donohue (1999); Duggan (2001).
46. Bartley and Cohen (1998). The extreme bounds analysis simply estimates the effect of the law using all combinations of the Lott and Mustard explanatory variables and documents whether the resulting estimates are always nonzero. When the dummy variable model was used, Bartley and Cohen found that only violent crime and assault fell consistently (although perhaps the Lott inverted V story can explain some of this discrepancy). Moody (1999) also provides an extended inquiry into the Lott aggregated dummy variable model for the county data set (with the coding errors) for the period 1977–92 and finds that the shall-issue laws are associated with lower violent crime in various permutations of this aggregated dummy variable model over the early time period.
47. Moody (1999); Benson and Mast (2001); and Plassman and Tideman (2000).
48. Ayres and Donohue (1999).

the debate—there is now much more evidence on the issue than was available to almost any of the researchers who have previously examined the more-guns, less-crime hypothesis. Ayres and Donohue have shown how important the extension of the Lott and Mustard data set is to an assessment of the validity of the earlier Lott and Mustard work, and none of the researchers just discussed were aware of the Ayres and Donohue finding that running the Lott and Mustard models for the period 1991–97 generates uniform estimates of *increased* crime associated with shall-issue laws.[49] The very sharply different results between regressions run for early and late legalizers show that aggregated regression models will be misspecified.

Indeed, the lead and lag analysis discussed earlier shows that, particularly for the county data set, there is evidence of a serious problem of endogeneity or omitted variable bias, since the prepassage dummies are frequently large, positive, and statistically significant. Moreover, pre- and postpassage comparisons based on the lead and lag analysis did not provide support for any story that shall-issue laws reduced crime.

The evidence from the disaggregated state-specific estimates for the 1977–97 data should put to rest any notion that shall-issue laws can be expected to lower crime.[50] The overwhelming story that leaps out from the eight figures (looking at four crimes with both the dummy variable and the spline models) is that most states experienced *increases* in crime from the passage of shall-issue laws. In other words, if one simply runs a disaggregated state-specific version of the Lott and Mustard models on the full 1977–97 data set, a few states will be shown to have decreases in crime, but most will not, and the statistically significant estimates of increased crime will far outweigh the significant estimates of crime decreases.

If one had previously been inclined to believe the Lott and Mustard results, one might now conclude that the statistical evidence that crime will rise when a shall-issue law is passed is at least as compelling as the prior evidence that was amassed to show it would fall. However, there are still enough anomalies in the data that warrant caution. Admittedly, the updated disaggregated data push toward a more-guns, more-crime conclusion, but that model still does not address the endogeneity or omitted variable problems that seem to be lurking in the results shown in tables 8-5 and 8-6. Moreover, the figure 8-4 dummy variable disaggregated model shows that widespread increases in property crime follow the adoption of shall-issue laws, but there is no internally consistent theory that would explain this effect.[51] When a regression predicts both a potentially plau-

49. Ayres and Donohue (forthcoming).

50. Ayres and Donohue (forthcoming).

51. In the linear trend disaggregated model (not shown), shall-issue laws are still associated with property crime increases, although they are less pronounced than for the dummy variable model.

sible finding (that shall-issue laws increase violent crime) and an implausible one (that the same laws also increase property crime), my confidence in the regression is weakened.

The overall evidence suggests to me that broad (and conflicting) crime swings that occurred in the late 1980s and 1990s happened to correlate with the passage of shall-issue laws, and the panel data model seems unable to separate out the contribution of the relatively minor influence of the shall-issue law from the major impacts of these broad swings. With data problems making it unclear whether the county or state data are more reliable, with the lack of good instruments available to directly address the problems of endogeneity and the lack of good controls available to capture the criminogenic influence of crack, it is hard to make strong claims about the likely impact of passing a shall-issue law. The tidal swings in crime rates during the late 1980s and the 1990s have both helped stimulate passage of shall-issue laws as a fearful population searches for relief from anxiety and obscured what the true effect of these laws on crime has been.

COMMENT BY
David B. Mustard

More than seven years ago John Lott and I decided to examine the impact of shall-issue laws on crime and accidental deaths. As someone who passionately disliked firearms and who fully accepted the conventional wisdom that increasing the gun ownership rate would necessarily raise violent crime and accidental deaths, I thought it obvious that passing these laws would cause a host of problems. It is now almost six years since I became convinced otherwise, and John Lott and David Mustard concluded that shall-issue laws reduce violent crime and have no impact on accidental deaths.[52] Since then we have distributed the data to about seventy groups of scholars and policymakers, thus facilitating an extensive research agenda concerning the efficacy of right-to-carry laws. John Donohue's chapter first evaluates the basic Lott-Mustard arguments and the subsequent research, and second, provides some new empirical work.

Lott-Mustard and Subsequent Research

An overview of the right-to-carry scholarly research in the past six years is a good start. One fundamentally important point is how much the terms of the debate

52. Lott and Mustard (1997).

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have been significantly altered. Before this explosion of research, many presumed that shall-issue laws would increase crime. However, since Lott-Mustard no empirical research has made a case for shall-issue laws increasing crime. Instead, the literature has disputed the magnitude of the decrease and whether the estimated decreases are statistically significant. This work is notable in the broader gun literature because right-to-carry laws are the first gun law to produce an empirically verifiable reduction in criminal activity. The empirical work in refereed scholarly journals presents a much stronger case for the efficacy of shall-issue laws to reduce crime than any other gun control law. From a public policy perspective, if one believes there is insufficient evidence to endorse concealed-carry laws, then to be logically consistent one must also oppose the implementation of waiting periods, safe-storage laws, and other gun laws even more adamantly.

Given the sizable empirical research devoted to this issue and the hundreds of thousands of regressions that have been run, the small number of positive and statistically significant estimates is absolutely striking. Even if one uncritically accepts the most negative reviews of Lott-Mustard at face value, there is still more evidence that shall-issue laws reduce, rather than raise, crime. For example, Mark Duggan, widely recognized as producing one of the most critical papers, reports thirty regressions of the impact of right-to-carry laws on violent crime. Only one of the thirty coefficient estimates is positive and statistically significant (robbery in one specification). In contrast, fourteen of the thirty have negative and statistically significant coefficient estimates, and most of the rest are negative and statistically insignificant.[53] Similarly Daniel A. Black and Daniel S. Nagin obtain a positive and significant coefficient in one specification for assaults but only while using the problematic quadratic estimation procedure. However, this same table reports thirteen negative and statistically significant coefficient estimates, and the remaining estimates are disproportionately negative and statistically insignificant.[54]

Donohue's chapter starts by discussing the basic model and methodology of Lott-Mustard. Unfortunately, many of the criticisms have already been addressed extensively in the literature.[55] Some criticisms were even discussed in the original Lott-Mustard article. Because space constraints limit the number and depth of the issues that I can address, I encourage you to investigate these additional sources more thoroughly in evaluating Donohue's chapter.

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53. Duggan (2001). Although only twelve are designated as statistically significant in the table, rape and assault in specification 2 are also statistically significant given the reported estimates of the coefficients and standard errors.

54. Black and Nagin (1998).

55. Bronars and Lott (1998); Lott (2000); Lott and Whitley (2001); articles in "Guns, Crime, and Safety" issue of *Journal of Law and Economics* 44 (2, pt. 2) (October 2001).

Like many critics, Donohue contends that different results for the impact of shall-issue laws on property crime undermine the Lott-Mustard work. He dramatically states, "Lott might respond that . . . murderers and rapists shifted over to committing property crime" and that the initial argument asserted, "that Shall-Issue laws induced massive shifts by thwarted murderers and rapists toward property crime." Regrettably, these misrepresentations of the original work continue to be made even though Lott and I have repeatedly asserted, "No one believes that hard-core rapists who are committing their crimes only for sexual gratification will turn into auto thieves."[56] Results of differing signs in no way indict our work. In the original paper we maintained that the deterrent effect should be larger on violent crime than on property crime, so the total effect on violent crime should be more negative than on property crime. Because financial gain is an important motive in some violent crimes there may be some substitution to property crime. However, to the extent that offenders reduce their involvement in all illegal activity as a result of the laws, property offenses may also decrease. Therefore, the theoretical prediction is ambiguous. In some specifications in the original paper property crimes increase, in others there is no effect, and in some there is a decrease. In writing the cost-benefit portion of the paper, we emphasized the results showing the effect of the law on property crime was positive (which also showed the smallest drops in violent crime), because we sought a lower bound on the total benefit and biased the findings *against* our conclusion that the laws provide net social benefits. Consequently, if shall-issue laws have no impact or actually reduce crime, the benefits of the law are even larger than we estimated.

Similarly, Donohue highlights another frequently repeated, yet incorrect, statement about how the relatively small decline in robbery as a result of shall-issue laws, "constitutes a major theoretical problem for Lott's interpretation." These comments about robbery neither acknowledge our initial arguments about how robbery should be affected, nor respond to Lott's subsequent arguments.[57] To briefly reiterate, the theoretical effect of shall-issue on robbery is ambiguous, because the offense category is composed of seven types of robberies. Only one of these categories involves the robbery of one person of another in a public place, which is the most likely type of robbery to be deterred by concealed carry. Clearly, the theory predicts that this type of crime should decrease. However, the theoretical prediction about the entire classification of robbery is not so clear. The other types of robbery could increase if as a result of right-to-carry laws offenders substitute from street robbery to other forms of robbery. Consequently, the effect of the law on the total category is ambiguous.

56. Lott (2000, p. 134).
57. Lott and Mustard (1997, note 26).

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One last example is that utilizing the arrest rate as a control variable in the crime rate regressions is problematic. However, Donohue does not mention that Lott and Mustard include extensive explanations of these problems, that the original article tested the robustness of the results to the inclusion of the arrest rate in a number of ways, or that Lott further tests the sensitivity of the results to different arrest rate specifications.[58] These papers show that the qualitative results were robust to omitting arrest rates from the regression, using moving averages of arrest rates, using predicted values of arrest rates, and examining large counties that had well-defined arrest rates. Furthermore, Donohue's discussion of the arrest rate misses an important point. Omitting arrest rates may generate a truncation problem because many counties with zero crime rates will be included in the regression. By construction it is impossible for a shall-issue law to reduce crime in a county that has no crime, no matter how effective the law is.[59]

Post-1992 Analysis

Donohue's second principal objective is to examine the results when the data are extended to 1997. Of all the empirical papers that examine the impact of right-to-carry laws, Donohue's chapter is unique, because it is the first to argue that the laws may increase crime. Tables 8-1 through 8-6 in his chapter present this evidence by portraying the coefficient estimates and standard errors of a series of leads and lags before and after the law passes. He contends that adding subsequent years of data demonstrates that there are differential effects between the early and late adopters of laws. I outline three central concerns about this analysis.

First, Donohue neither discusses nor controls for very important changes in right-to-carry laws. There are at least four trends that have made it more costly for law-abiding citizens to protect themselves. One, fees have increased substantially. For example, the average fee for states that implemented laws since 1994 was about 2.5 times greater than the states that adopted right-to-carry laws from 1985 to 1992. Two, the training requirements for obtaining permits have increased significantly. Of the eight states that adopted their laws before 1960, only one state had any training requirement. Of the laws adopted between 1985 and 1992, only half the states required training, which on average was relatively short. In sharp contrast, most of the states that passed laws since 1994 require training periods, and the average length of those periods is relatively long. Three, there are fewer places in which licensed individuals are legally permitted to carry. Other than the areas prohibited by federal laws, early states had few, if any, excluded

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58. Lott and Mustard (1997); Lott (2000).
59. Plassman and Tideman (2001).

areas. While many states that adopted their laws between 1985 and 1992 have few restrictions, the states since 1994 typically have extensive lists of excluded areas. Pennsylvania, which passed its law in 1989, excludes only courthouses and some government buildings, while Texas, which passed its law six years later, lists forty-eight places where carrying a concealed weapon is forbidden. Fourth, states that passed their laws later generally have more punitive penalties for carrying in unauthorized places. By raising the cost that law-abiding citizens bear in carrying a concealed weapon for self-protection, these four trends decrease the number of law-abiding citizens who can carry and the opportunities each license holder has to use a weapon for self-defense. Consequently, there are strong theoretical reasons for expecting the later laws to have different effects than earlier laws. Future empirical research should control for these changes and test the degree to which such provisions affect the carrying and crime rates. To the extent that these more restrictive laws reduce the carrying rate and the opportunities for self-defense, laws implemented later may be less efficacious.

A second concern about the new empirical work is that although it is important to know whether the coefficient estimates in the postlaw years are positive or negative, it is also important to understand how they compare to the prelaw estimates. For example, if the prelaw coefficient estimate is 8.5, and the postlaw estimate 5.5, the law may have lowered the crime rate in shall-issue states relative to the other states. To show these intertemporal effects more clearly, figure 8-5 plots the coefficient estimates from Donohue's table 8-5 county-level regression covering the 1977 to 1997 period. This figure clearly shows that all four violent crime rates plunge precipitously after the law is adopted. During the prelaw period, the murder rates are the same in shall-issue and non-shall-issue counties. After the law goes into effect, the murder rate for shall-issue counties drops dramatically. Crime rates for the other three offenses (rape, robbery, and assault) increase in the right-to-carry states before the law and plummet after the law. These drops are not simply reversions to the mean as some have suggested, because the postlaw rates for all three offenses are markedly lower than any of the prelaw rates.

Lott addressed this prelaw increase in crime in various ways in his many papers. Some methods include dropping the years immediately before and after the passage of the law, estimating regressions with instrumental variables and two-stage least squares, including nonlinear time trends, and showing that the postlaw crime rates drop far below the prelaw trend. Stephen Bronars and Lott used another strategy when they showed that when a given state passed a right-to-carry law, the crime rates in surrounding states increase.[60] There is no theoretical reason why the adoption of a law in one state should be a function of neighboring

60. Bronars and Lott (1998).

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Figure 8-5. *"Entire Period" Coefficient Estimates*

Percent change

Years before and after law

Source: John Donohue, chapter 8, in this volume.

crime rates. Last, if gun laws are adopted in response to random periods of high crime, other gun laws should exhibit similar drops in postlaw crime. However, shall-issue laws are unique among gun laws in that they are the only ones that show these large decreases in postlaw crime.

My last concern about Donohue's allegation that allowing law-abiding citizens to protect themselves increases crime rates is his lack of articulating and documenting a clear mechanism through which such an increase would occur. The most frequently articulated claim is that permit holders will use their guns to commit crimes instead of using their guns for self-defense. However, many years of evidence across different states and time periods overwhelmingly rejects such claims. In Multnomah County, Oregon, only 1 of 11,140 permit holders was arrested for a crime during a four-year period—an annual rate of only 0.2 incidents for every 10,000 holders.[61] The annual rate in Florida over a seven-year period was even lower at 0.1. In Virginia as of the beginning of 1997, not a

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61. Lott and Mustard (1997).

single concealed-carry permit holder had committed a violent crime. In North Carolina through 1997, permit-holding gun owners had not had a single permit revoked as a result of use of a gun in a crime. In South Carolina through 1997, only one permit holder had been indicted for a felony, a charge that was later dropped. Mustard showed that even those who vehemently opposed shall-issue laws have been forced to acknowledge that license holders are extremely law abiding and pose little threat.[62] Glenn White, president of the Dallas Police Association, twice lobbied against the proposed right-to-carry law, but after it finally passed he acknowledged, "I'm a convert." The president and the executive director of the Florida Chiefs of Police and the head of the Florida Sheriff's Association admitted that despite their best efforts to document problems arising from the law, they were unable to do so. Speaking on behalf of the Kentucky Chiefs of Police Association, Lt. Col. Bill Dorsey stated, "We haven't seen any cases where a [concealed-carry] permit holder has committed an offense with a firearm."[63] Many who believed that concealed-carry permit holders would threaten society actively tried to document that danger. However, they were compelled to change their minds as they observed law-abiding citizens who have no mental health histories, pay fees, and give authorities personal information do not use their weapons for inappropriate purposes. Much of the debate about concealed carry has involved detailed comments about empirical specifications and statistical estimation procedures, which has often left the average person confused. However, sometimes the most straightforward evidence, namely, the lack of criminality among law- abiding citizens who carry concealed weapons, is the most convincing and easy to understand.

COMMENT BY
Willard Manning

John J. Donohue's chapter examines the sensitivity of the results in earlier work by John Lott and his colleagues on the impact of laws granting a right to carry

62. Mustard (2001).

63. Scott Parks, "Charges against Texans with Gun Permits Rise. Law's Supporters, Foes Disagree on Figures' Meaning," *Dallas Morning News,* December 23, 1997, p. A1; Steve Patterson, "Concealed-Weapons Law Opponents Still Searching for Ammunition," *Florida Times-Union,* May 9, 1998, pp. A1, A3; Terry Flynn, "Gun-Toting Kentuckians Hold Their Fire," *Cincinnati Enquirer,* June 16, 1997, p. A1. Kentucky state police trooper Jan Wuchner is also quoted as saying that he has "heard nothing around the state related to crime with a gun committed by permit holders. There has been nothing like that I've been informed of."

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a concealed weapon on several different measures of criminal activity.[64] His primary concern is with the sensitivity of the results to a series of specification and analytical issues, especially with how time trends are modeled, with special attention given to allowing for state-specific time trends. Much of the earlier work assumes an additive effect of the law against the backdrop of a time trend common to states that already had a right-to-carry law, enacted such a law during the period, or did not have one during the period. He also raises other concerns about data quality and the inclusion of an additional endogenous explanatory variable. His results indicate that some of the conclusions in the seminal paper by Lott and Mustard and other publications by Lott are not robust to specification changes.

Instead of only critiquing Donohue's chapter, in this comment I examine a set of issues common to the original work and to Donohue's chapter in this volume.[65] My focus is on econometric or statistical issues that can lead to biases in the estimates of the coefficients, the standard errors and inference statistics for the models, or both. I consider four areas:

— Correlated errors—going beyond fixed effects;
— Multiple comparisons;
— Endogeneity of the right-to-carry laws; and
— General concerns about estimation and interpretation of log models.[66]

The first two are serious because both Donohue and Lott seem to have a false sense of confidence in their results. The results are not as statistically significant as they indicate and may not be significantly different from zero at all. The third and fourth raise the prospect that the estimates themselves are biased. Some of the following remarks are based on my own analysis of the state-level version of the data that Lott provided to me earlier.[67]

Correlated Errors

The data employed here and in earlier work separately by Donohue and Lott involve sixteen or more years of data for states, standard metropolitan statistical

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64. Lott (2000); Lott and Mustard (1997).

65. Lott and Mustard (1997); Lott (2000).

66. My original comments also included a concern about the endogeneity of the incarceration variable as an explanatory variable. Apparently, excluding the variable does not alter the results appreciably.

67. I have used these data in an applied regression course offered to students at the University of Chicago because the dataset exhibits a number of estimation problems. By using the state-level data, I do not have to deal with problems of zeroes at the county level. That is even more complicated than the ones dealt with here.

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areas (SMSAs), or counties within states. This panel characteristic raises the prospect that the error terms for a state (or a county) are correlated over time if some unobserved factors are stable over time within a cluster (for example, state) or changing slowly. Both sets of authors have addressed this problem by employing a standard panel data solution with state (or county) fixed effects but no other correction for autocorrelated error terms within state. They also employ fixed effects for year to deal with the complex time trends in crime rates. There has been some exploration of state-specific time trends.

Over short periods, fixed or random effects may provide a good approximation to the variance-covariance matrix for the error term within a state. For a short period, slow-moving changes in unobservables in the error term will not change much. However, over longer periods of time, the approximation may be poor. I examined the autocorrelation function for the residuals from the fixed effects models for the two summary measures—violent crime and nonviolent crime per capita. The results indicate that the error structure within a state has a more complex form of autocorrelation than that indicated by a simple fixed-effects-only model. Moreover, it does not appear to fit a fixed effect combined with an autoregressive (AR) error model, such as an AR(1).

This raises the prospect that the standard errors and inference statistics for these models are biased because no further correction beyond the inclusion of fixed effects was made for autocorrelated errors.[68] Leaving out such a correction can have a pronounced effect on the efficiency of the estimates and bias in the standard errors and other inference statistics, especially if the key variables are time trended. Bias in the inference statistics can go either way depending on whether the remaining correlation in the residuals after adding the fixed effects for states and time has the same sign as the time trend in covariates (the x's) net of fixed effects. The direction and magnitude depend on the specific data.

There are several alternatives available to correct the inference statistics. Two options are relatively easy to implement. The first is to conduct a bootstrap of the analysis, bootstrapping all of the observations for each state as a cluster, rather than bootstrapping individual state-year observations. The second alternative is to use general estimating equations (GEE) after determining the form of the autocorrelation after a fixed effect for each state has been included.[69]

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68. The various papers report either weighted least squares results under a fixed-effect specification or weighted with robust standard errors (corrected for heteroscedasticity using the sandwich estimator or Eicker-Huber-White correction).

69. Diggle and others (1994).

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My analysis of the state-level data on violent crimes indicates that the reported standard errors from the fixed effects models for the right to carry a concealed weapon are biased toward zero, and the reported t statistics are biased away from zero by about 30 percent.[70]

Multiple Comparisons

One of the most common practices in applied work is that the authors make multiple comparisons in a paper (with all of the comparisons having the same nominal significance level), without any further correction for having made multiple comparisons. This is a problem in both the Donohue chapter and the Lott and Mustard papers, and in the Lott book. The problems may be severe because there are two summary measures (violent and property crime separately) and seven alternative, less aggregated measures that are reported.

Failure to correct for multiple comparisons causes the true significance level to be much less than the nominal level would suggest. If there are seven comparisons, then a nominal 5 percent standard applied to each is actually more like a 30 percent standard. The former is usually considered statistically significant if it is met, while the latter is considered statistically insignificant, and not noteworthy unless one is looking for a null finding.

There are two alternative solutions to the multiple comparison problem. One is to use a Bonferroni bound, dividing the nominal α level of 5 percent by seven to achieve a true nominal 5 percent combined over all seven comparisons. This is equivalent to using a 0.7 percent nominal level for each of the comparisons. This implies that the t statistics have to exceed 2.69 rather than 1.96 to achieve an overall significance level of 5 percent. This approach tends to overcorrect if the errors across the equations are not independent. No correction is needed if the error terms are perfectly correlated. The correlations of the errors across equations are on the order of 0.4 or less for the disaggregated measures and 0.6 for the aggregated measures.

The second alternative is to use Zellner's seemingly unrelated regression approach.[71] In this case, one can use an F test to determine the statistical significance of the right-to-carry variables jointly across all equations.

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70. I have not determined the magnitude of the correction for the county-level analysis. There it seems very unlikely that a county-specific fixed effect will be sufficient to correct for both temporal and spatial correlation within state at a point in time or over time. To capture both for the county-level data, one would probably have to bootstrap clusters of all of the observations in the counties in a state as a group to correct the standard errors and other inference statistics.

Given the positive intrastate correlation within a state, I would expect the full correction for the county-level data to be even larger than the correlation for the state-level data.

71. Greene (2000, sec. 15.4).

If we combine the corrections for multiple comparisons and a more compli-
cated form of autocorrelation, then the results should have *t* statistics that are about
50–60 percent of their reported value.[72] With such a correction, the results at the
top of tables 8-2 and 8-4 appear to be statistically significantly different from zero
about as often as one would expect if they had occurred at random.

Endogeneity

Both the chapter by Donohue and the prior work by Lott and by Lott and Mus-
tard include endogenous explanatory variables.[73] The primary variable of inter-
est is endogenous—the right to carry a concealed weapon.[74] Given the primary
research interest, inclusion of the laws is unavoidable.

Is there a simultaneous equation bias caused by the endogeneity of enacting the
right-to-carry laws? The use of fixed effects for state and year and of specific state
slopes for time is not enough to capture why the right-to-carry law was enacted.
It is the change in the law that is of interest, given that the fixed effects approach
only relies on within-state variation in the laws to estimate the effect of the right-
to-carry laws. Lott and Mustard recognize this issue and use instrumental variable,
two-stage least squares (IV/2SLS) solutions to eliminate the simultaneous equa-
tions bias. The difficulty is that their instruments are questionable. The papers do
not provide compelling evidence or arguments to indicate whether the instru-
ments meet the econometric criteria for proper instruments.[75] For instrumental
variables, the burden of proof is on the proponent of the specific model.

Lott and Mustard should provide solid evidence and arguments for the sta-
tistical merits of their instruments. With data with this much autocorrelation in
the dependent and independent variables, one cannot use leads and lags to meet
the IV/2SLS criteria. If suitable instruments cannot be found, then the authors

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72. Given the concerns discussed in note 4, the corrections for the county-level data reported in
table 8-3 would be even larger.
73. Lott (2000); Lott and Mustard (1997).
74. In addition, Lott and his colleagues include a measure based on arrests and Donohue includes
incarcerations as covariates; the main results are largely insensitive to the inclusion of the additional en-
dogenous variables.
75. The major requirements for the instrumental variables in the linear model to yield consis-
tent estimates of the effect of the endogenous explanatory variable on the outcome of interest are the
instruments correlated with the endogenous explanatory variable(s); the instruments do not con-
ceptually belong in the equation of interest nor are they proxies for variables which should be in the
equation of interest but are omitted from the specification; the instrument is uncorrelated with the
error term in the equation of interest; and the instruments are not weak in the sense of Staiger and
Stock (1997) or Bound and others (1995). See Angrist, Imbens, and Rubin (1996) for a fuller ex-
position of the requirements.

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and their critics should consider using the bounding approach of Charles Man-
ski to deal with the simultaneous equations bias in their estimates.[76]

TNSTAAFL . . . There's No Such Thing as a Free . . .

In this case, the econometric equivalent of the free lunch is a free log transfor-
mation of the dependent measure. Several authors, including Donohue, Lott and
Mustard, and Lott, have used models with logged dependent variables to deal
with skewness in the dependent measures or to obtain estimates of proportional
effects of the right-to-carry laws on the outcomes (crime rates). Although such
transformation is widespread in applied econometrics, its use in conjunction with
ordinary least squares (OLS) or other least squares estimators can generate biased
inferences about the effect of various covariates (x's) on the ultimate outcome of
interest, the underlying dependent variable y, as distinct from inferences on $\ln(y)$.
In general, OLS with $\ln(y)$ is estimating the geometric mean function (condi-
tional on x), rather than the arithmetic mean function. Ultimately, the public
and public figures are concerned about $E(\text{crime per capita}|x)$, not the response
of the log crime rate. Mathematically, the problem is that: $E(\log(y)|x) \neq \log$
$(E(y|x))$. If we exponentiate both sides, we may have two quite different results.
One econometric problem that can lead to this discrepancy is heteroscedasticity
in the error term ε from the log scale regression model: $\ln(y) = x\beta + \varepsilon$, where the
variance of ε is some function of the covariates x.

   People are used to dealing with heteroscedasticity as a problem that biases
standard errors and other inference statistics. Correcting such statistics via the
sandwich estimator is commonplace.[77] However, such a correction does not deal
with the bias of going from the OLS on $\ln(y|x)$ to statements about $E(y|x)$. Con-
sider the following example, where the underlying error term ε is normally dis-
tributed with a variance: $\sigma^2(x)$, which is not a constant. In general, the expected
value of y given x is:

$$E(y) = e^{x\beta}E(e^{\varepsilon})$$
$$\neq e^{x\beta}$$

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76. Manski (1990).

77. The sandwich estimator is also known as the Eicker-Huber-White correction, or some com-
bination of the three.

unless $\sigma^2 = 0$. If the error term is normally distributed, then the expected value of y given x is:

$$E(y) = e^{x\beta + 0.5\sigma^2(x)}$$
$$> e^{x\beta}$$

The former is the arithmetic mean, while the latter is the geometric mean.

If the covariate x is a continuous measure then the marginal effect of x on $E(y|x)$ is:

$$\frac{\partial(y|x)}{\partial x_i} = \left[e^{\left(x\beta + 0.5\,\sigma^2(x)\right)} \right] \left(\beta_i + 0.5 \frac{\partial \sigma^2(x)}{\partial x_i} \right)$$

The second term is the one that has to be added to make the retransformation from the log scale to the raw scale give the correct, unbiased estimate of the incremental effect.

If there are two treatment groups or we are interested in the effect of an indicator variable, the formulation is slightly different. If there are two groups, A and B, where $\ln(y)_G \sim N(\mu_G, \sigma_G^2)$, with $G = A$ or B, then the contrast between the two groups is:

$$\frac{E(y_A)}{E(y_B)} = e^{(\mu_a - \mu_B) + 0.5(\sigma_A^2 - \sigma_B^2)}$$

Under homoscedasticity (σ^2 = a constant)

$$\frac{E(y_A)}{E(y_B)} = e^{(\mu_a - \mu_B)}$$

The second of these is the usual way of doing comparisons in log OLS models. However it is unbiased if and only if the two groups have (the same) error variance. The extension to multiple covariates does not alter the concern that heteroscedasticity can lead to bias when the results are retransformed unless a suitable correction is made.[78]

In the case of the Donohue and Lott formulations applied to the state-level data, the error is not heteroscedastic in the right-to-carry law variables themselves. However, the errors are heteroscedastic in year and some of the other vari-

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78. Manning (1998).

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ables; the specific variables vary depending on whether we are dealing with violent or property crimes. This could influence both the detrending of the data for secular change and some of the secondary hypotheses.

There are two alternatives that can be employed. The first is to find the functional form for the expectation of e^ε as a function of x to apply as a correction factor[79] or to employ a suitable variation of Duan's smearing estimator.[80] The second is to employ one of the generalized linear models with a log link,[81] which would provide estimates directly of $\log(E(y|x))$. These GLM estimates can be exponentiated to obtain $\ln(E(y|x))$ with all the usual interpretations of log model results, but without the complications caused by using least squares on $\log(y)$.

My examination suggests that the correct family for these data from the set of available GLMs suggests an overdispersed Poisson model. Further, it also appears that the log is not the correct link function if the concern is skewness in the error, because the residuals from their models are significantly skewed left. The log transformation overcorrects for skewness. A better power transformation could be the square root.

Conclusions

Donohue indicates that the earlier results by Lott and Mustard and by Lott may not be robust to a variety of specification and data issues.[82] The sensitivity of the findings could have major implications for the policy debate on right-to-carry concealed weapons. I share some of his concerns. For example, Donohue indicates that there are important differences in time trends before the right-to-carry laws were enacted. My own estimates also suggest differences pre- and postenactment by states that enacted during the sixteen-year interval.

But I find that both the critique and the original work suffer from several problems that could bias the coefficient estimates and the inference statistics. There are three major areas of concern. First, there may be a major simultaneous equations bias in the earlier estimates, as well as in Donohue's chapter on the effect of enacting right-to-carry laws.[83] Second, the precision of the findings (t, F, and p values) in the earlier work by Lott and by Lott and Mustard and in Donohue's chapter are substantially overstated because of failure to capture the full

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79. Manning (1998).
80. Duan (1983).
81. McCullagh and Nelder (1986); Mullahy (1998); Blough and others (1999).
82. Lott and Mustard (1997); Lott (2000).
83. Donohue also discusses the instrumental variable analysis reported by Lott and Mustard (1997).

autocorrelation structure and making multiple comparisons without suitable adjustment. Correcting for both of these may be sufficient to cause the results from the original analysis and those in the critique to be statistically insignificant. Thus, if I can paraphrase Gertrude Stein, "there may be no there there."

Finally, Donohue's and my results indicate that there is a need to check whether the model estimated "fit" the data. Seemingly innocuous specification choices or decisions about how to deal with autocorrelated errors seem to substantially influence the findings in terms of both the estimates themselves and their statistical significance.

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ELSEVIER

Concealed-Gun-Carrying Laws and Violent Crime: Evidence from State Panel Data

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A recent study concludes that permissive concealed-handgun-carrying (or "shall-issue") laws have sharply reduced crime rates, including the rate of homicide. The method of the study has been critiqued by several authors. In this paper, I report a quite different approach that exploits the minimum age requirements for concealed-carry permits to more effectively control for unobserved variables that may vary over time. Because even permissive concealed-carry states require permit holders to meet minimum age requirements, any deterrent benefits from these laws should be concentrated among adults and, therefore, should be reflected in the gap between adult and juvenile victimization rates. My results suggest that shall-issue laws have resulted, if anything, in an *increase* in adult homicide rates.   © 1998 by Elsevier Science Inc.

I. Introduction

Crime is one of the American public's top priorities,[1] a source of concern and frustration that has translated into individual as well as collective action. Motivated in large part by fear of crime, between 35% and 40% of all American households keep a total of 127 million long guns and 65 million handguns [Cook and Ludwig (1997)], despite uncertainty about whether such widespread gun ownership increases or decreases public safety [Zimring and Hawkins (1997a)]. For the owner, firearms may be used for protection against intruders, yet keeping a gun also seems to be a risk factor for unintentional injury, suicide, and homicide [Vernick et al. (1997)]. Keeping a gun also may impose costs and benefits on others. High rates of gun ownership may produce

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[1]For example, a USA Today/CNN/Gallup poll from January 5 to 7, 1996 ($N = 1000$), found that 66% of voters listed violent crime as an issue that would be a "high priority" in deciding whom to vote for, second only to the quality of public education (67%). (*USA Today*, "Ideal citizens go face to face," by Richard Wolf, January 22, 1996, p. 6D).

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general deterrence effects, for example by reducing the frequency with which burglars rob occupied homes. On the other hand, over 500,000 firearms are stolen each year, and keeping guns out of dangerous hands is made more difficult by over 2 million private transfers of second-hand guns annually [Cook et al. (1995); Cook and Ludwig (1997)]. In a recent survey, 85% of those without guns and 40% of gun owners report that they would feel less safe if more people in their community obtained a gun [Hemenway et al. (1995)].

Given the uncertainty surrounding the benefits and costs of widespread gun owner-ship, it is noteworthy that many states have responded to the crime problem by expanding the opportunities of private citizens to arm themselves in public. To date, 31 states have enacted "shall-issue" laws, which require local law enforcement authorities to issue concealed-handgun-carrying permits to any applicant who meets a set of specified criteria related to age, criminal history, and mental illness [Jost (1997)]. The number of states with shall-issue laws is likely to increase in the near future, as suggested by the consideration of shall-issue legislation in California and eight other states during 1997 [Hill (1997)].

The net effects of shall-issue laws are as difficult to predict as those of widespread gun ownership, though shall-issue laws have an even greater potential for positive and negative externalities. If gun carrying increases once these laws are passed, homicide rates may increase as guns are substituted for less lethal weapons in hostile confronta-tions [Zimring (1968); Cook (1991)]. Shall-issue laws also could cause homicides to increase if higher rates of gun carrying among potential victims causes criminals to arm themselves with greater frequency [Cook (1991)]. On the other hand, if shall-issue laws cause more citizens to carry handguns, then the expected costs associated with com-mitting crimes may increase. An increase in the costs of crime may deter some criminal activity [Lott and Mustard (1997)], particularly as the number of permits issued within a state increases over time. It is also possible that the publicity surrounding the passage of the law may be sufficient to cause criminals to revise their perceptions of the costs of crime,[2] in which case any deterrent benefits may surround changes in the legal regime.

Unfortunately, there is currently little empirical evidence on the relationship be-tween shall-issue laws and crime. A recent study by John Lott and David Mustard (1997) analyzes county-level panel data for 1977 through 1992 and finds evidence that shall-issue laws are negatively correlated with crime rates, including homicide. The authors conclude that "concealed handguns are the most cost-effective method of reducing crime thus far analyzed by economists" (p. 65). However, their method has been critiqued by several authors. Their study seems to suffer from model specification problems that will bias their estimates, a point that receives empirical support from Black and Nagin's (1998) reanalysis of the Lott and Mustard data.

In this paper, I present the results of a quite different approach to examining the effects of shall-issue laws on crime that exploits the fact that each shall-issue state enforces a minimum-age requirement for obtaining a concealed-carry permit to help control for the effects of unobserved variables. Because juveniles will not be eligible for concealed-carry permits even after shall-issue laws are passed, any deterrent benefits from these laws should be concentrated among adults. Any deterrent benefits of these laws should, therefore, reveal themselves in the difference in homicide victimization rates between adults and juveniles. My sample includes observations through 1994, an

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[2]Zimring and Hawkins (1997b) call this an "announcement effect."

important extension of Lott and Mustard, because some of the shall-issue states studied in their sample enacted these laws as late as 1991. My results suggest that shall-issue laws have resulted, if anything, in an *increase* in adult homicide rates.

The paper is organized as follows. The next section offers a critical review of the available evidence on shall-issue laws. The third section reviews the data and empirical strategy used in this paper, as well as the results of my analysis. The fourth section offers a discussion of my findings.

II. Previous Research

The effects of shall-issue laws on crime will depend, in part, on how concealed-handgun carrying changes when such laws are passed. Although almost nothing is known on this point, most gun carrying in the United States seems to occur without benefit of a concealed-carry permit. Cook and Ludwig (1997) find that 7.5% of American adults carried a firearm on their person or in a motor vehicle at some point during 1994. By way of comparison, a total of 1.4% of adults had obtained a concealed-carry permit in Florida 7 years after that state passed a shall-issue law,[3] and a recent review of other estimates suggests that in 12 of 16 shall-issue states fewer than 2% of adults had obtained permits [Hill (1997)]. Presumably, some fraction of those who apply for permits carried illegally before the shall-issue law was passed, so the number of permits issued may overstate the degree to which gun carrying changes. The effects of shall-issue laws on the prevalence of gun carrying are likely to be small.

Lott and Mustard (1997) examine the effects of shall-issue laws on crime by applying regression models to a panel dataset of all counties in the United States from 1977 through 1992.[4] Their dependent variables include the natural logarithm of several violent and property crime rates. Explanatory variables include age, race, *per capita* income, population, people per square mile, and *per capita* spending on social programs to proxy for poverty, though whether these proxy variables should be positively or negatively correlated with an area's level of material deprivation is not clear.[5] The variables also include year-specific dummy variables to capture changes in the U.S. crime rate over time, county-specific dummy variables to capture unobserved county "fixed effects," and the county's arrest ratio to control for other policy changes that may affect crime.[6] Lott and Mustard find that shall-issue laws are, in general, negatively correlated with violent crimes and are positively correlated with property crimes.

Yet, Lott and Mustard's (1997) analysis may suffer from bias from omitted variables for at least two reasons. First, the Lott and Mustard fixed-effects approach cannot control for unobserved factors that influence county crime trends but are not fixed over time. Crack is one example of a factor that is not explicitly controlled for in the Lott and

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[3]Calculated from permit figures reported in McDowall, et al. (1995, p. 194) together with population estimates from the U.S. Statistical Abstracts (1995, Table 34).

[4]McDowall et al. (1995) estimate the effects of shall-issue laws on crime rates using data from three states. Because their approach is susceptible to the same biases as that of Lott and Mustard, I restrict my attention to the problems with the Lott and Mustard estimates based on national data.

[5]A given level of *per capita* social spending may reflect a large number of pre-government-transfer poor who each receive a relatively meager transfer payment, or a small number of pretransfer poor who each receive a relatively generous transfer payment; the implications for the level of material deprivation are obviously different.

[6]The problems with using arrest ratios in this way have been well known since Blumstein et al. (1978). Yet in practice the Lott and Mustard results do not seem sensitive to the inclusion or exclusion of the arrest ratio [Black and Nagin (1998)].

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Mustard study, is likely to be different between shall-issue states such as Idaho and other states such as California and New York, and is unlikely to have fixed effects over time [Zimring and Hawkins (1997b)].[7] Other examples include gang activity [Klein (1995)] and, as noted above, poverty. Second, passage of a shall-issue law presumably reflects a jurisdiction's preferences for anticrime measures, which may manifest themselves in other government anticrime responses beyond passage of shall-issue legislation. Lott and Mustard include policy variables that are likely to capture only a subset of the many possible public-sector responses to crime.[8]

Empirical evidence that Lott and Mustard's (1997) analysis produces biased estimates comes from Black and Nagin (1998). By applying a formal model mis-specification test that exploits the panel structure of the dataset [Heckman and Hotz (1989)], Black and Nagin find evidence to suggest that the Lott and Mustard regression model is unable to control for all of the factors that cause crime rates to differ between shall-issue states and other states *before* these laws are adopted. As a result, Lott and Mustard's estimates for the effects of shall-issue laws will reflect in whole or part the effects of omitted factors that are not captured by their regression model.[9]

Lott and Mustard (1997) present an additional set of regressions that uses two-stage least squares (2SLS) methods in an attempt to control for the omitted variables highlighted by Black and Nagin's analysis. To produce unbiased estimates for the effects of shall-issue laws, their 2SLS approach requires that lagged crime rates (or changes in crime over time), the proportion of a state that belongs to the National Rifle Association or voted Republican in the most recent Presidential election, and *per capita* (and per crime) police resources will only affect a county's crime rate by influencing the state's shall-issue law status. Nagin (1978) offers a relevant discussion of why many of the variables used by Lott and Mustard are unlikely to be valid for this purpose. Unfortunately, Lott and Mustard do not present the results of statistical tests such as those discussed in Hausman (1983) or Newey (1985), which could shed light on the validity of their estimation procedure.

Yet, some evidence that the Lott and Mustard 2SLS estimates are biased comes from their implausibly large magnitudes [Lott and Mustard (1997), Table 11]: The estimates imply that passage of a shall-issue law will reduce homicides by 67%, rapes by 65%, and assaults by 73%.[10] In sum, Lott and Mustard's analysis seems to suffer from bias and, as

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[7]How to conceptualize and measure drug market activity is not obvious. Lott and Mustard (1997) experiment with drug prices as an additional covariate, though they ultimately reject this model specification because of missing data problems. Drug prices may be positively correlated with criminal activity if, as Lott and Mustard (1997, note 50) suggest, higher drug prices make addicts more prone to commit crimes to finance their habits. On the other hand, prices could be negatively correlated with criminal activity if low prices reflect the frequency of and (potentially violent) competition among drug suppliers. Unfortunately, as Kleiman and Smith (1990, p. 102) note, "[N]o city has anything resembling a quantitatively accurate description of its own drug problem."

[8]In addition to controlling for arrest ratios and (in some cases) burglary and robbery rates, Lott and Mustard (1997) experiment with including variables for sentencing enhancements for crimes committed with weapons, handgun purchase waiting periods, conviction rates, and sentence lengths (apparently available only for Oregon).

[9]Lott and Mustard (1997) also experiment with a model specification that includes a county's burglary or robbery rate as an additional explanatory variable to control for omitted variables. In unpublished calculations, Black and Nagin find that this model specification is also rejected using the Heckman and Hotz test (Dan Black, personal communication).

[10]Lott and Mustard (1997) report that the "percent of a standard deviation change in the endogenous variable [logged crime rate] that can be explained by a 1 standard deviation change in the exogenous variable [predicted probability of enacting a shall-issue law]" (p. 47). The implied effects on crime rates from passing a shall-issue law can

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a result, is unlikely to provide reliable information about the effects of shall-issue laws on crime.

III. Empirical Methods and Results

This section presents the results of a new test for the causal effects of shall-issue laws using state homicide data disaggregated by age. After reviewing the data, I discuss why my estimation approach may help control for the omitted variables problems that seem to plague Lott and Mustard (1997). Then, I show that there is little evidence to suggest that shall-issue laws have reduced homicide victimization rates for adults.

Data

The dataset used in this paper contains information for each state in the United States from 1977 through 1994. Of the various crime rates that may be used in assessing the effects of shall-issue laws, homicide is widely considered to be measured most accurately [Cook and Laub (1997)] and, as such, is the focus of the analysis presented here. Annual state-by-state homicide counts are taken from vital statistics reports compiled by the U.S. Department of Health and Human Services. State population data are taken from the Statistical Abstracts for the United States, while data on the age distribution within each state are from the Census Bureau's Population Estimates and Population Distribution Branches.[11] Descriptive statistics for these data can be found in Table 1.

Lott and Mustard (1997) classify the following states as having enacted shall-issue laws between 1987 and 1991: Florida (1987); Georgia (1989); Idaho (1990); Maine (1985); Mississippi (1990); Montana (1991); Oregon (1990); Pennsylvania (1989); Virginia (1988); and West Virginia (1989). As Lott and Mustard note, whether Virginia and Maine should be included in this list is unclear, because Maine passed a series of modifications to its concealed-carry laws starting in 1981, and Virginia enacted additional shall-issue legislation on July 1, 1995, that eliminated the previous law's "need-to-carry" requirement and greatly increased the rate at which permits were issued [Hill (1997); Webster et al. (1997)]. The appropriate treatment of Pennsylvania in my sample is also complicated, because the shall-issue law exempts Philadelphia [Lott and Mustard (1997)].

Several additional states had shall-issue laws in place at the start of my sample period (Alabama, Connecticut, Indiana, New Hampshire, North Dakota, South Dakota, Vermont, and Washington). Although I present descriptive statistics for these states in what follows, identification of the effects of shall-issue laws using estimation approaches that control for state fixed effects (as does my empirical strategy) will rest on the states that change their laws during the sample period.

The minimum age requirement for obtaining a concealed-carry permit in those states that changed their laws from 1987 to 1991 is 18 in Maine, Montana, and West Virginia and is 21 in the others. For my empirical analysis, I define juvenile homicide victimization rates as those involving victims between the ages of 12 and 17. I exclude homicides to younger children because they tend to have characteristics that are quite different from those involving older children or adults, though replicating the analysis presented

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be calculated as $e^{\beta} - 1$ for the coefficient β on the shall-issue variable, because the dependent variable is the logarithm of the crime rate [for example, see Kennedy (1993), p. 106]. Thanks to Daniel Nagin for this point.

[11]Annual state population estimates taken from the U.S. Department of Commerce web page, http://www.census.gov/population/www/estimates/statepop.html.

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TABLE 1. Descriptive statistics for state data

	Homicide rate (per 100,000 population)	Adult (21+) homicide rate (per 100,000 adults)	Youth (12–17) homicide rate (per 100,000 youth)
U.S., 1977–1994	9.35	11.17	5.93
Non-shall-issue states, 1977–1994	9.75	11.73	6.07
Rates for states with concealed-carry laws before 1977,* for 1977–1994	6.68	8.18	3.68
Rates for states that implemented concealed-carry laws between 1987–1991,† for the period before these laws went into effect	10.96	13.89	4.08
Rates for states that implemented concealed-carry laws between 1987–1991,† for the period after these laws went into effect	9.95	11.48	7.62

Notes: All means were calculated using state population figures as weights. Homicide counts taken from U.S. Vital Statistics, population counts taken from U.S. Census Bureau.

*States with shall-issue laws before 1977: Alabama, Connecticut, Indiana, New Hampshire, North Dakota, South Dakota, Vermont, and Washington.

†States that implemented shall-issue laws between 1987 and 1991: Florida, Georgia, Idaho, Mississippi, Montana, Oregon, and West Virginia. Pennsylvania, Maine, and Virginia are excluded from the sample for reasons discussed in the text.

below using victimization rates for all those under 18 years of age produces qualitatively similar results.[12]

Estimation Strategy

Of primary concern with previous research such as Lott and Mustard (1997) are the difficulties involved in controlling for unobserved or difficult-to-measure factors that influence local crime rates but change over time. One way to address the problem of unobserved, time-varying factors is suggested by the requirement in each shall-issue state that permit holders be at least 18, or more typically 21, years of age. As a result, the probability of encountering an armed juvenile (the costs of committing crime against juveniles) should be largely unaffected by shall-issue laws. Any deterrent benefits from these laws thus should be concentrated among adults and should be reflected by a decrease in the difference between adult and juvenile victimization rates (that is, adult rates should decrease relative to juvenile rates).

Both the standard fixed-effects approach and the empirical strategy used here can be illustrated using Table 2, adapted from Joyce and Kaestner (1996). The standard fixed-effects approach consists of comparing the rate of change in adult homicide victimization rates in shall-issue states (*a-b*) with the change in non-shall-issue states (*e-f*) to control for unobserved state fixed effects that cause crime rates to differ between

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[12]Thanks to an anonymous referee for this suggestion.

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TABLE 2. Differences-in-differences-in-differences model

	Pre-shall-issue	Post-shall-issue	Difference
Shall-issue states			
Adults ("treatment")	b	a	(a-b)
Juveniles ("control")	d	c	(c-d)
Difference in differences			(a-b) − (c-d)
Non-shall-issue states			
Adults ("treatment")	f	e	(e-f)
Juveniles ("control")	h	g	(g-h)
Difference in differences			(e-f) − (g-h)
DDD			[(a-b) − (c-d)] − [(e-f) − (g-h)]

Source: Joyce and Kaestner (1996). Cell entries represent homicide rates per 100,000 for the group defined at left.

shall-issue states and other states by the same amount each period. Yet, the fixed-effects approach will not address the effects of unobserved variables that differ between shall-issue states and other states and that vary over time. For example, suppose that crack use and gang activities have increased more substantially during the sample period in states without shall-issue laws relative to states that have such laws. Fixed-effects comparisons will reveal that adult homicide rates have grown more slowly in shall-issue states $[(a-b) < (e-f)]$, even if shall-issue laws have no effect on crime.

The "difference-in-difference-in-difference" (DDD) estimation strategy exploits the fact that juveniles are not eligible to obtain gun-carrying permits after shall-issue laws are passed but will still be affected by other fixed and time-varying state-specific factors that influence crime victimization rates. Juveniles thus provide a natural "control group" for examining the effects of shall-issue laws (the "treatment") on adults who are 21 years of age and older (the "treatment group"). The difference between the change in adult homicide victimization rates and the change in juvenile rates $[(a-b) − (c-d)]$ differences out the effects of both fixed and time-varying factors that cause both adult and juvenile rates to change over time, and it will reflect only those factors that act on the difference between adult and juvenile homicides. To control for the possibility that there are nationwide changes in the differences between adult and juvenile homicide victimization rates that are independent of the shall-issue laws, the difference in the adult-juvenile trends in shall-issue states are compared with the difference in the adult-juvenile trends in other states $[(a-b) − (c-d) − (e-f) − (g-h)]$. The DDD estimator thus isolates those factors that are unique to shall-issue states (such as shall-issue laws) that will cause adult homicide rates to decrease relative to the rates for juveniles.[13]

More formally, the proposition that shall-issue laws reduce adult homicide victimization rates suggests that $[(a-b) − (c-d) − (e-f) − (g-h)]$ will be negative, which can be tested by estimating the following regression model:

$$y_{it} = \theta_0 + \theta_1(Exper_i) + \theta_2(Adult_i) + \theta_3(Post_t) + \theta_4(Exper_i*Adult_i) + \theta_5(Adult_i*Post_t)$$

$$+ \theta_6(Exper_i*Post_t) + \theta_7(Exper_i*Post_t*Adult_i) + v_{it} \qquad (1)$$

The sample used to estimate equation (1) will include two observations for each state (i) for each period (t); one corresponds to the state's juvenile homicide victimization

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[13]The DDD estimator is discussed further in Card (1992), Gruber (1994), and Joyce and Kaestner (1996).

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TABLE 3. Differences-in-differences-in-differences regression model*

	Pre-shall-issue	Post-shall-issue	Difference
Shall-issue states			
Adults ("treatment")	$(\theta_0 + \theta_1 + \theta_2 + \theta_4)$	$(\theta_0 + \theta_1 + \theta_2 + \theta_3 + \theta_4 + \theta_5 + \theta_6 + \theta_7)$	$(\theta_3 + \theta_5 + \theta_6 + \theta_7)$
Juveniles ("control")	$(\theta_0 + \theta_1)$	$(\theta_0 + \theta_1 + \theta_2 + \theta_3 + \theta_4 + \theta_5 + \theta_6 + \theta_7)$	$(\theta_3 + \theta_6)$
Difference in differences			$(\theta_5 + \theta_7)$
Non-shall-issue states			
Adults ("treatment")	$(\theta_0 + \theta_2)$	$(\theta_0 + \theta_2 + \theta_3 + \theta_5)$	$(\theta_3 + \theta_5)$
Juveniles ("control")	(θ_0)	$(\theta_0 + \theta_3)$	(θ_3)
Difference in differences			(θ_5)
DDD			$(\theta_5 + \theta_7) - (\theta_5) = (\theta_7)$

Source: Modification of Joyce and Kaestner (1996). Cell entries represent homicide rates per 100,000 for group defined at left.

Regression model:

$$y_{it} = \theta_0 + \theta_1(Exper_i) + \theta_2(Adult_i) + \theta_3(Post_t) + \theta_4(Exper_i*Adult_i) + \theta_5(Adult_i*Post_t) + \theta_6(Exper_i*Post_t)$$

$$+ \theta_7(Exper_i*Post_t*Adult_i) + v_{it}$$

y_{it} = homicide victimization rate for observation (either adult or juvenile) in state (i), period (t)

$Exper_i$ = 1 if state (i) enacts shall-issue law during sample period, 0 otherwise

$Adult_i$ = 1 if observation corresponds to adult victimization rate, 0 if juvenile rate

$Post_t$ = 1 if observation occurs in post-shall-issue law period, 0 if pre-shall-issue law period

rate in period (t), whereas the other corresponds to the adult rate in period (t). That is, with a data sample consisting of N states in the panel, with observations on the states for T periods that span changes in shall-issue law status in a subset of states, then equation (1) is estimated using $2NT$ observations. The variable y_{it} represents a homicide rate measure for state (i) in period (t), whereas $Adult_i$ equals 1 if the observation is for adult homicide rates (zero otherwise), $Exper_i$ is equal to 1 if state (i) adopts a shall-issue law during the sample period (zero otherwise), and $Post_t$ equals 1 if the period is after the shall-issue laws have been enacted (zero otherwise). Equation (1) is estimated using state populations as weights to control for heteroskedasticity in the regression residuals [Greene (1993)].

The parameters in this regression model will capture fixed factors that reflect differences between shall-issue states and other states during the sample period (θ_1), differences between adult and juvenile homicide rates (θ_2), trends over time in homicide rates (θ_3), differences in the effects of fixed-state factors on adults versus juveniles (θ_4), differences in the trends of adult versus juvenile homicide rates over time (θ_5), and differences in homicide trends over time between shall-issue states and other states (θ_6). The key parameter of interest is θ_7, which represents $[(a\text{-}b) - (c\text{-}d) - (e\text{-}f) - (g\text{-}h)]$, the effects of the shall-issue law on the difference between adult and juvenile homicide rates in states that do adopt a shall-issue law during this period versus those that do not.

That the estimate for θ_7 from equation (1) represents an estimate for the quantity $[(a\text{-}b) - (c\text{-}d) - (e\text{-}f) - (g\text{-}h)]$ can be seen with the help of Table 3, which is identical to Table 2 except that the homicide rates are now expressed in terms of the parameters underlying equation (1). For example, the expected value of adult homicide victimization rates in shall-issue states after these laws are passed is given by $[a = (\theta_0 + \theta_1 + \theta_2 +$

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$\theta_3 + \theta_4 + \theta_5 + \theta_6 + \theta_7)]$, because each of the dummy variables underlying equation (1) will be equal to one in this case. The expected value of juvenile homicide victimization rates in states that never pass these laws, during the period before the adoption of shall-issue laws by the shall-issue states, is equal to $[h = (\theta_0)]$, because none of the dummy variables are "switched on" in this case. Taking the difference between adult and juvenile homicide trends over time in shall-issue states, and subtracting from this the difference between adult and juvenile homicide trends in non-shall-issue states, leaves us with θ_7.

Note also that the DDD approach differs in important ways from that used in Section IV-C of Lott and Mustard (1997), in which they apply their standard regression model to data for 1977 through 1992 to examine whether shall-issue laws change the age composition of homicide victimizations. They find a negative, but not statistically significant, relationship between shall-issue laws and the proportion of murder victims above some age level, though the specific age cutoff and regression coefficients are not reported. Yet, the strategy of using the ratio of adult to total homicides will not help control for unobserved state factors that vary over time.[14]

Empirical Results

Figure 1 provides a graphical representation of my results. The graph shows trends in the difference between adult (age 21 and over) and juvenile (ages 12 to 17) homicide victimization rates over time for those states that passed a shall-issue law during the period 1977 to 1994 (Florida, Georgia, Idaho, Mississippi, Montana, Oregon, and West Virginia), those that did not have a shall-issue law in effect during this period, and those that had enacted a shall-issue law before the sample period (Alabama, Connecticut, Indiana, New Hampshire, North Dakota, South Dakota, Vermont, and Washington). The sample excludes Pennsylvania, Virginia, and Maine because of the uncertainty surrounding how these states should be classified.

As noted above, any deterrent benefits of shall-issue laws should manifest themselves as a decrease in the difference between adult and juvenile homicide rates. Moreover, any change in the adult-juvenile difference should be greater in shall-issue states than in other states if the shall-issue laws themselves exert any influence on adult homicide rates beyond those factors that affect adult homicide nationwide. However, as seen in Figure 1, adult and juvenile homicide rates converged throughout the United States during the 1980s, and the rate of this convergence in shall-issue states after these laws were passed (1987–1991) does not seem to be noticeably different than the rates observed in other states. Figure 1 thus presents informal evidence that shall-issue laws did not serve to reduce adult homicide rates.

The results of testing this proposition more formally by estimating regression equation (1) are shown in Table 4. The one complication is the proper definition of the pretreatment and posttreatment periods. Because the states that passed shall-issue laws between 1987 and 1991 passed these laws in different years, "the" treatment period actually consists of a several-year window. In my preferred regressions, I define the 10

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[14]This can be seen by imagining two separate regression equations with adult and juvenile homicide rates as the dependent variables of interest and the various explanatory variables on the right-hand side. The regression equation with the ratio of adult to total homicides can be written as the ratio of the adult equation divided by the adult plus juvenile equations, with a residual term that still includes unobserved, time-varying state effects that influence adult and juvenile rates equally. These terms will be purged with my differencing strategy.
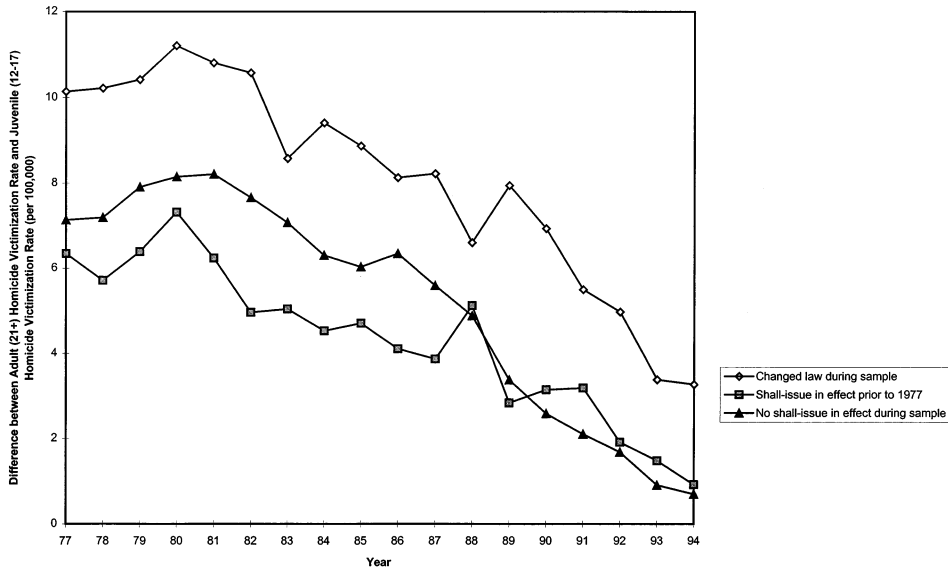
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FIG. 1. Difference Between Adult (21+) and Juvenile (12–17) Homicide Victimization Rates, 1977–1994. States that enacted shall-issue laws during the sample period are as follows: Florida (1987), Georgia (1989), Idaho (1990), Mississippi (1990), Montana (1991), Oregon (1990), and West Virginia (1989). States with shall-issue laws in effect during entire sample period are: Alabama, Connecticut, Indiana, New Hampshire, North Dakota, South Dakota, Vermont, and Washington. Excluded from the sample are Maine, Virginia, and Pennsylvania (see text).

years before Florida's implementation of its shall-issue law as the "pretreatment" period (1977 through 1986), and the 3 years after Montana's shall-issue law as the "posttreatment" period (1992 through 1994).

Because this analysis compares homicide rates that are averaged over several pretreatment and posttreatment years, the method is not well suited for determining whether shall-issue laws have immediate versus gradual effects on crime. If the effects of shall-issue laws change over time, for example because the number of concealed-carry permits issued within a state increases, then the posttreatment effect will reflect the average treatment effect for states with these laws in place for different lengths of time. Any bias that may arise from time-varying treatment effects will be exacerbated by including those states that enacted shall-issue laws before 1977 in the comparison (no change in shall-issue regime) group, because the change in the comparison-group homicide rates in this case may in part reflect changes in the shall-issue "dose" in some comparison-group states. As a result, these states are excluded from the my analytic sample, though below I examine the sensitivity of my estimates to the treatment of these states.

The regression results shown in Table 4 reveal that parameter θ_7, which captures the effects of shall-issue laws on adult homicide rates, is slightly positive, implying an increase of around one-sixth of a homicide per 100,000 adults. With an average adult homicide victimization rate of 11.17 per 100,000 in the United States for 1977 through 1994, this implies an increase of 1.4%. Because the sample of states that change their laws from 1977 to 1994 is relatively small, the standard errors around this point estimate

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TABLE 4. Differences-in-differences-in-differences regression results

Explanatory Variable†	Coeff.	Controls for	Estimate (standard error)
Exper = 1 if state ever passes shall-issue law (=0 else)	θ_1	Fixed factors which differ between shall-issue and other states	−1.53 (0.53)*
Adult = 1 if observation is for adult homicide rates (=0 if observation is for juvenile homicide rate)	θ_2	Differences in levels between adult and juvenile homicide rates	7.19 (0.26)*
Post = 1 if period is after implementation of shall-issue laws	θ_3	Trends over time in homicide rates	4.80 (0.44)*
Adult × *Exper*	θ_4	Differences in shall-issue state fixed-effects on adult versus juvenile homicide rates	2.62 (0.73)*
Adult × *Post*	θ_5	Differences in trends of adult versus juvenile homicide rates over time	−6.10 (0.52)*
Exper × *Post*	θ_6	Differences in homicide trends in shall-issue v. other states over time	−1.43 (1.01)
Exper × *Post* × *Adult*	θ_7	Effects of shall-issue laws on adult homicide rates relative to juvenile homicide rates	0.16 (1.42)
N			1,039
Adjusted R^2			0.64

Notes: Preprogram years included in the model are 1977 through 1986. Postprogram years included in the model are 1992 through 1994. The regression model also includes a constant term, the percentage of state population living in poverty, the percentage of state that is African-American, the state *per capita* personal income (measured in 1987 constant dollars), and the percentage of the state population living in urban areas, and it is estimated using state population counts as weights. Shall-issue states are Florida, Georgia, Idaho, Maine, Mississippi, Montana, Oregon, Virginia, and West Virginia. The sample excludes states with shall-issue laws enacted before the sample period (Alabama, Connecticut, Indiana, New Hampshire, North Dakota, South Dakota, Vermont, and Washington), as well as Maine, Pennsylvania, and Virginia (see text). "Pretreatment" period is defined as 1977 to 1986, "posttreatment" period is defined as 1992 to 1994.

* = significant at 1%.

†Dependent variables: Adult (21 and older) and juvenile (12–17) homicide victimization rates per 100,000 population.

are somewhat large. The standard errors imply that the point estimate is not statistically significant, with a 95% confidence interval of −2.68 to 3.00 homicides per 100,000. Yet even fairly small standard errors (such as those produced by Lott and Mustard's county-level ordinary least squares analysis) would imply that these estimates are consistent with positive, negative, or nonexistent effects of shall-issue laws on adult homicides.

As shown in Table 5, the results are not qualitatively different when states with shall-issue laws enacted before 1977 are included in the comparison group for the analysis, when the natural logarithm of the adult and juvenile victimization rates are

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Table 5. Sensitivity analysis of DDD regression results

Difference in regression model from that used in Table 4	Estimated effect (standard error) of shall-issue laws on adult homicide rates (per 100,000)
Alternative weighting variable	
Use adult (21+) rather than total population as weighting variable	0.15 (1.62)
Alternative functional form	
Use natural logarithm of homicide victimization rates	−0.04 (0.19)
Alternative definitions of "pre" and "post treatment" periods	
"Pre-law" period defined as 1982–1986	0.35 (1.65)
"Pre-law" period defined as 1980–1986	0.24 (1.55)
"Post-law" period defined as 1992	0.67 (1.98)
"Post-law" period defined as 1992–1993	0.26 (1.62)
"Post-law" period defined as 1993–1994	−0.09 (1.63)
Alternative "comparison state" groupings	
Include states with shall-issue laws on books before 1977 in comparison group	−0.05 (1.33)
Alternative "shall-issue" state groupings	
Include Pennsylvania as shall-issue state	1.20 (1.23)
Include Virginia as shall-issue state	0.53 (1.30)
Include Maine as shall-issue state	0.43 (1.39)
Drop Florida	0.76 (1.86)
Drop Georgia	1.18 (1.60)
Drop Idaho	0.05 (1.46)
Drop Mississippi	0.11 (1.48)
Drop Montana	−0.01 (1.45)
Drop Oregon	−0.29 (1.50)
Drop West Virginia	−0.29 (1.47)

Notes: Standard errors in parentheses. Results presented above taken from estimating regression equations similar to those underlying Table 4; coefficients presented above correspond to the variable in the last row of Table 4.

used rather than the raw values,[15] or when the adult (rather than total) populations are used as regression weights. The results are also generally not sensitive to the choice of pretreatment and posttreatment periods, though the exclusion of data from 1993 and 1994 causes the estimated positive effect of shall-issue laws on homicide to become even larger. When Pennsylvania, Virginia, or Maine are included, in turn, as shall-issue states, the estimated effect of shall-issue laws on adult homicides becomes even more positive, though the idiosyncracies in how these laws were enacted makes interpretation of these results difficult.

Previous research has found that the shall-issue "treatment effects" implied by the Lott and Mustard model vary quite substantially across states [Black and Nagin (1998)].

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[15]Using the natural logarithm for the homicide victimization rates is complicated somewhat by the fact that several states reported no homicides to victims ages 12 to 17 for some of the years between 1977 and 1994. Because the logarithm of 0 is undefined, I substitute the logarithm of (0.1) in these cases. Substitution of the logarithm of yet smaller values will increase the implied difference between adult and juvenile homicides when there are no juvenile homicide cases and will serve to make the shall-issue coefficient more negative.
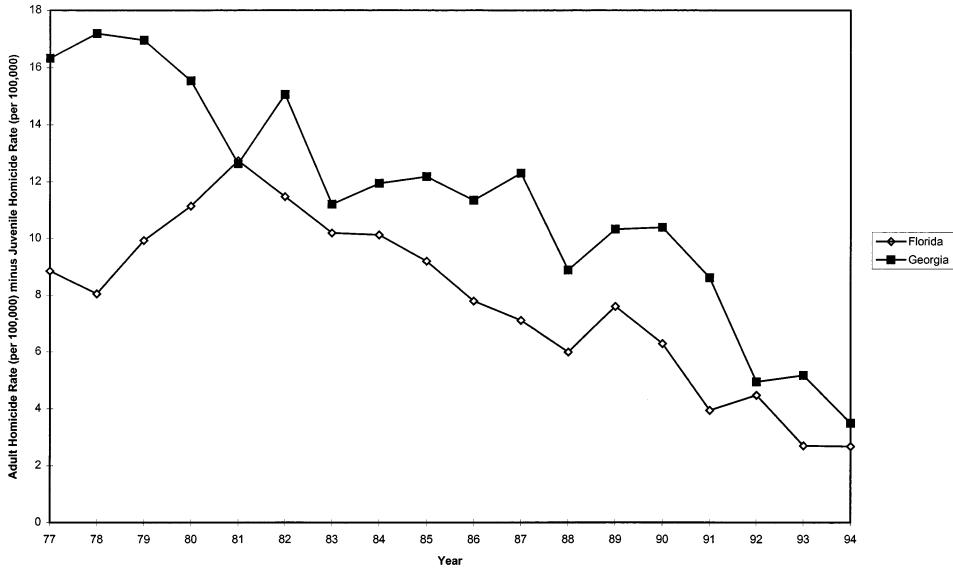
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FIG. 2. Difference in Adult (21+) minus Juvenile (12–17) Homicide Victimization Rates in Florida and Georgia, 1977–1994. Florida enacted shall-issue law in 1987, while Georgia enacted shall-issue law in 1989.

This finding may reflect heterogeneity across states that is not captured by the Lott and Mustard regression model, including differences in the way that shall-issue laws are written or enacted and the rate at which citizens within a state obtain concealed-carry permits. For example, state shall-issue laws vary with respect to fingerprint and safety training requirements, as well as to permit application fees, and even to the degree to which carrying privileges are restricted within some counties in a state [National Rifle Association (1998)]. Estimates for the proportion of adults who have been issued permits range from 0.2 percentage points in Mississippi to as high as 6.0% in South Dakota [Hill (1997)]. Although most of the permit holders in shall-issue states seem to be middle-aged white men, there does seem to be some variation across states in the age distribution of those holding permits [Hill (1997)].

   Figures 2 and 3 provide informal evidence that the effects of shall-issue laws may vary across states. Figure 2 presents trends in the difference between adult and juvenile homicide victimization rates in Florida and Georgia, those states with the most noticeable changes in the difference between adult and juvenile homicide rates. However, as seen in Figure 3, even after enacting shall-issue laws the remaining states reflect the kind of cyclicality in homicide rates that is typical in the United States [Blumstein (1995)].

   The sensitivity of my estimates to the exclusion of each shall-issue state in turn is shown in Table 5. As suggested by Figures 2 and 3, evidence for any crime-reducing benefits are concentrated in Florida and Georgia: The exclusion of these states causes the estimated effect of shall-issue laws on adult homicides to become even more positive. This finding is consistent with Black and Nagin (1998), who note that many of the negative shall-issue effects estimated by Lott and Mustard (1997) disappear once Florida is excluded from the sample. The results are generally not sensitive to excluding any of the other shall-issue states from the sample, or even to excluding such atypical

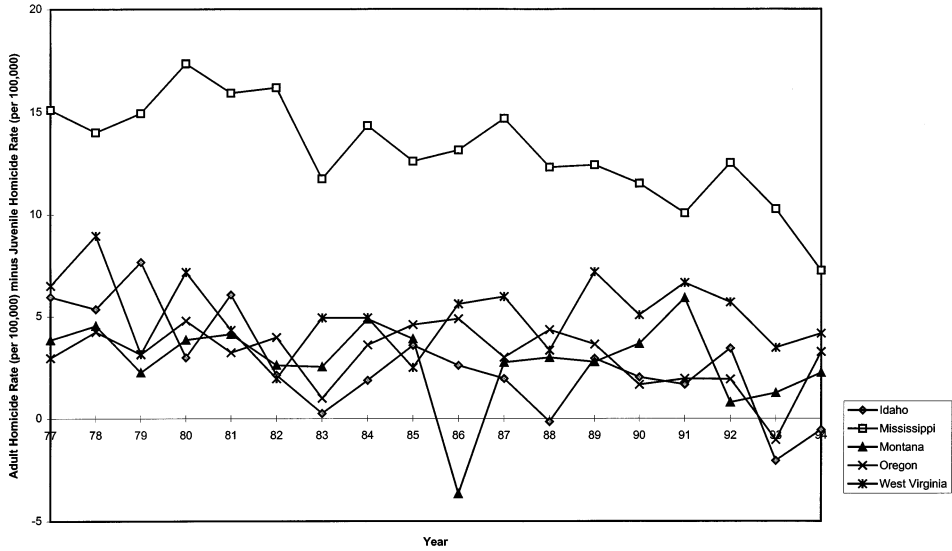252   *Concealed-gun-carrying laws and violent crime*

FIG. 3. Difference between Adult (21+) and Juvenile (12–17) Homicide Victimization Rates for Idaho, Mississippi, Montana, Oregon, and West Virginia, 1977–1994. Idaho, Mississippi, and Oregon enacted shall-issue laws in 1990, whereas Montana enacted a shall-issue law in 1991 and West Virginia in 1989.

non-shall-issue states as California or New York, with estimated effects that are consistently no larger than one-third of a homicide in absolute value. Taken together, this analysis produces little evidence that shall-issue laws reduce crime and suggests that these laws are as likely to cause crime to increase as to decrease.

IV. Discussion

Whether "shall-issue" laws that liberalize concealed-handgun-carrying requirements cause crime rates to increase or to decrease has become an increasingly important public policy question, as a growing number of states adopt or consider such legislation. The widely publicized study of Lott and Mustard (1997) suggests that shall-issue laws reduce crime and save lives and money. However, as I have argued above, the Lott and Mustard study does not seem to have controlled adequately for omitted variables and other problems and, as a result, is unlikely to provide reliable information about the effects of shall-issue laws on crime.

In this paper, I present the results of an alternative test for the effects of shall-issue laws on homicide rates that exploits the fact that juveniles are not eligible for concealed-carry permits to control for time-varying unobserved state factors. The results of my analysis suggest that shall-issue laws have resulted, if anything, in an increase in adult homicide rates.

What explains the difference between the findings in Lott and Mustard (1997) and those presented here? My use of state-level rather than county-level data is unlikely to explain the difference, inasmuch as Lott and Mustard's analysis of state-level data using their fixed-effects regression approach produces results that are similar to their county-level analysis. The additional 2 years of data that I use (1993 and 1994) also do not seem to explain the difference across studies, because excluding data from 1993 and 1994 in

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my analysis causes the estimated positive effect of shall-issue laws on adult homicides to become even larger.

I believe that the most compelling explanation for the differences between the results in Lott and Mustard (1997) and those presented here is that my estimation strategy is able to more adequately control for unobserved state variables that vary over time. Lott and Mustard's (1997) analysis is susceptible to bias from any unobserved state or county factor that varies over time, which in fact seems to be the case on the basis of Black and Nagin's (1998) analysis and the implausibility of Lott and Mustard's 2SLS results. In contrast, only social or public policy changes that are unique to shall-issue states, concurrent with the implementation of these laws, and that affect the difference between adult and juvenile homicide rates may impart bias to the estimates presented here. It is also possible that some criminals change their behavior after shall-issue laws are passed and now either victimize juveniles instead of adults or leave crime altogether, in which case my estimates may be subject to a slight negative or positive bias, respectively. The possibility of some unmodeled heterogeneity in my estimates is suggested by the sensitivity of the estimates to the exclusion of Florida and Georgia from the sample; these sample restrictions cause the estimated positive effect of shall-issue laws on homicide to become even larger. My results are generally robust to dropping other shall-issue and non-shall-issue states from the analytic sample.

The omitted variables problems highlighted in this paper are of general concern in evaluating the effects of anticrime efforts and seem even more severe than the problems involved in evaluating other areas of public policy such as education. In both crime and education, many of the important factors that influence policy outcomes vary at the local level. In the area of education policy, the government has invested substantial resources to collect rich data at levels as disaggregated as the school or student. In contrast, many of the important factors that influence crime are not measured, are not systematically compiled by government agencies, or are unusually difficult to measure. Even sophisticated measurement techniques such as fixed-effects or 2SLS models may produce biased estimates in the face of these problems, given that many of the unmeasured factors that cause crime are likely to vary over time and that valid instrumental variables are difficult to find. Public policymakers should be made aware of the unique identification problems in evaluating anticrime policies such as concealed-carry laws and should recognize that even elaborate studies such as Lott and Mustard (1997) may not provide reliable information. There may be many reasons for state and federal legislators to support shall-issue laws, but the belief that these laws reduce crime should not be one of them.

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EASING CONCEALED FIREARMS LAWS: EFFECTS ON HOMICIDE IN THREE STATES*

DAVID MCDOWALL
COLIN LOFTIN
BRIAN WIERSEMA**

I. INTRODUCTION

Restrictions on carrying concealed weapons are among the most common gun control policies.[1] These statutes limit who may have a deadly weapon—usually a handgun—hidden on their person when outside the home. By reducing access to guns in public, concealed weapons laws seek to make firearms less available for violence.[2]

Details of concealed weapons laws vary greatly among localities, but most approaches fall into two categories. One of these is a discretionary system, sometimes called "may issue" licensing.[3] Under this policy, legal authorities grant licenses only to those citizens who can establish a compelling need for carrying a gun.

The other approach is a non-discretionary, or "shall issue," system.[4] Here the authorities *must* provide a license to any applicant who meets specified criteria. Because legal officials are often unwilling to allow concealed weapons, adopting a shall issue policy usually increases the number of persons with permits to carry guns.[5]

In 1985, the National Rifle Association announced that it would

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[1] *See* JAMES D. WRIGHT ET. AL., UNDER THE GUN: WEAPONS, CRIME, AND VIOLENCE IN AMERICA 243-72 (1983); Gary Kleck & E. Britt Patterson, *The Impact of Gun Control and Gun Ownership Levels on Violence Rates*, 9 J. QUANTITATIVE CRIMINOLOGY 249 (1993).

[2] *See, e.g.,* Franklin E. Zimring, *Firearms, Violence, and Public Policy*, 265 SCI. AM. 48 (1991).

[3] GARY KLECK, POINT BLANK: GUNS AND VIOLENCE IN AMERICA 411-14 (1991).

[4] *Id.*

[5] Paul H. Blackman, Carrying Handguns for Personal Protection: Issues of Research and and Public Policy (presented at the Annual Meeting of the American Society of Criminology (Nov. 1985)).

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lobby for shall issue laws.[6] Several states, including Florida, Missis-
sippi, and Oregon, have since changed from may issue to shall issue
systems. Advocates of shall issue laws argue that such laws will both
prevent crime and reduce homicides.[7]

This Article examines the frequency of homicides in the large
urban areas of Florida, Mississippi, and Oregon, before and after their
shall issue laws began. The analysis provides no support for the idea
that the laws reduced homicides; instead, it finds evidence of an in-
crease in firearm murders.

II. The Laws

On October 1, 1987, Florida adopted a shall issue law that greatly
expanded eligibility to carry a concealed weapon.[8] The new statute
required the state to grant a concealed weapon license to any quali-
fied adult who had taken a firearms safety course. Those persons with
a history of drug or alcohol abuse, a felony conviction, mental illness,
physical inability, or who were not Florida residents were disqualified
from obtaining a license.

Prior to the passage of the Florida shall issue law, county officials
set their own standards for concealed carrying. Throughout the state,
about 17,000 persons held permits, including 1,300 in Dade county
(Miami) and 25 in Hillsborough county (Tampa).[9] The number of
licenses rose steadily after the passage of the new law, reaching
141,000 in September 1994.[10]

Mississippi adopted a shall issue law on July 1, 1990.[11] The Missis-
sippi law was similar to the Florida law, except that it did not require
firearms safety training. Mississippi's earlier law was highly restrictive,
generally allowing only security guards to have concealed weapons.[12]
In contrast, the new law is more lenient; by November 1992, the state
had issued 5,136 new licenses.[13]

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[6] *Id.; see also* G. Ray Arnett, *Sincerely, GRA*, 133 Am. Rifleman 7 (1985).

[7] *See, e.g.*, Wayne LaPierre, Guns, Crime, and Freedom 29-39 (1994); David B. Kopel,
Hold Your Fire: Gun Control Won't Stop Rising Violence, 63 Pol'y Rev. 58 (1993).

[8] Fla. Stat. ch. 790.06 (1992). *See* Richard Getchell, *Carrying Concealed Weapons in Self-
Defenses: Florida Adopts Uniform Regulations for the Issuance of Concealed Weapon Permits*, 15 Fla.
St. U.L. Rev. 751 (1987).

[9] *See* Lisa Getter, *Accused Criminals Get Gun Permits*, Miami Herald, May 15, 1988, at 1A;
Stephen Koff & Bob Port, *Gun Permits Soar Through Loopholes*, St. Petersburg Times, Jan. 7,
1988, at A1.

[10] Florida Department of State, Division of Licensing, Concealed Weapons/Fire-
arm License Statistical Report for Period 10/01/87 to 09/30/94 (1994).

[11] Miss. Code Ann. § 45-9-101 (1991).

[12] David Snyder, *New Miss. Gun-Permit Law Raises Visions of Old West*, Times-Picayune
(New Orleans), Aug. 13, 1990, at A1.

[13] Grace Simmons, *Police Want Concealed Guns Banned From Cars*, Clarion-Ledger (Jack-

Oregon adopted a shall issue law on January 1, 1990, in a compromise between supporters and opponents of stricter gun control measures.[14] Oregon's new law required county sheriffs to provide a concealed handgun license to any qualified adult who had taken a firearms safety course. People who could not obtain a license included: those with outstanding arrest warrants, those on pretrial release, those with a history of mental illness, or those with a felony or recent misdemeanor conviction.

In addition to easing laws on concealed carrying, Oregon's new law also tightened standards for buying a gun. While the old law barred convicted felons from owning handguns, the new law prohibited convicted felons from owning any type of firearm. Oregon's new law also lengthened the waiting period for handgun purchases and required more detailed background checks. It further prohibited most persons ineligible for a concealed handgun license from obtaining any firearm.

Before the passage of the new law in 1991, Oregon's sheriffs issued concealed handgun licenses at their discretion. In 1989, there were fewer than 500 licensed carriers in Clackamas, Multnomah, and Washington counties, the core of the Portland metropolitan area.[15] By October 1993, the number of licenses in these counties grew to 16,000.[16]

III.   POSSIBLE EFFECTS OF SHALL ISSUE LICENSING ON CRIME

While the shall issue policies clearly increased the number of persons licensed to carry concealed weapons in Florida, Mississippi, and Oregon, their effects on crime are less obvious. There are grounds to believe that crime might increase, decrease, or remain the same after a shall issue law is passed.

Shall issue licensing might reduce crime by deterring criminal offenders. Criminals generally wish to avoid victims who may be carrying guns.[17] Knowledge that many citizens have concealed weapons could discourage attempts at crime, especially crimes against strangers and crimes in public areas.

On the other hand, shall issue licensing also might raise levels of criminal violence. This is so because shall issue laws increase the

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son), Nov. 11, 1992, at A1.

[14] OR. REV. STAT. § 166.291-§166.295 (1991). *See also,* Rhonda Canby, *1989 Oregon Gun Control Legislation,* 26 WILLAMETTE L. REV. 565 (1990).

[15] Bill MacKenzie, *Packin' the Heat,* OREGONIAN (Portland), Nov. 4, 1993, at A1.

[16] *Id.*

[17] *See, e.g.,* JAMES D. WRIGHT & PETER H. ROSSI, ARMED AND CONSIDERED DANGEROUS: A SURVEY OF FELONS AND THEIR FIREARMS 141-59 (1986).

number of persons with easy access to guns. Zimring and Cook argue that assaults are often impulsive acts involving the most readily available weapons.[18] As guns are especially deadly weapons, more firearm carriers might result in more homicides.

Advocates of shall issue licensing cite figures showing that few legal carriers misuse their guns.[19] Yet greater tolerance for legal carrying may increase levels of illegal carrying as well. For example, criminals have more reason to carry firearms—and to use them— when their victims might be armed.[20] Further, if permission to carry a concealed weapon is easy to obtain, citizens and law enforcement officials may be less apt to view illegal carrying as a serious offense.

Still, shall issue licensing may be irrelevant to crime. Even in areas with shall issue policies, only a small fraction of adults have licenses to carry guns. Many citizens keep guns in their homes, and police officers often carry guns when off-duty and in plain clothes. The increase in available firearms due to shall issue licensing may be of little consequence.

IV. Existing Evidence on the Effects of Shall Issue Licensing

Most empirical discussions of shall issue licensing compare homicides in Florida before and after the beginning of its law. Homicide is the most accurately recorded crime, reducing the influence of measurement error on the comparison. Florida adopted its law earlier than did the other states, providing more time to study the effects.

All existing comparisons of Florida homicide rates before and after the passage of the Florida shall issue law found that Florida homicides decreased after the shall issue law. The National Rifle Association, for example, notes that Florida's homicide rate fell by 21% when comparing 1987 with 1992.[21]

Although the Florida experience appears to support a deterrent effect, the existing comparisons suffer from several weaknesses. First,

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[18] Franklin Zimring, *Is Gun Control Likely to Reduce Violent Killings?*, 35 U. Chi. L. Rev. 721 (1968); Philip J. Cook, *The Technology of Personal Violence, in* 14 Crime & Jus.: Ann. Rev. Res. 1 (Michael Tonry ed., 1991).

[19] *See, e.g.,* LaPierre, *supra* note 7, at 36-38; Jeffrey R. Snyder, *A Nation of Cowards,* 113 Pub. Interest 40 (1993). *See also* Florida Department of State, *supra* note 10.

[20] In a survey of prison inmates, Wright and Rossi found that a majority of gun-carrying criminals cited armed victims as an important motivation for their actions. Wright & Rossi, *supra* note 17, at 150. Of course, criminals rarely will know with certainty if a potential victim has a concealed gun. Even unarmed victims may therefore be more vulnerable to harm.

[21] National Rifle Association, Institute for Legislative Action, Fact Sheet: Carrying Concealed Firearms (CCW) Statistics (1994). *See also* LaPierre, *supra* note 7, at 33; Kopel, *supra* note 7, at 63; George F. Will, *Are We 'A Nation of Cowards'?*, Newsweek, Nov. 15, 1993, at 92-93.

these studies all use Uniform Crime Report data compiled by the Federal Bureau of Investigation (FBI). In 1988, the FBI did not publish crime counts for Florida. Evaluations based on the FBI data thus must ignore 1988 or use estimates of the 1988 total. This is important because 1988 was the first full year after the law's passage.[22]

Second, the existing evaluations use short time series of annual data. Even in Florida, there are few annual observations after the law began, and most comparisons only include those years immediately prior to the law's passage. Because crime increases and decreases over time due to the operation of many factors, comparisons using short time series are highly prone to the influence of chance events that briefly push homicides above or below their average levels.

Third, the existing comparisons examine total homicide rates for the entire state. If some areas respond differently to the laws than do others, a statewide analysis may miss important effects. For example, the influence of shall issue laws may be greatest in urban settings where crime is most prevalent. If this is true, including rural areas in an analysis would make it more difficult to detect changes in violence. Similarly, combining firearms and other weapon homicides might mask effects unique to one type of murder.

Fourth, most existing studies compare homicide levels before the shall issue law only with levels in 1991 or later. In February 1991, Florida adopted background checks of handgun buyers, and in October 1991, it began a waiting period for handgun purchases.[23] Comparisons that use only 1991 or later years cannot separate the effects of the shall issue law from those of the other two laws. The reductions in homicides that these studies claim may as easily be due to the other policies as to shall issue licensing.

In short, current evaluations leave much room for doubt about the effects of the Florida law. The shall issue laws in Mississippi and in Oregon have not received even this limited attention. A more detailed analysis using data from all three states would allow stronger inferences about the impact of the policies.

V. RESEARCH DESIGN

A. STUDY DESIGN AND DATA

Similar to existing evaluations of shall issue licensing, this study used an interrupted time series design to estimate average homicide

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[22] In addition, from 1988 through 1991 Florida did not report data to the FBI that distinguished firearms homicides from homicides by other means. Existing comparisons use only total homicide counts.

[23] FLA. STAT. chs. 790.065, 790.0655 (1992).

levels before and after shall issue policies began.[24] We studied patterns in Florida, Mississippi, and Oregon. In addition, we analyzed monthly homicide counts and examined only large urban areas within the three states. To find if the laws influenced gun deaths differently, firearm homicides were separated from homicides by other means.

We conducted analyses for Dade (Miami), Duval (Jacksonville), and Hillsborough (Tampa) counties in Florida, and for Hinds (Jackson) county in Mississippi. Because there were relatively few homicides in Multnomah county (Portland), we combined Clackamas, Multnomah, and Washington counties in Oregon. For each area, we used death certificate data compiled by the National Center for Health Statistics (NCHS) to count monthly homicides through December 1990.[25] Health departments in Florida, Mississippi, and Oregon provided additional cases from January 1991, to December 1992.

For all areas except Miami, we studied the period between January 1973 and December 1992 (240 months). We confined our Miami analysis to January 1983 through December 1992 (120 months) because of an unually sharp increase in homicide rates in May 1980 after an influx of Cuban refugees. In late 1982 the rates appeared to stabilize.[26]

In total, there were 177 months before the shall issue law in Jacksonville and Tampa, and 57 months before the shall issue law in Miami. For all three Florida cities there were 63 months after the law. In Mississippi there were 210 pre-law months and 30 post-law months. In Oregon there were 204 pre-law months and 36 post-law months.

To remove the effects of systematic variation from each time series, we developed autoregressive integrated moving average (ARIMA) noise models.[27] The noise models allow for variables, such as poverty or age structure, which influenced homicides both before and after the legal changes. If not controlled, these variables may bias inferences about the laws.

After developing suitable noise models, we added intervention models to measure changes in homicides following the shall issue laws.[28] We considered three intervention models: an abrupt permanent change model, a gradual permanent change model, and an ab-

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[24] *See* THOMAS D. COOK & DONALD T. CAMPBELL, QUASI-EXPERIMENTATION: DESIGN AND ANALYSIS ISSUES FOR FIELD SETTINGS 207-32 (1979).

[25] Department of Health and Human Services, National Center for Health Statistics, Inter-University Consortium for Political and Social Research, Mortality Detail Files, 1968 to 1990 (1993).

[26] Still, we reached similar conclusions when we analyzed all 240 months of Miami data.

[27] GEORGE E. P. BOX ET AL., TIME SERIES ANALYSIS: FORECASTING AND CONTROL (3d ed. 1994).

[28] *Id.* at 462-69.

rupt temporary change model.[29] For each series, the abrupt permanent change model provided the best fit to the data.[30]

Our analysis avoids the major problems of previous comparisons. The NCHS data collection system is independent of the FBI, allowing us to use 1988 Florida homicide counts.[31] The long monthly time series provides more stable estimates of homicide patterns before and after the shall issue laws began. By studying firearms and other weapon murders separately in several areas, we can more precisely isolate any changes due to the laws.

B.   THREATS TO VALIDITY AND SUPPLEMENTARY ANALYSIS

Interrupted time series studies are among the strongest non-experimental research designs.[32] Still, as is true with any design, time series studies do not eliminate all threats to valid inference.

Perhaps the most important threat to the design's validity is "history," the possibility that a permanent change in another variable produced an observed effect.[33] For example, suppose that each area adopted other policies that influenced crime when they began their shall issue laws. These policies then would be confounded with the laws, and they would be historical threats to validity.

The major method we used to avoid historical threats was replication of the analysis in five metropolitan areas. An unnoticed historical event may have increased or decreased homicides in any single area after its shall issue law began. Yet if similar outcomes occur in several different places after the laws, historical events become a less plausible explanation of the change.[34] With a consistent set of results, an historical explanation would require that each area witness permanent changes in other causes of homicide at about the time its law began. These changes would have to influence homicides in the same way in each area, increasing them in all five areas or decreasing them in all five areas.

The areas in our study are geographically separated and demographically diverse, and they adopted their laws at three different

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[29] *See* DAVID McDOWALL ET AL., INTERRUPTED TIME SERIES ANALYSIS 83-85 (1980.

[30] *Id.* at 83-85 (discussing criteria for selecting the best-fitting model).

[31] For a description of the FBI and NCHS data collection systems, see Marc Riedel, *Nationwide Homicide Data Sets: An Evaluation of the Uniform Crime Reports and the National Center for Health Statistics Data, in* MEASURING CRIME: LARGE-SCALE, LONG-RANGE EFFORTS 175 (Doris Layton MacKenzie et al. eds., 1990).

[32] *See* DONALD T. CAMPBELL & JULIAN C. STANLEY, EXPERIMENTAL AND QUASI-EXPERIMENTAL DESIGNS FOR RESEARCH 37-43 (1963).

[33] COOK & CAMPBELL, *supra* note 24, at 211.

[34] CAMPBELL & STANLEY, *supra* note 32, at 42 (pointing out that the natural sciences heavily rely on time series designs, and use replications to rule out rival hypotheses).

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times. While the replications cannot entirely rule out history, a consistent set of results would greatly narrow the range of historical events that could account for an effect. On the other hand, a varied pattern of results, with large increases or decreases in only one or two areas, would support an historical explanation.

Beyond replication, we used two additional methods to assess historical threats. First, we searched for other legal changes, especially changes in firearms laws, which might affect homicides. The most significant laws we found were Florida's background check, adopted in February 1991, and waiting period, adopted in October 1991.[35]

Florida's waiting period and background check laws began more than three years after shall issue licensing, leaving little data to estimate their effects. Still, we included these laws in a supplementary analysis to verify that they were not confounded with the licensing policy. Because the waiting period followed the background checks closely in time, we considered them as a single law that began in February 1991.

As a second check on historical threats, we estimated models that included homicide counts for the entire United States as an additional independent variable. This analysis studied whether homicide changes in the five areas simply mirrored national patterns; that is, homicide levels may have changed after the laws only because of events common to the nation as a whole. If this were true, the shall issue laws would not influence homicides net of the national counts.

We could obtain national homicide counts only through the end of 1991.[36] This limits the amount of data after the shall issue laws, especially in Mississippi and Oregon. Still, the national analysis provides an idea of whether broad historical events can explain any observed local changes.

Besides considering historical threats, we also conducted a supplementary analysis that used homicide rates instead of homicide counts. The population of all five areas grew over the study period, especially in the Florida cities. Homicide counts thus may have changed after the laws in part because of increases in the populations at risk.

To remove the influence of population, we estimated models for homicide rates per 100,000 persons. Only annual population figures

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[35] FLA. STAT. chs. 790.065, 790.065 (1992). As we noted earlier, Oregon changed several other features of its firearms laws when it adopted shall issue licensing. Because these other changes began with the shall issue policy, we cannot separately estimate their effects.

[36] Department of Health and Human Services, National Center for Health Statistics, Inter-University Consortium for Political and Social Research, Mortality Detail Files, 1968 to 1991 (1994).

were available, so we aggregated homicides in each area by year.[37] Because the annual data provided few cases to study changes in rates, we next pooled all five areas using a fixed effects analysis of variance model.[38] This created a single set of data, with seventy observations before the laws and twenty after the laws.[39] As in the main analysis, we then estimated separate equations for firearm homicides and for homicides by other methods.

In the pooled equations we first removed the mean homicide rates for each area and year. This controls for constant rate differences between the areas and for events that similarly influenced rates across all areas in a given year.[40] We then included intervention variables to measure the effects of the shall issue and (for the Florida cities) background check and waiting period laws.

VI.   RESULTS

Estimates of the effects of the shall issue laws on the monthly homicide counts appear in Table 1. To simplify the presentation, we report only the means before the laws and the changes in homicides after the laws began.[41]

The results in Table 1 show that firearms homicides increased in four of the five areas in the post-law period. Except the increase in Miami and the decrease in Portland, these changes were statistically significant ($p < .05$). Expressed as percentages, the changes varied from a decrease of 12% (Portland) to an increase of 75% (Jacksonville).[42] Considering each area as a replication of the same experiment, gun homicides increased by an average of 26%. An inverse normal combined test of statistical significance easily rejected the null

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[37] For 1973-1978 we used county-level population estimates from U.S. DEPARTMENT OF COMMERCE, BUREAU OF THE CENSUS, STATISTICAL ABSTRACT OF THE UNITED STATES (various years). For 1980-1992 we used unpublished Census Bureau estimates. The Census Bureau did not estimate county populations in 1979, and we interpolated values for that year.

[38] *See* CHENG HSIAO, ANALYSIS OF PANEL DATA (1986).

[39] Florida and Mississippi began their laws in the middle of the year. In the annual analysis we placed the interventions for these states at the first full year after the laws, 1988 for the Florida cities and 1991 for Jackson. Oregon's law began in January 1990, so we placed Portland's intervention at 1990.

[40] HSIAO, *supra* note 38, at 138-40.

[41] An appendix that describes the analysis in more detail is available from the authors.

[42] The NCHS data include civilian justifiable homicides, in which private citizens killed criminals during attempted felonies. We thus cannot dismiss the possibility that part of the rise in firearms murders was due to permit holders who shot offenders in self-defense. Still, justifiable homicides are rare, and it is not plausible that they could account for the bulk of the increase. According to FBI data for 1992, there were 262 justifiable handgun homicides in the entire United States, 1.7% of the 15,377 firearm murders. *See* FEDERAL BUREAU OF INVESTIGATION, CRIME IN THE UNITED STATES, 1992, at 15-22 (1993).

hypothesis of zero overall change.[43]

In contrast to gun homicides, homicides by other means did not show a consistent pattern of effects. Homicides without firearms increased in Tampa and Jacksonville, but they fell in the other three areas. Across all five areas, the average change in homicides without guns was an increase of less than 1%. In combination, this change was statistically insignificant.

Table 2 contains the analysis for the Florida cities that includes the state's waiting period and background check laws. These results provide no evidence that the original estimates were due to confounding between the other laws and shall issue licensing. Adding the other laws slightly increased the coefficients for the shall issue policy, but it did not alter their statistical significance.

Although not central to our study, it is worth noting that the levels of each Florida firearms series decreased after the waiting period and background checks began. Yet homicides without guns also fell in two cities, and the policies should influence only firearm crimes. The results do not point to any strong conclusions about the waiting period and background check laws.

Table 3 presents the analysis that adds national homicide counts to control for patterns in the United States as a whole.[44] In each area, there was a positive relationship between local homicide patterns and patterns in the nation. Still, including the national counts only modestly changed the estimates for shall issue licensing.

Finally, Table 4 reports the results for the annual homicide rates. Here the coefficient for the shall issue policies is the average effect across all five cities. Gun homicides increased on average by 4.5 per 100,000 persons, a value significantly different from zero. In contrast, murders without guns decreased insignificantly. Gun homicides fell insignificantly following Florida's waiting period and background check laws, while other weapon homicides increased.

VII. Discussion

Across the five areas, firearms homicides increased in the aftermath of the shall issue laws. In contrast, homicides without guns remained steady. These findings were little altered when we considered other laws, controlled for variations in national homicide counts, and

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[43] *See* Larry V. Hedges & Ingram Olkin, Statistical Methods for Meta-Analysis 39-40 (1985). The test assumes that the replications are independent. Because we include three cities from the same state in the analysis, this is probably only approximately correct.

[44] Because the national counts were not stationary in level, we used their first differences in this analysis. *See* Box, *supra* note 27, at 89-130 for a discussion of nonstationary time series models.

allowed for population change.

The pattern of results leads us to two conclusions, one stronger than the other. The stronger conclusion is that shall issue laws do not reduce homicides, at least in large urban areas. If there were such a decrease, other events would have to push murders up strongly enough to mask it in all five areas that we studied. Such events are possible, of course, but we believe that they are extremely unlikely.

The weaker conclusion is that shall issue laws raise levels of firearms murders. Coupled with a lack of influence on murders by other means, the laws thus increase the frequency of homicide. This interpretation is consistent with other work showing that policies to *discourage* firearms in public may help prevent violence. For example, studies by Pierce and Bowers and by O'Carroll et al. found that laws providing mandatory sentences for illegal gun carrying reduced firearms crimes in Boston and Detroit.[45] Similarly, Sherman et al. found that gun crimes fell during a Kansas City program that confiscated firearms from people who carried them outside their homes.[46]

Despite this evidence, we do not firmly conclude that shall issue licensing leads to more firearms murders. This is so because the effects varied over the study areas. Firearms homicides significantly increased in only three areas, and one area witnessed an insignificant decrease. In combination, the increase in gun homicides was large and statistically significant. Yet we have only five replications, and two of these do not clearly fit the pattern.

The statistical significance of the combined results aside, the analysis implies that shall issue policies do not *always* raise levels of gun murder. Sometimes, at least, local conditions operate to blunt any effects. The areas without significant increases, Portland and Miami, may be unusual, but we lack the data to examine whether this is true.

Stated in another way, we cannot completely dismiss historical events as an explanation of the increases in firearms murders. One would need a complex theory to explain how history could mask a *decrease* in homicides after the laws. Historical accounts of the apparent *increase* might be much simpler. One would then be left with the hypothesis that the effects of the laws are nil.

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[45] Glenn L. Pierce & William J. Bowers, *The Bartley-Fox Gun Law's Short-Term Impact on Crime in Boston,* 455 ANNALS AM. ACAD. POL. & SOC. SCI. 120, 120-37 (1981); Patrick W. O'Carroll et al., *Preventing Homicide: An Evaluation of the Efficacy of a Detroit Gun Ordinance,* 81 AM. J. PUB. HEALTH 576 (1991).

[46] Lawrence W. Sherman et al., *The Kansas City Gun Experiment,* NATIONAL INSTITUTE OF JUSTICE RESEARCH IN BRIEF (Jan. 1995). Sherman and associates note that about 20% of the seized firearms were legally carried.

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A more definitive analysis should be possible in the future. Besides Mississippi and Oregon, six other states have adopted shall issue laws based on the Florida model. Four of these—Alaska, Idaho, Montana, and Wyoming—have small populations and low levels of criminal violence.[47] As a result, it would be difficult to perform a statistically meaningful analysis of changes in homicides after their laws began.

Yet, two more populous states, Arizona and Tennessee, enacted shall issue licensing in 1994.[48] Given several years of experience with the laws in these areas, future research could provide more certain estimates of the effects on firearms violence.

Between January 1995 and March 1995, the legislatures of Arkansas, Utah, and Virginia sent shall issue laws to their Governors for signature.[49] Similar laws were pending in an additional fourteen states, including California, Illinois, and Texas.[50] Given this level of interest, it is likely that shall issue licensing will continue to receive attention in the future.

While our analysis does not allow a firm conclusion that shall issue licensing increases firearms homicides, it does suggest caution about these laws. Some observers consider strict limits on firearms outside the home to be among the most effective forms of gun control.[51] Beyond any influence on violence, the policies are easy to enforce and they do not inconvenience most gun owners. When states weaken limits on concealed weapons, they may be giving up a simple and effective method of preventing firearms deaths.

[47] ALASKA STAT. §§ 18.65.700-8.65.720 (1994); IDAHO CODE § 18-3302 (1993); MONT. CODE ANN. § 45-8-321 (1993); WYO. STAT. § 6-8-104 (1994).

[48] ARIZ. REV. STAT. ANN. § 13-3112 (1994); TENN. CODE ANN. § 39-17-1315 (1994).

[49] Roger Worthington, *Support Mounting for Concealed Guns*, CHI. TRIB., Mar. 6, 1995, at A1.

[50] Sam Howe Verhovek, *States Seek to Let Citizens Carry Concealed Weapons*, N.Y. TIMES, Mar. 6, 1995, at A1.

[51] *See* Mark H. Moore, *The Bird in Hand: A Feasible Strategy for Gun Control*, 2 J. POL'Y ANALYSIS & MGMT. 185 (1983); SAMUEL WALKER, SENSE AND NONSENSE ABOUT CRIME: A POLICY GUIDE 179-198 (2d ed. 1989).

Table 1

MEAN NUMBERS OF HOMICIDES PER MONTH, BY JURISDICTION AND METHOD, BEFORE AND AFTER IMPLEMENTATION OF SHALL ISSUE LICENSING

Type of Homicide and Location	Before the Shall Issue Law no./mo.	Change After the Shall Issue Law*			
		no./mo.	SE	%	t-Statistic
Firearm					
Miami	25.88	0.79	1.09	+3	0.73
Jacksonville	6.24	4.78	0.61	+75	7.84
Tampa	4.91	1.10	0.44	+22	2.50
Portland area	2.79	−0.34	0.35	−12	−0.98
Jackson	3.64	1.57	0.47	+43	3.34
Mean change = +26.2%	Inverse normal combined Z = −6.01, p < .0001				
Other Methods					
Miami	9.58	−0.73	0.63	−8	−1.16
Jacksonville	2.85	1.03	0.32	+36	3.22
Tampa	2.74	0.48	0.42	+17	1.14
Portland area	2.46	−0.58	0.38	−24	−1.53
Jackson	1.34	−0.30	0.27	−22	−1.11
Mean change = −0.2%	Inverse normal combined Z = +0.25, p = .8023				

* Difference between the mean number of homicides per month before implementation of the shall issue law and the mean number after its implementation.

Table 2

MEAN NUMBERS OF HOMICIDES PER MONTH IN FLORIDA AREAS, BY JURISDICTION AND METHOD, BEFORE AND AFTER IMPLEMENTATION OF SHALL ISSUE LICENSING AND WAITING PERIOD AND BACKGROUND CHECK LAWS

Type of Homicide and Location	Before the Laws no./mo.	Change After the Shall Issue Law*			Change After the Waiting Period and Background Check Laws**		
		no./mo.	SE	t-Statistic	no./mo.	SE	t-Statistic
Firearm							
Miami	25.88	2.25	1.19	1.89	−3.99	1.51	−2.64
Jacksonville	6.21	6.10	0.61	10.00	−3.11	0.86	−3.62
Tampa	4.91	1.35	0.52	2.60	−0.68	0.77	−0.88
Other Methods							
Miami	9.60	0.11	0.53	0.21	−2.48	0.68	−3.65
Jacksonville	2.86	1.25	0.38	3.29	−0.60	0.56	−1.07
Tampa	2.74	0.42	0.49	0.86	0.17	0.72	0.24

* Difference between the mean number of homicides per month before implementation of the shall issue law and the mean number after its implementation, controlling for the waiting period and background check laws.
** Difference between the mean number of homicides per month before implementation of the waiting period and background check laws and the mean number after their implementation, controlling for the shall issue law.

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Table 3

MEAN NUMBERS OF HOMICIDES PER MONTH, BY JURISDICTION AND METHOD, BEFORE AND AFTER IMPLEMENTATION OF SHALL ISSUE LICENSING, CONTROLLING FOR NATIONAL HOMICIDE COUNTS

Type of Homicide and Location	Constant no./mo.	Change After the Shall Issue Law*			Coefficient for National Homicide Counts**		
		no./mo.	SE	t-Statistic	slope	SE	t-Statistic
Firearm							
Miami	25.86	1.55	1.12	1.38	0.0144	0.0063	2.29
Jacksonville	6.23	5.36	0.64	8.37	0.0015	0.0019	0.79
Tampa	4.91	1.17	0.49	2.39	0.0014	0.0015	0.93
Portland area	2.80	−0.44	0.42	−1.05	0.0015	0.0014	1.07
Jackson	3.62	1.61	0.57	2.82	0.0011	0.0013	0.85
Other Methods							
Miami	9.62	−0.43	0.55	−0.78	0.0010	0.0051	0.20
Jacksonville	2.86	0.96	0.35	2.74	0.0181	0.0214	0.85
Tampa	2.75	0.81	0.45	1.80	0.0077	0.0205	0.38
Portland area	2.46	−0.28	0.43	−0.65	0.0039	0.0019	2.05
Jackson	1.35	0.22	0.27	0.81	0.0027	0.0013	2.08

* Difference between the mean number of homicides per month before implementation of the shall issue law and the mean number after its implementation, controlling for national homicide counts.
** Slope estimate for influence of national homicide counts, controlling for the shall issue law.

Table 4

POOLED ANNUAL HOMICIDE RATES, BEFORE AND AFTER IMPLEMENTATION OF SHALL ISSUE LICENSING AND WAITING PERIOD AND BACKGROUND CHECK LAWS

Firearms Homicide Rate Per 100,000			
	Coefficient Estimate	SE	t-Statistic
Shall Issue Licensing	4.52	1.75	2.58
Waiting Period and Background Check	−3.25	2.09	−1.55
Constant	11.20	0.53	21.13

Other Methods Homicide Rate Per 100,000			
	Coefficient Estimate	SE	t-Statistic
Shall Issue Licensing	−0.16	0.75	−0.21
Waiting Period and Background Check	1.81	0.90	2.01
Constant	5.02	0.23	21.83

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👤 **PEOPLE**			
Population			
Population estimates, July 1, 2015, (V2015)		213,016	321,418,820
Population estimates, July 1, 2014, (V2014)		207,590	318,857,056
Population estimates base, April 1, 2010, (V2015)		200,850	308,758,105
Population estimates base, April 1, 2010, (V2014)		200,850	308,758,105
Population, percent change - April 1, 2010 (estimates base) to July 1, 2015, (V2015)		6.1%	4.1%
Population, percent change - April 1, 2010 (estimates base) to July 1, 2014, (V2014)		3.4%	3.3%
Population, Census, April 1, 2010		200,849	308,745,538
Age and Sex			
Persons under 5 years, percent, July 1, 2014, (V2014)		5.8%	6.2%
Persons under 5 years, percent, April 1, 2010		6.3%	6.5%
Persons under 18 years, percent, July 1, 2014, (V2014)		21.7%	23.1%
Persons under 18 years, percent, April 1, 2010		22.7%	24.0%
Persons 65 years and over, percent, July 1, 2014, (V2014)		11.5%	14.5%
Persons 65 years and over, percent, April 1, 2010		9.8%	13.0%
Female persons, percent, July 1, 2014, (V2014)		51.3%	50.8%
Female persons, percent, April 1, 2010		51.2%	50.8%
Race and Hispanic Origin			
White alone, percent July 1, 2014, (V2014) (a)		75.9%	77.4%
White alone, percent, April 1, 2010 (a)		63.2%	72.4%
Black or African American alone, percent, July 1, 2014, (V2014) (a)		3.0%	13.2%
Black or African American alone, percent, April 1, 2010 (a)		2.6%	12.6%
American Indian and Alaska Native alone, percent, July 1, 2014, (V2014) (a)		1.8%	1.2%
American Indian and Alaska Native alone, percent, April 1, 2010 (a)		1.1%	0.9%
Asian alone, percent, July 1, 2014, (V2014) (a)		13.8%	5.4%
Asian alone, percent, April 1, 2010 (a)		13.0%	4.8%
Native Hawaiian and Other Pacific Islander alone, percent, July 1, 2014, (V2014) (a)		0.6%	0.2%
Native Hawaiian and Other Pacific Islander alone, percent, April 1,		0.5%	0.2%

2010 (a)		
Two or More Races, percent, July 1, 2014, (V2014)	5.0%	2.5%

⚠ This geographic level of poverty and health estimates are not comparable to other geographic levels of these estimates

Some estimates presented here come from sample data, and thus have sampling errors that may render some apparent differences between geographies statistically indistinguishable. Click the Quick Info 🛈 icon to the left of each row in TABLE view to learn about sampling error.

The vintage year (e.g., V2015) refers to the final year of the series (2010 thru 2015).
Different vintage years of estimates are not comparable.

(a) Includes persons reporting only one race
(b) Hispanics may be of any race, so also are included in applicable race categories
(c) Economic Census - Puerto Rico data are not comparable to U.S. Economic Census data

D Suppressed to avoid disclosure of confidential information
F Fewer than 25 firms
FN Footnote on this item in place of data
NA Not available
S Suppressed; does not meet publication standards
X Not applicable
Z Value greater than zero but less than half unit of measure shown

QuickFacts data are derived from: Population Estimates, American Community Survey, Census of Population and Housing, Current Population Survey, Small Area Health Insurance Estimates, Small Area Income and Poverty Estimates, State and County Housing Unit Estimates, County Business Patterns, Nonemployer Statistics, Economic Census, Survey of Business Owners, Building Permits.

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👤 PEOPLE			
Population			
Population estimates, July 1, 2015, (V2015)		1,501,335	321,418,820
Population estimates, July 1, 2014, (V2014)		1,482,026	318,857,056
Population estimates base, April 1, 2010, (V2015)		1,418,742	308,758,105
Population estimates base, April 1, 2010, (V2014)		1,418,742	308,758,105
Population, percent change - April 1, 2010 (estimates base) to July 1, 2015, (V2015)		5.8%	4.1%
Population, percent change - April 1, 2010 (estimates base) to July 1, 2014, (V2014)		4.5%	3.3%
Population, Census, April 1, 2010		1,418,788	308,745,538
Age and Sex			
Persons under 5 years, percent, July 1, 2014, (V2014)		6.7%	6.2%
Persons under 5 years, percent, April 1, 2010		7.1%	6.5%
Persons under 18 years, percent, July 1, 2014, (V2014)		24.4%	23.1%
Persons under 18 years, percent, April 1, 2010		25.6%	24.0%
Persons 65 years and over, percent, July 1, 2014, (V2014)		12.8%	14.5%
Persons 65 years and over, percent, April 1, 2010		11.2%	13.0%
Female persons, percent, July 1, 2014, (V2014)		51.1%	50.8%
Female persons, percent, April 1, 2010		51.0%	50.8%
Race and Hispanic Origin			
White alone, percent July 1, 2014, (V2014) (a)		64.6%	77.4%
White alone, percent, April 1, 2010 (a)		57.5%	72.4%
Black or African American alone, percent, July 1, 2014, (V2014) (a)		10.9%	13.2%
Black or African American alone, percent, April 1, 2010 (a)		10.4%	12.6%
American Indian and Alaska Native alone, percent, July 1, 2014, (V2014) (a)		1.5%	1.2%
American Indian and Alaska Native alone, percent, April 1, 2010 (a)		1.0%	0.9%
Asian alone, percent, July 1, 2014, (V2014) (a)		15.8%	5.4%
Asian alone, percent, April 1, 2010 (a)		14.3%	4.8%
Native Hawaiian and Other Pacific Islander alone, percent, July 1, 2014, (V2014) (a)		1.2%	0.2%
Native Hawaiian and Other Pacific Islander alone, percent, April 1,		1.0%	0.2%

2010 (a)

ⓘ Two or More Races, percent, July 1, 2014, (V2014)	6.0%	2.5%

⚠ This geographic level of poverty and health estimates are not comparable to other geographic levels of these estimates

Some estimates presented here come from sample data, and thus have sampling errors that may render some apparent differences between geographies statistically indistinguishable. Click the Quick Info ⓘ icon to the left of each row in TABLE view to learn about sampling error.

The vintage year (e.g., V2015) refers to the final year of the series (2010 thru 2015).
Different vintage years of estimates are not comparable.

(a) Includes persons reporting only one race
(b) Hispanics may be of any race, so also are included in applicable race categories
(c) Economic Census - Puerto Rico data are not comparable to U.S. Economic Census data

D Suppressed to avoid disclosure of confidential information
F Fewer than 25 firms
FN Footnote on this item in place of data
NA Not available
S Suppressed; does not meet publication standards
X Not applicable
Z Value greater than zero but less than half unit of measure shown

QuickFacts data are derived from: Population Estimates, American Community Survey, Census of Population and Housing, Current Population Survey, Small Area Health Insurance Estimates, Small Area Income and Poverty Estimates, State and County Housing Unit Estimates, County Business Patterns, Nonemployer Statistics, Economic Census, Survey of Business Owners, Building Permits.

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http://www.census.gov/quickfacts/table/PST045215/06067,00[6/3/2016 9:30:31 AM]

United States Court of Appeals for the Ninth Circuit

Office of the Clerk
95 Seventh Street
San Francisco, CA 94103

Information Regarding Judgment and Post-Judgment Proceedings

Judgment
- This Court has filed and entered the attached judgment in your case. Fed. R. App. P. 36. Please note the filed date on the attached decision because all of the dates described below run from that date, not from the date you receive this notice.

Mandate (Fed. R. App. P. 41; 9th Cir. R. 41-1 & -2)
- The mandate will issue 7 days after the expiration of the time for filing a petition for rehearing or 7 days from the denial of a petition for rehearing, unless the Court directs otherwise. To file a motion to stay the mandate, file it electronically via the appellate ECF system or, if you are a pro se litigant or an attorney with an exemption from using appellate ECF, file one original motion on paper.

Petition for Panel Rehearing (Fed. R. App. P. 40; 9th Cir. R. 40-1)
Petition for Rehearing En Banc (Fed. R. App. P. 35; 9th Cir. R. 35-1 to -3)

(1) A. Purpose (Panel Rehearing):
- A party should seek panel rehearing only if one or more of the following grounds exist:
  - ► A material point of fact or law was overlooked in the decision;
  - ► A change in the law occurred after the case was submitted which appears to have been overlooked by the panel; or
  - ► An apparent conflict with another decision of the Court was not addressed in the opinion.
- Do not file a petition for panel rehearing merely to reargue the case.

B. Purpose (Rehearing En Banc)
- A party should seek en banc rehearing only if one or more of the following grounds exist:

►   Consideration by the full Court is necessary to secure or maintain uniformity of the Court's decisions; or

►   The proceeding involves a question of exceptional importance; or

►   The opinion directly conflicts with an existing opinion by another court of appeals or the Supreme Court and substantially affects a rule of national application in which there is an overriding need for national uniformity.

(2)   Deadlines for Filing:

- A petition for rehearing may be filed within 14 days after entry of judgment. Fed. R. App. P. 40(a)(1).
- If the United States or an agency or officer thereof is a party in a civil case, the time for filing a petition for rehearing is 45 days after entry of judgment. Fed. R. App. P. 40(a)(1).
- If the mandate has issued, the petition for rehearing should be accompanied by a motion to recall the mandate.
- *See* Advisory Note to 9th Cir. R. 40-1 (petitions must be received on the due date).
- An order to publish a previously unpublished memorandum disposition extends the time to file a petition for rehearing to 14 days after the date of the order of publication or, in all civil cases in which the United States or an agency or officer thereof is a party, 45 days after the date of the order of publication. 9th Cir. R. 40-2.

(3)   Statement of Counsel

- A petition should contain an introduction stating that, in counsel's judgment, one or more of the situations described in the "purpose" section above exist. The points to be raised must be stated clearly.

(4)   Form & Number of Copies (9th Cir. R. 40-1; Fed. R. App. P. 32(c)(2))

- The petition shall not exceed 15 pages unless it complies with the alternative length limitations of 4,200 words or 390 lines of text.
- The petition must be accompanied by a copy of the panel's decision being challenged.
- An answer, when ordered by the Court, shall comply with the same length limitations as the petition.
- If a pro se litigant elects to file a form brief pursuant to Circuit Rule 28-1, a petition for panel rehearing or for rehearing en banc need not comply with Fed. R. App. P. 32.

- The petition or answer must be accompanied by a Certificate of Compliance found at Form 11, available on our website at www.ca9.uscourts.gov under *Forms*.
- You may file a petition electronically via the appellate ECF system. No paper copies are required unless the Court orders otherwise. If you are a pro se litigant or an attorney exempted from using the appellate ECF system, file one original petition on paper. No additional paper copies are required unless the Court orders otherwise.

Bill of Costs (Fed. R. App. P. 39, 9th Cir. R. 39-1)
- The Bill of Costs must be filed within 14 days after entry of judgment.
- See Form 10 for additional information, available on our website at www.ca9.uscourts.gov under *Forms*.

Attorneys Fees
- Ninth Circuit Rule 39-1 describes the content and due dates for attorneys fees applications.
- All relevant forms are available on our website at www.ca9.uscourts.gov under *Forms* or by telephoning (415) 355-7806.

Petition for a Writ of Certiorari
- Please refer to the Rules of the United States Supreme Court at www.supremecourt.gov

Counsel Listing in Published Opinions
- Please check counsel listing on the attached decision.
- If there are any errors in a published <u>opinion</u>, please send a letter **in writing within 10 days** to:
  - ► Thomson Reuters; 610 Opperman Drive; PO Box 64526; St. Paul, MN 55164-0526 (Attn: Jean Green, Senior Publications Coordinator);
  - ► and electronically file a copy of the letter via the appellate ECF system by using "File Correspondence to Court," or if you are an attorney exempted from using the appellate ECF system, mail the Court one copy of the letter.

Form 10. Bill of Costs ....*(Rev. 12-1-09)*

United States Court of Appeals for the Ninth Circuit

BILL OF COSTS

This form is available as a fillable version at:
http://cdn.ca9.uscourts.gov/datastore/uploads/forms/Form%2010%20-%20Bill%20of%20Costs.pdf.

<u>*Note:*</u> If you wish to file a bill of costs, it MUST be submitted on this form and filed, with the clerk, with proof of service, within 14 days of the date of entry of judgment, and in accordance with 9th Circuit Rule 39-1. A late bill of costs must be accompanied by a motion showing good cause. Please refer to FRAP 39, 28 U.S.C. § 1920, and 9th Circuit Rule 39-1 when preparing your bill of costs.

[   ] v. [   ] 9th Cir. No. [   ]

The Clerk is requested to tax the following costs against: [   ]

Cost Taxable under FRAP 39, 28 U.S.C. § 1920, 9th Cir. R. 39-1	REQUESTED *(Each Column Must Be Completed)*				ALLOWED *(To Be Completed by the Clerk)*			
	No. of Docs.	Pages per Doc.	Cost per Page*	TOTAL COST	No. of Docs.	Pages per Doc.	Cost per Page*	TOTAL COST
Excerpt of Record			$	$			$	$
Opening Brief			$	$			$	$
Answering Brief			$	$			$	$
Reply Brief			$	$			$	$
Other**			$	$			$	$
			TOTAL:	$			**TOTAL:**	$

* *Costs per page*: May not exceed .10 or actual cost, whichever is less. 9th Circuit Rule 39-1.

** *Other*: Any other requests must be accompanied by a statement explaining why the item(s) should be taxed pursuant to 9th Circuit Rule 39-1. Additional items without such supporting statements will not be considered.

Attorneys' fees **cannot** be requested on this form.

Continue to next page

Form 10. Bill of Costs - *Continued*

I, [   ] , swear under penalty of perjury that the services for which costs are taxed were actually and necessarily performed, and that the requested costs were actually expended as listed.

Signature [   ]

("s/" plus attorney's name if submitted electronically)

Date [   ]

Name of Counsel: [   ]

Attorney for: [   ]

(To Be Completed by the Clerk)

Date [   ]   Costs are taxed in the amount of $ [   ]

Clerk of Court

By: [   ] , Deputy Clerk