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Shooting Down the "More Guns, Less Crime" Hypothesis

Ian Ayres* & John J. Donohue III**

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INTRODUCTION

In a remarkable paper published in 1997, John Lott and David Mustard managed to set the agenda for much subsequent dataset work on the impact of guns on crime in America by creating a massive dataset of crime across all U.S. counties from 1977 through 1992 and by amassing a powerful statistical argument that state laws enabling citizens to carry concealed handguns had *reduced* crime.[1] The initial paper was followed a year later by an even more comprehensive and sustained argument to the same effect in a book solely authored by John Lott entitled *More Guns, Less Crime* (now in its second edition).[2] The work by Lott and Mustard has triggered an unusually large set of academic responses, with talented scholars lining up on both sides of the debate.[3] Indeed, a panel of the National Academy of Sciences has been convened to sort through the now large body of conflicting studies.

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1. John R. Lott, Jr. & David B. Mustard, *Crime, Deterrence, and Right-to-Carry Concealed Handguns*, 26 J. LEGAL STUD. 1 (1997). A law that allows a citizen to carry a concealed handgun if he or she can demonstrate a need to a government official is a discretionary, or "may-issue," law. The "shall-issue" laws are designed to eliminate discretion on the part of governmental officials by requiring them to issue a permit to carry concealed handguns unless specific and easily verifiable factors dictate otherwise. Essentially, these "concealed-carry laws" enable adults without serious criminal records or identified mental illness to carry concealed handguns in virtually all public places. For a listing of the states that have shall-issue laws (according to two different sets of coding), see Appendix Table 1.

2. JOHN R. LOTT, JR., MORE GUNS, LESS CRIME: UNDERSTANDING CRIME AND GUN-CONTROL LAWS (2d ed. 2000).

3. Among the articles that are supportive of the more guns, less crime thesis are Bruce L. Benson & Brent Mast, *Privately Produced General Deterrence*, 44 J.L. & ECON. 1 (2001); Stephen G. Bronars & John R. Lott, Jr., *Criminal Deterrence, Geographic Spillovers, and the Right to Carry Concealed Handguns*, 88 AM. ECON. REV. 475 (1998); Carlisle E. Moody, *Testing for the Effects of Concealed Weapons Laws: Specification Errors and Robustness*, 44 J.L. & ECON. 799 (2001); David B. Mustard, *The Impact of Gun Laws on Police Deaths*, 44 J.L. & ECON. 635 (2001); John R. Lott, Jr. & William M. Landes, *Multiple Victim Public Shootings*, *at* http://papers.ssrn.com/sol3/papers.cfm?abstract_id=272929 (last modified June 10, 2001).

Articles that raise doubts about the Lott and Mustard findings include Dan A. Black &

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But in the world of affairs rather than ideas, it did not take long for the National Rifle Association (NRA) and politicians across the country to seize upon the work of Lott and Mustard to oppose efforts at gun control and advance the cause of greater freedom to carry guns. For example, in the same year that the initial article was published, Senator Larry Craig (R-Idaho) introduced The Personal Safety and Community Protection Act, which was designed to facilitate the carrying of concealed firearms by nonresidents of a state who had obtained valid permits to carry such weapons in their home state. Senator Craig argued that the work of John Lott showed that arming the citizenry via laws allowing the carrying of concealed handguns would have a protective effect for the community at large because criminals would find themselves in the line of fire.[4] On May 27, 1999, Lott testified before the House Judiciary Committee that the stricter gun regulations proposed by President Clinton either would have no effect or would actually cost lives,[5] and a number of Republican members of Congress have since included favorable references in their speeches to Lott's work.[6] Moreover, Lott has also testified in support of concealed gun laws before several state legislatures, including Nebraska (1997), Michigan (1998), Minnesota (1999), Ohio (2002), and Wisconsin (2002).[7]

This past summer, Lott's work was favorably cited in a letter to Attorney General John Ashcroft, signed by eighteen state attorneys general, in support of Ashcroft's decision to interpret the Second Amendment as protecting the right of individuals to bear arms. The letter concluded with the following statement:

> As the chief law enforcement officers of our respective states, we wish to make one final point that is outside the scope of constitutional analysis.

Daniel S. Nagin, *Do Right-to-Carry Laws Deter Violent Crime?*, 27 J. LEGAL STUD. 209 (1998); Hashem Dezhbakhsh & Paul H. Rubin, *Lives Saved or Lives Lost? The Effects of Concealed-Handgun Laws on Crime*, 88 AM. ECON. REV. 468 (1998); Mark Duggan, *More Guns, More Crime*, 109 J. POL. ECON. 1086 (2001); Jens Ludwig, *Concealed-Gun-Carrying Laws and Violent Crime: Evidence from State Panel Data*, 18 INT'L REV. L. & ECON. 239 (1998); Franklin Zimring & Gordon Hawkins, *Concealed Handguns: The Counterfeit Deterrent*, RESPONSIVE COMMUNITY, Spring 1997, at 46.

4. 143 CONG. REC. S5109 (daily ed. May 23, 1997) (statement of Sen. Craig). Although Congress has not yet adopted this legislation, it was reintroduced in 2000 by Congressman Cliff Stearns (R-Florida), who also specifically cited Lott's work. 146 CONG. REC. H2658 (daily ed. May 9, 2000) (statement of Rep. Stearns).

5. *Gun Regulations Can Cost Lives: Hearing Before the Subcomm. on Crime of the House Comm. on the Judiciary*, 106th Cong. (1999) (statement of John R. Lott, Jr.).

6. *See, e.g.*, 146 CONG. REC. S349 (daily ed. Feb. 7, 2000) (statement of Sen. Craig); 145 CONG. REC. H8645 (daily ed. Sept. 24, 1999) (statement of Rep. Doolittle).

7. Of these states, only Michigan has adopted a right-to-carry law—the law took effect on January 1, 2001. *See Michigan's New Concealed Weapon's Law: Referendum Petition Drive Under Way*, FOCUS (Michigan Catholic Conference, Lansing, Mich.), Jan. 2001, at 1, *available at* http://www.micatholicconference.org/pdf/FOCUS_Jan_2001.pdf. The Wisconsin Assembly adopted the proposed law supported by Lott on February 26, 2002, but the Senate has not yet passed the bill. *See* A.B. 675, 95th Leg. Sess., Reg. Sess. (Wis. 2001), *available at* http://www.legis.state.wi.us/2001/data/AB675hst.html.

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> Simply put, your position on the Second Amendment is a sound public policy decision. There is an increasing amount of data available to support the claim that private gun ownership deters crime. That evidence comes both from the United States (particularly as highlighted in the empirical research of John Lott) and from abroad.[8]

Lott has also drawn upon his scholarly work to become a major popular commentator in the wake of various instances of violence and mayhem. After sixteen individuals were killed in a school shooting in Germany in April 2002, Lott attacked the strict gun control measures in Europe claiming that "[t]he problem with such laws is that they take away guns from law-abiding citizens, while would-be criminals ignore them, leaving potential victims defenseless."[9] After the attacks of September 11, 2001, Lott similarly argued that "fears of having guns on planes are misplaced. The special, high-velocity handgun ammunition used on planes packs quite a wallop but is designed not to penetrate the aluminum skin of the plane." Noting that "[s]tates that pass concealed handgun laws experience drops in violent crimes, especially in multiple victim shootings—the type of attack most associated with terrorism," Lott argued that "[t]he use of guns to stop terrorists shouldn't be limited to airplanes. We should encourage off-duty police, and responsible citizens, to carry guns in most public places. Cops can't be everywhere."[10] Clearly, Lott's message has been widely heard. This Article will explore whether the message is in fact true.

In addition to their statistical work, Lott and Mustard have also compiled a large body of anecdotal evidence concerning instances where law-abiding citizens have used guns to capture or thwart dangerous criminals. It may well be the case that many advocates of gun control have been inattentive to the possible benefits—in terms of protection or psychological comfort—that have at times been achieved by those lawfully carrying concealed weapons. Among the many anecdotes in Lott's book designed to emphasize this point, consider the case of Suzanna Hupp, who was sitting in a cafeteria in Killeen, Texas in 1991 when a gunman crashed his car into the restaurant and began shooting the patrons. Although Hupp had a gun in her car, which she believes might have been used to stop the killer, her parents and twenty-one others died in the

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8. Letter from Bill Pryor, Alabama Attorney General, to John Ashcroft, United States Attorney General (July 8, 2002), *available at* http://www.ago.state.al.us/ag_items.cfm?Item=81.

9. John R. Lott, Jr., *Blown Away: Gun Control Misfires in Europe, What's Behind the Massacres in Germany, France and Switzerland?*, WSJ.com Opinion Journal (May 4, 2002), *at* http://www.opinionjournal.com/extra/?id=105002026. Supporters of tighter controls on guns note, however, that "among rich countries, the U.S. far surpassed any other country in firearm-related deaths—with 30,419 reported in 1998, or 11.3 per 100,000 people. That compared with 83 gun-related deaths in Japan and 197 in the United Kingdom during the same period, with rates of 0.1 and 0.3 per 100,000, respectively." Rachel Zimmerman, *Study Finds Violence Took 1.6 Million Lives in 2000*, WALL ST. J., Oct. 3, 2002, at D5.

10. John R. Lott, Jr., *Only Guns Can Stop Terrorists*, WALL ST. J., Sept. 28, 2001, at A14.

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massacre. Following the horrific event, she verbally attacked those who had "legislated me out of the right to protect my family members."[11] Going on to become a legislator herself, Hupp now always carries a firearm, and she is trying to extend the right to carry concealed handguns (now legal in Texas in part due to her efforts) to the few remaining places where they are banned, such as in churches, university campuses, and public schools.

Of course, we do not know whether Hupp would have been carrying her gun on her person that day if she had had the legal right to do so (the fact that she carries it now is not dispositive on that question), or if she would have been able to save her parents or others (rather than just become another casualty by staying and fighting instead of escaping out a shattered window to safety, as she did). It is clear, though, that her loss was terrible, and having a concealed-carry law in effect at the time *might* have reduced the carnage in that situation (or might have added one or more other victims).

While Lott and Mustard have energetically catalogued the situations in which armed citizens have protected themselves or others, they never acknowledge cases on the other side of the ledger where the presence of guns almost certainly led to killings.[12] In the end, one must acknowledge that there are both costs and benefits to either allowing or prohibiting the carrying of handguns, and the task for the scholar is to try to determine which effects dominate.

The existence of a widely cited study based on the statistical analysis of a massive dataset that is invoked in both political and popular circles as an argument against most forms of gun control suggests that careful scrutiny of the empirical evidence is warranted. Lott and Mustard based their analysis on the current state-of-the-art technique of micro-econometric evaluation—a panel data model with fixed effects.[13] That is, Lott and Mustard began by collecting

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11. Hector Tobar, *To Texas Lawmaker, Guns Are the Answer*, L.A. TIMES, Mar. 16, 2001, at A21.

12. For example, the nightmare scenario for those asserting the value of defensive use of guns is not mentioned: the case of the Japanese exchange student, Yoshihiro Hattori, who was on his way to a Halloween party in October 1992 when he mistakenly approached the wrong house and was shot to death by the homeowner, Rodney Peairs. Peairs, who was later found civilly liable for the boy's wrongful death after an acquittal at his criminal trial, was at home with his family when Hattori and his American host mistakenly rang the doorbell in search of the party. Peairs's wife answered and, apparently frightened by the costume, yelled to her husband to get his gun. Peairs shot Hattori dead after warning him to "freeze," a phrase the young man apparently did not understand. A Baton Rouge, Louisiana judge awarded more than $650,000 in damages to Hattori's parents, saying there was "no justification whatsoever" for the killing of the 16-year-old boy. Adam Nossiter, *Judge Awards Damages in Japanese Youth's Death*, N.Y. TIMES, Sept. 16, 1994, at A12.

13. There are actually two "fixed effects." The first is a dummy variable for each county or state that is designed to reflect any unvarying trait that influences crime in that county or state yet is not captured by any of the other explanatory variables. The second is a set of "year fixed effects," which are dummy variables included for each year of the dataset to capture any national influence on crime that is not captured in any of the other explanatory

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data over a period of years (1977-1992) for individual states and counties across the United States, and then used panel data regression techniques to estimate the effect of the adoption of shall-issue laws, controlling for an array of social, economic, and demographic factors.[14] In earlier work, we commented on concerns that we had about model reliability based on Lott's analysis of 1977-1992 data evaluating the effect of the adoption of shall-issue laws in ten states.[15] We opined on the potential theoretical and empirical infirmities in that analysis, and noted the value in further study given that more state adoptions and the passage of time would likely either strengthen Lott's case if it were true or weaken it if it were false. Having extended the state data through 1999 and the county dataset through 1997, we are now able to test that prediction. We conclude that Lott and Mustard have made an important scholarly contribution in establishing that these laws have not led to the massive bloodbath of death and injury that some of their opponents feared. On the other hand, we find that the statistical evidence that these laws have reduced crime is limited, sporadic, and extraordinarily fragile. Minor changes of specifications can generate wide shifts in the estimated effects of these laws, and some of the most persistent findings—such as the association of shall-issue laws with increases in (or no effect on) robbery and with substantial increases in various types of property crime—are not consistent with any plausible theory of deterrence. Indeed, the probabilistic underpinnings of statistical analysis suggest that running regressions for nine different crime categories to see if there is any measurable impact on crime will, by chance alone, frequently generate estimates that on their face are "statistically significant." Therefore, it

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variables, but which might be expected to affect all jurisdictions equally. A full list of the variables included in the regressions (other than year and county dummies) and their summary statistics is included in Appendix Table 2. Panel data is data that varies both across time and across jurisdictions. A fixed effects regression adds separate controls for every time period and for every individual jurisdiction and sees whether, after controlling for these individual jurisdiction and time effects, some other characteristic was associated with crime. *See* WILLIAM H. GREENE, ECONOMETRIC ANALYSIS 303, 466 (2d ed. 1993) (describing fixed effects regressions on panel data).

14. Lott, in his book and in a variety of articles, also supports his more guns, less crime thesis with other types of evidence. For example, he collected data at the city and state levels to test whether jurisdictions of different sizes exhibited a reduction in crime when a shall-issue law was adopted. *See* LOTT, *supra* note 2, at 169, 190. Lott was also able to secure permit data from 10 states that he used to test whether counties with more concealed handgun permits have larger reductions in crime. *Id.* at 178. In addition, Lott has concluded from other work that counties located next to states passing shall-issue laws experience an increase in crime just as the passing states experience a decrease. Bronars & Lott, *supra* note 3, at 479. Although our disaggregated analysis below will cast doubt on this finding, it should still be noted that any estimate of the overall effect on crime from these laws will be biased toward making them look more beneficial than they are if they cause crime to shift from passing to nonpassing states. We do not have these additional types of data and therefore do not analyze them in this Article.

15. Ian Ayres & John J. Donohue III, *Nondiscretionary Concealed Weapons Laws: A Case Study of Statistics, Standards of Proof, and Public Policy*, 1 AM. L. & ECON. REV. 436 (1999).

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may well be the case that the scattered negative coefficients for various violent crime categories, which on their face suggest that crime decreases with passage of shall-issue laws, should be thought of as statistical artifacts.[16] While we do not want to overstate the strength of the conclusions that can be drawn from the extremely variable results emerging from the statistical analysis, if anything, there is stronger evidence for the conclusion that these laws increase crime than there is for the conclusion that they decrease it.

The remainder of this Article is divided into five Parts. Part I analyzes the theory underlying Lott's empirical project, discussing the ways in which shall-issue laws could dampen—or increase—crime. Part II delves into a host of methodological issues that a researcher must confront in estimating the impact of law on crime. Using a state dataset with several additional years of information, this section demonstrates that while the simplest regression models suggest that crime has tended to increase more when states adopt shall-issue laws, these results vary over different time periods (yielding the opposite results before crime began dropping faster in the 1990s in the nonadopting states) and are surprisingly sensitive to inclusion of seemingly extraneous right-hand side control variables. While the Lott and Mustard model based on state data gives some support for the view that shall-issue laws can lower rates of murder and rape, better models undermine this conclusion. The most robust findings from the most up-to-date state data are that various property crimes rise with passage, although the absence of a compelling theoretical justification for this result raises concerns about the predictive validity of the models. Part III turns to Lott's own county dataset to assess the extent to which the "more guns, less crime" result persists in less-constrained specifications with additional years of data. Part IV explores even less-constrained regressions, in particular estimating state-specific effects, and concludes that the core finding of more guns, less crime is reversed once the statistically preferable state-specific regression models are used. Part V discusses a hierarchy of possible conclusions to emerge from our empirical work and provides an illustration of how state-specific regression models can potentially provide more nuanced policy recommendations across states than are possible with more customary aggregated models.

16. While a primer in regression analysis is beyond the scope of this Article, a crime regression would traditionally regress a measure of crime as a left-hand side variable onto a linear combination of right-hand side attributes (called control variables) that are assumed to be exogenous influences on crime. In such a regression, a negative coefficient on a variable indicating that a jurisdiction had a shall-issue law in effect would tend to indicate that the effect of the law was associated with a reduction in crime.

As Milton Friedman stated: "I have long had relatively little faith in judging statistical results by formal tests of statistical significance. I believe that it is much more important to base conclusions on a wide range of evidence coming from different sources over a long period of time." Daniel Hamermesh, *The Craft of Labormetrics*, 53 INDUS. & LAB. REL. REV. 363, 376 n.11 (2000).

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I. THEORETICAL ISSUES CONCERNING THE EFFECT OF INCREASED CARRYING OF CONCEALED HANDGUNS

Given the massive extent of gun ownership in this country, coupled with the fact that the United States is exceptional in only one aspect of its crime problem—its high rate of lethal violence—it might at first appear that guns must be a part of the problem. But over the last decade, a number of scholars have offered theoretical and empirical support for the notion that allowing law-abiding citizens to carry concealed handguns (unobservable to potential criminals) can deter criminal behavior.[17] The theory is that criminals will be willing to arm themselves whether or not this is lawful, so that laws designed to restrain gun ownership and carrying will only serve to protect criminals, who will have a lessened fear of encountering armed resistance to their criminal designs. Allow law-abiding individuals to carry guns, so the theory goes, and the costs of engaging in criminal activity will rise, thereby dampening the amount of crime.

Lott and Mustard's empirical project is grounded in the important theoretical insight that hidden precautions by potential victims can generate powerful general deterrence effects. Visible precautions by potential victims may simply tend to displace crime toward victims who take less precaution, while unobservable precautions (silent alarms, gasoline kill switches, Lojack) make potential criminals generally more reluctant to commit crime. Thus, while the conventional wisdom focuses on the danger that more guns pose to the citizenry, the new critique emphasizes the protective effect that spills over from those who carry concealed weapons. Because criminals cannot know in advance who is armed with a concealed weapon, their risk goes up in an encounter with any potential victim. Note, then, that even though the open carrying of handguns might only divert criminals from potential victims with guns to those without them, legalizing the concealed carrying of weapons holds out at least the potential of reducing crime rather than merely shifting its incidence.

The first rejoinder to this view is that shall-issue laws allow anyone of a certain age without an officially documented problem of mental health or criminal record to secure a permit to carry a concealed weapon; this is not a particularly exacting standard. A moment's reflection on one's own acquaintances would likely suggest the names of numerous angry or intemperate individuals who could pass the "shall-issue" test even though the prospect of their carrying a concealed weapon would not be likely to enhance one's sense of personal security. Still, Lott and Mustard have a fairly good reply to this point: The number of crimes committed by those individuals who have obtained permits appears to be rather small (although it is doubtless higher

17. *See* GARY KLECK, POINT BLANK: GUNS AND VIOLENCE IN AMERICA (1991); Daniel D. Polsby, *The False Promise of Gun Control*, ATLANTIC MONTHLY, Mar. 1994, at 57.

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than official records would suggest as the identity of the perpetrator in a substantial proportion of crimes is never discovered).

But even if no one securing a concealed-carry permit ever used it to commit a crime, there are still a number of avenues by which the passage of a concealed-carry law could stimulate crime. First, even if the adoption of a shall-issue law increased the riskiness of criminal activity and thereby dampened the number of criminals, it might also *increase* the number of criminals who decided to carry weapons themselves (by hypothesis, illegally) and also might *increase* the speed at which a criminal decides to shoot or disable potential victims (as the presence of armed victims increases the cost of hesitation once a criminal engagement has been launched). Therefore, the number of murders and aggravated assaults can rise if criminals respond to shall-issue laws by packing more heat and shooting quicker. Arming the citizenry can encourage an arms race, leading more criminals to carry even higher-powered weapons and to discharge them more quickly when threatened.[18]

Second, even when no criminal act is initially contemplated, the injection of a gun into an angry dispute, perhaps in lawful defense, might escalate a minor dispute into a criminal homicide or a serious wounding.[19] As an earlier president of the Connecticut Chiefs of Police Association stated, "We are concerned about the increasing availability of handguns and the ease with which a person can get a pistol permit. . . . [A] permit is dangerous in the hands of a neophyte who goes to a bar and shows off his phallic symbol to the

18. John J. Donohue III & Steven D. Levitt, *Guns, Violence, and the Efficiency of Illegal Markets*, 88 AM. ECON. REV. 463 (1998).

19. For an illustration of this occurrence, consider the story of Skip Olson, 58, and his roommate and friend of 25 years, Michael Jurisin, 50. The two Palo Alto, California residents were fighting about rent payments when Jurisin took out a handgun. Olson grabbed Jurisin's gun and shot him in the back of the head on February 17, 1998. Olson was later convicted of second-degree murder. One suspects that if neither man had owned a gun, no one would have been murdered. Of course, one might object that this example (and the aforementioned Hattori case, *supra* note 11) show only that having a gun around can lead to needless killings, but not that concealed-carry laws lead to needless killings. Killings in the home will still occur even without shall-issue laws, since no permit is needed to have guns there in most states (although shall-issue laws may increase the attractiveness of guns and lead to more guns being in the home as well as on the highway). Perhaps a better example, then, is the recent triple murder followed by a suicide in Arizona, where a veteran with a concealed handgun permit in Arizona, who was performing poorly in a nursing program, chose to murder the teachers who gave him low grades. While one suspects that the teachers were correctly identifying the student's limited capacity for success in a healing profession, they were certainly victims of his ability to carry concealed handguns to the school. John Broder, *Arizona Gunman Chose Victims in Advance*, N.Y. TIMES, Oct. 30, 2002, at A20. Still, one suspects that the killer was the sort of person who would have had access to guns in any event, and thus, given his intent to kill, might not have been dissuaded from doing so even if Arizona law had prohibited carrying concealed weapons. Ultimately, one hopes that the statistical evidence can answer the question of which set of effects the competing anecdotes illustrate is more common.

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boys."[20] Indeed, there was a bit of a scandal in Connecticut in 1977 when it was revealed that Michael O'Brien—deemed by the federal organized crime strike force special prosecutor as one of the "two most important criminals in the Hartford area" and convicted for racketeering, extortion, and gambling—had obtained a right to carry a concealed weapon with the support of letters of recommendation from certain major political figures in the state.[21] This suggests that those who are able to secure handgun permits are not always model citizens, and that at least some criminals find it useful to have the legal right to carry weapons.

Third, with some estimates suggesting that as many as one million or more guns are stolen each year, we know that putting more guns in the hands of the law-abiding population necessarily means that more guns will end up in the hands of criminals.[22] In fact, with guns being a product that can be easily carried away and quickly sold at a relatively high fraction of the initial cost, the presence of more guns can actually serve as a stimulus to burglary and theft.[23] Even if the gun owner had a permit to carry a concealed weapon and would never use it in furtherance of a crime, is it likely that the same can be said for the burglar who steals the gun?

Fourth, allowing citizens to carry concealed weapons imposes burdens on police in that they must ascertain whether the gun is being carried legally. Officers of the Illinois State Police have indicated that their job would be complicated if private citizens were permitted to carry guns as they would need to spend time confirming whether the guns were being legally carried.[24] As it stands now in Illinois, anyone caught with a gun in public is violating state law and can be immediately brought into custody without the need for further investigation, which the state police believe has been a powerful tool for taking criminals off the streets.[25] According to James Jacobs, "[t]he possibility of ratcheting up street-level policing to seize more unlawful guns [perhaps through new technologies that can allow police to detect guns from some

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20. Lincoln Millstein, *Police Toughen Criteria for Getting Gun Permit*, HARTFORD COURANT, Jan. 15, 1978, at 1.

21. Andrew Kreig, *Pair Gets 3 to 10 Years in Prison in Racketeering Case*, HARTFORD COURANT, July 6, 1977 at 3.

22. Mark Duggan reports that nearly 500,000 guns are stolen in the United States every year. Duggan, *supra* note 3, at 1094. Others estimate the figure to be as much as three times that number.

23. *See* Philip J. Cook & Jens Ludwig, *Guns and Burglary*, *in* EVALUATING GUN POLICY: EFFECTS ON CRIME AND VIOLENCE 74, 75-76 (Philip J. Cook & Jens Ludwig eds., 2003).

24. Donohue has served as a consultant to the Illinois State Police on matters relating to a claim of racial profiling.

25. Lott also identifies another mechanism by which shall-issue laws might induce crime: They may embolden citizens to frequent high-crime areas (or enter areas at night) that would be avoided were they not carrying a gun, thereby increasing possible exposure to criminal acts. While we would lament the increased crime in this situation, the greater willingness to move about would be a benefit associated with the right to carry weapons.

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distance away] is complicated by the passage of state 'shall-issue' laws . . . ."[26] Finally, accidental deaths and suicides are obviously aided by the presence of guns, and these costs could conceivably outweigh any benefits of shall-issue laws in reducing crime.[27] Extensive empirical study is needed to assess the relative magnitudes of the likely conflicting effects of a state's decision to permit citizens to carry concealed weapons.

II. EMPIRICAL ISSUES IN ESTIMATING THE EFFECT OF INCREASED CARRYING OF CONCEALED HANDGUNS

It is probably useful to begin our empirical investigation by giving a sense of the nature of the difficulties in trying to uncover concealed-carry laws' ultimate impact on crime. To this end, look at Figure 1, which shows the pattern of robbery over the period from 1977 through 1999 in four groups of states: the four states that had shall-issue laws prior to 1977, the eight adopters between 1977 and 1989, the seventeen adopters between 1990 and 1999, and the twenty-two states (including the District of Columbia) that have never adopted a shall-issue law.[28] Robbery is a good place to start our inquiry because it is committed in public more than any other crime, and should be the crime most likely to decline if the Lott and Mustard story of deterrence has any plausibility.[29] The first thing one notices in this Figure is that there is a broadly similar pattern for the four groups (particularly for the top three state groupings): Robbery rates rose until about 1980 then dipped, began rising yet again starting in roughly the mid-1980s, followed by a decline in the 1990s. This pattern suggests that there are some factors operating across the entire nation that tend to push crime up and down in broad waves lasting from five to ten years. The second point that leaps out from the Figure is that the twenty-two states that have not adopted shall-issue laws have had much higher rates of robbery than states that allow the carrying of concealed handguns, at least until recently (more about this later). Note that this is not what Lott and Mustard mean when they suggest more guns, less crime. They realize, as sophisticated

26. JAMES JACOBS, CAN GUN CONTROL WORK? 205 (2002)

27. D. Hemenway & M. Miller, *Association of Rates of Household Handgun Ownership, Lifetime Major Depression, and Serious Suicidal Thoughts with Rates of Suicide Across U.S. Census Regions*, 8 INJURY PREVENTION 313 (2002) (concluding that higher rates of gun ownership lead to higher rates of suicide).

28. As Appendix Table 1 indicates, there has been some dispute as to whether certain states have a shall-issue law. The Figure 1 graphs are constructed using the shall-issue coding supplied by Jon Vernick and Lisa Hepburn, which we will refer to as "Vernick's coding." Jon S. Vernick & Lisa M. Hepburn, *State and Federal Gun Laws: Trends for 1970-99*, *in* EVALUATING GUN POLICY, *supra* note 23, at 345. Appendix Table 1 sets forth Vernick's coding (column D) and an alternative coding that is closer to the coding used by Lott and Mustard (column A) that we will refer to as "Lott's coding."

29. Lott and Mustard have tried to argue that because some robberies are of banks and other commercial entities that are already protected by armed guards, the predicted effect of shall-issue adoption on robbery is uncertain. We are not persuaded by this claim.

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researchers, that in 1977, the twenty-two never-adopting states had double the robbery rate of the other states for reasons having nothing to do with their lack of shall-issue laws. Indeed, only four of the other twenty-nine states allowed the carrying of concealed handguns at that time. The main story is that robbery occurs more frequently in large, densely populated urban areas. Thus, one could not hope to establish the effect of a shall-issue law by looking only at which states have such laws and which do not at any one point in time—a so-called "cross-section" analysis. Even the most zealous supporters of shall-issue laws should realize that introducing the right to carry concealed handguns could not cut the robbery rates by anything close to one-half. Clearly, other factors explain the large differences in the core rates of crime in the different sets of states.

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FIGURE 1A: ROBBERY RATES FOR STATES BY PASSAGE OF SHALL-ISSUE LAW WEIGHTED BY STATE POPULATION (VERNICK'S CODING)

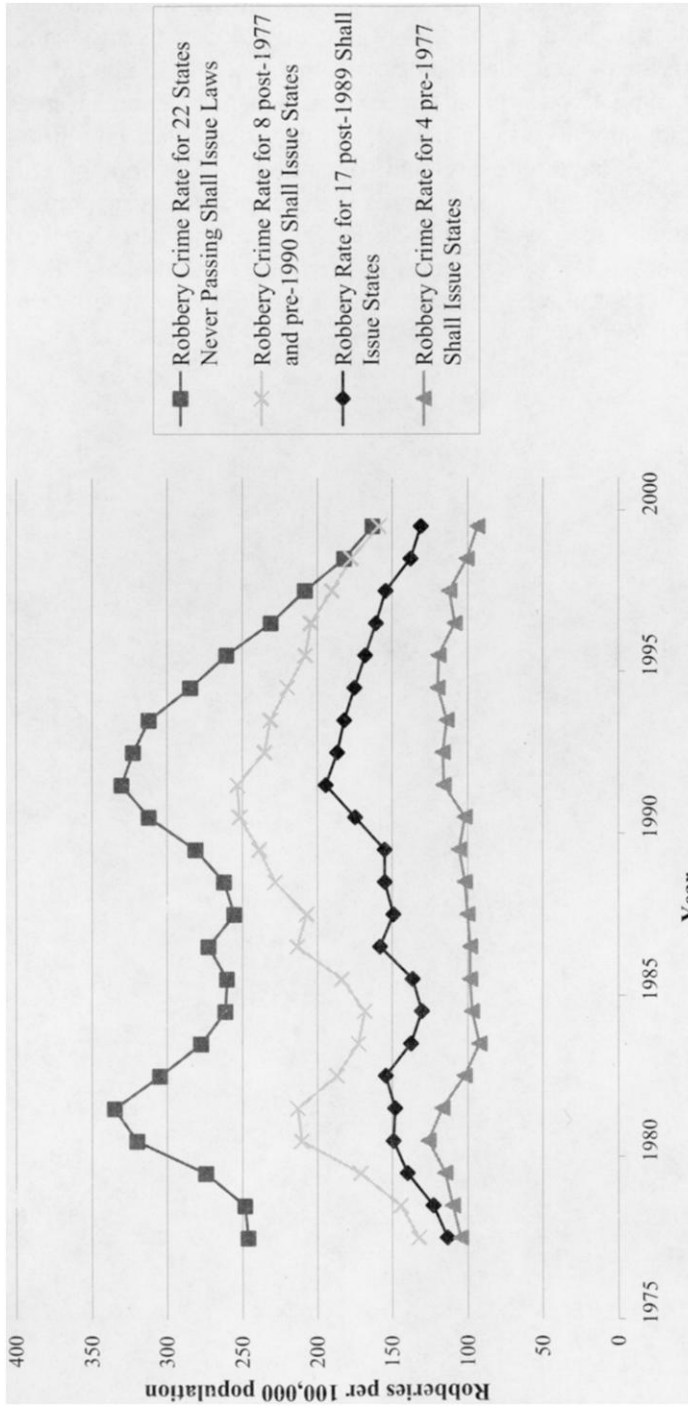

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FIGURE 1B: VIOLENT CRIME RATES FOR STATES BY PASSAGE OF SHALL-ISSUE LAW WEIGHTED BY STATE POPULATION (VERNICK'S CODING)

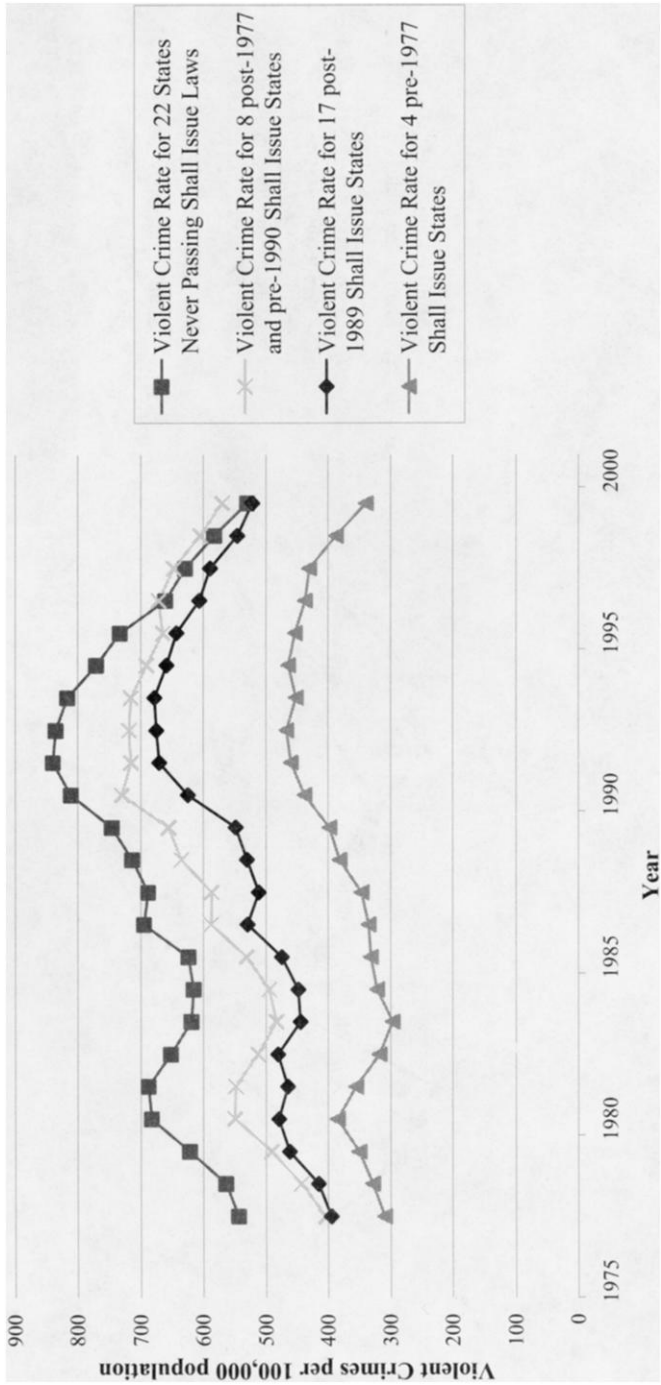

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FIGURE 1C: MURDER RATES FOR STATES BY PASSAGE OF SHALL-ISSUE LAW WEIGHTED BY STATE POPULATION (VERNICK'S CODING)

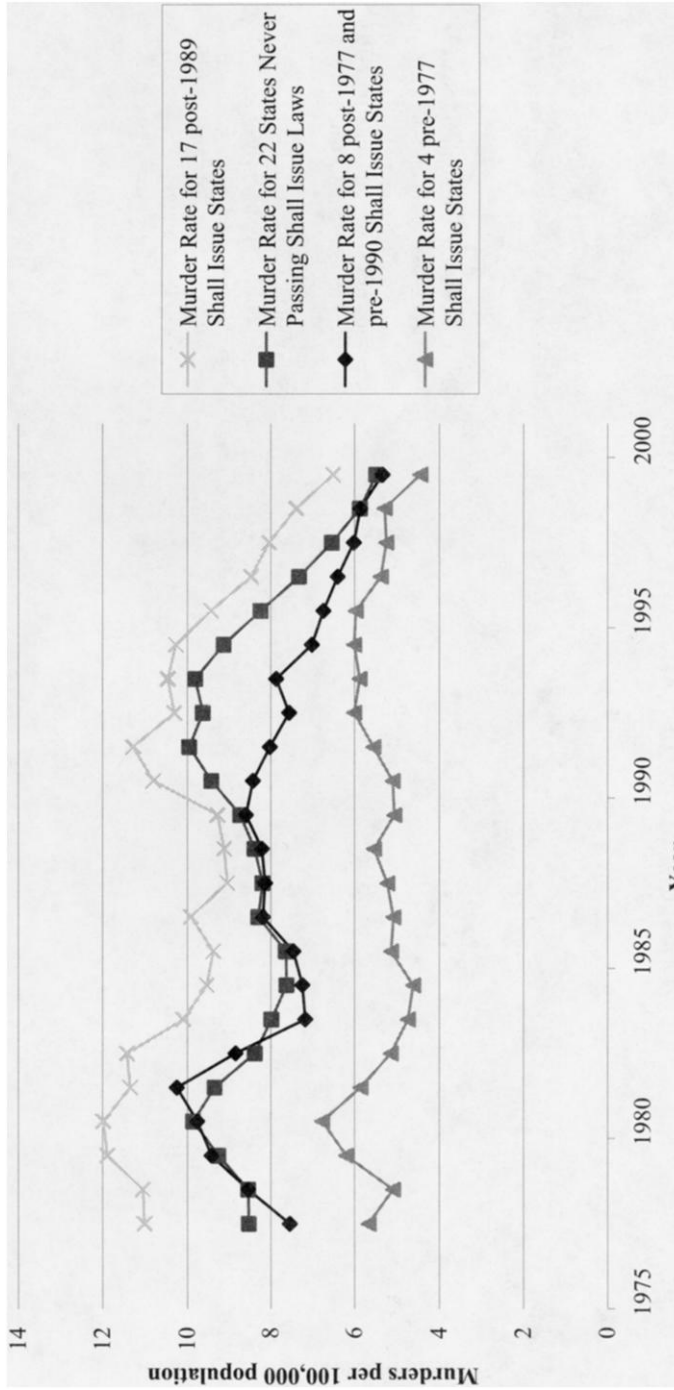

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FIGURE 1D: RAPE RATES FOR STATES BY PASSAGE OF SHALL-ISSUE LAW WEIGHTED BY STATE POPULATION (VERNICK'S CODING)

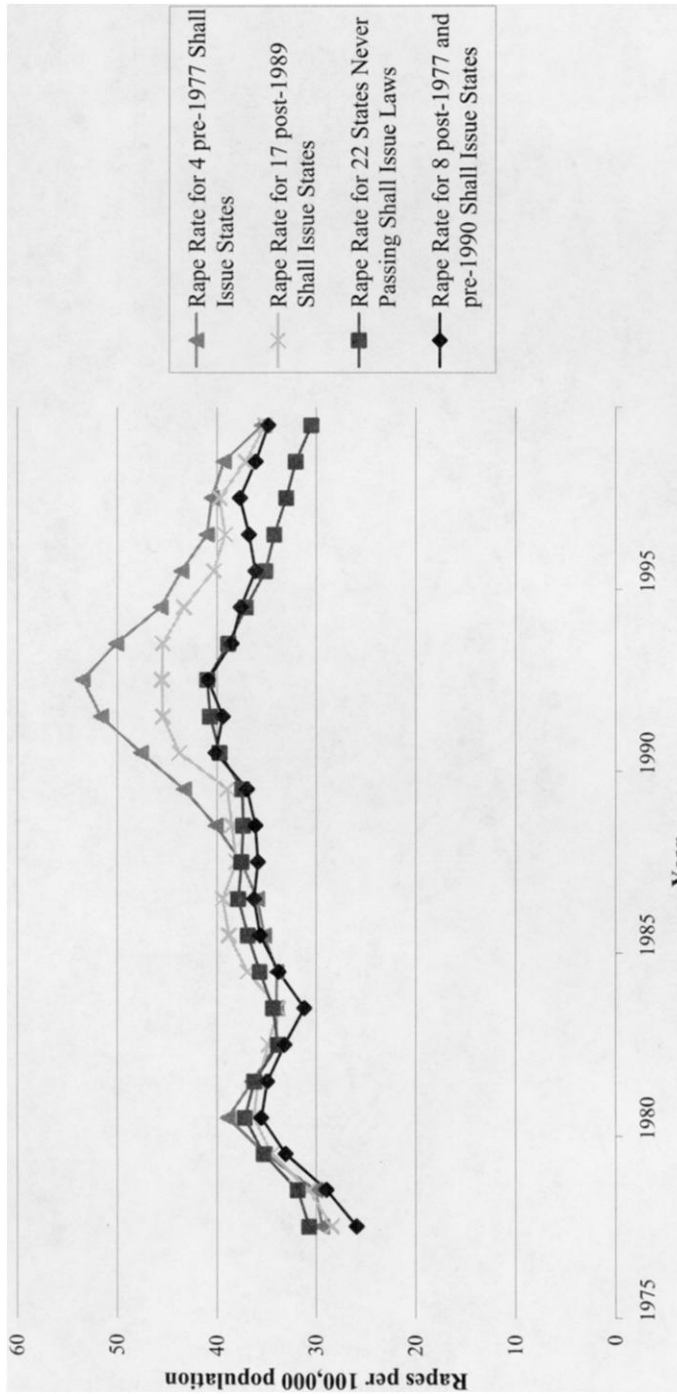

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FIGURE 1E: AGGRAVATED ASSAULT RATES FOR STATES BY PASSAGE OF SHALL-ISSUE LAW WEIGHTED BY STATE POPULATION (VERNICK'S CODING)

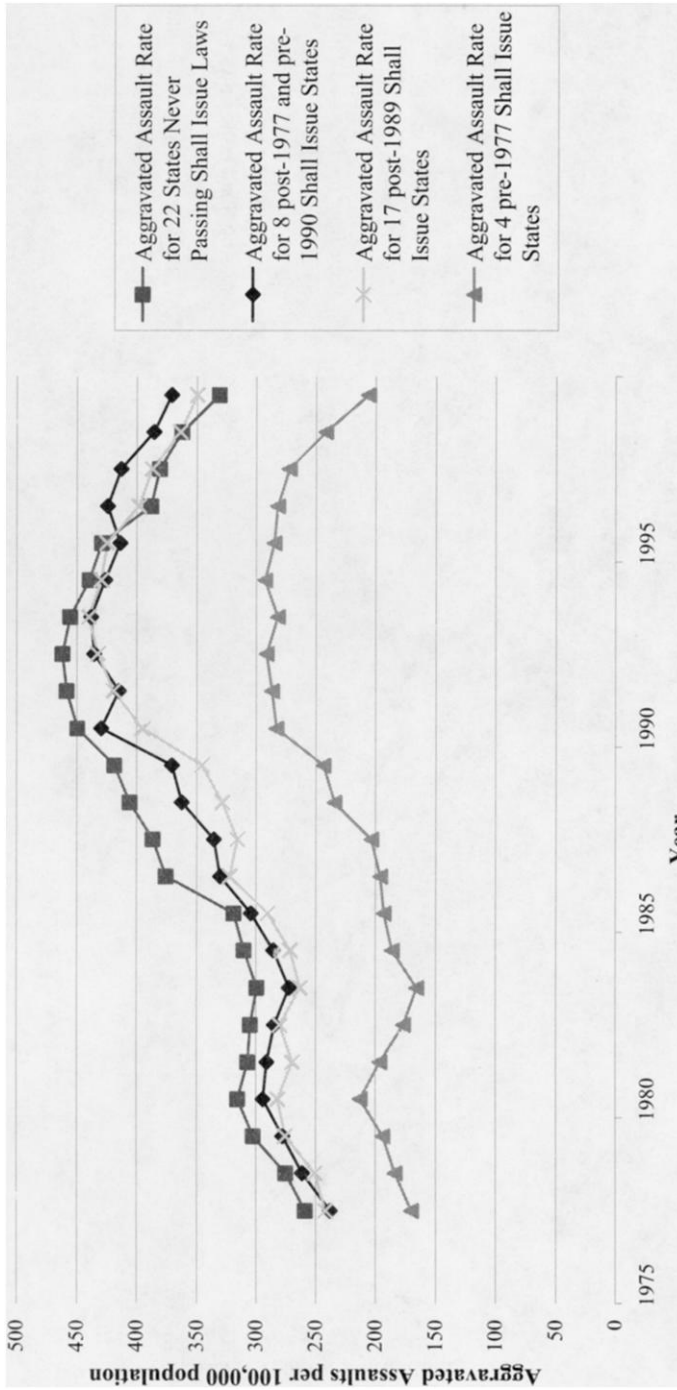

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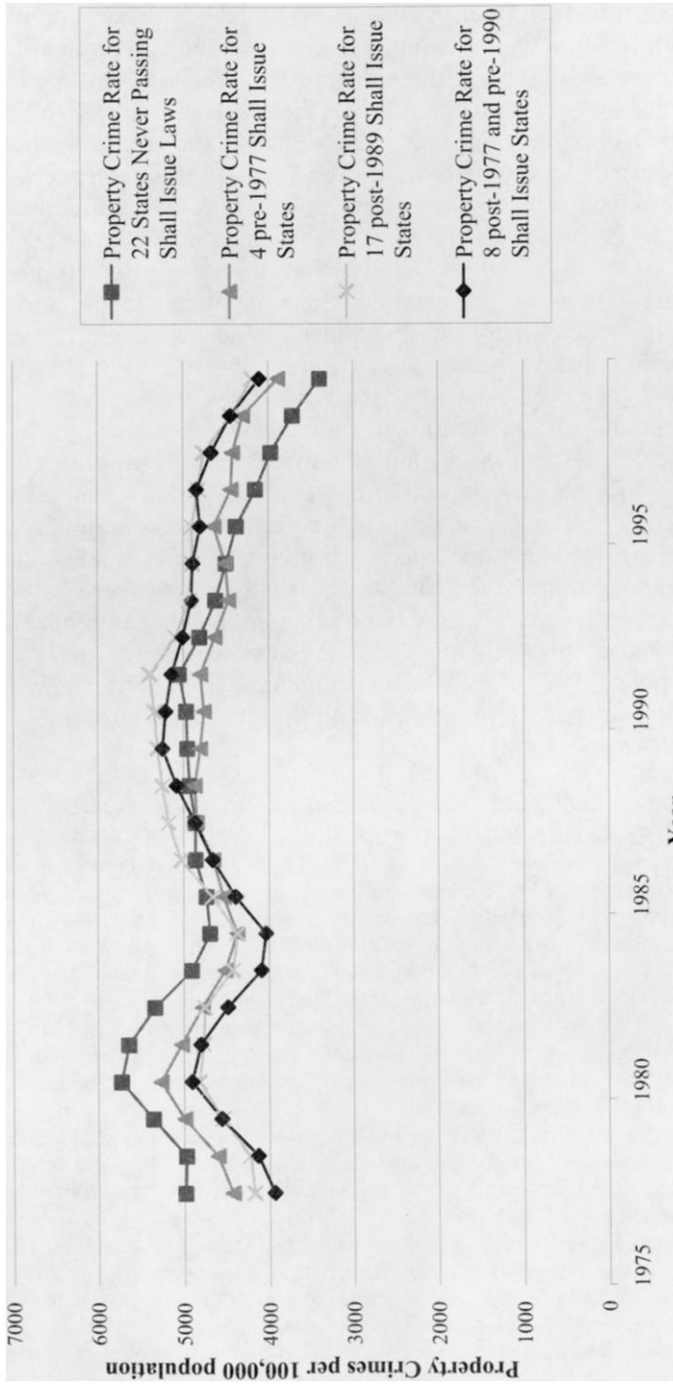
FIGURE 1F: PROPERTY CRIME RATES FOR STATES BY PASSAGE OF SHALL-ISSUE LAW WEIGHTED BY STATE POPULATION (VERNICK'S CODING)



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Given this fact, is there anything that can be said about the likely effect on robbery of adopting a concealed-carry law? Certainly, one can say nothing definitive from merely examining the graphical evidence, and we will explain why shortly. It is worth noting, however, that the only group of states to have experienced a substantial drop in robbery over this time period (albeit one punctuated by two sharp upturns) was the group of twenty-two states that never adopted shall-issue legislation. These states experienced a whopping drop in robberies of roughly one-third, a drop so large that the never-adopting states went from having by far the highest robbery rates of the four groups in 1977 to ending up in 1999 with the same robbery rate as the eight adopters over the period from 1977-1989.[30] Indeed, if one were forced to make causal attributions from this graphical data, one might conclude that shall-issue laws tend to increase robbery rates. Similar conclusions could be derived from an examination of the other crime categories depicted in Figures 1b through 1f.[31] But, there is no need to rely on visual inspection alone, since the statistical tool of regression can do more formally and precisely what the graphical analysis is trying to do—control for the initially different levels of crime and the common national forces acting on crime to see whether shall-issue adoption has any systematic effect on crime. We ran just such a regression model that controlled only for the average crime rate in the state and the common national influence each year and found that adoption of a shall-issue law was associated with an almost sixteen percent *increase* in robbery.[32] Indeed, as Table 1, line 2 shows, running these same parsimonious regressions across all nine crime categories for the period 1977-1999 yields results that, with only a single exception, are uniformly statistically significant and positive, suggesting—however naively—that shall-issue laws *increase* crime.[33]

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30. A somewhat similar pattern can be seen in Figure 1b for violent crime. While the never-adopters experienced a small *drop* in violent crime, the three other sets of shall-issue states experienced violent crime *increases* over this period, and for the adopters after 1977, those increases were sizable. Indeed, while both sets of post-1977 shall-issue adopters started out with equal levels of violent crime that were substantially below the level in the 22 never-adopting states, by 1999 the never-adopters had a violent crime rate about equal to that of the late-adopters and clearly below that of the adopters in the 1977-1989 period.

31. Consider the pattern of rape in the four state groups, shown in Figure 1d. While the never-adopting states started out with the highest level of rape among the four groups, they ended the period with the lowest level. The same pattern can be found, yet even more dramatically, for property crime.

32. The more technical description of this regression model would be a panel data model—that is, one combining crime data over a period of time with cross-section data for 50 states and the District of Columbia—controlling only for "state- and year-fixed effects." *See supra* note 13.

33. The sole exception is murder, which has a small negative but wholly statistically insignificant coefficient. The other crime rate increases estimated in Table 1, line 2 range from a low of 3.9% for aggravated assault to a high of 23.2% for auto theft. In order to conserve space, our Tables will only report coefficients of interest relating to the impact of the shall-issue law. But the interested reader can find the complete regression output for all the regressions in this Article (as well as the underlying STATA do files and datasets for independent verification) on the Internet at http://www.law.yale.edu/ayres.

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TABLE 1: THE ESTIMATED IMPACT OF SHALL-ISSUE LAWS ON CRIME
STATE DATA (VERNICK'S CODING), USING STATE AND YEAR EFFECTS ONLY

	Violent Crime	Murder	Rape	Aggravated Assault	Robbery	Property Crime	Auto Theft	Burglary	Larceny
Time Period: (1977-1992)									
1.	-3.4%	**-8.3%**	0.5%	**-9.5%**	4.8%	**7.3%**	**8.7%**	**9.4%**	**6.5%**
	(2.1%)	(3.7%)	(3.2%)	(2.2%)	(3.9%)	(1.8%)	(3.8%)	(2.8%)	(1.8%)
Time Period: (1977-1999)									
2.	**8.7%**	-1.6%	**6.2%**	**3.9%**	**15.9%**	**16.1%**	**23.2%**	**14.4%**	**16.0%**
	(1.8%)	(3.1%)	(2.4%)	(1.8%)	(2.9%)	(1.9%)	(3.1%)	(2.3%)	(1.8%)

Notes: The dependent variable is the ln(crime rate) named at the top of each column. The data set is comprised of annual state-level observations (including the District of Columbia). 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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Unfortunately, life is not so simple. First, note that the story that was just told was very sensitive to the time frame we examined. If our evaluation had been conducted using data only through 1992, which was the case for the initial Lott and Mustard paper, then we would not have seen the dramatic drop in robbery in the nonadopting states. As Table 1, line 1 shows, over the 1977-1992 period, the simple regression would have suggested that shall-issue laws were associated with statistically significant *decreases* in murders and aggravated assaults. Second, the Table 1 predictions would be valid estimates of the impact on crime of the adoption of a shall-issue law only if two conditions held: (1) each state had a fixed underlying rate of crime that only changed when some omnipresent national influence of crime pushed that rate up or down by the same amount everywhere or when a shall-issue law was adopted; and (2) the fact and timing of adoption of a shall-issue law was not influenced by the current trend or level of crime in a state.[34]

Figure 1 provides some visual support for part of the first condition in that one can discern the presence of strong influences on crime that seem to operate across all states.[35] But the second part of the first condition—the claim that the only additional statewide influence on crime beyond the fixed state effect and the national year effect is the adoption of a shall-issue law—is obviously heroic. Some of the major measurable influences on crime, such as economic conditions, poverty rates, police activities, rates of incarceration, or demographics, may vary across states. For example, some states may decide to get tougher on crime by increasing their rates of incarceration faster than other states. If such factors *both* change in different ways across states *and* influence crime significantly, then the claim that the only changes in crime are caused by uniform national influences or the shall-issue legislation is undermined. More complete regression models will be used below to try to control for these other plausible influences on crime, and to the extent that this effort is successful, the regressions will (hopefully) succeed where reliance on graphs could fail. But note that the correctness of the regression results will be imperiled if there are influences—such as, say, the criminogenic influence of the introduction of crack cocaine—that impact differentially across states and are not controlled for in the regression model, yet are correlated with the adoption of a shall-issue law.

34. As those familiar with tests of the effectiveness of medicinal drugs or medical interventions know, the gold standard for ascertaining the effect of a treatment is to randomize individuals into treatment and control groups and observe if there are statistically significant differences in outcomes across the two groups. In the medical context, the benefits of this approach would be lost if individuals were free to chose whether to take the treatment. Similarly, in our evaluation of the impact of a legal change, the regression results can be misleading when legislatures are free to enact or reject the shall-issue law instead of having the law randomly assigned to different states. This issue, referred to as the problem of endogenous state adoption, is addressed at greater length in Part II.E, *infra*.

35. The pattern is very clear for robbery and violent crime. *See* figs. 1a, 1b.

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One can get a glimpse of how this problem of omitted variable bias might be operating in this context by examining Figure 1c, which shows the pattern of murder rates for the four state groupings. Note that murder rates spiked into the early 1990s in the two top groups (which either never adopted a shall-issue law or adopted one in the 1990s) but did not show the same increase in the group of states that adopted shall-issue laws in the 1977-1989 period. The Table 1 regressions would attribute the greater run-up in murder starting in the late 1980s to the failure of the top two groups to adopt shall-issue laws—hence Table 1, line 1 found the shall-issue laws to be associated with a substantial 8.3% *decrease* in murder over the period of 1977-1992. But an alternative explanation is that the crack cocaine problem[36] drove up crime in certain highly urban areas in states that were reluctant to enact a shall-issue statute while largely bypassing more rural states that were adopting such laws. If that were the case, the regression would identify a relationship between higher crime and the failure to adopt a shall-issue law when the real cause would have been the influence of crack, for which the regression failed to control.[37]

The second issue relevant to estimating the effect of a shall-issue law is whether the law can be thought of as being adopted as a random treatment—that is, the law is an exogenous event—rather than being part of the endogenous system that influences crime and the various responses to crime. In other words, if spikes in crime lead to demands for the right to carry handguns (which turns out to be the case), then adoptions of shall-issue laws are not exogenous but are endogenous to the variable that we are trying to explain, which is crime itself. In this event, the regression might attribute subsequent drops in crime, even if merely reflecting a return to more normal rates of crime, to the passage of the law.[38] This endogeneity problem is a vexing one in all studies examining the impact of a legal change, and perhaps particularly so when examining a law's impact on crime (because politicians like to be seen as "solving" crime problems by passing laws). We will also try to address this issue—admittedly with only limited success—later in this Article.

A. *Introducing Explanatory Variables into the Panel Data Model*

Putting aside the endogeneity problem for now, we begin our exploration of the panel data regressions with the finding that shall-issue laws seem to increase crime—although, as Table 1 revealed, we might have thought differently about the impact on murder and aggravated assault if we had had to

36. *See* THE CRIME DROP IN AMERICA (Alfred Blumstein & Joel Wallman eds., 2000).

37. Lott's control for state population density (population per square mile), shown in Appendix Table 2, will not likely capture the full effects of the influence of crack on crime across states.

38. The "return to more normal rates of crime" after a sudden upturn is referred to as regression to the mean.

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rely on data ending in 1992 rather than 1999. But before we can repose confidence in conclusions from graphical evidence or the parsimonious regressions of Table 1, we must probe whether introducing controls for other possible influences on crime might reverse this initial conclusion. For this to happen, the omitted variables would have to be correlated with both the passage of shall-issue laws as well as with higher crime. Before turning to these results, we must first address a few details about the nature of the regression models (specification) and the explanatory (or control) variables.

1. *Model specification.*

Although the regression model employed by Lott and Mustard is for a panel dataset with both state (or county) and year fixed effects, even within this structure, a number of decisions need to be made concerning how one models the impact of the law. In trying to determine the impact of the passage of shall-issue laws, one would ordinarily begin by asking whether crime was on average higher or lower after the law went into effect, controlling for all the explanatory variables that are thought to have an impact on crime in the state. This is the so-called "dummy variable" model, which posits that the law will have a fixed (once-and-for-all) percentage impact on crime—that is, the law will raise or lower crime by, say, five percent. Lott and Mustard begin their analysis with this dummy variable model but also explore a second specification in which they attempt to estimate whether the passage of the law will cause a break in the time path of crime, causing it to tip up or down depending on whether the law raises or lowers crime. In conducting this trend (or "spline") analysis, Lott and Mustard estimate what the average linear time trend of crime is before the law passes, and then probe whether this trend changes after passage.[39]

The dummy variable model. If Lott and Mustard were correct that the passage of a shall-issue law reduces crime, one might see a sudden and persistent drop in crime that would be captured by a postpassage dummy in the panel data regression. Since typically the dependent variable in these regressions will be the natural log of the crime rate, the coefficient on the postpassage dummy variable can be interpreted as the percentage change in crime associated with the adoption of the law.[40] Lott interprets negative coefficients on the postpassage dummy variable to imply that prospective criminals anticipate the dangers they would face in trying to prey upon a more armed population and that they would reduce their criminality in response. Of course, one way that prospective criminals could reduce any increased personal

39. Instead of reporting regression coefficients, with regard to his linear specification, Lott only reports the change in the before and after linear trends, *see* Lott's original table 4.8, and with regard to quadratic terms in the time trend, he graphs the before and after quadratic effects, *see* Lott's original figures 4.5-4.9.

40. Table 1 estimates are from just such a model (albeit controlling only for state and year fixed effects) and can thus be interpreted in this manner.

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risk to them from such a law would be to continue their life of crime in a more hospitable jurisdiction (presumably one without a shall-issue law).[41]

The Lott spline model. Again assuming Lott and Mustard are correct that the shall-issue laws reduce crime, one might observe crime rates falling as individuals applied to the relevant state officials to secure the right to carry concealed weapons and then in fact began carrying them. Since this process would unfold gradually, one might expect to see a gradual and continuing decrease in crime—at least until the increase in the number of citizens carrying lawful concealed weapons came to an end. In this model (the trend or "Lott spline" model), a time trend would emerge after passage reflecting a dampening effect on crime that grew as the number of concealed handguns being carried increased.

In response to criticisms about the robustness of the dummy variable (or "static") specification results,[42] Lott has correctly noted that, as a theoretical matter, shall-issue laws could still dampen the trend of crime without showing any effect in the simple dummy regressions if crime were to follow an "inverted V" pattern. For example, Figure 2 depicts a case in which the crime rate increased for five years in a state before the law's passage and then symmetrically declined in the five years after the law's passage. In this situation, the static regression would estimate no systematic impact of the law (as depicted by the constant estimated horizontal lines before and after passage) even though there is a large change in the before and after slopes.[43]

Lott argues that the "inverted V" theory is borne out in the data, and, indeed, the graphs in his book of the trends in crime for the states adopting shall-issue laws appear to show that the linear trends in robbery and other crime categories were increasing prior to passage of the laws and fell thereafter. But to sort out whether the dummy variable model, the linear trend model, or any other model can support Lott's story that shall-issue laws *caused* a downward shift in the trend of violent crime, we examine Lott's estimated static and trend effects using more data (extending Lott's dataset through 1999 for our state data analysis and through 1997 for our county data analysis) and less-constrained econometric specifications.

---

41. Note that the simple panel data results cannot distinguish between an effect of a shall-issue law that reduces crime overall versus one which only shifts it to another jurisdiction. Obviously, crime transfers are much less desirable from a policy perspective than crime reductions. Bronars & Lott, *supra* note 3, claim to have found such geographic substitution, but our disaggregated analysis below casts doubt on their findings.

42. *See* Ayres & Donohue, *supra* note 15.

43. *Id.* at 445. Note that Figure 2 defines the "true impact" of the law by the difference between the prepassage linear trend and the observed postpassage time path of crime. Note the need for assumptions about the unobserved counterfactual—what would have happened had the law not been passed. *See* John J. Donohue III, *The Search for Truth: In Appreciation of James J. Heckman*, 27 LAW & SOC. INQUIRY 23 (2002).

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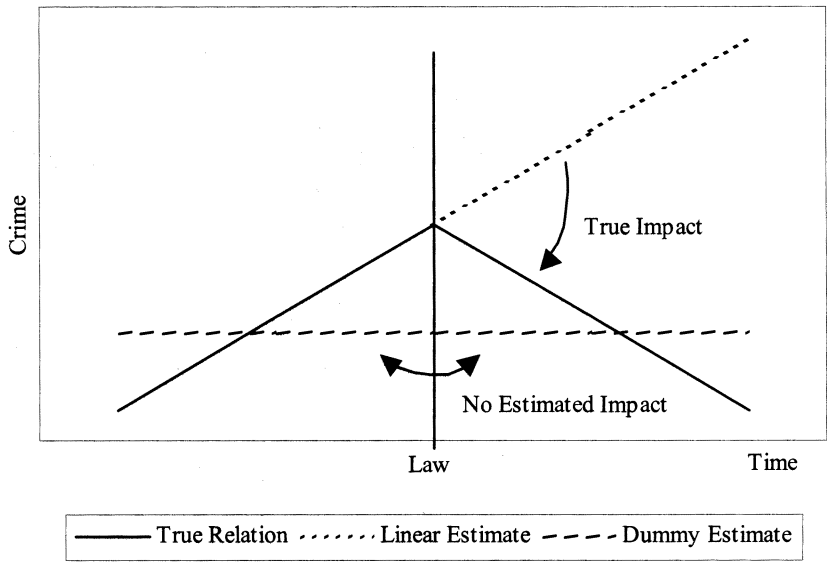
FIGURE 2: HOW DIFFERENT MODELS ESTIMATE THE LAW'S EFFECT WHEN THE PATH OF CRIME IS AN INVERTED V



The hybrid (or main effect plus trend) model. While Lott and Mustard employ both of the preceding models, a third, more general model is actually a hybrid of the preceding two in that it allows a postpassage dummy to capture the main effect of the law but also allows the law to change the linear trend in crime for adopting states. This model could be important if an "announcement effect" initially scares some criminals into fearing possible victim or bystander retaliation but that the ultimate effect is that more guns leads to more serious criminal acts—perhaps as fist fights end with someone dead or seriously injured instead of with a bloodied nose.[44] Under this scenario, one might even see an initial drop in crime followed by a subsequent turn around as the number of concealed guns being carried and crime increase in tandem. Note that although Lott does not employ this model,[45] it can be used to test whether one or both of the first two models are appropriate.[46]

44. Zimring and Hawkins first characterized the announcement and the increased-risk effects in 1997. *See* Zimring & Hawkins *supra* note 3.

45. Lott, however, did use the hybrid specification in his analysis of geographic substitution. *See* Bronars & Lott, *supra* note 3, at 250.

46. 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.

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The hybrid model will generate two estimated effects that could either be reinforcing (both the dummy and trend have the same sign), or in conflict (in that one effect is positive and the other is negative). We will later see empirical evidence suggesting that regression results indicating that the main effect of the law is pernicious while the trend effect is benign are likely generated by some model misspecification rather than evidence that the law actually generated this pattern.[47] Lott does suggest a way in which a pernicious main effect could be followed by a benign long-term trend effect, but this argument is unconvincing. In discussing his findings that public shootings increase for a few years after passage of nondiscretionary handgun laws, Lott suggests that there might have been a type of temporal substitution: People planning such shootings might "do them sooner than they otherwise would have, before too many citizens acquire concealed-handgun permits."[48] But we find temporal substitution to be an unlikely explanation for the aggregate behavior of violent criminals.[49]

The opposite pattern—a negative dummy and a positive time trend—could occur in a number of ways. For example, this pattern would emerge if the announcement effect dampens crime more powerfully than any effect initially generated by the actual higher risk to the potential lawbreakers of confronting an armed citizen, but over time this effect is overwhelmed by the stimulus to crime that greater gun prevalence generates.[50] Similarly, this pattern would be seen if an initial announcement effect led to a drop in crime, followed by a return to previous levels of crime as the salience of the new law recedes from the consciousness of criminals. The Lott spline model, in contrast, essentially posits no announcement effect and assumes a gradually growing risk to lawbreakers generates a reduction in crime (the Lott thesis) or that the gradual pernicious influence of more guns in the hands of hotheads or criminals leads to more shootings and opportunistic criminal acts. Of course, if both influences are operating—an announcement effect dampening or increasing crime and an increased risk effect (reducing crime if the risk is to lawbreakers and increasing

47. To foreshadow the later empirical evidence, the problem is revealed in Figure 3a, in which one sees that the effect of the shall-issue law is completely absent until after the tenth year (when many late-adopting states drop out of the analysis). At that point, the regression perceives that crime has dropped sharply (although this is a pure selection effect). The result is that the regression line has to tilt sharply down, and this downward tilt is accommodated through an ostensible jump upwards in the main effect.

48. LOTT, *supra* note 2, at 102.

49. We can conceive of a more plausible potential theoretical mechanism for generating a positive main effect and a negative trend: In the wake of passage of a shall-issue law the increased gun carrying generates more harmful, offensive uses of the these weapons but that over time criminals are deterred by the potential defensive uses of guns.

50. As noted above, *see supra* note 22, a very large number of guns are stolen each year. If shall-issue laws encourage more lawful gun ownership, they probably lead to an increase in the number of guns that are stolen. Note that these thefts could undo any initial benign effect of the law in a way that leads to a negative postpassage dummy (as crime initially dropped) and a positive time trend (in light of the increased arming of criminals).

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it if the risk is to the public)—then the Lott spline model would be misspecified.

2. *Control variables.*

Lott tries to control for an array of measurable factors—listed in Appendix Table 2a—that might influence crime rates, such as the arrest rate for a particular crime category in a state or county, the level of income, and various demographic measures. Since many of these variables make little difference to the analysis, it is not worth arguing about whether they should be included or not.[51] A potentially problematic variable employed by Lott and Mustard is the arrest rate, which is used to capture any changing deterrence that might be resulting from alterations in the intensity or effectiveness of police or prosecutorial resources. Reliance on the arrest rate (measured as the ratio of total arrests for any particular crime to the total number of occurrences of that crime) can lead to a large amount of data being excluded because of the realization of the dependent variable, which is always problematic.[52] Accordingly, we examined the sensitivity of the Lott results to using the state incarceration rate as an alternative and, in some ways, preferable measure of the likelihood of punishment.[53] To dampen problems of endogeneity, we used

51. A minor variable that attempts to measure the amount of income maintenance per capita given to those over age 65 under certain social welfare programs is problematic in that the reason for inclusion is uncertain and attempts to replicate and extend this data beyond 1992 were unsuccessful. Since its inclusion or omission matters little to the analysis before 1992, we just excluded it in all reported regressions.

52. In these regressions, the dependent (or left-hand side) variable that the regressions are trying to predict is the crime rate. But if the crime rate is zero, Lott's methodology causes the observation to be dropped from the sample.

Mustard has argued that this exclusion, based on the undefined arrest rate in zero crime counties, is actually a good thing: "Omitting arrest rates may generate a truncation problem because many counties with zero crime rates will be included in the regression. By construction it is impossible for a shall-issue law to reduce crime in a county that has no crime, no matter how effective is the law." David B. Mustard, *Comment* to John J. Donohue, *The Impact of Concealed-Carry Laws*, *in* EVALUATING GUN POLICY, *supra* note 23, at 325, 328. We disagree with this point. If a county has no crime, then adopting a shall-issue law can have no crime-reducing benefit. If one is interested in ascertaining what the average impact on crime is from the adoption of the law, one needs to include all jurisdictions, including those that derive no benefit. To take the extreme case, assume that the vast majority of counties had no crime, and a few had substantial crime that dropped by 10% when a shall-issue law was adopted (while by assumption there was no benefit when such laws were adopted in the zero crime areas). Lott and Mustard would argue that shall-issue laws cause crime to fall by 10%, even though the true drop in crime in all the adopting areas would only be a miniscule fraction of that figure.

53. The disadvantage of the incarceration rate data is that it is only available on a statewide (as opposed to countywide) basis, and therefore may be less useful in the county data analysis. In part for this reason, we primarily rely on the incarceration rate when conducting analyses on state data and the arrest rate when conducting analyses on county data.

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the lagged value of the incarceration rate.[54] But whether one uses arrest rate or incarceration rate, one must acknowledge the possible problem that instead of being a truly independent variable, either of these measures will in fact be influenced by the trends in crime. Therefore, we also estimated models in which both of these explanatory variables are omitted as not being truly exogenous controls.[55]

B. *Comparing the Results Using the Zheng and Lott Controls*

Our parsimonious Table 1 regressions for the period 1977-1999 confirmed what the graphs of Figure 1 had shown—that shall-issue laws were associated with statistically significant higher crime rates in eight of the nine crime categories. We can now enrich the simple model by adding controls for other factors that might be influencing crime over this period. There is a vast array of different variables that might be entered into a crime equation, and we begin by using the specification employed by a gifted researcher at Stanford, Wentong Zheng, who has been working with state crime data to ascertain the impact of state lotteries on crime. Zheng generously shared his dataset and programs and coded in the states that adopted shall-issue laws. The resulting regressions control for the following state-wide attributes: population, the percentage of black population; the percentage of metropolitan population; the percentage of population aged fifteen to seventeen, eighteen to twenty-four, and twenty-five to thirty-four respectively; per capita income; unemployment rate; the percentage of population under the poverty line; number of prisoners in state prisons (lagged one year); number of sworn police officers (once-lagged); and per capita alcohol consumption.[56] While one might argue that some different explanatory variables should be included or that the included ones should be treated differently, the advantage of using this specification is that it was adopted by a different researcher for a different crime paper and therefore it is immune to the argument that the explanatory variables were chosen to influence the estimated effect of shall-issue laws.

The results of this analysis are shown in Table 2a. Let's begin with line 1, which is the "dummy variable model," in which the regression estimates the change in crime from the prepassage to the postpassage period while controlling for the influence of Zheng's explanatory variables. Comparing the results in line 1 to the simple regressions of Table 1, line 2, we see that the general story that, over the 1977-1999 time period, shall-issue laws are

54. Lagging of a right-hand side variable is a standard econometric technique that attempts to make the variable more exogenous to the determination of the dependent variable (in this case, the crime rate).

55. In general, as indicated in our discussions of Tables 5 and 6, below, the only explanatory variables that tended to have a large impact on the estimated effect of shall-issue laws were the demographic variables.

56. Summary statistics for these variables are presented in Appendix Table 2b.

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associated with *higher* rates of crime comes through, although less powerfully than before and primarily for robbery and property crimes.[57] While before, eight of nine estimates were positive and statistically significant, now seven of nine are positive, of which only five are statistically significant. The two negative effects—for murder and rape—are statistically insignificant.

Lines 2 and 3 of Table 2a provide alternative spline and hybrid methods (discussed above) for estimating the effect of the shall-issue laws by testing whether there is any break in the trend of crime (line 2) or allowing both a change in the level as well as a change in the trend in crime (line 3). The line 2 results essentially wipe out any effect, leaving only one statistically significant variable—robbery, which switched in sign from positive to negative in going from line 1 to line 2. When both effects are estimated in the line 3 hybrid model, they become somewhat anomalous, showing a positive main effect in most of the crime categories, but some negative trend effects.[58]

Table 2b adopts the same estimation approach as in Table 2a, with one change. Since the panel data model is essentially a comparison of the period before and after adoption of the shall-issue law, there is some danger that if crime is trending up (or down), the Table 2a regressions might erroneously attribute a preexisting trend to the adoption of a law. Consequently, the Table 2b results control for any preexisting state trends. The results are similar to those in Table 2a, but the anomalous pattern of strong positive main effects and negative trend effects in the hybrid model is eliminated. Importantly, the robbery effect comes through as positive and statistically significant in both the dummy variable model and the hybrid model when one controls for state fixed trends.

57. The results shown in Table 2 use Vernick's coding. Wentong Zheng also ran similar regressions in which he used Lott's coding for the period of his original dataset (1977-1992) and Vernick's coding for 1993-1999. This different coding led to similar results, except that murder became significant and negative.

58. We previously indicated that such a pattern was likely to be a sign of model misspecification. *See supra* text accompanying note 47.

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TABLE 2A: THE ESTIMATED IMPACT OF SHALL-ISSUE LAWS ON CRIME
ZHENG'S STATE DATA, 1977-1999 (VERNICK'S CODING)

Time Period (1977-1999)	Violent Crime	Murder	Rape	Aggravated Assault	Robbery	Property Crime	Auto Theft	Burglary	Larceny
1. Dummy variable model:	2.3%	-1.5%	-1.7%	0.1%	**6.1%**	**8.2%**	**15.1%**	**5.0%**	**8.3%**
	(1.6%)	(2.2%)	(1.6%)	(1.9%)	(2.2%)	(1.2%)	(2.2%)	(1.5%)	(1.2%)
2. Lott spline model:	-0.6%	-0.8%	0.6%	-0.6%	**-2.0%**	-0.1%	-0.7%	-0.6%	0.1%
	(0.4%)	(0.5%)	(0.4%)	(0.5%)	(0.5%)	(0.3%)	(0.5%)	(0.3%)	(0.3%)
3. Hybrid model:									
Postpassage dummy	2.4%	**6.4%**	**-5.6%**	0.0%	**7.8%**	**4.3%**	**11.1%**	**4.2%**	**3.0%**
	(1.8%)	(2.5%)	(2.2%)	(2.3%)	(2.8%)	(1.4%)	(2.8%)	(1.9%)	(1.4%)
Trend effect	-0.8%	**-1.2%**	**1.0%**	-0.6%	**-2.5%**	-0.4%	**-1.5%**	**-0.9%**	-0.1%
	(0.4%)	(0.6%)	(0.4%)	(0.5%)	(0.6%)	(0.3%)	(0.5%)	(0.3%)	(0.3%)

Notes: The dependent variable is the ln(crime rate) named at the top of each column. The data set is comprised of annual state-level observations (excluding the District of Columbia). State- and year- fixed effects are included in all specifications. All regressions are weighted by state population. Standard errors (in parentheses) are computed using the Huber-White robust estimate of variance. Coefficients that are significant at the .10 level are underlined. Coefficients that are significant at the .05 level are displayed in bold. Coefficients that are significant at the .01 level are both underlined and displayed in bold. We thank Wentong Zheng for generously sharing his data set, model specification, and do-files.

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TABLE 2B: THE ESTIMATED IMPACT OF SHALL-ISSUE LAWS ON CRIME CONTROLLING FOR STATE TRENDS
ZHENG'S STATE DATA, 1977-1999 (VERNICK'S CODING)

Time Period (1977-1999)	Violent Crime	Murder	Rape	Aggravated Assault	Robbery	Property Crime	Auto Theft	Burglary	Larceny
1. Dummy variable model:	1.2%	2.5%	**-3.0%**	-0.9%	**5.1%**	**2.2%**	**4.4%**	1.6%	2.0%
	(1.4%)	(2.0%)	(1.4%)	(1.7%)	(2.3%)	(1.1%)	(2.0%)	(1.6%)	(1.1%)
2. Hybrid model:									
Postpassage dummy	-1.3%	1.9%	**-4.7%**	**-4.9%**	**5.5%**	1.7%	**4.8%**	2.3%	0.9%
	(1.5%)	(2.1%)	(1.5%)	(1.8%)	(2.4%)	(1.1%)	(2.0%)	(1.5%)	(1.1%)
Trend effect	**2.3%**	0.6%	**1.6%**	**3.8%**	-0.3%	0.5%	-0.4%	-0.7%	**1.0%**
	(0.5%)	(0.7%)	(0.4%)	(0.5%)	(0.8%)	(0.3%)	(0.6%)	(0.4%)	(0.4%)

Notes: The dependent variable is the ln(crime rate) named at the top of each column. The data set is comprised of annual state-level observations (excluding the District of Columbia). State- and year- fixed effects are included in all specifications. All regressions are weighted by state population. Standard errors (in parentheses) are computed using the Huber-White robust estimate of variance. Coefficients that are significant at the .10 level are underlined. Coefficients that are significant at the .05 level are displayed in bold. Coefficients that are significant at the .01 level are both underlined and displayed in bold. We thank Wentong Zheng for generously sharing his data set, model specification, and do-files.

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So while our simplest Table 1 regressions (with controls for only state and year fixed effects) suggested that shall-issue laws substantially increase crime, moving to a full regression model in Table 2, we see that the results become less stark. Adding Wentong Zheng's set of explanatory variables has generally maintained the story that robbery and property crime rates are higher with adoption of shall-issue laws, while the results for the four other violent crimes are more mixed across the five different models shown. If one adopts the view that the results should be robust across both Tables 2a and 2b before they can be accepted, one would probably conclude that shall-issue adoption *may* be associated with lower amounts of rape—the hybrid results with state trends are ambiguous—but there is no consistent evidence of lower crime in any other crime category. At the same time, there is more robust statistical evidence of higher property crime, although the theoretical rationale for such an effect is obscure.

It is now worth exploring whether the Lott and Mustard models (which differ from Zheng's only in terms of the explanatory variables used) will generate similar results. Therefore, we ran our version of the Lott model (which includes a host of additional right-hand side variables, using incarceration rates instead of arrest rates) on this same expanded state dataset covering the years 1977-1999. Somewhat surprisingly, the results (shown in Table 3) appear far stronger for the Lott and Mustard thesis than those depicted in Table 2.[59] For example, while the dummy variable model of Table 2 showed five statistically significant positive variables (and none that were negative), the same model in Table 3 has two positive and two negative estimates that are statistically significant. Indeed, in the hybrid results of Table 3 almost all of the positive and significant coefficients from Table 2 disappear. Again, using the standard of robustness across models, the strongest results are that rape goes down and larceny goes up with the adoption of shall-issue laws. Despite the anomaly of the initial positive effect followed by a downward trend in the hybrid model of Table 3b, the Lott model also suggests that murder falls with shall-issue adoption. While Table 3 provides the first support for some drops in violent crime (for rape and murder), it still gives no consistent evidence of any decline in robbery, which is the one area in which one would most expect the deterrent impact proposed by Lott and Mustard to show up if it were in fact important.

59. To ensure that the different shall-issue law coding schemes mentioned in footnote 24 are not driving the results, Appendix Table 3 replicates the Table 3 results using Lott's shall-issue law coding. The results are substantially similar.

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TABLE 3A: THE ESTIMATED IMPACT OF SHALL-ISSUE LAWS ON CRIME
STATE DATA, 1977-1999 (VERNICK'S CODING), USING INCARCERATION RATES

Time Period (1977-1999)	Violent Crime	Murder	Rape	Aggravated Assault	Robbery	Property Crime	Auto Theft	Burglary	Larceny
1. Dummy variable model:	**-3.4%**	-1.7%	**-7.2%**	-4.1%	-1.2%	1.8%	**4.5%**	-2.1%	**2.8%**
	(1.6%)	(2.0%)	(1.6%)	(2.1%)	(2.1%)	(1.0%)	(1.9%)	(1.4%)	(1.0%)
2. Lott spline model:	-0.5%	**-1.0%**	**-0.8%**	-0.7%	**-1.3%**	0.2%	0.3%	-0.1%	0.1%
	(0.4%)	(0.5%)	(0.4%)	(0.5%)	(0.5%)	(0.2%)	(0.4%)	(0.3%)	(0.2%)
3. Hybrid model:									
Postpassage dummy	-2.7%	1.5%	**-6.9%**	-4.4%	3.3%	1.3%	3.5%	-0.2%	1.3%
	(1.8%)	(2.3%)	(2.1%)	(2.3%)	(2.5%)	(1.2%)	(2.4%)	(1.7%)	(1.2%)
Trend effect	-0.4%	**-1.0%**	-0.4%	-0.4%	**-1.5%**	0.1%	0.0%	-0.1%	0.1%
	(0.4%)	(0.5%)	(0.4%)	(0.5%)	(0.5%)	(0.2%)	(0.4%)	(0.3%)	(0.2%)

Notes: The dependent variable is the ln(crime rate) named at the top of each column. The data set is comprised of annual state-level observations (including the District of Columbia). 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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TABLE 3B: THE ESTIMATED IMPACT OF SHALL-ISSUE LAWS ON CRIME CONTROLLING FOR STATE TRENDS
STATE DATA, 1977-1999 (VERNICK'S CODING), USING INCARCERATION RATES

Time Period (1977-1999)	Violent Crime	Murder	Rape	Aggravated Assault	Robbery	Property Crime	Auto Theft	Burglary	Larceny
1. Dummy variable model:	-1.7%	2.4%	**-4.5%**	**-4.6%**	4.3%	**2.7%**	2.4%	2.1%	**2.9%**
	(1.5%)	(2.3%)	(1.6%)	(1.8%)	(2.4%)	(1.2%)	(2.1%)	(1.5%)	(1.1%)
2. Hybrid model:									
Postpassage dummy	**-4.3%**	**5.4%**	**-5.4%**	**-8.6%**	4.8%	**2.8%**	**4.5%**	2.2%	**2.8%**
	(1.7%)	(2.4%)	(1.8%)	(2.1%)	(2.5%)	(1.2%)	(2.1%)	(1.5%)	(1.2%)
Trend effect	**2.2%**	**-2.5%**	0.7%	**3.4%**	-0.5%	-0.1%	**-1.8%**	-0.1%	0.1%
	(0.4%)	(0.7%)	(0.5%)	(0.6%)	(0.7%)	(0.3%)	(0.6%)	(0.4%)	(0.3%)

Notes: The dependent variable is the ln(crime rate) named at the top of each column. The data set is comprised of annual state-level observations (including the District of Columbia). 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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The question merits investigation: Why do the results of the Lott model (Table 3) support the Lott thesis more than the results of the Zheng model (Table 2)? The reason turns out to be somewhat surprising. As Table 2 documents, the two models include some different explanatory variables that one might think could have important implications. For example, Lott controls for population density and transfer payments to the poor, while Zheng controls for police, poverty, unemployment, and alcohol consumption. But these differences in the substantive controls turn out to be largely unimportant. Interestingly, as we will discuss in the next section, what drives the entire difference between Tables 2 and 3 is that Lott includes a large number of potentially duplicative demographic variables. Indeed, the array is so extensive as to make multicollinearity a serious issue.[60] Specifically, while Zheng's model controls for percent black and three age groupings, Lott's has thirty-six separate demographic percentages, breaking down each of three different race categories—black, white, and neither black nor white—and both sexes into six separate age categories from age ten up. The sensitivity of the results to the inclusion or exclusion of an array of highly collinear demographic variables serves as a cautionary tale to those who conduct or rely upon panel data models of crime. Probably no one examining either Wentong Zheng's work or that of Lott and Mustard would suspect that conclusions reached from their models would be sensitive to these seemingly second-order demographic controls.

C. *Sensitivity of the Lott Results to Time Period and Inclusion of Demographic Controls*

The results so far suggest that both a sparse model with no controls other than state and year fixed effects, as well as a reasonable specification developed for another context (thereby eliminating the possibility of data mining to achieve a desired result) undermine the Lott and Mustard more guns, less crime hypothesis. At the same time, though, their own specification generates some superficially supportive results in that murder and rape rates seem to be lower after passage of shall-issue laws. We say "superficially supportive" because a number of factors, including the failure of the model to show a drop in robbery, cast doubt on the causal story that they advance.

The first problem with the Lott story—as suggested in various graphs of Figure 1—is that during the 1990s crime in non-shall-issue states fell far more than in shall-issue states. Indeed, when we limit our focus to the thirteen states that adopted shall-issue laws between 1991 and 1999 as shown in Table 4, we see that the Lott story does not hold up during this period. The only robust

60. Multicollinearity exists when right-hand side variables are strongly (positively or negatively) correlated with each other. *See* GREENE, *supra* note 13, at 266.

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finding we can see in Table 4 across both panels is that three of the four property crime categories are higher with shall-issue law adoption.[61]

We have already alluded to the second problem with the Lott story: The results are incredibly sensitive to the inclusion of various seemingly unimportant demographic controls. The first line of Table 5a shows the results from estimating a dummy variable model using all of Lott's explanatory variables (again replacing arrest rate with the incarceration rate), except his large array of demographic controls. Here the results look very much like Table 1, line 2 in showing that shall-issue laws are associated with (statistically significantly) *higher* rates of crime in every category but murder. In other words, the similarity of the Table 5a, line 1 results and the Table 1, line 2 results suggests that the addition of the incarceration rate, state population and density, the controls for transfer payments, and real per capita state income essentially had no effect on the story depicted in the various graphs of Figure 1 (that shall-issue laws were associated with more crime). Each successive line of Table 5a augments the model estimated in line 1 by adding additional demographic controls. By the time the full array of demographic controls is entered, five of the eight strong positive estimates of line 1 have flipped the sign used by Lott and Mustard to become negative (although only violent crime and rape have become statistically significant in that direction). Even the crime categories, such as property, auto theft, and larceny, which retain their positive signs, still drop very sharply in size. For example, the estimated increase in auto theft from adopting a shall-issue law is 24.1% in line 1 of Table 5a, but only 4.5% in line 9.

What explains the enormous sensitivity of Lott's results to the inclusion of minor demographic controls? Table 5b gives insight into this question by replicating the analysis of Table 5a, while controlling for fixed state trends. Now we see much closer agreement with the results as we move down the nine regressions listed. Apparently, then, Lott and Mustard's thirty-six demographic variables mimic time trends in crime that we can control for directly with our controls for state trends.

61. We also replicated this Table using the alternative coding of the shall-issue laws shown in Appendix Table 1, column A. The resulting Appendix Table 4 yields quite similar results, except that rape is robustly negative in the two panels.

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TABLE 4A: THE ESTIMATED IMPACT OF SHALL-ISSUE LAWS ON CRIME
STATE DATA, 1991-1999 (VERNICK'S CODING), USING INCARCERATION RATES

Time Period (1991-1999)	Violent Crime	Murder	Rape	Aggravated Assault	Robbery	Property Crime	Auto Theft	Burglary	Larceny
1. Dummy variable model:	0.6%	**7.1%**	-2.8%	-2.1%	**9.0%**	**4.1%**	**7.3%**	**4.6%**	**3.0%**
	(2.3%)	(3.5%)	(1.8%)	(3.0%)	(2.4%)	(1.5%)	(3.0%)	(1.5%)	(1.4%)
2. Lott spline model:	0.2%	0.3%	0.2%	-0.1%	0.3%	1.0%	1.5%	1.2%	0.4%
	(1.2%)	(1.8%)	(1.2%)	(1.5%)	(1.3%)	(0.7%)	(1.5%)	(0.8%)	(0.7%)
3. Hybrid model:									
Postpassage dummy	0.9%	5.9%	-0.5%	-1.0%	5.6%	**4.1%**	**8.9%**	**4.1%**	3.3%
	(2.2%)	(4.1%)	(2.2%)	(3.0%)	(2.9%)	(1.9%)	(3.9%)	(2.0%)	(1.7%)
Trend effect	0.1%	-0.4%	0.2%	0.0%	-0.3%	0.5%	0.6%	0.7%	0.1%
	(1.2%)	(1.8%)	(1.3%)	(1.6%)	(1.3%)	(0.7%)	(1.5%)	(0.9%)	(0.7%)

Notes: The dependent variable is the ln(crime rate) named at the top of each column. The data set is comprised of annual state-level observations (including the District of Columbia). 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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TABLE 4B: THE ESTIMATED IMPACT OF SHALL-ISSUE LAWS ON CRIME CONTROLLING FOR STATE TRENDS
STATE DATA, 1991-1999 (VERNICK'S CODING), USING INCARCERATION RATES

Time Period (1991-1999)	Violent Crime	Murder	Rape	Aggravated Assault	Robbery	Property Crime	Auto Theft	Burglary	Larceny
1. Dummy variable model:	-0.1%	1.0%	-0.4%	-2.0%	3.0%	**3.6%**	**<u>8.6%</u>**	2.1%	<u>3.1%</u>
	(1.9%)	(4.3%)	(1.8%)	(2.5%)	(2.4%)	(1.7%)	(3.2%)	(1.8%)	(1.7%)
2. Hybrid model:									
Postpassage dummy	-0.3%	2.3%	2.1%	-2.3%	1.4%	**4.4%**	**<u>9.7%</u>**	2.0%	**4.1%**
	(2.1%)	(4.5%)	(2.2%)	(2.7%)	(2.6%)	(1.9%)	(3.4%)	(1.9%)	(1.8%)
Trend effect	0.4%	-2.0%	**<u>-4.0%</u>**	0.4%	2.5%	-1.2%	-1.8%	0.2%	-1.5%
	(1.4%)	(2.5%)	(1.5%)	(1.9%)	(1.6%)	(1.0%)	(1.9%)	(1.2%)	(1.0%)

Notes: The dependent variable is the ln(crime rate) named at the top of each column. The data set is comprised of annual state-level observations (including the District of Columbia). 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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TABLE 5A: THE ESTIMATED IMPACT OF SHALL-ISSUE LAWS ON CRIME
STATE DATA, 1977-1999 (VERNICK'S CODING), USING INCARCERATION RATES
ADDING ON DEMOGRAPHIC CONTROLS TO DUMMY VARIABLE MODEL

Time Period (1977-1999) Controls Used:	Violent Crime	Murder	Rape	Aggravated Assault	Robbery	Property Crime	Auto Theft	Burglary	Larceny
1. Every nondemographic control	**9.3%**	-0.1%	**6.4%**	**4.1%**	**16.5%**	**15.4%**	**24.1%**	**13.4%**	**15.0%**
	(1.8%)	(2.5%)	(2.0%)	(1.8%)	(2.6%)	(1.6%)	(2.9%)	(2.0%)	(1.5%)
2. (1) + demographic groups for bl males 10-19 & w males 10-19	**8.1%**	0.6%	**4.9%**	**3.7%**	**13.9%**	**12.7%**	**20.7%**	**10.6%**	**12.4%**
	(1.8%)	(2.4%)	(1.8%)	(1.9%)	(2.3%)	(1.4%)	(2.6%)	(1.7%)	(1.3%)
3. (2) + demographic groups for bl males 20-29 & w males 20-29	**4.9%**	0.3%	2.9%	0.4%	**9.9%**	**9.7%**	**18.3%**	**6.6%**	**9.5%**
	(1.7%)	(2.4%)	(1.9%)	(1.9%)	(2.2%)	(1.2%)	(2.5%)	(1.6%)	(1.2%)
4. (3) + demographic groups for bl males 30-49 & w males 30-49	2.9%	-2.0%	-0.7%	-0.3%	**6.9%**	**7.0%**	**13.7%**	**4.7%**	**6.7%**
	(1.5%)	(2.3%)	(1.5%)	(1.8%)	(2.1%)	(1.0%)	(2.2%)	(1.5%)	(1.0%)
5. (4) + demographic groups for bl females 10-49 & w females 10-49	2.4%	-0.9%	**-3.0%**	-0.2%	**6.8%**	**7.4%**	**16.6%**	**4.4%**	**7.2%**
	(1.6%)	(2.1%)	(1.5%)	(1.9%)	(2.2%)	(1.1%)	(2.3%)	(1.5%)	(1.0%)
6. (5) + demographic groups for neither bl nor w males 10-49	1.3%	0.2%	**-4.4%**	-0.9%	**5.4%**	**5.9%**	**13.7%**	1.7%	**6.2%**
	(1.6%)	(2.0%)	(1.5%)	(2.1%)	(2.2%)	(1.1%)	(2.1%)	(1.4%)	(1.1%)
7. (6) + demographic groups for neither bl nor w females 10-49	-0.7%	-0.9%	**-5.3%**	-2.7%	3.2%	**4.5%**	**12.0%**	0.5%	**4.8%**
	(1.5%)	(2.0%)	(1.5%)	(2.0%)	(2.1%)	(1.0%)	(2.0%)	(1.4%)	(1.0%)
8. (7) + demographic groups for bl males over 50 & w males over 50	-1.3%	-1.3%	**-5.5%**	-2.3%	1.4%	**3.7%**	**10.8%**	-0.5%	**4.0%**
	(1.5%)	(2.0%)	(1.6%)	(2.0%)	(2.1%)	(1.0%)	(2.1%)	(1.4%)	(0.9%)
9. All variables	**-3.4%**	-1.7%	**-7.2%**	-4.1%	-1.2%	1.8%	**4.5%**	-2.1%	**2.8%**
	(1.6%)	(2.0%)	(1.6%)	(2.1%)	(2.1%)	(1.0%)	(1.9%)	(1.4%)	(1.0%)

Notes: The dependent variable is the ln(crime rate) named at the top of each column. The data set is comprised of annual state-level observations (including the District of Columbia). State- and year- fixed effects are included in all specifications. All regressions are weighted by state population. Standard errors (in parentheses) are computed using the Huber-White robust estimate of variance. Coefficients that are significant at the .10 level are underlined. Coefficients that are significant at the .05 level are displayed in bold. Coefficients that are significant at the .01 level are both underlined and displayed in bold. The previously excluded demographic variables that are added into the line 9 regressions are black females over age 50, white females over age 50, neither black nor white females over age 50, and neither black nor white males over age 50.

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TABLE 5B: THE ESTIMATED IMPACT OF SHALL-ISSUE LAWS ON CRIME CONTROLLING FOR STATE TRENDS
STATE DATA, 1977-1999 (VERNICK'S CODING), USING INCARCERATION RATES
ADDING ON DEMOGRAPHIC CONTROLS TO DUMMY VARIABLE MODEL

Time Period (1977-1999) Controls Used:	Violent Crime	Murder	Rape	Aggravated Assault	Robbery	Property Crime	Auto Theft	Burglary	Larceny
1. Every nondemographic control	1.1%	1.9%	**-3.7%**	-0.8%	4.6%	**2.5%**	**4.7%**	2.1%	**2.3%**
	(1.5%)	(2.1%)	(1.5%)	(1.7%)	(2.4%)	(1.1%)	(2.1%)	(1.6%)	(1.1%)
2. (1) + demographic groups for bl	0.0%	0.2%	**-3.7%**	-2.4%	4.0%	2.0%	**4.2%**	1.8%	1.7%
males 10-19 & w males 10-19	(1.5%)	(2.1%)	(1.5%)	(1.7%)	(2.4%)	(1.1%)	(2.1%)	(1.6%)	(1.2%)
3. (2) + demographic groups for bl	-0.2%	0.0%	**-3.8%**	-2.5%	3.7%	1.9%	3.9%	1.7%	1.6%
males 20-29 & w males 20-29	(1.5%)	(2.2%)	(1.4%)	(1.6%)	(2.5%)	(1.2%)	(2.2%)	(1.5%)	(1.2%)
4. (3) + demographic groups for bl	0.9%	1.9%	-2.8%	-1.6%	**5.9%**	**3.0%**	3.6%	2.8%	**2.8%**
males 30-49 & w males 30-49	(1.6%)	(2.2%)	(1.5%)	(1.7%)	(2.5%)	(1.2%)	(2.2%)	(1.6%)	(1.2%)
5. (4) + demographic groups for bl	0.4%	0.9%	**-3.4%**	-1.8%	**5.5%**	**3.4%**	3.9%	**3.5%**	**3.2%**
females 10-49 & w females 10-49	(1.5%)	(2.2%)	(1.4%)	(1.6%)	(2.5%)	(1.2%)	(2.2%)	(1.6%)	(1.1%)
6. (5) + demographic groups for	0.6%	1.5%	**-3.2%**	-2.1%	**6.2%**	**3.7%**	**4.3%**	**3.7%**	**3.4%**
neither bl nor w males 10-49	(1.5%)	(2.3%)	(1.5%)	(1.6%)	(2.5%)	(1.2%)	(2.2%)	(1.6%)	(1.1%)
7. (6) + demographic groups for	0.1%	0.6%	**-3.6%**	-2.5%	**5.7%**	**3.0%**	3.8%	2.8%	**3.0%**
neither bl nor w females 10-49	(1.5%)	(2.2%)	(1.5%)	(1.6%)	(2.6%)	(1.2%)	(2.2%)	(1.5%)	(1.1%)
8. (7) + demographic groups for bl	-0.5%	0.6%	**-3.4%**	-2.8%	4.4%	**2.8%**	2.7%	2.2%	**3.1%**
males over 50 & w males over 50	(1.4%)	(2.3%)	(1.5%)	(1.7%)	(2.4%)	(1.1%)	(2.1%)	(1.5%)	(1.1%)
9. All variables	-1.7%	2.4%	**-4.5%**	**-4.6%**	4.3%	**2.7%**	2.4%	2.1%	**2.9%**
	(1.5%)	(2.3%)	(1.6%)	(1.8%)	(2.4%)	(1.2%)	(2.1%)	(1.5%)	(1.1%)

Notes: The dependent variable is the ln(crime rate) named at the top of each column. The data set is comprised of annual state-level observations (including the District of Columbia). State- and year- fixed effects are included in all specifications. All regressions are weighted by state population. Standard errors (in parentheses) are computed using the Huber-White robust estimate of variance. Coefficients that are significant at the .10 level are underlined. Coefficients that are significant at the .05 level are displayed in bold. Coefficients that are significant at the .01 level are both underlined and displayed in bold. The previously excluded demographic variables that are added into the line 9 regressions are black females over age 50, white females over age 50, neither black nor white females over age 50, and neither black nor white males over age 50.

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Assume, though, that Lott's decision to include thirty-six demographic controls is excessive and that the appropriate number of these highly collinear variables is some smaller number. For example, since men commit over ninety percent of the violent crime, perhaps it would be enough to control only for the percent of white males in each of six separate age groupings above ten as well as the percentage of black males in each of these six age categories, for a total of twelve demographic controls. One could follow Lott in including additional controls for the percentage of white or of black females in each of these six age groupings for an additional twelve more controls, but each black male category is already highly correlated with each corresponding black female category and the same correlation applies for whites as well. There is little justification for going even further in following Lott by putting in the percentage of the population that is neither white nor black in twelve other age-sex categories.

Table 6 mimics the Lott and Mustard model of Table 3, but limits the demographic controls from Lott's thirty-six to the six age categories above age ten for white males and the same six age categories for black males—a total of twelve controls. Now it is hard to find *any* crime category that seems to have a robustly lower crime rate across the various models of panel A and panel B. Larceny and property crime seem to be robustly higher with the adoption of shall-issue laws, and rape may be lower (although the results including state trends are mixed for this crime). Overall, the results seem much more supportive of a *positive* impact on crime after the adoption of shall-issue laws than a negative one. Perhaps one can argue that it is important to include all of the other twenty-four highly collinear demographic variables to return to the (already weak) results of Table 3, but results that are sensitive to inclusion or exclusion of these marginal control variables must be treated with skepticism.

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TABLE 6A: THE ESTIMATED IMPACT OF SHALL-ISSUE LAWS ON CRIME
STATE DATA, 1977-1999 (VERNICK'S CODING), USING INCARCERATION RATES
INCLUDING ONLY BLACK AND WHITE MALES OVER AGE 10 AS DEMOGRAPHIC CONTROLS

Time Period (1977-1999)	Violent Crime	Murder	Rape	Aggravated Assault	Robbery	Property Crime	Auto Theft	Burglary	Larceny
1. Dummy variable model:	1.0%	-0.5%	**-3.7%**	-1.1%	3.8%	**6.0%**	**13.3%**	**3.0%**	**5.7%**
	(1.5%)	(2.2%)	(1.5%)	(1.9%)	(2.2%)	(1.1%)	(2.4%)	(1.5%)	(1.0%)
2. Lott spline model:	-0.5%	-0.5%	-0.1%	-0.7%	**-1.5%**	0.0%	0.7%	-0.1%	-0.2%
	(0.4%)	(0.6%)	(0.4%)	(0.5%)	(0.5%)	(0.3%)	(0.5%)	(0.4%)	(0.2%)
3. Hybrid model:									
Postpassage dummy	0.6%	**6.8%**	**-5.9%**	-1.5%	**6.0%**	**3.7%**	**9.8%**	2.8%	**2.6%**
	(1.7%)	(2.6%)	(2.0%)	(2.1%)	(2.6%)	(1.2%)	(2.7%)	(1.8%)	(1.2%)
Trend effect	-0.5%	-0.9%	0.3%	-0.6%	**-1.9%**	-0.2%	0.1%	-0.3%	-0.3%
	(0.4%)	(0.6%)	(0.4%)	(0.5%)	(0.5%)	(0.3%)	(0.5%)	(0.4%)	(0.3%)

Notes: The dependent variable is the ln(crime rate) named at the top of each column. The data set is comprised of annual state-level observations (including the District of Columbia). 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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TABLE 6B: THE ESTIMATED IMPACT OF SHALL-ISSUE LAWS ON CRIME CONTROLLING FOR STATE TRENDS STATE DATA, 1977-1999 (VERNICK'S CODING), USING INCARCERATION RATES INCLUDING ONLY BLACK AND WHITE MALES OVER AGE 10 AS DEMOGRAPHIC CONTROLS

Time Period (1977-1999)	Violent Crime	Murder	Rape	Aggravated Assault	Robbery	Property Crime	Auto Theft	Burglary	Larceny
1. Dummy variable model:	0.0%	1.8%	-2.9%	-2.4%	4.5%	**3.0%**	2.6%	2.5%	**3.2%**
	(1.4%)	(2.2%)	(1.5%)	(1.6%)	(2.3%)	(1.1%)	(2.0%)	(1.5%)	(1.1%)
2. Hybrid model:									
Postpassage dummy	-2.3%	3.2%	**-3.9%**	**-5.9%**	**4.7%**	**2.9%**	3.9%	3.0%	**2.7%**
	(1.6%)	(2.3%)	(1.6%)	(1.9%)	(2.4%)	(1.1%)	(2.0%)	(1.6%)	(1.1%)
Trend effect	**2.3%**	-1.3%	**1.1%**	**3.4%**	-0.3%	0.0%	**-1.3%**	-0.4%	0.4%
	(0.5%)	(0.7%)	(0.5%)	(0.6%)	(0.7%)	(0.3%)	(0.6%)	(0.4%)	(0.3%)

Notes: The dependent variable is the ln(crime rate) named at the top of each column. The data set is comprised of annual state-level observations (including the District of Columbia). 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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Since the Figure 1a-1f graphs, the Table 1 regressions, the Table 5 regressions with the nondemographic controls, and the Table 6 regressions with twelve of Lott's demographic controls all show a largely *positive* relationship between shall-issue law adoption and crime over the 1977-1999 period, it becomes important to ascertain what about the demographic controls is capturing some true phenomenon that would reverse our assessment of the crime effects generated by shall-issue laws. Ordinarily, if the inclusion of an explanatory variable has such a dramatic effect on one's estimate, one would explore whether the estimated effect of that explanatory variable makes sense. In this case, though, the 36 demographic controls are so highly collinear that it is impossible for the regression to provide meaningful results for any given demographic control.[62] Thus, all we are left with is a set of estimated effects on the demographic controls resembling output from a random number generator, and the knowledge that, for some unknown reason, these demographic controls matter considerably. We can think of no reason to prefer the regressions in Table 3, which might be deemed to provide weak support to Lott and Mustard, to those in Table 6, which are devastating to the more guns, less crime thesis.[63]

While virtually the only evidence shown so far that offers even weak support for the Lott and Mustard thesis is Table 3, there is a statistical argument that even these results inaccurately overstate their statistical significance. A recent comment by Willard Manning of the University of Chicago suggests that when, as here, one is estimating nine different regressions, one should make

---

62. For example, if one regresses the percent black, age 10 to 19 on the other demographic controls, the R-squared value is 0.9999. R-squared is a measure of the "goodness of fit," that is, how accurately the regression was able to account for movements in the dependent variable. *See* GREENE, *supra* note 13, at 152. An R-squared of 99% would indicate that the regression right-hand side variables were able to explain 99% of the variance in the left-hand side variable.

Consequently, there is a very severe multicollinearity problem that makes it impossible to interpret any of the coefficients on the individual demographic controls (although ordinarily one would not expect this to invalidate the estimated shall-issue effects). Indeed, when one looks at the estimated effects of a change in the demographic variables on violent crime from the Table 3 regressions, one finds utterly bizarre results. For example, an increase of one percentage point in the percentage of black males aged 30-39 would be expected to almost double the violent crime rate, while a similar increase in the percentage of black males aged 40-49 would lead to a drop in violent crime of 60%. Similarly, increasing the percentage of black males aged 50-64 would cause violent crime to jump by 145%, but increasing the percentage of black males over age 65 would lead to a 78% decline in violent crime. These nonsense results prevent us from understanding why the demographic controls can influence the estimates of shall-issue adoption so strongly.

63. In response to the claim that the unpredictable estimated impacts of the demographic controls suggest model misspecification, Lott explains that "[w]hat I have tried to do is 'overcontrol' for all possible demographic factors to make sure that any effects attributed to the right-to-carry laws are not arising because I have accidentally left out some other factor." LOTT, *supra* note 2, at 144. Ironically, the effects that Lott attributes to these laws occur not because he has left out any demographic variable, but primarily because he has put them in.

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adjustment for the fact that multiple comparisons are being made in assessing statistical significance:

> Failure to correct for multiple comparisons causes the true significance level to be much less than the nominal level would suggest. If there are seven comparisons, then a nominal 5 percent standard applied to each is actually more like a 30 percent standard. The former is usually considered statistically significant if it is met, while the latter is considered statistically insignificant, and not noteworthy unless one is looking for a null finding.[64]

Correcting for the presence of these multiple comparisons and for autocorrelation in crime across years, Manning found that the estimates of the effect of shall-issue laws "should have *t* statistics that are about 50-60 percent of their reported value. With such a correction, the results [estimating the effect of a shall-issue law] appear to be statistically significantly different from zero about as often as one would expect if they had occurred at random."[65] Again, this is not a ringing endorsement of the more guns, less crime hypothesis. If one were to apply both such corrections to the Table 3 results, there would simply be no evidence of any crime-reducing benefit associated with the adoption of shall-issue laws. We ran the regressions of Table 3 using the Bhargava correction for serial correlation, which did greatly reduce the statistical significance of the resulting estimates. In none of the five sets of regressions was there any statistically significant effect for robbery or any property crime, and only for row 2 in Table 3b (the hybrid model for state trends) do we see anything more than isolated significance. In that model, violent crime and aggravated assault appear to rise, while rape and murder appear to fall.

D. *Problems with Unequal Years of Data from Early and Late Adopters in the Pre-Post Comparison*

Thus far our regressions have attempted to generate an estimate for the impact of shall-issue laws that are aggregated across all adopting states. Essentially, we are implementing a difference-in-difference design, in which we compare how crime changes over time in states that do not have shall-issue laws (or that had such laws prior to our data period) to how crime changes in states that adopt shall-issue laws during our data period. An important aspect of this investigation is to select "before" and "after" periods for pre-post comparisons, and thus far we have used the entire 1977-1999 panel to calculate the aggregated pre-post difference for shall-issue states. This can create problems, though, because shall-issue laws were adopted as early as 1985 and

64. Willard Manning, *Comment* to Donohue, *supra* note 52, at 331, 334.

65. *Id.* at 335. A "t-statistic" is a measure of the statistical significance of an individual regression coefficient. *See* GREENE, *supra* note 13, at 161. A reduction in the absolute value of a t-statistic would indicate that the coefficient (and therefore the effect of the variable in question) was less statistically significant.

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as late as 1996, which implies that for some states, observations from nineteen years before adoption to eighteen years after adoption would be used to form a pre-post contrast. This can be potentially problematic when a very limited sample of states in the very early and very late years may be tilting the regression line unduly. Table 7 shows the periods of time for which pre- and postpassage data is available for the twenty-five adopting jurisdictions (in Vernick's coding). It should be immediately apparent that while every state except Maine is used in identifying the effect on crime over the period from eight years prior to three years after passage, only three states are entering into the estimate of crime nineteen years before passage: Texas, Kentucky, and South Carolina. Thus, Texas will disproportionately influence the population-weighted prepassage effect because it will be counted in fifteen more yearly observations than the Maine observations during the same prepassage period. Conversely, in the postpassage period, Florida, Georgia, and Pennsylvania, all fairly populous states, will influence the postpassage estimates more highly both by virtue of their substantial populations and because they will have from ten to twelve yearly observations that will be averaged into the postpassage effect compared with the mere three years of observations appearing for Texas, Kentucky, and South Carolina.

One crude way to show that this factor is important is to simply "dummy out" the periods more than eight years prior to or more than three years after the passage of a shall-issue law.[66] This approach essentially will enable the estimates of the aggregated effect of shall-issue laws on crime to come primarily from the twelve-year span that begins eight years prior to passage and extends through the year of passage to the three years after passage. Table 8 shows the results of this exercise. The bottom line seems to be that rape may decrease with the adoption of shall-issue laws (the results are somewhat mixed across the two panels), but larceny and burglary rates are robustly larger. Indeed, if one accepts the estimates from the hybrid model controlling for state trends, shall-issue laws would be deemed (at least by the third year) to have *increased* violent crime, aggravated assault, robbery, property crime, burglary, and larceny. There is one negative estimate: rape. Its effect is not statistically significant in the third year, however, and it turns positive in the fourth.[67]

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66. To "dummy out" is to add separate dummy controls—here, for years more than eight years before or three years after shall-issue adoption. By creating individual dummy variables for the stated years before and after adoption, we are ensuring that our overall estimates of the impact of the law will not be influenced by observations from those years.

67. Appendix Table 5 replicates the Table 8 results using Lott's shall-issue law coding. The results are similar, but with two changes: The Appendix Table 5 results weaken the Lott and Mustard thesis since robbery is *positive* and statistically significant across both panels (as opposed to just in Panel B of Table 8), but the results strengthen the thesis in that rape is more robustly negative. Because of the difference in coding for Maine between Lott and us, we reran Table 8 using Lott's 1985 adoption date for Maine. The results were virtually identical (albeit somewhat stronger for the view that shall-issue laws increase crime in Panel B, while generating statistically significant (0.05 level) reductions in rape and aggravated

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TABLE 7: YEARS AND STATES INCLUDED IN THE 1977-1999 TIME PERIOD RELATIVE TO SHALL-ISSUE LAW ADOPTION STATE DATA (VERNICK'S CODING OF THE SHALL-ISSUE DUMMY)

Number of years before or after passage

	-19	-18	-17	-16	-15	-14	-13	-12	-11	-10	-9	-8	-7	-6	-5	-4	-3	-2	-1	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18
ME(1981)																•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•
ND(1985)												•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•				
SD(1985)												•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•				
UT(1986)											•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•					
FL(1987)										•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•						
GA(1989)								•	•		•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•								
PA(1989)								•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•								
WV(1989)								•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•								
ID(1990)							•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•									
MS(1990)							•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•									
OR(1990)							•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•									
LA(1991)						•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•										
MT(1991)						•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•										
AK(1994)			•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•													
AZ(1994)			•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•													
TN(1994)			•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•													
WY(1994)			•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•													
AR(1995)		•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•														
NC(1995)		•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•														
NV(1995)		•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•														
OK(1995)		•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•														
VA(1995)		•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•														
TX(1996)	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•															
KY(1996)	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•															
SC(1996)	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•	•															

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TABLE 8A: THE ESTIMATED IMPACT OF SHALL-ISSUE LAWS ON CRIME
STATE DATA, 1977-1999 (VERNICK'S CODING), USING INCARCERATION RATES ADDING IN
DUMMIES FOR YEARS MORE THAN 8 YEARS BEFORE OR 3 YEARS AFTER SHALL-ISSUE LAW ADOPTION

Time Period (1977-1999)	Violent Crime	Murder	Rape	Aggravated Assault	Robbery	Property Crime	Auto Theft	Burglary	Larceny
1. Dummy variable model:	-2.7%	0.6%	**-5.1%**	-4.1%	2.2%	2.0%	**4.3%**	-0.4%	**2.5%**
	(1.7%)	(2.0%)	(1.8%)	(2.1%)	(2.2%)	(1.1%)	(2.1%)	(1.4%)	(1.1%)
2. Lott spline model:	-0.1%	-0.5%	0.5%	-0.7%	-0.4%	0.4%	0.2%	**0.9%**	0.2%
	(0.6%)	(0.7%)	(0.5%)	(0.7%)	(0.7%)	(0.3%)	(0.6%)	(0.4%)	(0.3%)
3. Hybrid model:									
Postpassage dummy	-2.4%	1.3%	**-7.2%**	-4.1%	3.8%	1.3%	3.4%	-0.2%	1.3%
	(1.9%)	(2.3%)	(2.1%)	(2.3%)	(2.6%)	(1.3%)	(2.5%)	(1.6%)	(1.3%)
Trend effect	0.0%	-0.6%	0.8%	-0.6%	-0.5%	0.3%	0.1%	**0.9%**	0.2%
	(0.6%)	(0.7%)	(0.5%)	(0.7%)	(0.7%)	(0.3%)	(0.6%)	(0.4%)	(0.3%)

Notes: The dependent variable is the ln(crime rate) named at the top of each column. The data set is comprised of annual state-level observations (including the District of Columbia). 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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TABLE 8B: THE ESTIMATED IMPACT OF SHALL-ISSUE LAWS ON CRIME CONTROLLING FOR STATE TRENDS STATE DATA, 1977-1999 (VERNICK'S CODING), USING INCARCERATION RATES ADDING IN DUMMIES FOR YEARS MORE THAN 8 YEARS BEFORE OR 3 YEARS AFTER SHALL-ISSUE LAW ADOPTION

Time Period (1977-1999)	Violent Crime	Murder	Rape	Aggravated Assault	Robbery	Property Crime	Auto Theft	Burglary	Larceny
1. Dummy variable model:	-1.5%	3.5%	**-4.4%**	**-4.8%**	**5.4%**	**3.0%**	3.1%	3.0%	**3.0%**
	(1.6%)	(2.4%)	(1.6%)	(1.9%)	(2.5%)	(1.2%)	(2.2%)	(1.5%)	(1.2%)
2. Hybrid model:									
Postpassage dummy	**-4.5%**	4.8%	**-6.0%**	**-8.8%**	4.8%	**2.8%**	4.2%	2.0%	**2.8%**
	(1.7%)	(2.5%)	(1.8%)	(2.1%)	(2.6%)	(1.2%)	(2.2%)	(1.6%)	(1.2%)
Trend effect	**3.4%**	-1.5%	**1.8%**	**4.6%**	0.7%	0.3%	-1.3%	**1.1%**	0.2%
	(0.5%)	(0.9%)	(0.6%)	(0.7%)	(0.9%)	(0.4%)	(0.8%)	(0.5%)	(0.4%)

Notes: The dependent variable is the ln(crime rate) named at the top of each column. The data set is comprised of annual state-level observations (including the District of Columbia). State- and year- fixed effects are included in all specifications. All regressions are weighted by state population. Standard errors (in parentheses) are computed using the Huber-White robust estimate of variance. Coefficients that are significant at the .10 level are underlined. Coefficients that are significant at the .05 level are displayed in bold. Coefficients that are significant at the .01 level are both underlined and displayed in bold.

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To underscore the message that the observations from the adopting states at the two ends of the time span, either well before passage or well after passage, are causing mischief when estimating a single aggregated impact of the shall-issue laws, it may be helpful to illustrate this point graphically. Figures 3a through 3i plot the estimated annual effect on various crime categories for every year from nineteen years prior to adoption to seventeen years after adoption.[68] Figure 3b shows the results for murder and it should immediately be apparent that the period from eight years before to three years after passage evidences relatively little movement in this crime category (as the Table 8 regression results would suggest). However, outside this time frame (especially in the last years), one sees large swings in the estimated effect of shall-issue laws on murder. Of course, the thought that shall-issue laws caused crime to drop by almost twenty-five percent in the thirteenth year after passage and then caused it to increase by almost twenty-one percent in the fourteenth year is obviously untenable. These wild swings are caused not by any true impact of shall-issue laws but by the selective dropping out from the individual year estimates of states that adopted the law more recently, leaving only the shrinking number of earliest adopters to identify the particular annual impact. Whether one looks at murder, aggravated assault, rape, or robbery, the final year estimate at fourteen years after passage always swings wildly—sometimes up, sometimes down—and the earliest prepassage estimates also show erratic movements (note the jump in the first year for aggravated assault in Figure 3d). Another way to get at this same problem of unbalanced panel data is simply to undertake a disaggregated analysis, which estimates different shall-issue impacts for individual states. We will undertake just this analysis when we later turn to analyze the county data, but first we will address an issue that we previously set aside.

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assault, and increases in property crime, auto theft, and larceny in line 1 of Panel B). Running the same regressions for the period from four years prior to adoption to three years after adoption (thereby capturing all 25 of the adopting states under Vernick's coding) yields comparable results.

68. In other words, rather than simply having a single postpassage dummy, as in our customary dummy variable model, we now define a separate dummy variable for each individual year before or after adoption of a shall-issue law, and then plot these individual estimates in Figures 3a through 3i, with the effect of the law normalized to equal zero in the year of passage (year 0).

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FIGURE 3A: VIOLENT CRIME—NORMALIZED EFFECT BY YEAR RELATIVE TO ADOPTION (VERNICK'S CODING)

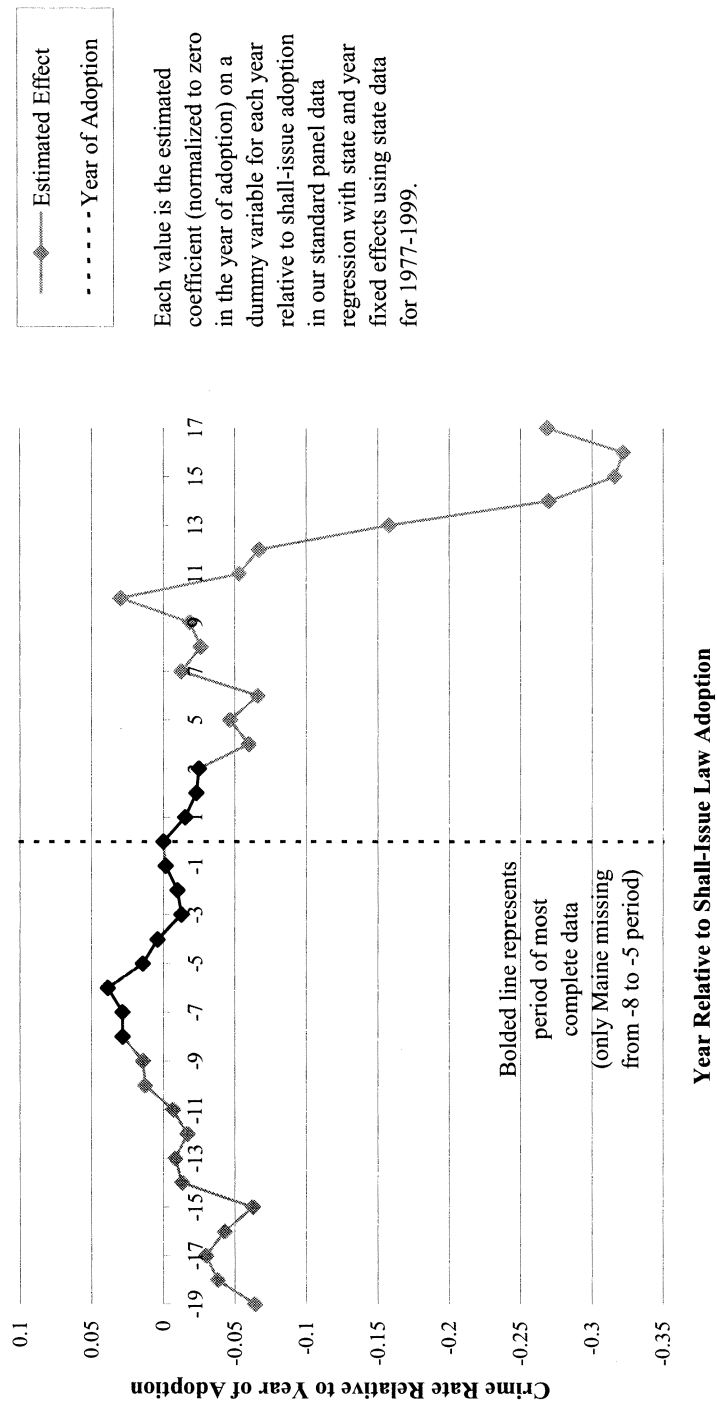

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FIGURE 3B: MURDER—NORMALIZED EFFECT BY YEAR RELATIVE TO ADOPTION (VERNICK'S CODING)

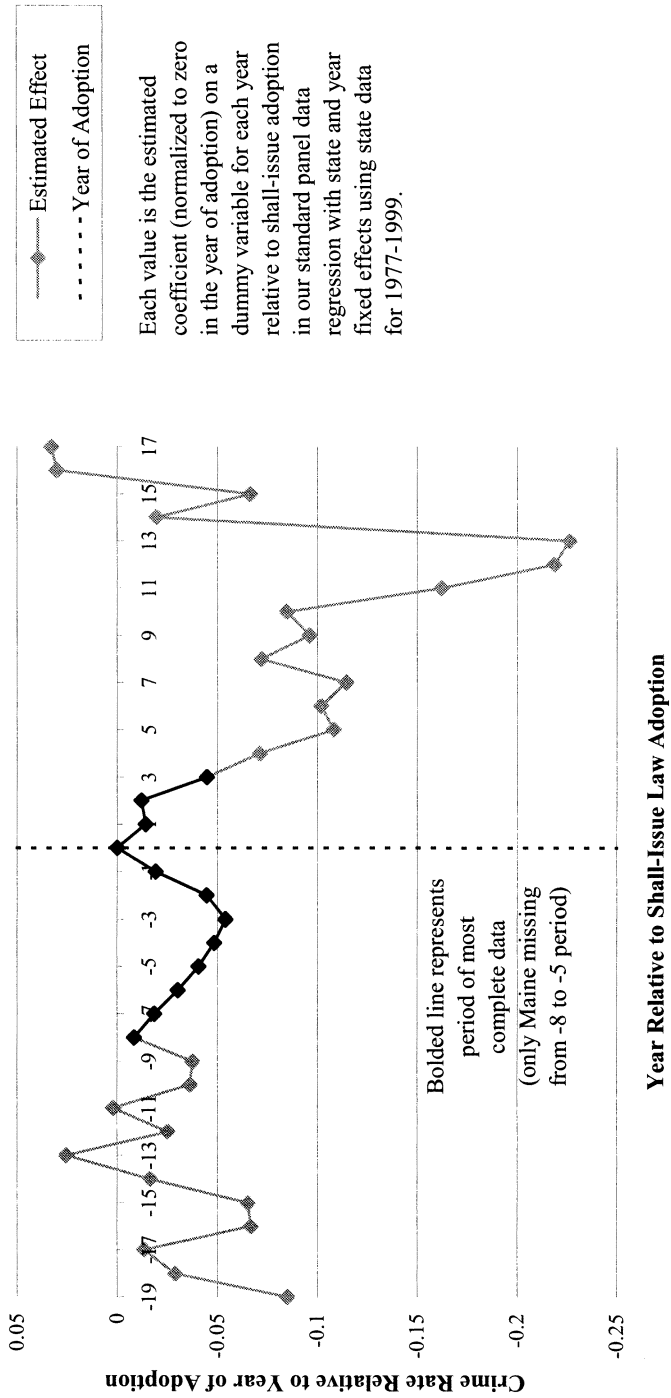

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FIGURE 3C: RAPE—NORMALIZED EFFECT BY YEAR RELATIVE TO ADOPTION (VERNICK'S CODING)

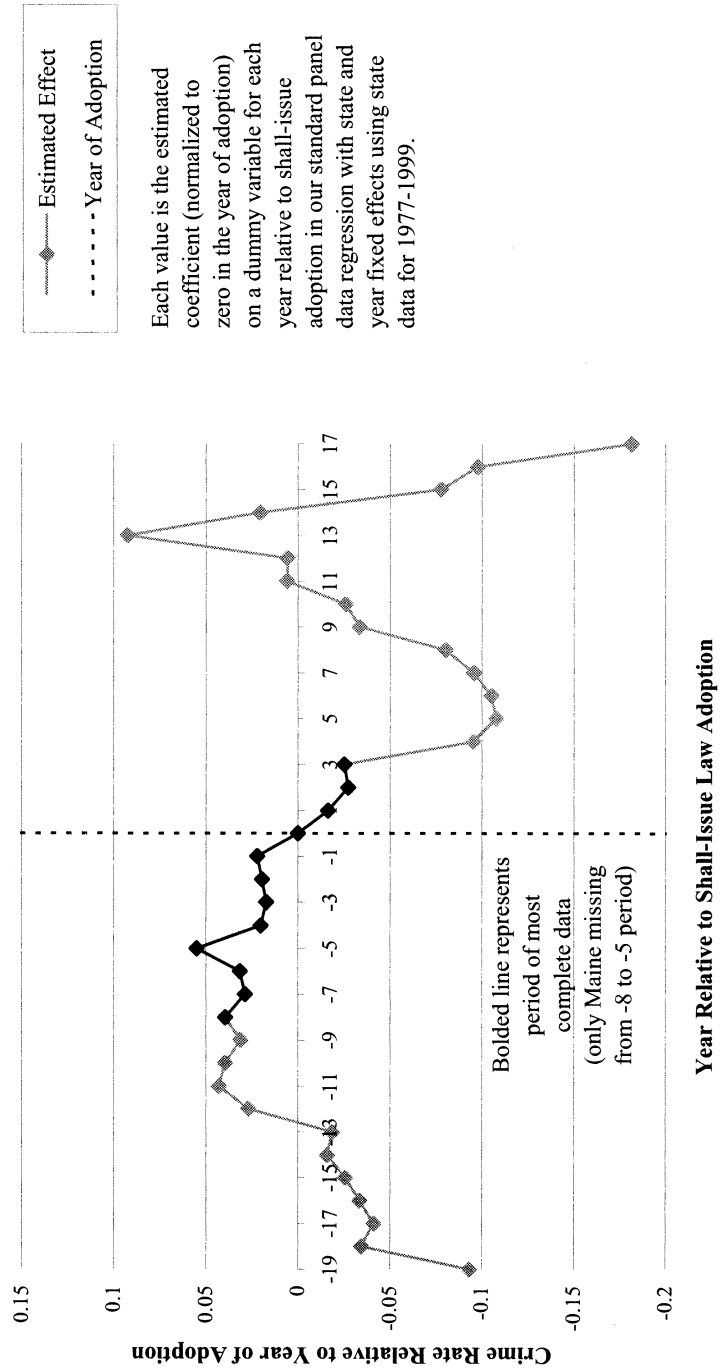

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FIGURE 3D: AGGRAVATED ASSAULT—NORMALIZED EFFECT BY YEAR RELATIVE TO ADOPTION (VERNICK'S CODING)

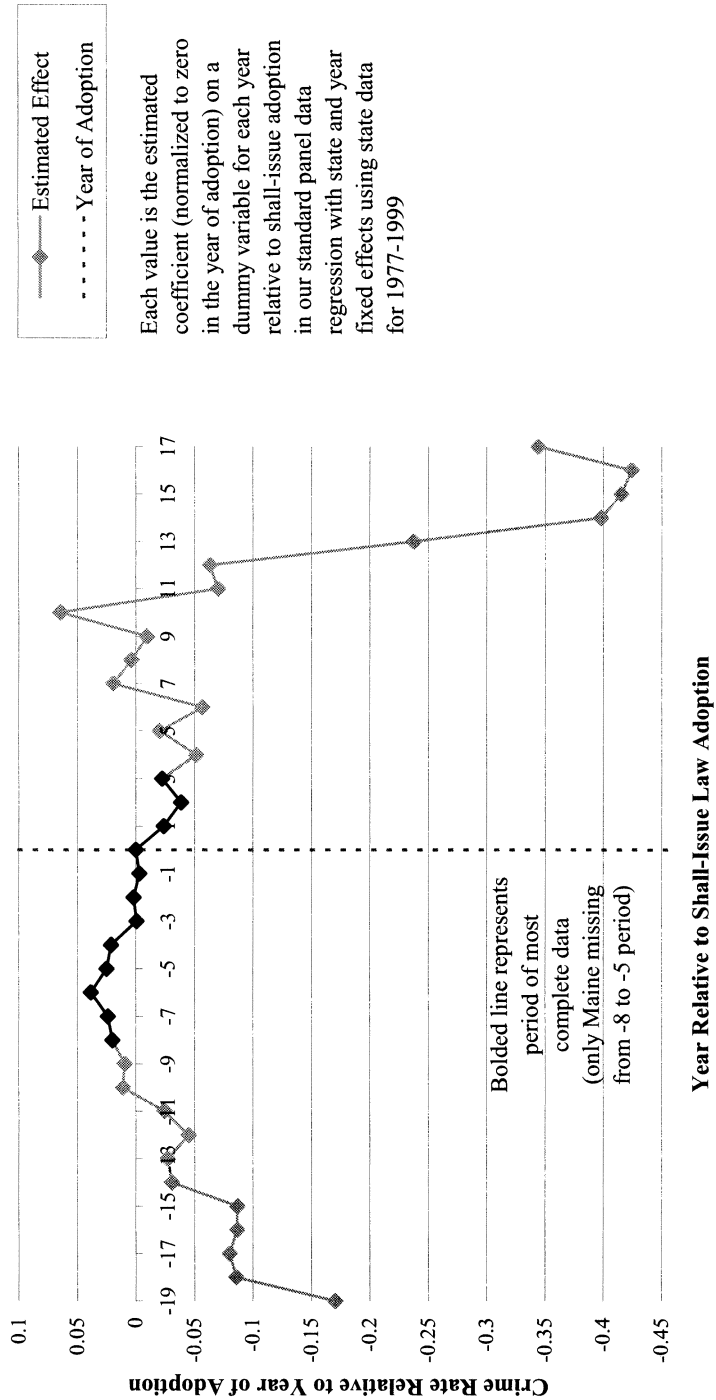

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FIGURE 3E: ROBBERY—NORMALIZED EFFECT BY YEAR RELATIVE TO ADOPTION (VERNICK'S CODING)

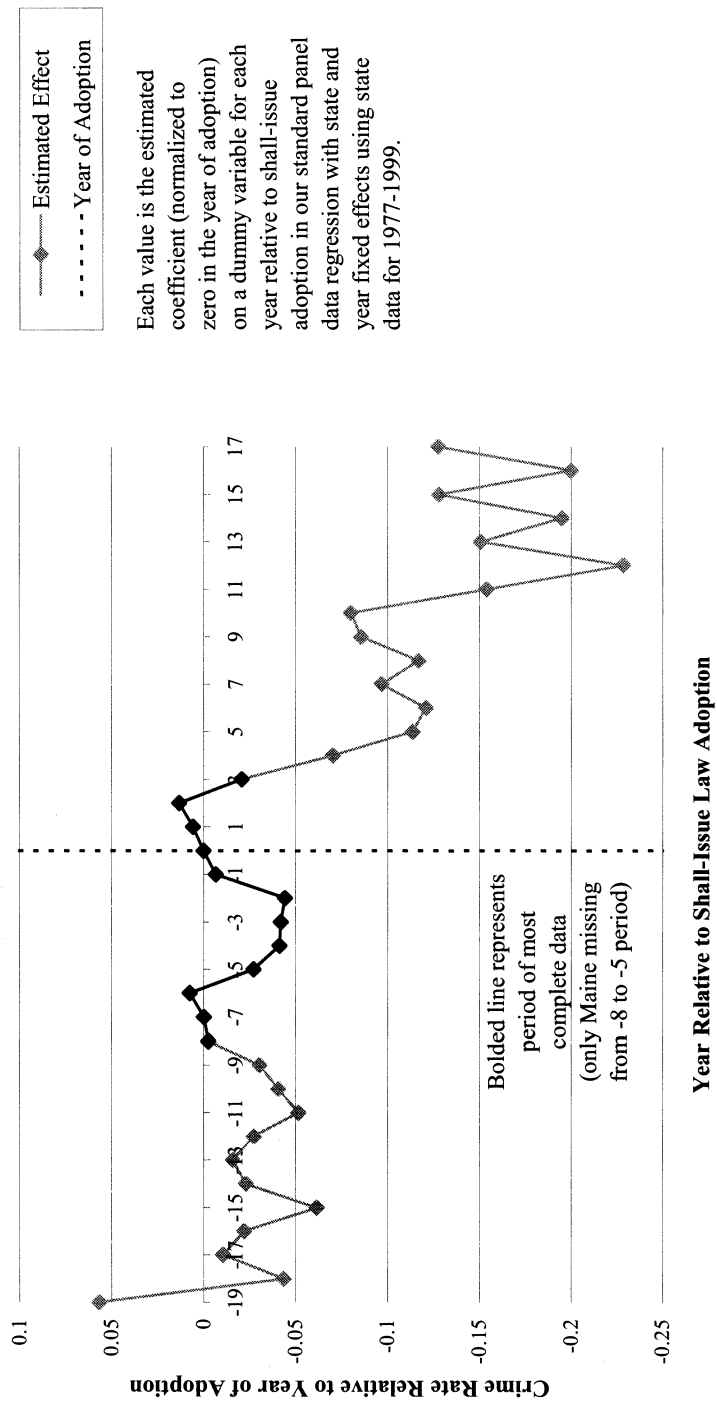

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FIGURE 3F: PROPERTY CRIME—NORMALIZED EFFECT BY YEAR RELATIVE TO ADOPTION (VERNICK'S CODING)

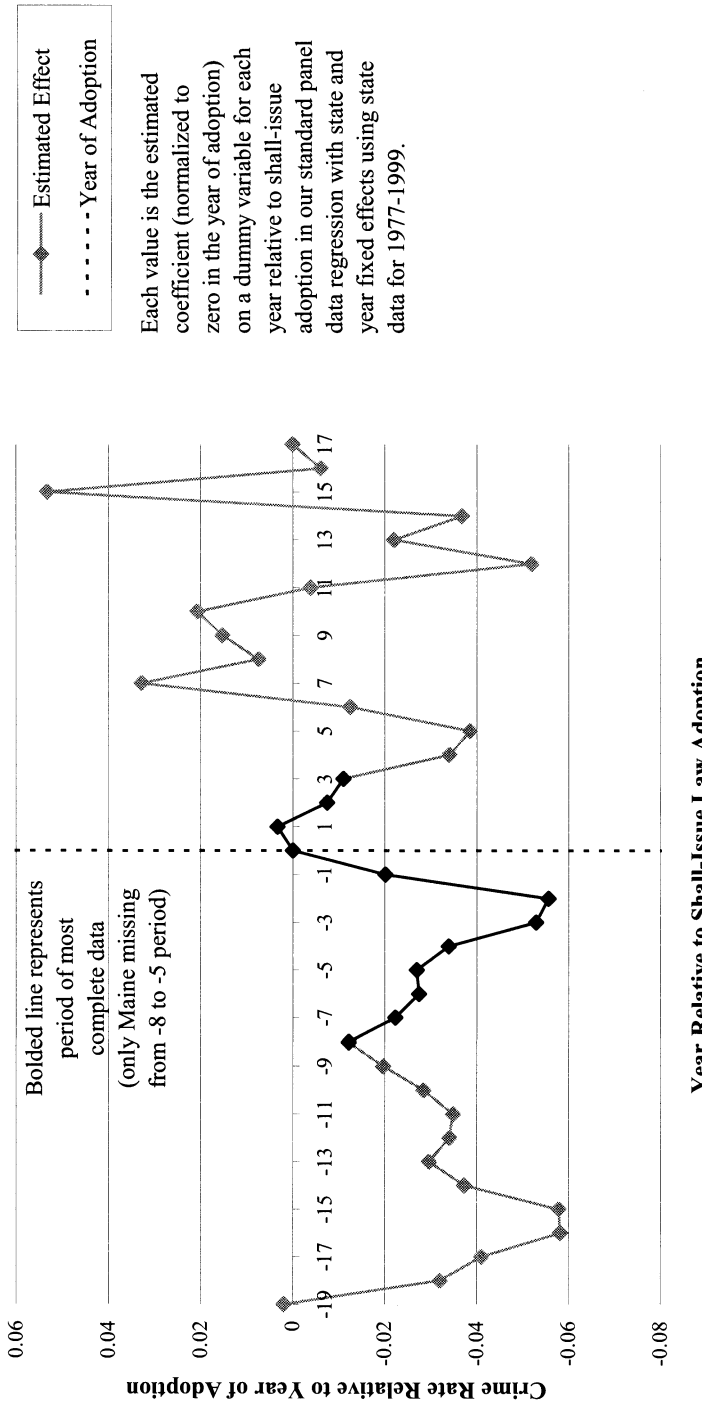

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FIGURE 3G: AUTO THEFT—NORMALIZED EFFECT BY YEAR RELATIVE TO ADOPTION (VERNICK'S CODING)

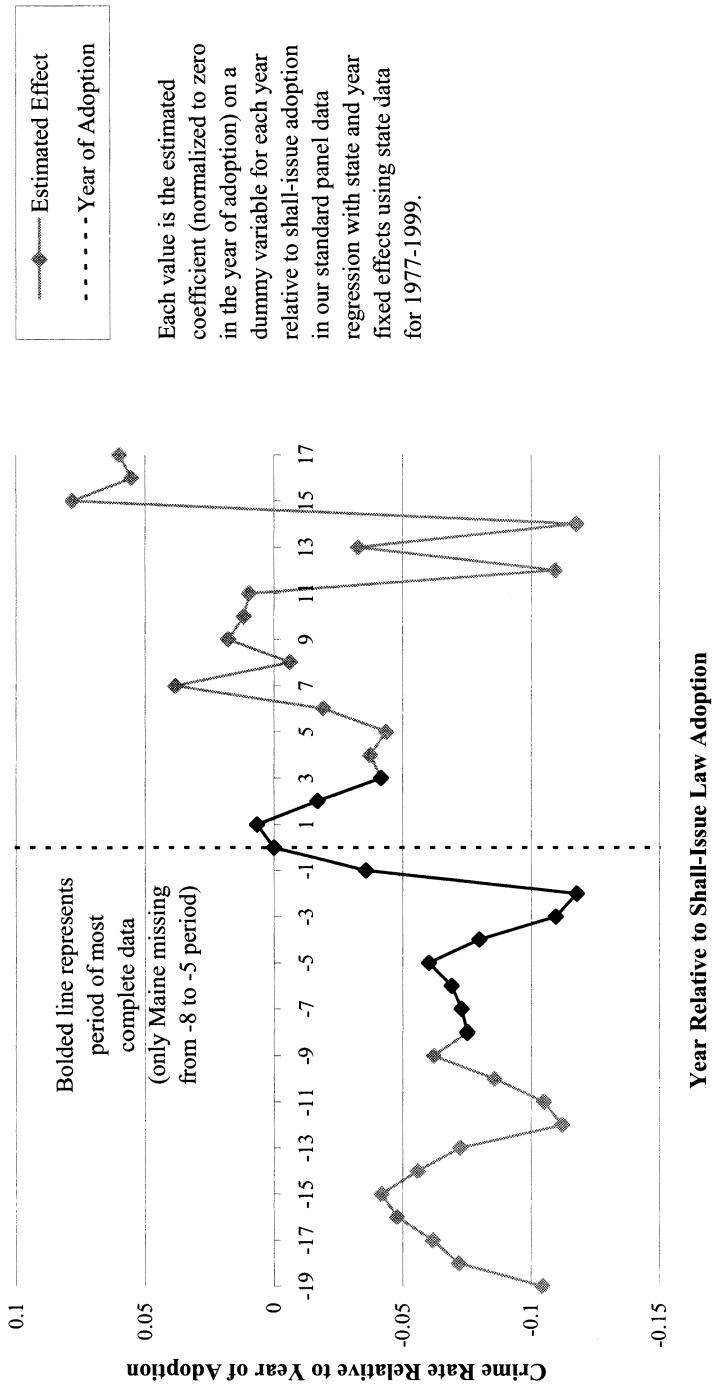

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FIGURE 3H: BURGLARY—NORMALIZED EFFECT BY YEAR RELATIVE TO ADOPTION (VERNICK'S CODING)

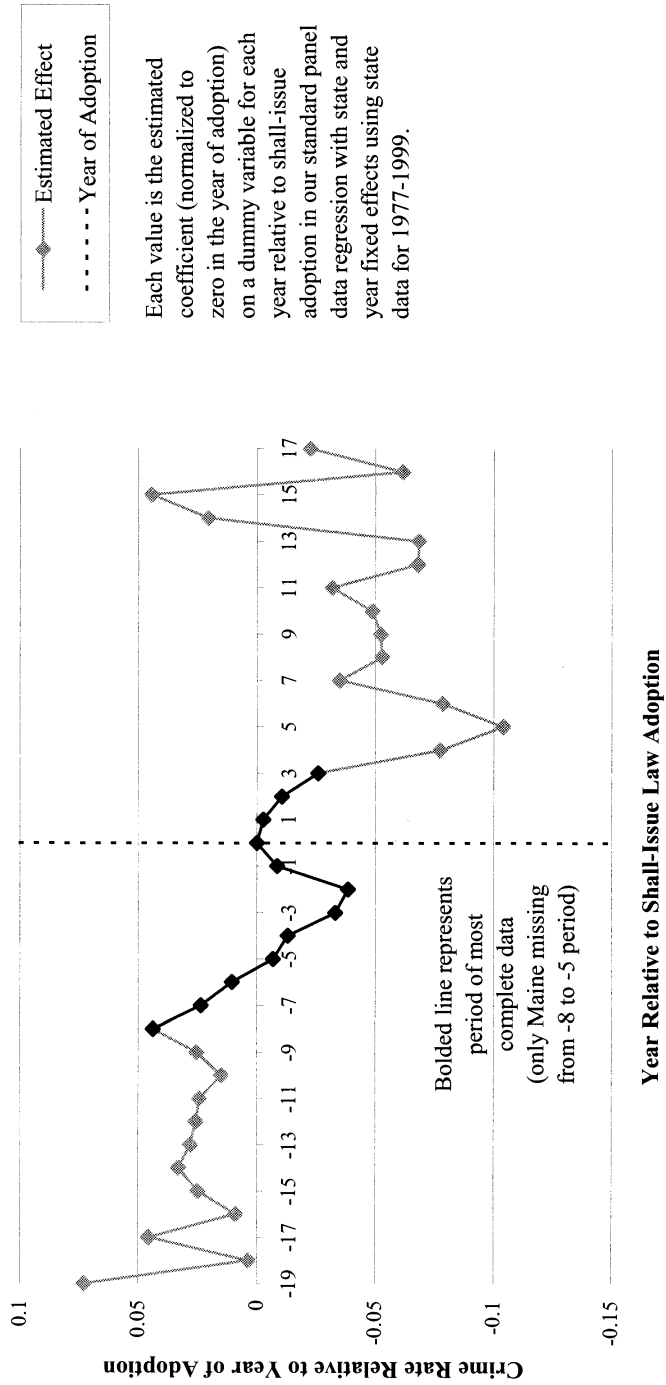

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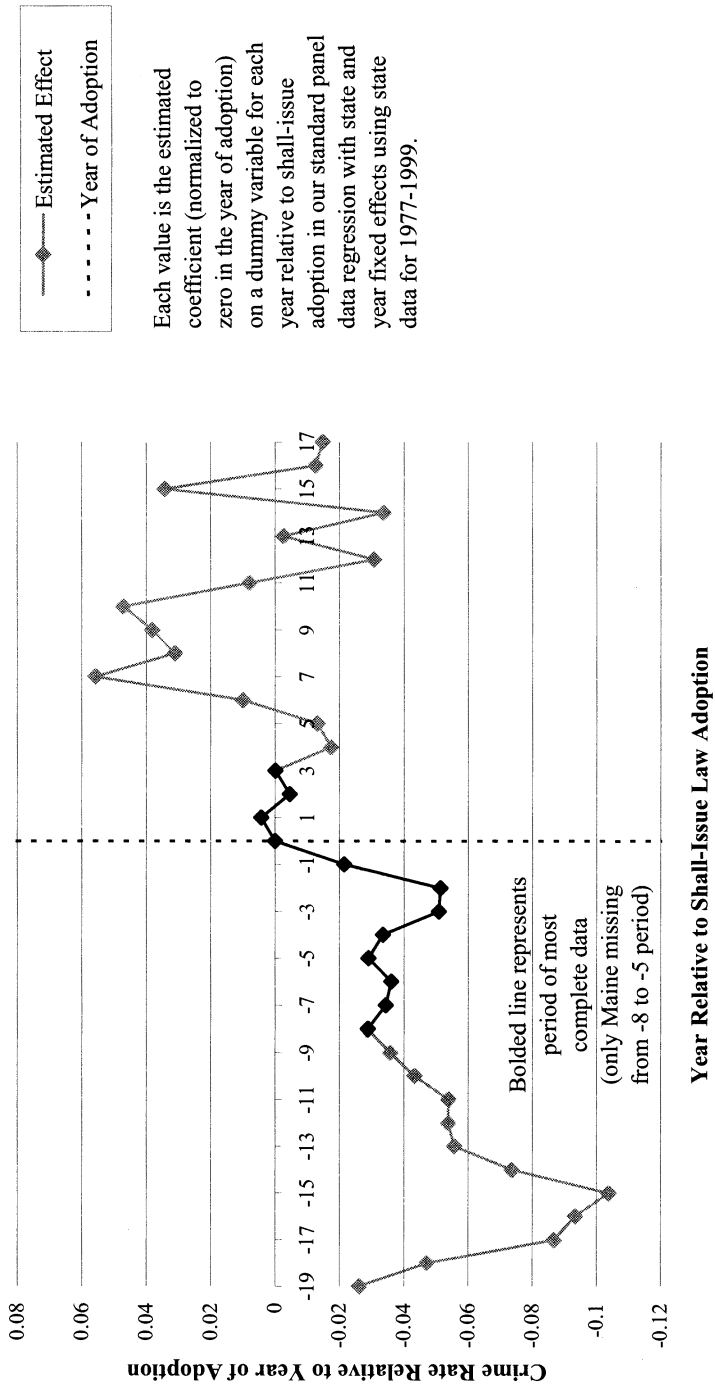
FIGURE 31: LARCENY—NORMALIZED EFFECT BY YEAR RELATIVE TO ADOPTION (VERNICK'S CODING)



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E. *Problems with Endogenous State Adoption*

A major concern of any analysis on the effect of shall-issue laws on crime is the possibility that crime correlates with shall-issue law adoption in a noncausal manner. Indeed, examining the graphs of Figure 3, one can see that in four of the five violent crime categories and for burglary, even before adopting states passed their shall-issue legislation, crime was substantially higher than the regression model would have otherwise predicted (given the full array of explanatory variables). This raises concerns about the reliability of the regression model, because the higher crime rate in the prepassage years may suggest that the laws were passed because crime was higher. That would imply that shall-issue law adoption is, in fact, endogenous to the crime rate. Lott is aware of this problem and indeed confirms it in his book, noting that shall-issue laws "have so far been adopted by relatively low-crime states in which the crime rate is rising."[69] He attempts to use the appropriate two-stage least squares (2SLS) technique to address the problem, and he states that his "results differ from all my previous estimates in one important respect: Nondiscretionary concealed-handgun laws are associated with large, significant declines in all nine categories."[70]

As Willard Manning and others have emphasized, the problem is that the statistical demands for the successful use of the 2SLS approach are exacting, and it is unlikely that Lott has satisfied them since his instruments cannot be considered convincingly exogenous.[71] Lott creates his instrumental variable by regressing the presence of a shall-issue law on violent and property crime rates and the change in those rates, percent of state population in the National Rifle Association (NRA), percent of state population voting for Republican presidential candidate, percent of blacks and whites in state population, total state population, dummies for the South, Northeast, and Midwest, and year dummies.[72]

The manner in which Lott performed his 2SLS analysis suggests that he considers his main instruments to be the violent and property crime rates and

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69. LOTT, *supra* note 2, at 120.

70. *Id.* at 118.

71. As Manning notes:

> The major requirements for the instrumental variables in the linear model to yield consistent estimates of the effect of the endogenous explanatory variable on the outcome of interest are the instruments correlated with the endogenous explanatory variable(s); the instruments do not conceptually belong in the equation of interest nor are they proxies for variables which should be in the equation of interest but are omitted from the specification; the instrument is uncorrelated with the error term in the equation of interest; and the instruments are not weak in the sense of Staiger and Stock (1997) or Bound and others (1995).

Manning, *supra* note 64, at 335 n.75.

72. LOTT, *supra* note 2, at 118.

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the change in those crime rates. However, these variables are obviously endogenous and cannot serve as unbiased instruments. His NRA and Republican presidential election variables are the most compelling, but these too are at least potentially endogenous. These variables correlate with factors such as an area's wealth and criminal tendencies, which in turn affect crime directly. We attempted to replicate Lott's 2SLS results using state-level data to determine how much of an effect the first-stage crime rate variables were having on the final results.[73]

Table 9, line 1 presents our replication of Lott's 2SLS estimates for the 1977-1992 time period. According to this method, shall-issue law adoption reduces violent crimes by 61%, murders by 43%, rapes by 20%, robberies by 51%, and aggravated assaults by 64%.[74] While these numbers should seem implausibly large, we were gratified to see that we had basically succeeded in replicating Lott's 2SLS results. While the excessively large predicted effects have led a number of researchers to dismiss these results immediately, we go on to show that the results are entirely driven by the inclusion of the inappropriate instruments: the logs of violent and property crime. Line 2 of Table 9 presents the results of the identical 2SLS regressions of line 1, except excluding the logs of crime rates as instruments. Notice that some of the most extreme negative coefficients from line 1 drop drastically. For example, the coefficient estimated for violent crime drops from –0.94 to –0.09, and that for aggravated assault drops from –1.18 to –0.12. The dramatic changes in the 2SLS estimates when the most flawed instruments are dropped indicates, as we suggested above, that this endogenous set of instruments was severely biasing

73. While it is unclear exactly how Lott performed these regressions, it appears that he instrumented the shall-issue law dummy using only the variables mentioned above, deviating from standard 2SLS procedure by failing to include all of the exogenous variables used in the second stage of the regressions. If he did include all the exogenous variables, then one would question introducing the regional dummy variables and the racial demographic percentages into the first-stage equation given the existing state dummy variables and extensive demographic controls. However, since excluding the exogenous variables from the first-stage regression can possibly bias the estimates, we avoided replicating this potential inaccuracy. Thus, there are a few differences between our regressions and Lott's. First, because we were unable to obtain NRA membership data, we were forced to leave this variable out. Fortunately, as we were able to roughly approximate Lott's results, this omission proved not to be a substantial problem. Second, instead of including the percentage of blacks and whites in the state population and the region dummies, we used the array of demographic variables and state dummies that Lott used in the second stage of his 2SLS regressions. Accordingly, our first stage consisted of regressing the shall-issue law dummy on the vector of demographic variables, the log of violent and property crime rates, the changes in the log of violent and property crime rates, and a vector of presidential election variables. This final vector was created in the exact way Lott created his: interacting the percentage of state population voting Republican with dummy variables for a four-year period around the election year. Since our effort is at replication, we also used Lott's coding of the shall-issue laws.

74. The precise estimated percentage effect on crime is given by the formula e^{B}-1, where B is estimated in Table 9. For values under about 0.2 in absolute value, B is a fairly good approximation of the proportionate change.

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the results. We then went one step further and performed another set of regressions that excluded the change in the crime rates as instruments, thereby leaving the matrix of political variables as the only remaining instrument. As shown in line 3 of Table 9, this approach generated estimates that shall-issue laws would strongly dampen murder, robbery, and aggravated assault rates.

The next three lines of Table 9 replicate these three 2SLS regressions for the 1977-1997 period. While the Lott approach to line 4 of Table 9 continues to show large drops in violent crime, rape, and aggravated assault, the line 4 numbers are much less favorable to the Lott and Mustard thesis than the line 1 estimates for murder, robbery, and all four property crimes. But while the drops in violent crime and aggravated assault resulting from the shall-issue laws that are estimated in line 4 are once again vastly too large to be plausible, the second and third lines calculated without clearly inappropriate instruments alter the results dramatically. Indeed, when the flawed instruments are dropped in lines 5 and 6, the estimated effect on crime of shall-issue law adoption is never significantly different from zero. Accordingly, the evidence from 2SLS estimation, in our view, offers no support for the more guns, less crime hypothesis.[75]

75. We are not claiming that the instruments used in our line 5 or 6 2SLS estimates meet the criteria for valid instruments set forth above. *See supra* note 71. Indeed, the political variable serving as the instrument in lines 3 and 6 is quite weak, raising the R-squared value in a first-stage regression by only 0.006 (from 0.815 to 0.821). We are confident, however, that the other instruments used by Lott that are dropped in the third regression for each time period in Table 9 profoundly violate these standards.

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TABLE 9: TWO-STAGE LEAST SQUARES ESTIMATES OF THE IMPACT OF SHALL-ISSUE LAWS, STATE DATA (LOTT'S CODING)

	Violent Crime	Murder	Rape	Aggravated Assault	Robbery	Property Crime	Auto Theft	Burglary	Larceny
Time Period (1977-1992) Shall Dummy Instruments:									
1. PV, CR, ΔCR	**-0.94**	**-0.56**	**-0.22**	**-1.18**	**-0.71**	-0.03	-0.23	**-0.17**	0.01
	(0.17)	(0.14)	(0.09)	(0.21)	(0.16)	(0.05)	(0.12)	(0.07)	(0.04)
2. PV, ΔCR	-0.09	**-0.43**	-0.04	-0.12	**-0.33**	-0.02	-0.15	-0.12	-0.01
	(0.08)	(0.16)	(0.10)	(0.12)	(0.14)	(0.06)	(0.14)	(0.09)	(0.06)
3. PV	-0.15	**-0.37**	-0.01	**-0.25**	**-0.32**	0.1	0.01	-0.00	0.11
	(0.08)	(0.16)	(0.11)	(0.12)	(0.15)	(0.07)	(0.14)	(0.09)	(0.06)
Time Period (1977-1997) Shall Dummy Instruments:									
4. PV, CR, ΔCR	**-0.68**	-0.12	**-0.26**	**-1.04**	-0.11	**0.21**	**0.29**	0.08	**0.19**
	(0.11)	(0.10)	(0.08)	(0.16)	(0.10)	(0.05)	(0.10)	(0.06)	(0.05)
5. PV, ΔCR	0.15	-0.02	-0.12	0.12	0.15	0.06	0.17	-0.10	0.05
	(0.11)	(0.13)	(0.10)	(0.12)	(0.14)	(0.07)	(0.15)	(0.09)	(0.06)
6. PV	-0.04	-0.05	-0.17	-0.12	0.04	0.10	0.21	-0.08	0.10
	(0.10)	(0.15)	(0.11)	(0.12)	(0.15)	(0.07)	(0.17)	(0.10)	(0.06)

Key to Dummy Instruments: PV=Political Variables CR=log(Crime Rates) ΔCR=Change in log(Crime Rates)

Notes: The dependent variable is the ln(crime rate) named at the top of each column. The data set is comprised of annual state-level observations. 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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III. MOVING FROM THE STATE TO THE COUNTY DATA

Up to this point we have focused exclusively on analyzing state data, which enabled us to use more current crime data—up through 1999. Lott and Mustard decided, however, to rest their analysis most heavily on county crime data rather than state crime data (although they do present some state data results as well).[76] The advantages of this approach are that (1) with over 3000 counties in the country as opposed to only fifty states, there is far more data with a county dataset than with a state dataset, which, *ceteris paribus*, should improve the precision of the estimates; and (2) the county fixed effects will explain a great deal of the fixed cross-sectional variation in crime across the country, thereby diminishing the inevitable problem of omitting some appropriate, but possibly unavailable, time-invariant explanatory variables.

There are also some disadvantages to using county data. First, the intervention of interest is generally a statewide phenomenon.[77] Using county data under these circumstances will likely exaggerate the amount of independent data available to the researcher, thereby possibly creating the appearance of statistical significance when in fact none exists.[78] Second, many of the explanatory variables are only measured on the state level, and, thus again, the county data analysis may be giving a false sense of precision. Third, Lott uses arrest rates (the ratio of arrests to crime in a county) as an explanatory variable, which leads to many counties being dropped from the analysis. This occurs because of missing arrest data in some cases, and also because the arrest rate is undefined for any county that experiences no crime in a particular category in a particular year (since the rate would have zero in the denominator in such cases). Thus, a substantial number of counties are thrown out of the Lott analysis by virtue of the realization of the dependent variable (if it is zero in a given year, that county is dropped from the analysis), which can potentially bias the results of the regression estimation.[79] Fourth, Maltz and Targonski

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76. *See, e.g.*, Lott & Mustard, *supra* note 1, at 55 (showing state-data-based estimates).

77. But not uniformly. Pennsylvania initially excluded Philadelphia from its 1989 shall-issue law. PA. STAT. ANN. tit 18, § 6109 (West 1989). In 1995, the law was extended to include Philadelphia. *See* 1995 Pa. Legis. Serv. 17 (West).

78. Indeed, when we reran both Lott's and our own specifications, clustering on the state level, we found dramatically lower statistical significance for the estimated impacts. The cluster procedure allows the regression to relate the variability across counties within a state for more refined estimation of the variance-covariance matrix. *See* STATA USER'S GUIDE 258-59 (Release 6). However, to be both conservative and consistent with Lott, all the regressions reported in this paper do not cluster by state.

79. The percentage of dropped observations (because of either missing numerators or zero denominators) vary for the individual crime categories, from a low of about 9% for auto-theft to a high of about 48% for murder. Fifty-seven percent of county/year observations have at least one of the arrest rates undefined or missing.

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consider the quality of UCR county-level data to be so poor that they dismiss Lott's work on that basis alone (at least if the data extend beyond 1992).[80]

By using our state data, we were able to extend our analysis through 1999, while our county dataset is only updated through 1997. But because there are some advantages to using county data and because Lott and Mustard rely most heavily on this data, we turn for the rest of this Article to an analysis of county data.

A. *Lott's County Data Analysis for 1977-1992*

1. *The dummy variable model.*

We begin by presenting Lott's initial regression results for county level data over the period from 1977-1992. Line 1 of Table 10 shows the predicted effect on nine crime categories using the dummy variable model. A quick examination of the line 1 results reveals that (1) four of the five categories of violent crime (the exception is robbery) have negative and statistically significant coefficients, suggesting that shall-issue laws reduce these types of violent crime by four to seven percent; and (2) all four property crimes have positive and statistically significant coefficients, suggesting that the laws increase property crime by two to nine percent. Lott accepts the regression results at face value and concludes that the passage of these laws causes criminals to shift from committing violent crime to committing property crime where, he argues, they are less likely to be shot since the victim is frequently not present when the crime occurs.[81] Thus, we see violent crime decreasing by 3.5% and murders falling by over twice that percentage, while property crime rises by over 5%. As we stressed in our 1999 review of Lott's work, however, the fact that robbery is not dampened by the adoption of a shall-issue law constitutes a major theoretical problem for Lott's interpretation of the results of the dummy variable model.[82] If there is to be the type of substitution away from violent crime that Lott predicts, one would expect that the new law would induce potential robbers to avoid confronting victims and shift to more stealthy property crime. Yet, as Table 10 reveals, we see no evidence of this effect.

80. Michael Maltz & Joseph Targonski, *A Note on the Use of County-Level UCR Data*, 18 J. QUANTITATIVE CRIMINOLOGY 297 (2002). Although in this portion of the Article we focus on county data for the period 1977-1997, we generally find, with some noted exceptions, broadly similar results when we either use the state dataset or confine the county results to the period not beyond 1992. We also note that to the extent, however unlikely, that the post-1992 break in the county crime data series to which Maltz and Targonski refer is relatively uniform across all counties, the year effects would control for this change. *See infra* note 90.

81. Lott & Mustard, *supra* note 1, at 18.

82. Ayres & Donohue, *supra* note 15, at 461.

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Hence, the dummy variable model undermines a key prediction that Lott offers to explain the Table 10, line 1 regression results for the period of 1977-1992.

In table 4.1 of his book,[83] Lott presents a version of the line 1 robbery regression showing that shall-issue laws reduce robbery by 2.2%, which is indicated to be statistically significant at the 0.10 level (considered marginally significant). But we have shown in previous work that this –2.2% figure is an error that results from a miscoding of the effect of the shall-issue laws. The problem with Lott's analysis was that, instead of following his articulated strategy of assuming that the effect of the law would emerge in the first year after passage, Lott coded the shall-issue law in that fashion only for Florida and Georgia, with all other states being coded so that the effect of the law begins in the year of passage. Correcting this error to adhere consistently to the articulated Lott protocol wipes out the size and significance of the estimated effect on robbery.[84]

83. LOTT, *supra* note 2, at 51.

84. Ayres & Donohue, *supra* note 15, at 451. We replicated Lott precisely with the coding error and then showed how the correction eliminates the robbery effect. The line 1 regressions in Table 10, *supra*, are identical to Lott's table 4.1 results with three exceptions, which are maintained in all the regressions presented here: (1) the coding error is corrected; (2) standard errors are corrected to adjust for heteroscedasticity; and (3) as noted *supra* note 51, one explanatory variable—the measure of the real per capita income maintenance, SSI and other, for those over sixty-five—was dropped. One can compare the results in table 1 of Ayres & Donohue, *supra* note 15, with those of Table 10 here to see that the only one of these changes that influences the basic story is the correction for the coding error.

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TABLE 10: THE ESTIMATED IMPACT OF SHALL-ISSUE LAWS ON CRIME, COUNTY DATA (LOTT'S CODING)

	Violent Crime	Murder	Rape	Aggravated Assault	Robbery	Property Crime	Auto Theft	Burglary	Larceny
Lott's Time Period (1977-1992)									
1. Dummy variable model:	**-3.5%**	**-7.4%**	**-4.7%**	**-5.3%**	-0.2%	**5.2%**	**8.7%**	**2.3%**	**5.9%**
	(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%
	(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%
	(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%
	(0.8%)	(1.5%)	(0.9%)	(1.0%)	(1.1%)	(0.6%)	(1.2%)	(0.6%)	(1.2%)
Enitre Time Period (1977-1997)									
4. Dummy variable model:	0.0%	**-7.7%**	**-3.2%**	-0.3%	-0.3%	**7.6%**	**10.8%**	1.6%	**9.4%**
	(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%**
	(0.2%)	(0.5%)	(0.4%)	(0.4%)	(0.4%)	(0.2%)	(0.4%)	(0.3%)	(0.4%)
6. Hybrid model:									
Postpassage dummy	0.0%	**6.9%**	**5.9%**	**5.9%**	3.6%	-0.7%	**9.0%**	**4.3%**	**5.3%**
	(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%**	**-3.4%**	**-4.1%**	-0.3%	**-1.9%**	**-3.1%**	**-1.7%**
	(0.3%)	(0.7%)	(0.5%)	(0.6%)	(0.6%)	(0.2%)	(0.6%)	(0.4%)	(0.5%)

Notes: The dependent variable is the ln(crime rate) named at the top of each column. The data set is comprised of annual county-level observations. County- and year- fixed effects are included in all specifications. All regressions are weighted by county population. 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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2. *Lott's trend (or spline) model.*

Lott responds to the point that shall-issue laws seem not to dampen robberies in the dummy variable model by arguing that a model that captures the change in the linear trend of crime, the Lott spline model, provides a better picture of the effect of the passage of a shall-issue law in this case. The only numbers that Lott reports in his book concerning his trend analysis are found in a single row of numbers representing the difference between the before-passage linear trend and after-passage linear trend for the states that passed shall-issue laws (hereinafter "passing states") (appearing in the book's table 4.8). Lott's regressions include year effect dummies, so the pre- and postpassage trend coefficients would capture linear movements in crime in the ten passing states apart from the general movements in crime for the nation as a whole (which would be captured by the general year dummies). Lott's message in his table 4.8 is that a trend analysis shows that shall-issue laws lower all crime categories—both violent and property—and in all cases but one (larceny) the reduction is statistically significant.[85]

Line 2 of Table 10 follows Lott's lead in testing whether there is a break in the linear crime trend in the year of passage.[86] We use a spline specification that is somewhat easier to interpret (than Lott's difference in pre- and posttrend test) because the statistical significance of any induced change in trend can be ascertained directly from the regression output (the t-statistic for the postpassage linear trend coefficient). Even though Lott did not use a spline

85. LOTT, *supra* note 2, at 76.

86. Panel A2 of Appendix Table 6 reproduces the methodology (although not the reported results) of the Lott trend analysis and reports both the before and after linear time trends (as well as their difference, which is presumed to be the estimated effect of the law). The discrepancies between Lott's table 4.8 and Appendix Table 6.A2 result from two different errors by Lott. First, Lott has informed us that he mistakenly wrote down the "Shall Trend After" coefficient for violent crimes instead of reporting the difference between the before and after coefficients. Second, Lott has erred in his reporting of the statistical significance of these effects, perhaps because he did not correct the standard errors for the presence of heteroscedasticity in the panel data. Lott reported that the change in trend owing to the shall-issue law for violent crime, aggravated assault, and auto theft was statistically significant at the level shown in the top row of Appendix Table 6. But Appendix Table 6.A2 shows, however, that the differences for these three crime categories were all insignificant.

This Appendix Table 6.A2 replication of Lott still ostensibly finds that the law generates a statistically significant reduction in the time trends (at, at least, the five percent significance level) in five of the nine crime categories tested. But Lott's regressions also incorrectly identify the passage date of three jurisdictions that adopted shall-issue laws, which makes the laws look more effective than they are. Lott coded the enactment dates in Oregon, Pennsylvania, and Virginia earlier than was proper. Appendix Table 1 shows discrepancies in Lott's shall-issue coding in his own dummy variable and trend models (columns B and C). The corrected regression estimates are presented in Appendix Table 6.A3, which shows that the shall-issue laws statistically significantly reduce crime in only three of the nine categories (murder, rape, and robbery). *Id.*

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specification himself, we call this the Lott spline model because it is equivalent to the linear trend model that he did employ in which he estimated pre- and postpassage trends and then subtracted them.[87]

Note that the story in line 2 of Table 10 is changed in a number of respects from that of line 1 (the dummy variable model). Instead of all violent crime (but robbery) falling and property crime rising, line 2 suggests that shall-issue laws have no effect on property crime (or overall violent crime and aggravated assault), but dampen murder, rape, and the heretofore unaffected robbery. Lott's discussion of the impact of shall-issue laws causing criminals to shift from committing violent to committing property crime is no longer central if the Lott spline analysis (Table 10, line 2) is the appropriate estimation approach.

3. *The hybrid model testing for main and trend effects.*

The different results between the dummy and trend models suggests the advisability of employing the more general hybrid model, which will enable us to test whether either of the more constrained models is statistically preferable. Consequently, we estimate regression 3 in Table 10, which is a less-constrained specification than either the dummy variable or the linear specification because it allows (and tests) for the existence of both a once-and-for-all announcement effect as well as a changed (linear) trend effect.[88] But for the four violent crime categories, we see a pattern that is potentially problematic—the main effect of the shall-issue laws is positive but over time this effect gets overwhelmed as the linear trend turns crime down. In other words, according to the hybrid model, in the year after passage the main effect of the shall-issue law is a 6.7% increase in violent crime, which is dampened by the 2% drop associated with the negative trend variable, for a net effect of 4.7% higher crime. After three and a half years, the conflicting effects cancel out at which point crime begins to fall. Our antennae are raised when we see this particular result of a positive main effect and a negative trend effect since there are empirical reasons to believe that this observed pattern is not truly reflecting an initial acceleration in crime followed by a drop, but rather model misspecification resulting from the selective dropping of states from the postpassage period in a way that tilts the trend regression line downward inappropriately.[89] This may suggest that even the most general form of the

87. For further explanation of this spline specification, see discussion *supra* text accompanying notes 42-50.

88. Contrary to the results of regression 1, the hybrid model confirms the prediction of regression 2 that the shall-issue laws have virtually no effect on any of the property crime categories (although we will soon see that this finding breaks down when we extend the analysis through 1997 in regression 6 of Table 10).

89. 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

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three crime models is still misspecified and, hence, that its results are unreliable.

B. *Extending Lott's County Data Through 1997*

Lott's initial analysis using 1977-1992 data captured the period in which only 10 states newly adopted shall-issue statutes, and therefore Lott's regression results should be taken as the predicted effect of the adoption of the law in these ten states. Between 1992 and 1996, however, fourteen more jurisdictions (thirteen states and Philadelphia) adopted the law (as shown in Column A of Appendix Table 1), and therefore one might hope to gain more accurate results by extending the period over which the effect of the law is estimated. Regressions 4-6 in Table 10 simply repeat the models of regressions 1-3 but now estimating them over the longer time period from 1977-1997 (and thus measure the effect of adoption of the law in twenty-three states and the city of Philadelphia).[90] Comparing (the dummy variable model in) lines 1 and 4 of Table 10, we see that adding more years of data weakens Lott's story. Importantly, violent crime is no longer negative, so the basic argument that the prospect of meeting armed resistance shifts criminals from violent crime to property crime is undermined. Lott might respond that murders fall by nearly eight percent and rape by over three percent, as murderers and rapists shifted over to committing property crime, thereby raising its prevalence by eight percent. But the suggestion that this pattern could be explained by the changed behavior of would-be murderers and rapists is not compelling.[91]

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in a straightforward manner: The announcement of the law scared potential criminals, thereby dampening crime initially, but as more guns got out on the street and/or as the fear subsided, crime ultimately turned up.

90. In the second edition of his book, Lott analyzes four additional years of data that allow him to test the effects of shall-issue laws in 13 additional states. LOTT, *supra* note 2, at 90. But he only reports results for this dataset from tests of the trend specification. It is important to emphasize that combining these later years of data with the original dataset is potentially problematic. The 1994 codebook for the (NACJD) crime data that both Lott and we use explicitly notes under a major heading, "Break in Series," that describes a new imputation procedure it will use from 1994 on and cautions: "These changes will result in a break in series from previous UCR county-level files. Consequently data from earlier year files should not be compared to data from 1994 and subsequent years . . . ." Maltz & Targonski, *supra* note 80, at 309 (quoting FBI, UNIFORM CRIME REPORTING PROGRAM DATA [UNITED STATES]: COUNTY-LEVEL DETAILED ARREST AND OFFENSE DATA, 1994, ICPRS 6669).

If the break in series caused a uniform jump up or down in crime that applied to all jurisdictions, then our year dummies would control for this problem. Unfortunately, it is generally unlikely that errors in crime data would be uniform across the country (or even random across the country); so the break in the series is a concern. *See supra* note 73 (discussing newer data).

91. Consider the case of Florida—one of the states that statistically is most conducive to the Lott story because the murder rate fell after the passage of a shall-issue law in 1987. Assume that every murder and rape that would have occurred but for the shall-issue law

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Indeed, the idea that a thwarted rapist would decide to switch to property crime because rape had become more dangerous (to the perpetrator) seems rather fanciful. The issue is important for the following reason. The dummy variable model regression on the full 1977-1997 period gives very strong significant results on a number of crimes—murder and rape being negative and property crime, auto theft, and larceny being positive. Yet if the theoretical explanation for this substitution pattern in crime is flawed, then the fact that we cannot believe the regression finding of a large jump in property crime as an effect of the shall-issue law suggests that we should not believe the accompanying regression finding of a substantial drop in murders and rapes. Again, the possibility of model misspecification seems to be a serious concern.

Interestingly, while adding five years of data weakens Lott's argument based on the dummy model (Table 10, line 1 versus line 4), it actually strengthens his story using the spline analysis (compare lines 2 and 5 in Table 10). Thus, we see in line 5 that every crime except property crime is both negative and significant. Moreover, in contrast to both dummy variable models, the Lott spline estimated effect for robbery for both time periods is negative and significant—an almost indispensable finding if the Lott deterrence story is in fact true.

The added five years of data also has a considerable impact on the estimates generated by the hybrid model. For Lott's initial time period, the hybrid model (Table 10, line 3) basically suggested that shall-issue laws were not affecting property crime or robbery but were having the potentially problematic conflicting effects on four other violent crimes. For the full time period, however, the potentially troubling pattern previously observed for violent crime also shows up for property crime (Table 10, line 6). For example, in the year after passage the line 6 regressions indicate that shall-issue laws increase murders by over 3% (the main effect of 6.9%, less the trend effect of 3.5%). Rather than seeing shifting from violent to property crime, we observe similar patterns for a number of crimes in which early increases in crime are followed by subsequent drops after a number of years. The primary conclusion that emerges from these six regressions is that there is a considerable degree of instability in the predicted effects of shall-issue laws as one adds five extra years of data or switches among the two models presented by Lott or the third hybrid model which includes both a dummy variable and a time trend effect.

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would have been committed by a different individual (that is, there were no multiple offenses for either of these crimes). This would imply that a total of 299 individuals did not commit a murder or rape (106 fewer murders, 193 fewer rapes) because of the law but that they each committed over 225 property crimes instead, thereby elevating property crime by 68,337. If a number of these rapes and murders represented multiple crimes by the same individual, then the number of property crimes that would have had to have been committed by each former rapist/murderer to generate this large property crime increase would rise commensurately. It seems unlikely that the shall-issue law could explain an increase in the amount of property crime of this magnitude.

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Because the hybrid model nests both the dummy variable and the spline models, it is possible to test whether the data reject the implicit constraints imposed by these more-restrictive specifications. Specifically, we can test the spline specification's implicit assumption that there is no announcement effect by looking to see whether the dummy variable coefficient in the hybrid model is statistically different than zero. The data strongly reject the spline specification in six of the nine hybrid regressions (in specification 6). The dummy variable model analogously assumes that there is no spline effect. This assumption is also rejected in eight of the nine hybrid regressions.[92]

In sum, the foundation of the Lott thesis essentially is captured in regressions 1 (dummy variable model) and 2 (spline model) of Table 10. While these results are not identical to those presented in Lott's book, these regressions are probably more authoritative because some apparent coding errors by Lott have been corrected. The results are not as stable as one might like, but if one were to examine only those two regressions, the evidence would tend to support Lott's thesis. Obviously, the analyst's task would be easiest if the regressions generated by three different models (dummy, spline, hybrid) for two different time periods (1977-1992 and 1977-1997) all conveyed essentially the same picture. Unfortunately, they do not. Importantly, both the dummy variable and spline models are essentially rejected by the data by virtue of the large and statistically significant positive effects on both terms in the hybrid models (Table 10, lines 3 and 6)—particularly for the full dataset. But the hybrid model's prediction of initial jumps in crime followed by subsequent declines in response to the adoption of a shall-issue law raises our concern about model misspecification as states that had adopted shall-issue laws close to the end of our data period drop from the estimates of the late postpassage effects.

C. *Replicating Table 10 While Controlling for State Trends*

We have previously discussed how one must take care to avoid generating statistically significant results that are really the spurious consequence of preexisting trends (for example, where an upward trend in crime is confused for a postpassage increase in crime). Table 11 replicates the Table 10 results while

92. It is interesting to compare the results of Table 3 (which uses state data for 1977-1999 and the incarceration rate instead of the arrest rate) with those of Tables 10 and 11 (which use county data for 1977-1997 and the arrest rate). Beginning with the dummy variable model and no state trends (Tables 3 and 10), the results are similar in each, although in the county data one sees stronger effects of dropping violent crime and increasing property crime. For the dummy model with state trends (Tables 3 and 11), though, the state results show drops in rape and aggravated assault and increases in auto theft and larceny, while the county results show only an *increase* in aggravated assault. When the hybrid model is used with state trends, the county results are clearly stronger for the Lott and Mustard thesis than the state data results—unless one is troubled by the positive main effects and negative trends found for the county data.

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controlling for these state trends. The first thing to note as we compare the Table 10 and 11 results for the dummy variable model in 1977-1997 is that without controlling for state trends, the shall-issue laws were seen to increase crime in three property crime categories and decrease in two violent crime categories, while after adding such controls the only statistically significant effect is that aggravated assault increases. The hybrid results for the same period tend to show positive main effects followed by negative trend effects, which raises the specification concerns we expressed earlier in our discussion of the end of period drops in estimated crime effects seen in the graphs of Figure 3. If we take that pattern as accurate, though, then after two to three years shall-issue laws would reduce murders, rapes, robberies, auto theft, and burglaries (while increasing aggravated assaults and larceny).

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TABLE 11: THE ESTIMATED IMPACT OF SHALL-ISSUE LAWS ON CRIME CONTROLLING FOR STATE TRENDS
COUNTY DATA (LOTT'S CODING)

	Violent Crime	Murder	Rape	Aggravated Assault	Robbery	Property Crime	Auto Theft	Burglary	Larceny
Lott's Time Period (1977-1992)									
1. Dummy variable model:	0.1%	**-8.7%**	-1.5%	3.4%	**-7.5%**	-1.4%	-1.2%	**-3.6%**	0.6%
	(1.6%)	(3.4%)	(2.1%)	(2.0%)	(2.2%)	(2.1%)	(2.2%)	(1.4%)	(4.5%)
2. Hybrid model:									
Postpassage dummy	**6.9%**	5.8%	5.5%	**6.0%**	6.3%	-0.1%	5.2%	1.1%	-3.2%
	(2.3%)	(5.3%)	(3.1%)	(3.0%)	(3.4%)	(1.9%)	(2.9%)	(2.0%)	(3.0%)
Trend effect	**-3.1%**	**-6.6%**	**-3.2%**	-1.2%	**-6.3%**	-0.6%	**-3.0%**	**-2.2%**	1.7%
	(0.8%)	(1.8%)	(1.1%)	(1.0%)	(1.3%)	(1.1%)	(1.2%)	(0.8%)	(2.5%)
Enitre Time Period (1977-1997)									
3. Dummy variable model:	1.5%	-0.2%	2.6%	**7.1%**	0.0%	-0.7%	3.5%	0.5%	4.0%
	(1.5%)	(2.3%)	(1.6%)	(1.8%)	(1.8%)	(1.3%)	(2.0%)	(1.3%)	(2.3%)
4. Hybrid model:									
Postpassage dummy	0.7%	**5.6%**	**6.6%**	**6.6%**	**5.2%**	-1.5%	**6.6%**	**4.4%**	**4.4%**
	(1.5%)	(2.7%)	(2.0%)	(2.2%)	(2.2%)	(1.2%)	(2.2%)	(1.7%)	(2.1%)
Trend effect	0.5%	**-3.9%**	**-2.7%**	0.4%	**-3.5%**	0.5%	**-2.1%**	**-2.7%**	-0.3%
	(0.4%)	(0.8%)	(0.6%)	(0.6%)	(0.7%)	(0.4%)	(0.7%)	(0.5%)	(0.7%)

Notes: The dependent variable is the ln(crime rate) named at the top of each column. The data set is comprised of annual county-level observations. County- and year- fixed effects are included in all specifications. All regressions are weighted by county population. 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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IV. ESTIMATING STATE-SPECIFIC PASSAGE EFFECTS

On the surface, the panel data regressions in Table 10 (and Table 11 for the hybrid model) appear to establish a prima facie case that shall-issue laws reduce crime (or, at least in the dummy variable model, reduce violent crime while increasing property crime). But all of the regressions presented so far have estimated an aggregated effect for the laws across all adopting states. It is well known, however, that aggregation can at times lead to misleading conclusions in statistical studies.[93] For example, the model would be misspecified if one tried to estimate a uniform effect from the shall-issue law while the law had systematically different effects across states. Moreover, as we have noted, the dangers of estimating a single aggregated effect are particularly acute in this case because a state that adopts a shall-issue law early in the data period will contribute fully to the estimated postpassage effect, while a state that adopts near the end of the period will have little weight. Since we know that the late adopters tended to experience crime increases, the aggregated analysis will give less weight to these states in estimating the overall effects of shall-issue laws.[94] Thus, what might look like a changing effect over time from the passage of the law may simply be a compositional effect as certain states drop out of the analysis.

One way to avoid these aggregation and compositional biases is to change the specification to estimate a state-specific effect for each state that adopts a shall-issue law.[95] In other words, we include in our regression for each crime category a separate postpassage dummy for each adopting state (as opposed to a single postpassage dummy pertaining to all adopting states). Building on our previous dummy variable model with state fixed trends (Table 11), we now use the full 1977-1997 county dataset to estimate the effect on nine crime categories for twenty-four jurisdictions that adopted shall-issue laws—a total of 216 estimates. Table 12 presents all of these estimates for all nine crime

93. Simpson's Paradox, also sometimes referred to as aggregation bias, is just one such example. *See* P.J Bickel, E.A. Hammel & J.W. O'Connell, *Sex Bias in Graduate Admissions: Data from Berkeley*, 187 SCIENCE 398, 400-01 (1975) (showing that while aggregate data suggested bias against female graduate applicants, the disaggregated data by department reversed this conclusion).

94. If we run the Table 10 regression on just the years 1991 to 1997, we find statistically significant increases in all nine crime categories.

95. Black & Nagin, *supra* note 3, at 211, were the first to run a disaggregated regression estimating state specific impacts of the shall-issue law. However, their analysis was limited to Lott's initial dataset, which only allowed them to test for the impacts on 10 passing states. They reported substantial heterogeneity in the law's impact for a sample limited to large counties, but their results differ from ours in that (for their limited dataset) the state specific impacts were more beneficial, but the impacts tended to be less statistically significant. They also limited their analysis to the dummy specification—which may misestimate the true impact of the law (because of Lott's inverted V argument or some other misspecification). In Table 12, we include state fixed trends as an added control.

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categories, while Figures 4 through 6 graphically depict the results for violent crime, murder, and property crime.[96] These state-specific "dummy variable" coefficients represent an even less-constrained specification than we saw in line 4 of Table 11 in that they separately estimate the impact of shall-issue laws for each of the twenty-four jurisdictions that passed the laws between 1977 and 1997. The Table 12 results reject the more constrained specifications of the aggregate regressions, which implicitly assumed that the impact of the shall-issue law was constant across jurisdictions. More importantly, the state specific estimates frequently undercut Lott's more guns, less crime thesis.

For every crime type there are more states where shall-issues laws produce a positive and statistically significant coefficient than states that produce a negative and statistically significant coefficient. For example, as Figure 4 reveals, while there are three states that experience a statistically significant drop in violent crime upon passage of the law, there are five states that experience a statistically significant increase. Overall, there are almost twice as many jurisdictions with an estimated increase in violent crime (fifteen) as those with an estimated decrease (nine).[97]

The second column of Table 12 and Figure 5 both reveal an even more dramatic disparity for murder: There are eight states with a statistically significant *increase* in murder while only four states exhibit a statistically significant decrease. Of the twelve jurisdictions that experienced a statistically significant effect on property crime from the passage of a shall-issue law, Figure 6 shows that eight experienced an *increase* in crime.

Stepping back, we see that out of the 216 estimated impacts in Table 12 (twenty-four jurisdictions by nine crime categories), sixty-eight exhibited statistically significant increases in crime while only twenty-seven exhibited statistically significant decreases. Overall, Table 12 shows 141 increases in crime versus only seventy-five decreases. The striking implication from this disaggregated analysis is that, according to this model, shall-issue laws increased crime in substantially more jurisdictions than they decreased crime.[98]

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96. As noted above, Philadelphia is treated as a separate jurisdiction, because the law became effective in the city of Philadelphia at a different time than for the rest of Pennsylvania. For convenience, we will still refer to state-specific estimates in referring to these 24 jurisdictions. *See supra* note 77.

97. Figure 4 also shows the estimated effect for the corresponding aggregated model (analogous to line 4 of Table 11), which is 1.5% (albeit statistically insignificant).

98. The same story comes through if one uses the Table 12 analysis on state data. In that event, there are only 207 effects (nine regressions x 23 jurisdictions) because we do not treat Philadelphia separately in the state data regressions. We find that there are 37 significant increases in crime versus nine significant decreases. Overall, 122 of the 207 effects were positive versus 85 negative.

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TABLE 12: THE JURISDICTION-SPECIFIC ESTIMATED IMPACT OF SHALL-ISSUE LAWS ON CRIME, DUMMY MODEL CONTROLLING FOR STATE TRENDS IN CRIME, COUNTY DATA (LOTT'S CODING)

Entire Period (1977-1997)	Violent Crime	Murder	Rape	Aggravated Assault	Robbery	Property Crime	Auto Theft	Burglary	Larceny
Maine (1985)	**-15.1%**	17.7%	9.8%	**-23.5%**	-10.9%	-6.3%	-6.2%	**-17.6%**	-7.2%
	(5.6%)	(17.8%)	(8.8%)	(7.0%)	(9.3%)	(3.4%)	(5.1%)	(4.0%)	(4.5%)
Florida (1987)	**-12.3%**	**-30.9%**	-9.1%	**11.9%**	**-15.4%**	-6.6%	9.4%	0.7%	18.7%
	(3.1%)	(7.3%)	(6.3%)	(5.1%)	(7.6%)	(7.5%)	(7.3%)	(5.7%)	(17.6%)
Virginia (1988)	-1.7%	9.4%	6.9%	-1.3%	-4.4%	3.6%	4.2%	0.4%	2.4%
	(3.0%)	(8.1%)	(4.3%)	(3.5%)	(3.7%)	(2.9%)	(4.3%)	(2.3%)	(4.4%)
Georgia (1989)	-4.5%	0.8%	**-14.8%**	1.4%	**-11.9%**	**-8.7%**	-6.5%	**-9.2%**	**-11.2%**
	(3.8%)	(7.7%)	(5.1%)	(4.1%)	(4.8%)	(3.1%)	(4.3%)	(3.3%)	(4.1%)
Pennsylvania (1989)	0.7%	-2.9%	2.7%	1.3%	4.1%	**5.9%**	0.6%	**4.5%**	**6.1%**
	(2.8%)	(7.0%)	(3.3%)	(3.7%)	(3.6%)	(2.0%)	(3.7%)	(2.1%)	(2.7%)
Philadelphia (1995)	9.0%	14.2%	3.7%	-0.6%	16.2%	-2.9%	10.7%	-7.1%	-4.3%
	(6.3%)	(10.3%)	(6.5%)	(7.0%)	(9.6%)	(9.8%)	(11.2%)	(7.6%)	(5.6%)
West Virginia (1989)	**15.2%**	-0.8%	6.5%	**26.0%**	-9.8%	**8.1%**	**-17.8%**	**9.5%**	**9.5%**
	(5.8%)	(10.1%)	(8.8%)	(7.9%)	(6.6%)	(2.9%)	(4.8%)	(3.2%)	(4.1%)
Idaho (1990)	5.5%	-4.2%	**23.9%**	5.6%	14.3%	3.6%	**10.7%**	-4.2%	1.0%
	(5.9%)	(20.3%)	(6.7%)	(6.4%)	(9.4%)	(3.2%)	(5.1%)	(4.1%)	(4.0%)
Mississippi (1990)	**34.9%**	**20.5%**	11.6%	**30.5%**	**39.5%**	11.7%	**36.2%**	**18.8%**	6.2%
	(8.3%)	(8.0%)	(7.7%)	(9.7%)	(7.8%)	(6.9%)	(8.6%)	(7.0%)	(7.5%)
Oregon (1990)	2.3%	**-24.0%**	-7.3%	**21.5%**	**-33.8%**	-7.0%	-10.1%	**-25.0%**	-4.5%
	(5.0%)	(9.6%)	(5.0%)	(7.7%)	(5.5%)	(4.0%)	(5.6%)	(4.8%)	(4.6%)
Montana (1991)	17.7%	**-60.0%**	-29.3%	27.6%	-3.1%	1.0%	0.2%	2.7%	-2.6%
	(19.6%)	(26.1%)	(30.3%)	(20.7%)	(29.6%)	(14.9%)	(15.2%)	(13.9%)	(15.7%)
Alaska (1994)	-0.8%	20.1%	-29.1%	8.0%	1.5%	10.3%	-4.4%	-3.4%	4.9%
	(16.8%)	(25.5%)	(19.7%)	(19.2%)	(14.6%)	(12.0%)	(16.7%)	(18.1%)	(10.4%)
Arizona (1994)	9.3%	**19.3%**	8.2%	6.6%	**21.2%**	**10.2%**	**22.9%**	**18.4%**	**9.9%**
	(5.1%)	(7.2%)	(6.1%)	(5.9%)	(5.9%)	(4.3%)	(10.4%)	(4.8%)	(4.1%)
Tennessee (1994)	**18.5%**	**20.9%**	**15.0%**	**25.6%**	**16.0%**	**11.2%**	11.3%	**13.5%**	**14.5%**
	(5.2%)	(6.2%)	(5.8%)	(6.2%)	(4.8%)	(3.7%)	(5.8%)	(4.1%)	(3.9%)
Wyoming (1994)	-3.6%	12.6%	10.8%	-7.3%	**23.6%**	**7.4%**	13.0%	**15.3%**	2.9%
	(7.6%)	(17.9%)	(10.3%)	(9.8%)	(12.0%)	(3.6%)	(6.7%)	(5.6%)	(4.1%)
Arkansas (1995)	**15.7%**	2.8%	**12.8%**	**26.8%**	6.5%	-4.6%	-0.8%	-4.5%	5.9%
	(7.1%)	(8.1%)	(6.2%)	(9.2%)	(6.5%)	(2.6%)	(4.8%)	(4.1%)	(3.1%)
Nevada (1995)	18.2%	**42.6%**	11.6%	24.7%	**18.7%**	**13.3%**	**19.1%**	**25.9%**	**13.4%**
	(10.4%)	(13.1%)	(11.5%)	(15.9%)	(8.2%)	(5.5%)	(9.6%)	(7.2%)	(5.7%)
North Carolina (1995)	6.2%	7.2%	5.3%	**13.9%**	4.5%	**-4.7%**	**19.7%**	-5.0%	**7.8%**
	(3.3%)	(5.6%)	(4.1%)	(4.0%)	(4.0%)	(2.4%)	(3.5%)	(3.0%)	(2.7%)
Oklahoma (1995)	-1.4%	7.9%	**11.8%**	4.4%	**-9.5%**	-4.9%	**-10.1%**	-3.5%	4.7%
	(4.7%)	(8.4%)	(4.9%)	(5.2%)	(4.6%)	(3.3%)	(4.6%)	(3.8%)	(4.6%)
Texas (1995)	**-11.6%**	**-18.3%**	0.8%	-6.5%	**-11.9%**	**-12.4%**	**-16.0%**	**-8.8%**	-3.0%
	(3.7%)	(4.6%)	(4.0%)	(5.8%)	(4.6%)	(3.0%)	(5.1%)	(3.4%)	(4.0%)
Utah (1995)	**21.3%**	**34.1%**	**17.4%**	**25.1%**	**21.5%**	3.3%	**38.9%**	**23.1%**	-3.6%
	(4.9%)	(10.0%)	(7.7%)	(5.7%)	(7.2%)	(3.4%)	(9.9%)	(5.1%)	(4.1%)
Kentucky (1996)	-2.8%	**43.4%**	-12.1%	-2.1%	**29.8%**	**-8.5%**	12.6%	-11.5%	**-14.2%**
	(12.0%)	(13.8%)	(7.7%)	(20.6%)	(8.4%)	(4.3%)	(17.1%)	(8.5%)	(3.7%)
Louisiana (1996)	14.2%	**34.2%**	**21.7%**	13.9%	**31.9%**	**17.0%**	**32.2%**	**22.9%**	**13.4%**
	(7.7%)	(7.9%)	(7.2%)	(11.3%)	(11.0%)	(6.0%)	(8.7%)	(6.8%)	(6.8%)
South Carolina (1996)	7.2%	**14.6%**	-2.9%	**11.8%**	11.3%	**8.2%**	**20.8%**	5.7%	3.0%
	(5.3%)	(7.2%)	(4.8%)	(5.9%)	(6.0%)	(3.5%)	(7.0%)	(3.6%)	(3.7%)
Summary									Totals
Negative & Significant	3	4	1	1	5	4	3	4	2	27
Negative & Not Significant	6	3	6	5	4	6	5	7	6	48
Positive & Not Significant	10	9	11	9	7	6	8	4	9	73
Positive & Significant	5	8	6	9	8	8	8	9	7	68
Weighted Average Effect:	1.0%	0.6%	2.0%	7.2%	1.0%	-1.1%	4.7%	0.4%	4.0%	

Notes: Weighted Average Effect is calculated by weighting the state-specific coefficients by their average population over the time period. See also notes for Table 11.

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FIGURE 4: IMPACT OF SHALL-ISSUE LAW ON VIOLENT CRIME
JURISDICTION-SPECIFIC DUMMY MODEL WITH STATE TRENDS, COUNTY DATA

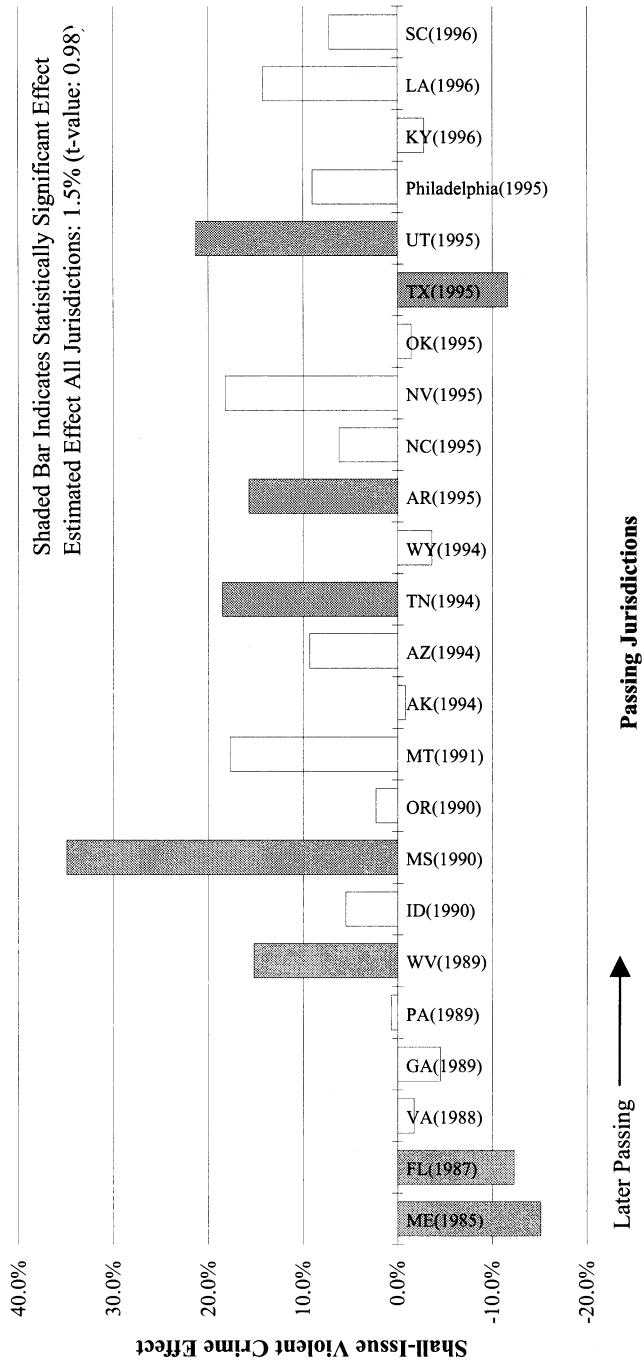

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FIGURE 5: IMPACT OF SHALL-ISSUE LAW ON MURDER
JURISDICTION-SPECIFIC DUMMY MODEL WITH STATE TRENDS, COUNTY DATA

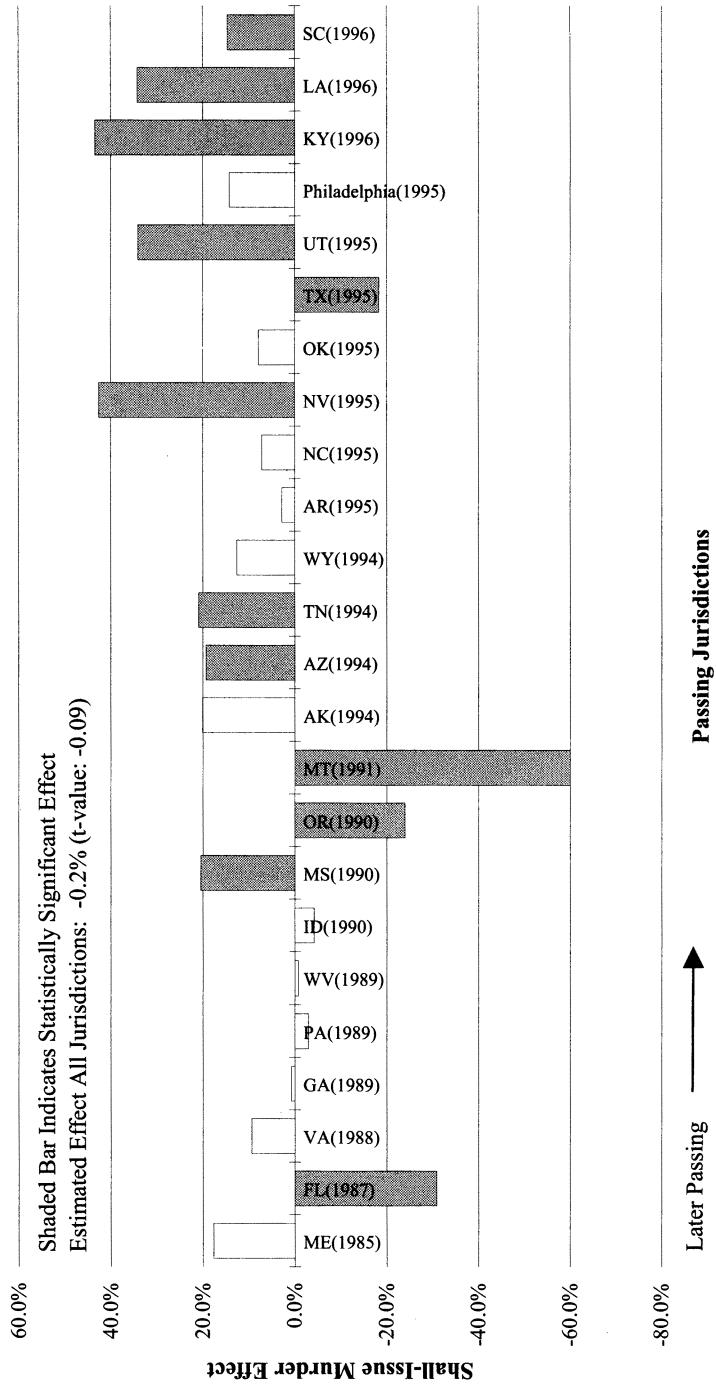

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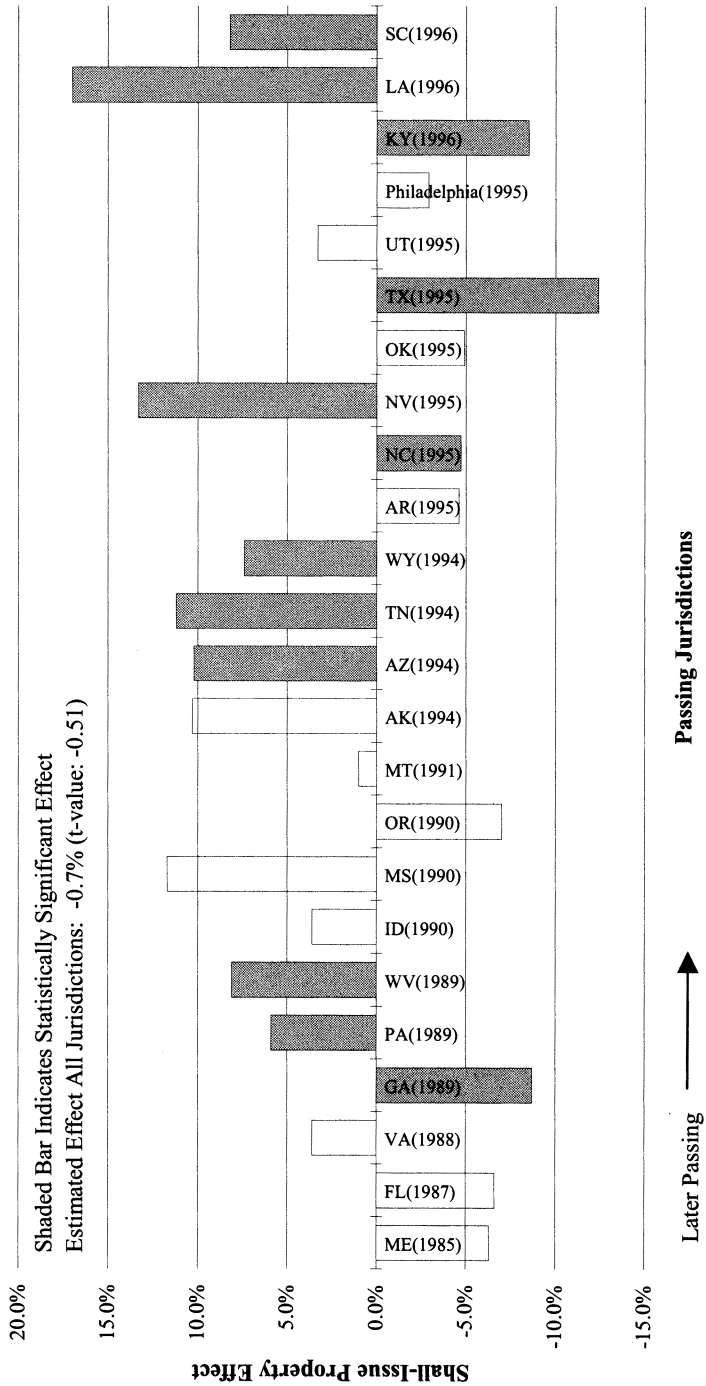
FIGURE 6: IMPACT OF SHALL-ISSUE LAW ON PROPERTY CRIME
JURISDICTION-SPECIFIC DUMMY MODEL WITH STATE TRENDS, COUNTY DATA



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How can it be that the disaggregated analysis in Table 12 is suggestive of crime increases while the aggregate model (Table 11, line 4) suggests statistically significant decreases for murder, rape, and robbery? First, note that weighting by population in the regression gives far greater influence in the regression to large states and that Texas and Florida (the two largest states) had large and statistically significant estimated drops in violent crime after they passed shall-issue laws.[99] As Table 10 indicated, the estimated aggregated effect on murder in the dummy variable model is a drop in crime of 7.7%. Running the aggregated regression without weighting by population lowers the estimated effect on murder from –7.7% to –5.1%. Hence, weighting clearly increases the apparent murder-reducing capacity of shall-issue laws in the aggregated dummy variable model, but it is not the entire story.

Second, as we have seen, the fact that a state adopts a shall-issue law earlier means that it will have a greater impact in the estimation of any postpassage dummy in the aggregated analysis. Thus, imagine a scenario under which only two states (with equal populations) adopt shall-issue laws—one in 1987 and another in 1996. Assume the effect in the two states is exactly opposite, in the early adopter crime *drops* by ten percent in the first year after passage and stays at that lower level through 1997, while in the late adopter crime *increases* by ten percent and will stay that way for ten years. In the disaggregated analysis, one will see equal and opposite impacts, suggesting no overall net effect on crime. This is also what the aggregated dummy variable analysis would show if the laws had been adopted at the same time. But the later adoption in the second state means that its impact will be diminished when the aggregated dummy variable model is estimated. Indeed, the aggregated

99. Even though one can interpret the coefficients on the individual state postpassage dummies as the percentage effect of the law on crime, one has to at least entertain the prospect that these estimates are picking up other changes in the states in question that happened to coincide with the passage of the shall-issue laws. This could happen for any state, but one that has been singled out on this ground is Florida because of the influx of roughly 125,000 largely male, uneducated, and young refugees from Cuba from 1980 through 1981 in the Mariel boatlift, which swelled crime in Florida in the early 1980s, presumably followed by at least some crime decline once the refugees had been fully integrated into the community (or removed from it). *See* David Card, *The Impact of the Mariel Boatlift on the Miami Labor Market*, 43 INDUS. & LAB. REL. REV. 245 (1990). Any such crime decline occurring after 1987 from this factor would be captured in the Lott analysis as the result of shall-issue law. Moreover, even if the effect of the Mariel boatlift had been completely dissipated by the time of passage as Lott has argued, the very sharp spike in crime that can be seen for Florida in figure 1(a) of Ayres and Donohue (1999) reveals that the prepassage fixed effect for Florida would be artificially elevated, biasing downward any estimated effect on the postpassage dummy. *See* John R. Lott, Jr., *The Concealed Handgun Debate*, 27 J. LEGAL STUD. 221, 232 (1998).

Of course, one cannot simply select the states that seemed to do well under the law for further evaluation, but it does suggest that some examination of whether there were any identifiable factors influencing crime in any of the states that appear to have large crime changes—whether positive or negative—at or around the time of the adoption of a shall-issue law might be worthwhile.

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effect in this hypothetical will be a drop in crime of nine percent because the ten years of a crime drop of ten percent will be averaged with the one year of the crime increase of ten percent.[100] As it turns out, two (large) early passing states (Florida and Georgia) experienced drops in murder, thus inordinately dragging down the estimated aggregate impact. But when we decouple the impact of the law on individual jurisdictions, a much different picture emerges.[101]

Lott might respond that these jurisdiction-specific dummy effects could understate the true impact of the law because his "inverted V" concern might operate on an individual state-by-state basis. While this specific concern is dampened somewhat by the inclusion of state-specific trends in our regressions, there is value in exploring whether the hybrid analysis is superior to the dummy variable model for the disaggregated analysis as it was for the aggregated analysis. Accordingly, we employed a disaggregated version of the hybrid specification, which estimates for each jurisdiction both an intercept effect and a trend effect. While only thirty percent of the estimated state-specific spline effects were statistically different than zero, we were able to reject in each of the nine crime type regressions the hypothesis that the twenty-four disaggregated spline effects were jointly equal. Thus, the regressions suggest that the implicit constraints of the disaggregated static model are once again too restrictive.

While we report the raw coefficients of these hybrid regressions in the Appendix,[102] Table 13 reports the net five-year impact of the law, annualized in order to facilitate comparison with the static model.[103] Turning to the

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100. Lott includes graphs in his second edition showing the distorting impacts of unbalanced datasets in estimating the impact of the law. LOTT, *supra* note 2, at 216. Note, too, that the regression will minimize the sum of the squared residuals, which implies that large outliers will be given disproportionate effect in aggregated models.

101. Our disaggregated results also substantially weaken the power of Lott and Bronars's geographic substitution result. Lott and Bronars use an aggregate specification to show that passage of the law caused crime to decrease in the passing states but increase in adjoining states because, they argue, of geographic substitution. Bronars & Lott, *supra* note 3, at 476-77. But this purported spillover result could simply be a byproduct of aggregation bias. Our disaggregated analysis demonstrates that passage of the law was likely associated with increases in many metro areas and with decreases in others. The Lott and Bronars story would only be true if crime fell on the shall-issue side of the metro border and rose on the non-shall-issue side, but nothing in their aggregated analysis would ensure this was the case. If the spillover regression were reestimated on a more disaggregated basis, we predict that most metro areas would show similar movements in crime in both the areas that were covered and uncovered by the law, which would be the exact opposite of the Lott and Bronars hypothesis of crime falling on one side of the border (in response to the shall-issue law) and rising on the other.

102. *See* Appendix Table 7.

103. To calculate the five-year impact of the shall-issue law under the hybrid specification it is necessary to add together the impacts of the intercept and trend terms for individual years and then sum the yearly impacts. For example, the predicted impact of a law for individual years is:

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substance of the disaggregated hybrid specification captured in Table 13, we find a pattern that is remarkably consistent with that presented in Table 12. With the disaggregated hybrid specification, just as in the static model, more states experienced statistically significant increases in crime than statistically significant decreases in crime after passing shall-issue laws. Overall, seventeen of the twenty-four states report a net increase in violent crime, and twenty-one out of twenty-four showed an increase in assault. Only one state (Florida) showed a statistically significant drop in violent crime while six states showed statistically significant increases in violent crime. Similarly, while eight states experienced a statistically significant increase in assault, not one experienced a statistically significant decrease. In fact, as before, every crime category reports more increases than decreases in crime. For example, there are six states showing a statistical increase in murder while only four report a statistical decrease. The disparity is even greater for rape, with four states displaying a statistical increase and only one state reporting a statistical decrease. Overall 150 (of the 216) tests indicate that the shall-issue laws increased crime, with fifty-nine of these estimates being statistically significant (at a five percent level). At the same time, only seventeen of the 216 estimated effects demonstrated a statistically significant decrease—a ratio of more than three increases to every one decrease.[104]

Year 1:1*beta(shall dummy in state X) + 1*beta(spline trend in state X)
Year 2:1*beta(shall dummy in state X) + 2*beta(spline trend in state X)
Year 3:1*beta(shall dummy in state X) + 3*beta(spline trend in state X)
Year 4:1*beta(shall dummy in state X) + 4*beta(spline trend in state X)
Year 5:1*beta(shall dummy in state X) + 5*beta(spline trend in state X)

where beta(shall dummy) and beta(spline trend) represent the estimated coefficients on the intercept and trend variables. Summing these individual year impacts together, we were able to calculate a net annualized five-year impact as: beta(shall dummy in state X) + 3*beta(spline trend in state X).

We also tested whether this linear combination of regression coefficients was statistically different than zero and report the results of this testing in Table 13.

104. These results were qualitatively unaffected when we instead calculated the four-year and the six-year annualized impact of the law. We also estimated the Table 13 results for state data, which generated 31 positive and statistically significant five-year annualized effects versus 18 negative and statistically significant effects. Overall, there were three more positive effects than negative effects, 105 vs. 102.

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TABLE 13: THE JURISDICTION-SPECIFIC ANNUALIZED FIVE-YEAR IMPACT OF SHALL-ISSUE LAWS ON CRIME, LINEAR HYBRID MODEL CONTROLLING FOR STATE TRENDS IN CRIME, COUNTY DATA (LOTT'S CODING)

Entire Period (1977-1997)	Violent Crime	Murder	Rape	Aggravated Assault	Robbery	Property Crime	Auto Theft	Burglary	Larceny
Maine (1985)	-4.9%	7.2%	19.8%	-15.3%	0.1%	0.5%	-4.6%	1.2%	-1.6%
Florida (1987)	**-10.7%**	**-25.9%**	-5.8%	**14.8%**	-9.0%	-5.1%	**14.0%**	5.9%	21.8%
Virginia (1988)	-0.8%	8.7%	8.0%	1.4%	-3.7%	3.9%	6.0%	0.7%	2.3%
Georgia (1989)	-7.1%	-2.8%	**-19.8%**	1.4%	**-17.6%**	**-11.5%**	-7.2%	**-14.2%**	**-13.2%**
Pennsylvania (1989)	1.7%	-1.7%	2.6%	1.8%	5.7%	**7.9%**	0.0%	**5.2%**	**8.5%**
Philadelphia (1995)	8.8%	22.0%	7.9%	11.9%	5.6%	**32.3%**	**33.8%**	-0.6%	-0.6%
West Virginia (1989)	**19.4%**	-2.3%	7.5%	**32.3%**	-7.5%	**8.6%**	**-17.0%**	**10.4%**	**11.1%**
Idaho (1990)	7.4%	-2.0%	**24.3%**	8.6%	16.2%	4.9%	**12.4%**	-2.6%	2.9%
Mississippi (1990)	**37.8%**	**20.6%**	10.5%	**34.8%**	**39.5%**	12.2%	**37.2%**	**17.3%**	6.6%
Oregon (1990)	4.6%	**-25.0%**	-8.5%	**25.8%**	**-33.3%**	-4.8%	-9.1%	**-23.8%**	-2.0%
Montana (1991)	23.8%	**-54.0%**	-31.0%	36.7%	4.7%	5.0%	6.0%	7.8%	2.4%
Alaska (1994)	3.9%	28.6%	-32.4%	24.9%	-12.2%	20.5%	-4.2%	-4.0%	14.1%
Arizona (1994)	11.9%	**17.8%**	**14.8%**	8.4%	**28.8%**	8.7%	17.7%	**23.9%**	**11.8%**
Tennessee (1994)	**26.5%**	**30.0%**	**21.7%**	**34.1%**	**19.8%**	**15.4%**	16.7%	**17.1%**	**20.0%**
Wyoming (1994)	8.2%	2.9%	1.5%	11.3%	**37.2%**	**9.9%**	13.8%	**22.9%**	7.2%
Arkansas (1995)	**43.2%**	33.0%	23.3%	48.2%	20.7%	-13.1%	0.5%	1.6%	3.5%
Nevada (1995)	15.4%	**49.1%**	24.6%	17.1%	18.1%	13.7%	25.3%	**33.1%**	8.7%
North Carolina (1995)	15.4%	11.5%	-6.3%	**21.3%**	8.3%	**-20.9%**	**19.3%**	**-15.3%**	3.0%
Oklahoma (1995)	-11.1%	1.5%	10.7%	-8.6%	-4.8%	-15.2%	-14.3%	-10.9%	4.3%
Texas (1995)	-17.2%	**-35.4%**	-9.6%	-14.0%	-14.8%	**-15.0%**	-21.7%	-9.9%	-4.1%
Utah (1995)	**41.5%**	11.8%	35.8%	**42.1%**	**51.2%**	3.2%	45.8%	**33.7%**	-4.3%
Kentucky (1996)	-1.0%	**41.9%**	-13.5%	0.2%	**29.5%**	-7.9%	12.4%	-12.4%	**-13.5%**
Louisiana (1996)	**16.1%**	**33.5%**	**20.3%**	16.9%	**32.1%**	**17.6%**	**33.0%**	**22.3%**	**14.6%**
South Carolina (1996)	8.7%	13.5%	-4.5%	**14.3%**	10.8%	**8.3%**	**21.4%**	4.5%	3.7%
Summary of Five-Year Effects									Totals
Negative & Significant	1	4	1	0	2	3	1	3	2	17
Negative & Not Significant	6	4	8	3	6	5	6	6	5	49
Positive & Not Significant	11	10	11	13	9	9	10	6	12	91
Positive & Significant	6	6	4	8	7	7	7	9	5	59
Weighted Average Effect:	2.7%	-1.0%	0.7%	9.1%	2.9%	-1.8%	5.1%	0.8%	4.7%	

Notes: Weighted Average Effect is calculated by weighting the state-specific coefficients by their average population over the time period. The dependent variable is the ln(crime rate) named at the top of each column. The data set is comprised of annual county-level observations. County- and year- fixed effects are included in all specifications. All regressions are weighted by county population. Coefficients that are significant at the .10 level are underlined. Coefficients that are significant at the .05 level are displayed in bold. Coefficients that are significant at the .01 level are both underlined and displayed in bold.

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Indeed, for the clear majority of states, shall-issue laws are associated with *increases* in crime for *all crime types*, and the statistically significant impacts are more than twice as likely to exhibit increases in crime. While the story of murder, rape, or robbery dropping can be found in the aggregated analyses of Tables 10 and 11, it is purely an artifact of the happenstance of early adoption that weights a few large states most heavily.[105] If one takes the population-weighted average effect for the twenty-four passing jurisdictions in Table 12 (the dummy variable specification), shall-issue laws are associated with *more* crime in eight out of the nine crime categories. For Table 13 (the hybrid specification), this is true for seven of the nine categories.[106]

We take these disaggregated (state-specific) hybrid regressions to be our most definitive results for the county based data. In a sense, our analysis of this county data has been an exercise in testing and rejecting a series of progressively less-constrained specifications. We began by rejecting the simple aggregate dummy variable and spline models in favor of the aggregate hybrid specification. We next rejected the constraint that the law had the same impact on early and late passing jurisdictions. We then rejected the decision to exclude state-specific trends. And finally we rejected the disaggregated dummy variable specification. The disaggregated hybrid model that we have finally settled on allows the data to reveal a variety of different impacts of the law—allowing separate intercept and trend effects for each of the twenty-four passing jurisdictions. And while we might have concerns that estimating this many impacts would rob the regressions of statistical significance (as we eat up degrees of freedom), we still find that over one third of the state/crime type tests (seventy-six out of 216) are statistically significant.[107]

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105. The disaggregated analysis is also amenable to the same kind of test of internal theoretical consistency that we undertook earlier with respect to the aggregate analysis. Looking again at property crimes and robbery, we see in Table 13 on a disaggregated basis that 16 (of 24) states experienced an increase in property crime and that 14 of those 16 states also experienced an increase in violent crime, of which five were statistically significant increases in violent crime. Of the other two states that experienced an increase in property crime but a decrease in violent crime, in only one was the decrease statistically significant. Once again, although shall-issue laws are generally associated with *increased* property crime, we have no plausible story to back up this effect. Table 13 shows neither a general shift from violent to property crime nor a more nuanced shift from robbery to property crime, as none of the 16 states that have estimated increases in property crime reported statistically significant decreases in robbery.

106. Note that the weighted average for murder is –1%, which does not represent a statistically significant difference from zero.

107. It is possible, of course, to estimate even less-constrained specifications that admit the possibility of higher order impacts. Indeed, we estimated a disaggregated quadratic hybrid that is identical to the disaggregated hybrid discussed above but which includes a prepassage quadratic term and a postpassage quadratic spline term. Estimating this quadratic-hybrid specification allowed us to test (1) whether the implicit restrictions of the (linear) hybrid are rejected by the less-constrained specification, and (2) whether the results of the (linear) hybrid were robust to the less-constrained specification. We found that the (linear) hybrid's implicit assumption of no quadratic postpassage effect was not decisively

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But in interpreting the results, it is important not to forget the lessons of Part II, which showed that the results of the state data regressions were incredibly contingent on an array of factors, such as the inclusion or omission of rather innocuous right-hand side controls. We found a similar fragility with regard to these county data regressions. Indeed, as we were double-checking our results, we discovered that we had omitted one of Lott's original controls: "Percentage of County Population That Was Not Black or White but Was Male and Aged 20-29"; and we were surprised to find that adding this extremely innocuous demographic variable decreased our estimate of the dollar impact that the law's passage had on crime (that we are about to report) by more than twenty percent. The nonrobustness of the results with respect to these attenuated demographic controls is a legitimate reason to discount or at least question this entire enterprise. Because the disaggregated county data still yields some jurisdictional pockets in support of Lott's more guns, less crime hypothesis, however, we soldier on to assess a possible net impact of the laws.

To get a better handle on the net impact of the law on all crime, we have estimated the dollar impact of the law on particular crime categories, using the same dollar value per crime that Lott used in his initial study.[108] Table 14 reports the annualized dollar impact of crime for each of the twenty-four jurisdictions and for each of the nine crime categories, as well as various aggregations of these amounts. Eighteen of the twenty-four states have estimated increased dollar harms, as shown in the "Total" column, which is also depicted in Figure 7. Viewed in aggregate, the Table suggests that the net annual impact of the law was to increase the dollar harm of crime by approximately $1 billion. This represents a "harm weighted" annual increase in crime of two percent that amounts to an annual burden of $9.63 on the average citizen in the passing states.

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rejected in that only 49 of the 216 coefficients were statistically different from zero (although the quadratic spline effects were jointly different from zero in eight of the nine regressions). But the basic results of the (linear) hybrid analysis discussed in the text remain unaltered: Calculating the net annualized five-year impact, we continued to find that the vast majority of the statistically significant impacts were positive (48 versus 20).

Lott has sharply criticized Black and Nagin's decision to estimate state-specific quadratic time trends in an aggregated dummy variable model. *See* LOTT, *supra* note 2, at 209; *see also* Black & Nagin, *supra* note 3, at 218. We agree that such a specification can understate the law's impact if the impact comes primarily through a kink or bend in the time trend. But Lott's criticism is not relevant to our quadratic-hybrid specification, which allows the law's impact to come through an intercept effect, a linear spline effect, or a quadratic spline effect.

108. In 1997 dollars, the harm of the following crimes was assumed to be: murder, $3,092,804; rape, $91,522; aggravated assault, $25,247; robbery, $8416; auto theft, $3892; burglary, $1472; and larceny, $389. LOTT, *supra* note 2, at 54-56.

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TABLE 14: ESTIMATES OF DOLLAR IMPACT OF CRIME (IN MILLIONS OF DOLLARS*) BASED ON JURISDICTION-SPECIFIC ANNUALIZED FIVE-YEAR IMPACT, HYBRID MODEL WITH STATE TRENDS (LOTT'S CODING)

	Murder	Rape	Aggravated Assault	Robbery	Violent Crime Total	Auto Theft	Burglary	Larceny	Property Crime Total	Total (Viol & Prop)	Per Capita Impact*	Harm-Weighted Percent Change
Maine (1985)	6.29	3.03	-5.69	0.00	3.63	-0.34	0.19	-0.18	-0.33	3.30	2.83	1.9%
Florida (1987)	-1098.84	-32.03	272.28	-32.53	-891.12	44.30	23.56	37.90	105.76	-785.37	-65.45	-10.0%
Virginia (1988)	122.27	11.33	3.08	-1.98	134.70	4.09	0.48	1.43	6.00	140.70	23.54	7.0%
Georgia (1989)	-69.61	-55.91	8.99	-25.55	-142.07	-11.62	-22.69	-12.90	-47.22	-189.29	-29.53	-4.8%
Pennsylvania (1989)	-14.56	5.11	7.99	3.71	2.24	0.00	4.83	5.43	10.26	12.50	1.21	0.7%
Philadelphia (1995)	293.94	5.59	21.16	6.42	327.10	31.32	-0.14	-0.11	31.07	358.18	239.28	19.5%
West Virginia (1989)	-8.64	2.38	11.90	-0.50	5.15	-1.93	1.78	1.15	1.00	6.15	3.41	1.3%
Idaho (1990)	-1.69	6.08	5.00	0.20	9.59	0.80	-0.31	0.32	0.80	10.39	10.22	5.2%
Mississippi (1990)	119.42	6.85	30.53	5.75	162.56	5.95	5.75	1.03	12.74	175.30	67.86	21.6%
Oregon (1990)	-83.65	-10.34	57.49	-11.49	-47.99	-4.61	-11.28	-0.78	-16.68	-64.67	-22.65	-7.6%
Montana (1991)	-22.40	-3.20	5.08	0.04	-20.48	0.28	0.35	0.14	0.77	-19.71	-24.37	-24.1%
Alaska (1994)	24.03	-7.71	14.46	-0.86	29.92	-0.45	-0.19	0.96	0.33	30.24	50.51	15.5%
Arizona (1994)	232.38	19.19	42.01	15.73	309.31	29.58	20.76	8.52	58.87	368.18	88.74	15.9%
Tennessee (1994)	406.25	46.22	194.58	17.31	664.36	17.61	13.10	9.67	40.38	704.73	136.63	28.7%
Wyoming (1994)	1.54	0.22	2.97	0.25	4.97	0.41	1.04	0.43	1.88	6.85	14.43	6.3%
Arkansas (1995)	264.73	19.69	114.59	5.43	404.43	0.16	0.58	0.95	1.69	406.12	163.70	32.6%
Nevada (1995)	246.05	20.87	35.86	7.55	310.32	11.20	9.78	1.82	22.80	333.12	217.79	35.7%
North Carolina (1995)	237.69	-13.27	162.87	8.93	396.22	16.65	-22.76	2.71	-3.40	392.83	54.66	11.3%
Oklahoma (1995)	18.48	14.30	-34.96	-1.53	-3.71	-9.05	-6.69	1.73	-14.01	-17.72	-5.42	-0.9%
Texas (1995)	-1854.87	-75.23	-283.89	-41.93	-2255.92	-88.61	-29.52	-10.09	-128.22	-2384.14	-127.49	-25.7%
Utah (1995)	27.81	26.84	43.35	5.57	103.56	13.31	7.73	-1.46	19.58	123.14	62.37	24.1%
Kentucky (1996)	125.77	-5.62	0.30	7.76	128.20	2.91	-2.38	-1.83	-1.31	126.89	32.69	22.1%
Louisiana (1996)	772.18	31.94	105.79	31.74	941.65	34.36	17.72	9.45	61.53	1003.18	231.10	29.2%
South Carolina (1996)	139.91	-7.74	103.07	5.89	241.12	13.27	3.16	1.88	18.30	259.42	69.80	12.0%
Totals**	-115.56	8.58	918.79	5.92	817.73	109.57	14.84	58.19	182.60	1000.34	9.63	2.0%
Simple Mean	-4.81	0.36	38.28	0.25	34.07	4.57	0.62	2.42	7.61	41.68	49.83	9.1%
Standard Deviation	502.83	26.22	100.07	15.52	588.18	24.42	12.81	9.04	41.88	613.17	92.14	16.5%
Number Positive	16	15	21	16	18	16	15	17	17	18		
Number Negative	8	9	3	8	6	7	9	7	7	6		

* Per Capita Impact is expressed in dollars, not millions of dollars.

** The entries for Per-Capita Impact and Harm-Weighted Percent Change are computed by dividing the total estimated impact ($1 billion) by the total population (in millions) and the total cost of crime (in millions of dollars for the 24 jurisdictions) respectively.

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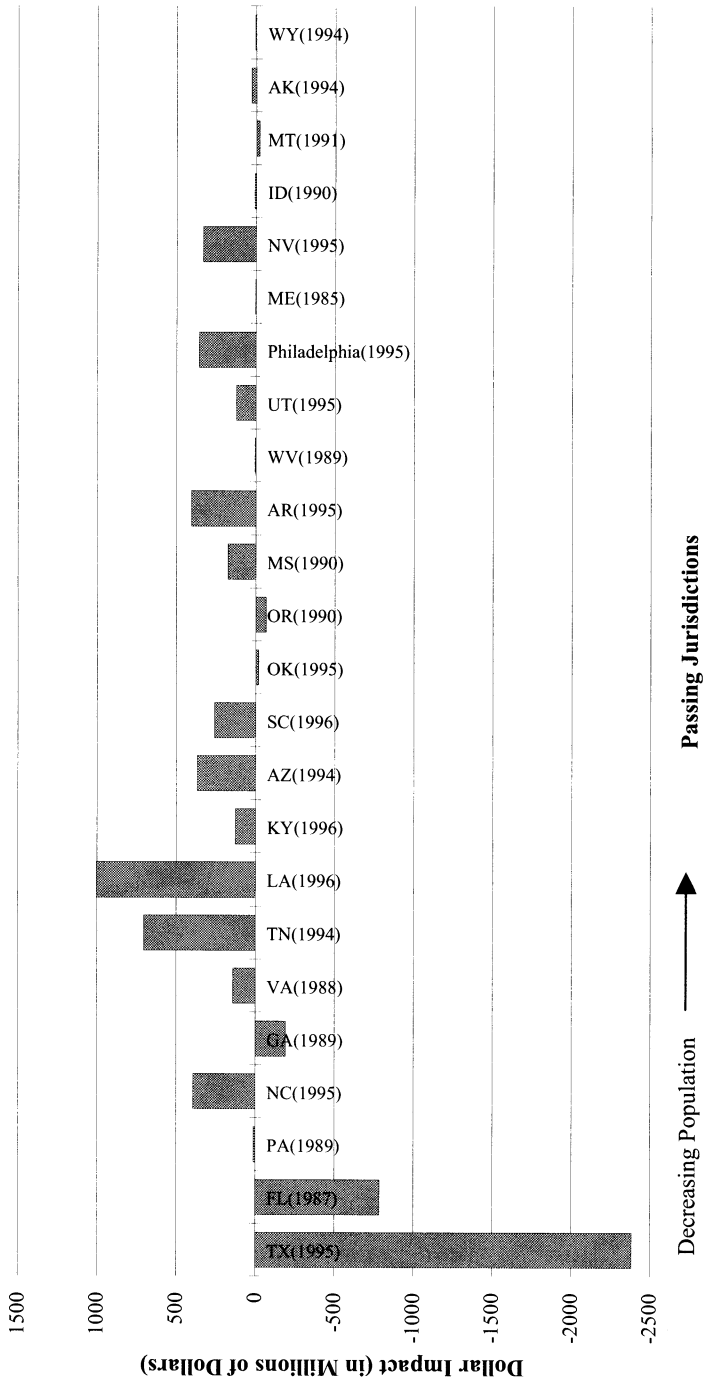
FIGURE 7: FIVE-YEAR ANNUALIZED AVERAGE DOLLAR IMPACT OF SHALL-ISSUE LAWS OVER ALL CRIME TYPES



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The Table also reveals substantial variation in the impact of the law. In Texas and Florida alone, the law is estimated to have *reduced* the annual cost of crime by more than $3 billion. Conversely, in Louisiana and Tennessee, the law is estimated to have *increased* the annual cost of crime by $1.7 billion. In the twenty-four adopting jurisdictions that we examined, the mean impact of the law was a $42 million increase in crime, but the standard deviation was more than fourteen times this amount ($613 million).[109]

These dollar figures price all of the estimated impacts of the law (reported above in Table 13) regardless of their statistical significance. An alternative way to estimate the aggregate impact of the law is to put a zero dollar value on all the impacts that are not sufficiently statistically significant. Table 15 reports the aggregate dollar impact of the law for alternative levels of significance—for both the dummy variable specification (originally reported in Table 13) and our preferred hybrid specification (of Table 14). The Table shows that the law continues to display a pernicious dollar effect even when we limit our focus to five-year impacts that were statistically significant at the ten-percent or five-percent level. At these levels, the static specification estimates a net annual impact on the order of $500 million, while the less-restrictive hybrid specification estimates an increased cost ranging between $3 and $524 million.[110]

We take these results to be generally devastating to Lott's more guns, less crime hypothesis. Estimating a less-constrained specification with more data, we find that more jurisdictions experience an increase than a decrease in crime and that the statistically significant increases outpace the statistically significant decreases by more than two-to-one in county data (and still by a substantial amount in state data).

109. One can see from Figure 7 that the two states showing the biggest dollar impact on crime are Texas (a crime decline of over $2 billion per year) and Louisiana (a crime increase of over $1 billion). Both adopted the law late in the data period, which implies that there is relatively little postpassage data with which to estimate these figures. It is quite likely that with more years of data, one would see the estimated effects for these two states move closer to the mean.

110. Only if we restrict attention to five-year impacts that were statistically significant at the one percent level do we find a net benefit from the law, with an estimated reduction in crime in the hybrid specification of $784 million. But here it is important to note again that the result is driven by just two states—Texas and Florida (with an estimated combined benefit of more than $3 billion)—which more than offset the estimated crime-increasing impact in Louisiana and Tennessee (approximately $1.7 billion).

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TABLE 15: ESTIMATED TOTAL DOLLAR IMPACT OF 24 SHALL-ISSUE LAWS (IN MILLIONS OF DOLLARS)

	Level of Filtering			
Model	All Estimates	10%	5%	1%
Dummy	1262.14	513.11	487.04	-22.22
Hybrid	1000.34	524.36	2.89	-784.33

Notes: These estimated dollar effects are based on the jurisdiction specific models with state trends. The Hybrid Model is from Table 13, where the $1 billion figure can be found under the row and column Total. The remaining totals and the Dummy Variable model is available upon request. Essentially, the first set of estimates (in the "All Estimates" column) takes every estimated jurisdiction-specific as reflecting the true impact of the shall-issue law on crime. The other three columns will filter out those estimates that are not statistically significant at the indicated level.

V. SOME INTERPRETATIONS AND SPECULATIONS

Our effort to find the statistically most appropriate model with which to assess the impact of shall-issue laws on crime has involved an extended odyssey as our testing, on a more comprehensive county dataset, has constantly pushed us towards more disaggregated and less restrictive models than the more aggregated and highly constrained models employed by Lott and Mustard. The lesson has been a sobering one in that the facially plausible models relied upon by Lott and Mustard that we present in Table 10 could well encourage a researcher or policymaker to believe that shall-issue laws reduce crime. Yet when we reached the end of the journey with our more complete data, the use of a statistically superior model—one that estimates jurisdiction-specific effects while estimating both main and trend effects (our "hybrid") and controlling for state fixed effects—reduces the initial conclusion to ashes. The best, albeit admittedly imperfect, evidence suggests that, for the majority of states, shall-issue laws are associated with higher levels of crime.

It is important, though, to be clear about the degree of confidence that we can repose in any particular interpretation of the evidence. In the end, we are left with a hierarchy of three conclusions that we will discuss in turn below.

1. There remains no robust, credible statistical evidence that the adoption of shall-issue laws will generally lower crime, and indeed the best, albeit admittedly imperfect, statistical evidence presented thus far points in the opposite direction: that the adoption of shall-issue laws will generally increase crime.

We believe that a fair evaluation of all the state and county evidence we have presented offers virtually no basis for believing that shall-issue laws reduce crime. While particular regressions at times predict drops in certain violent crimes following adoption, the overall regression evidence predicts

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increases in other crime categories, most frequently among the many types of property crime. Indeed, there is abundant reason to be concerned that the regressions are not performing particularly well, and plausible adjustments to the aggregated regressions—whether by using alternative model specifications, trying to control for selection effects of very late or very early adopters in the state analysis, or introducing state trends in the county data—largely eviscerate the more guns, less crime hypothesis. In particular, state-specific estimates generated on the 1977-1997 county data are more supportive of the view that shall-issue laws increase crime than that they decrease it.

While Lott and Mustard have tried to offer other types of evidence that can bolster their core statistical findings, if the foundation falls, the entire edifice will crumble. We believe we have shown that the foundation has collapsed. Whether further advances in statistical modeling or additional years of data to analyze more state adoptions (or repeals) of shall-issue laws will be able to resurrect the structure remains to be seen.

We hasten to add, however, that showing that superior statistical modeling on more complete data reverses the Lott and Mustard conclusion does not necessarily resolve the debate since "better" does not always imply "good enough." The estimates' lack of robustness with regard to innocuous changes in specifications provides grounds for rejecting the authoritativeness of any of the results. Therefore, another plausible conclusion from the evidence we have presented is:

2. While the best evidence suggests that shall-issue laws generally tend to increase crime, there is still too much uncertainty to make strong claims about their effects.

The dramatic reversal in findings in moving from aggregated (Table 10) to disaggregated (Table 13) hybrid specifications certainly reveals that many conclusions about the impact of shall-issue laws will be dependent on the particular statistical model that is employed. Some will be convinced that our disaggregated model is superior and, therefore, will accept the conclusions of the state-specific analysis. More cautious analysts will be concerned that the problems we have highlighted of data accuracy, model misspecification, endogeneity, and lack of robustness (such as the sensitivity of the state data results to exclusions of minor demographic controls) are too severe to confidently assert whether shall-issue laws dampen crime, increase crime, or have no overall effect on crime. We share these concerns, especially since the theoretical argument for one of the most robust findings—that the laws increase property crime—is not particularly compelling.[111] Thus, a plausible

111. The figures we provide show property crime rising as much in response to shall-issue laws as violent crime does (with virtually no evidence of the shifting from violent crime to property crime that was initially posited by Lott and Mustard). *See, e.g.*, LOTT *supra* note 2, at 19 ("When potential victims are able to arm themselves, some criminals turn away from crimes like robbery that require direct attacks and turn instead to such crimes as auto theft, where the probability of direct contact with victims is small."). Given the lack of

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interpretation of the existing evidence would be that even if we can muster evidence that shall-issue laws generally appear to increase crime, the uncertainty about whether the statistical models are working properly makes it difficult to make any strong claim about the impact, other than to say that it is not so huge that it can overwhelm any defects in the model.

At the end of the day, then, it is still possible that shall-issue laws have no effect—positive or negative—on crime. This is particularly so if one credits Willard Manning's suggested correction for the presence of these multiple comparisons and for autocorrelation in crime across years.[112] In addition, as discussed above, one reason states like Florida and Texas—both of which experienced large crime decreases after adoption—may have an estimated negative impact is simply that they passed the law in response to crime increases, and as crime reverted of its own accord to its normal levels, the regression inappropriately attributed this reversion to the passage of the law.

Earlier in this Article, we touched upon an alternative reason why even the best regression results may not be believable: the huge omitted variable problem represented by the upturn in crime following the advent of crack in certain urban areas in the mid- to late-1980s. If states adopting shall-issue laws in the 1980s had no major crack problem, while nonadopting states were more likely to have a crack problem, then that fact alone could make it appear that shall-issue laws reduced crime because crime would rise relatively faster in the nonadopting states (by virtue of the crack problem). In his book, Lott reported that the states adopting shall-issue laws tend to be Republican, have high NRA membership, and have low crime rates.[113] That does not sound like the sort of place where one finds the worst problems with crack. Without a plausible control for the influence of crack on crime rates, every crime regression is subject to a charge of inadequacy: A potentially important explanatory variable that is correlated with the adoption of shall-issue laws is being omitted.[114]

Misspecifications of this type make it difficult for the researcher to distinguish between the possibility that the law has no effect and the possibility that any effect of the law is beyond the current ability of researchers to identify. Ironically, however, either a "no effect" or "don't know the effect" assessment might be enlisted to argue normatively for adoption of the shall-issue law.

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evidence that robbery or other violent crimes have fallen, however, is the estimated increase in property crime due to shall-issue laws plausible?

112. A basic assumption of regression is that the difference between the regression prediction of the crime rate and the observed value, called the error, is not correlated across years. If it is, then one has the problem of autocorrelation, and more sophisticated statistical techniques are required to secure reliable estimates. *See* GREENE, *supra* note 13, at 147.

113. LOTT, *supra* note 2, at 120.

114. In his second edition, Lott admirably includes controls for individual year effects for five different regions (Northeast, South, Midwest, Rocky Mountains, and Pacific) to allow for more heterogeneous year effects for different parts of the country. *Id.* at 170. But Lott's less-constrained approach would not be sufficient to solve the crack problem (outlined above), which may very well have played out within the regions.

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While the first empirical assessment would tend to lead consequentialists to oppose the law,[115] the second assessment of ignorance (or even no effect) might provide a libertarian grounds to support such legislation. There are, however, many reasons why reasonable people who embrace the "don't know the effect" conclusion might nevertheless oppose the statute. Nonconsequenstialists and expressivists might oppose the law notwithstanding a lack of evidence that the law increases crime. Risk aversion or a concern that the law moves society away from a more global maximum of fewer weapons might also ground opposition. Finally, if the realization that thousands of citizens were carrying around concealed handguns generated fear or apprehension in the community, one might oppose shall-issue laws even if one could not prove that they increased crime. In a world where NRA members have bumper stickers stating "Keep Honking, I'm Reloading," the costs of intimidation of law-abiding citizens may become intolerable.[116]

Some may feel that just as many were willing to make policy choices on the basis of the initial Lott and Mustard study, there is even stronger reason to rely on our less-constrained models estimated on more years of data showing that after passage twice as many jurisdictions experienced a statistical increase as experienced a statistical decline in crime. Those who are willing to repose confidence in such results have to consider whether the disaggregated evidence might support a view that some states would benefit from shall-issue law adoption even though most would not. We will now evaluate the normative implications of this assessment of heterogeneous effects across states. While the first and second assessments militate towards across-the-board policies, the heterogeneity assessment might counsel towards a more nuanced, piecemeal adoption of the statute:

3. We should simply accept the twenty-four different jurisdiction-specific estimates and conclude that shall-issue laws increase crime in most states but reduce it in other states.

This is the most speculative of the three conclusions we discuss in that it violates Milton Friedman's admonition against accepting statistically

115. Lott, himself, is clearly in the consequentialist camp. *See id.* at 21 ("[T]he ultimate test: Does it save lives?").

116. David Mustard, *The Impact of Gun Laws on Police Deaths*, 44 J.L. & ECON. 635, 637 (2001), cites a 1999 incident in Phoenix in which a concerned citizen with a concealed gun permit (and the "Keep Honking . . ." bumper sticker) came to the assistance of a dying officer and helped capture three fleeing drug suspects. The citizen returned fire by shooting 14 shots into the car of the criminals, which "he thought was pretty good since he shot quite a few of them with his off hand, hanging out the window." While the apprehension of dangerous felons was an obvious benefit of the citizen's intervention, it is not hard to imagine how such behavior could have ended badly for some innocent bystanders. Simply because this episode ended happily does not mean that we should encourage such efforts. *See* Mark Shaffer, *"True Hero" Helps Nab Trio: Security Guard Reacted to "What I Thought Was the Right Thing to Do,"* ARIZ. REPUBLIC, Mar. 28, 1999, at A1.

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significant findings too readily.[117] On the face of the evidence presented in Table 13, there are a number of problems with accepting that crime fell with the adoption of shall-issue laws in even a limited number of states. There is only one state that shows a statistically significant decrease in at least two of the five violent crime categories without showing a statistically significant increase in another violent crime category (Georgia, showing drops in rape and robbery). Florida shows substantial drops in violent crime and murder but a statistically significant increase in aggravated assaults (all against the background of the potentially confounding influence of the Mariel boatlift), while Oregon shows huge drops in murder and robbery but a huge increase in aggravated assaults. Only two other states show a statistically significant drop in any other violent crime category (Texas and Montana, seeing enormous drops in murder). In other words, it is rare (only five out of twenty-four) to see any statistically significant evidence of declines in any violent crime category resulting from the adoption of a shall-issue law. Certainly, there is nothing to give one confidence that an overall drop in violent crime is likely to be spawned anywhere by the adoption of a shall-issue law. Indeed, the rare and seemingly haphazard pattern of statistically significant drops across isolated violent crime categories makes one think of these drops as more random noise than estimates upon which much confidence can be reposed that real effects have been identified. This is underscored by the fact that finding eight statistically significant drops in crime across the 120 estimates (twenty-four jurisdictions times five violent crime categories) in Table 13 is only modestly more than one would expect from a purely random process.

A. *Illustrating a Methodology for Deriving State-Specific Predictions*

One interesting consequence of conclusion three, however, is that it invites the researcher to investigate whether there may be some particular attributes about the small number of states for which crime drops were estimated that diminish the generally harmful effects of shall-issue laws enough so that they may actually dampen crime. If one overcomes the concern that the infrequent instances of crime reductions are merely random artifacts and concludes that there is valid information captured in the harm-weighted estimate of the total effect on crime for a state, we can investigate whether there is any pattern that explains which states show overall crime increases and which show decreases. Unfortunately, we have very little data to make this assessment. The county fixed-effects model which we have followed Lott in using to test for the law's impact only allows us to test for the impact of changes in the law within particular counties.[118] But now we are called upon to assess the determinants

117. *See supra* note 16.

118. Lott attempted to assess what characteristics were associated with larger or smaller impacts of the shall-issue law by interacting the law dummy variable with various

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of the law's impact across the passing states. In essence, we have just twenty-four observations (twenty-three passing states plus Philadelphia) on which to try to disentangle what caused the law to increase crime in some jurisdictions and decrease it in others.

Since we are now taking the output of potentially fragile regressions that were used to estimate the impact on crime of adopting a shall-issue law in twenty-four jurisdictions, then trying to run yet another regression based on this output having only twenty-four observations, we are now moving very far out on a branch. Still, there is value in at least detailing a methodology for deriving state-specific predictions of the effect of a law, even for those that have not adopted the legislation. Accordingly, we regressed various measures of the impact of shall-issue laws on a variety of state characteristics. Before reporting the results, let us emphasize that with just twenty-four observations we are certainly pushing the limits of data. The results that we report are not nearly as robust as our prior findings to the inclusion or exclusion of other variables.[119] Indeed, the data's resistance to explanation can be taken as yet more evidence that, notwithstanding our best efforts, the underlying model (the disaggregated hybrid version) is still misspecified.

With these caveats about the substantial limits in our data and the residual concerns with misspecification, we proceed to discuss the impact regressions of Table 16. The dependent variable in the first row regression is the harm-weighted percentage impact of the hybrid regression (reported above in Table 14), which ranges from a 25.7% *drop* in crime in Texas to a 35.7% *increase* in crime in Nevada.[120] We use the following six jurisdiction characteristics as explanatory variables: the year the law was adopted, population, log of violent crime rate, density (measured in population per square mile), and regional dummies.[121] Looking at the estimated coefficients, we see that later-passing

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demographic characteristics of the county (its density, its income, etc.). LOTT *supra* note 2, at 60-81. But this interaction specification in a fixed-effects model only allowed Lott to assess the impact of, say, changes of density *within* particular counties that had passed the law. Since, on average, there is very little within-county variation in density over a 20-year period, this specification can tell us very little about the relationship between the law's impact and densities across different counties or states.

119. The fact that we ran a number of alternative specifications that we did not report also suggests a "pretesting" effect that should lead us to discount even the nominal levels of significance reported in the regression.

120. The dispersion in the estimated effects of the 24 shall-issue laws is quite wide, which in itself shows that there is a considerable degree of noise in the estimate for any particular jurisdiction. As Black and Nagin comment in finding substantial dispersion in state-specific estimates for the 10 states adopting shall-issue laws during the 1977-1992 period: "Widely varying estimates such as these are classic evidence that, even beyond the assumption of homogeneous impacts across states, the model is misspecified." Black & Nagin, *supra* note 3, at 214.

121. Only two (South and West) of the four regions are reported in Table 16. There were no Midwest states that passed the law during this 20-year period (and the Northeast region was the excluded attribute).

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states are predicted to have a more deleterious impact, with each additional year adding more than 1.6 percentage points to the estimated impact (prob. = 0.07). This is a huge effect (a law passing ten years later would be expected to have a sixteen percentage point higher crime impact) and is qualitatively consistent with the aggregate results showing crime increases in the 1990s for later passing jurisdictions.[122]

The harm-weighted estimates that we use as regressors have the advantage of aggregating the individual crime category impacts in a natural way that weights the individual crime estimates commensurate with their underlying importance (more serious crimes are socially more "expensive" and therefore weighed more heavily). A disadvantage of the harm-weighted estimates is that they ignore the varying significance of the individual crime impact estimates. Returning to Figure 5 (which depicts the disaggregated dummy variable model estimates for murder from Table 12), one can readily identify the positive correlation between the estimated impact of the law on the murder rate and the year in which the law was adopted. We see that thirteen of the final fourteen passing states had estimated increases in murder (and seven of these were statistically significant) while only four out of the first ten passing states posted estimated increases in murder (and only one of those was statistically significant).[123] More generally, Table 14 shows that eleven of the last thirteen passing jurisdictions experienced *increases* in violent crime.

122. Query, though, whether this result tells us that the later adopters can expect more pernicious outcomes from adopting shall-issue laws or whether it is reflecting the sharp drop in crime in the postcrack world for the nonadopting states. The year of adoption is clearly a proxy for something—but it is uncertain whether it is a proxy for a feature of the state that will influence the impact of a shall-issue law (as the regression posits) or a proxy for the time period during which sharp declines from previous crack-induced crime hikes occurred in selected nonadopting states (i.e., having no relation at all to the impact of shall-issue laws).

123. In his second edition, Lott attributes the diminishing beneficial impact of the law in later-passing jurisdictions to heightened fees and training requirements that were imposed on permit applicants in the later passing states. LOTT, *supra* note 2, at 125-26. But while the imposition of greater obstacles could have explained a diminished beneficial trend, it does not explain why we find that later passing states generally experienced increases in crime. It also does not explain why Texas (which Lott notes requires 10 hours of training and charges the highest fee in the sample) was one of the great outliers in generating a beneficial impact. *See id.* at 175. We did not have available the fee and training data that Lott used and hence did not control for these attributes in the regressions reported in Tables 10 and 11.

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TABLE 16: EXPLAINING THE ESTIMATED PERCENTAGE IMPACT ON CRIME OF THE 24 SHALL-ISSUE JURISDICTIONS

		Explanatory Variables					
Dependent variable*		Density**	Population (Millions)	Log Violent Crime Rate	Year Adopted	South	West
Hybrid model harm-weighted impact		-0.026	**-0.028**	0.116	0.016	-0.119	-0.205
		(0.013)	(0.006)	(0.059)	(0.009)	(0.087)	(0.098)
Mean impact:	0.091						
Standard deviation:	0.165						
Hybrid model five-year murder impact		**-0.053**	**-0.042**	**0.212**	0.023	**-0.261**	**-0.427**
		(0.019)	(0.006)	(0.096)	(0.013)	(0.120)	(0.139)
Mean impact:	0.077						
Standard deviation:	0.247						
Dummy model harm-weighted impact		-0.027	**-0.02**	0.095	0.017	-0.115	-0.182
		(0.013)	(0.004)	(0.071)	(0.009)	(0.080)	(0.089)
Mean impact:	0.073						
Standard deviation:	0.146						
Dummy model murder impact		**-0.056**	**-0.032**	0.166	0.028	**-0.303**	**-0.422**
		(0.022)	(0.008)	(0.123)	(0.015)	(0.133)	(0.161)
Mean impact:	0.076						
Standard deviation:	0.237						

* For the values of the hybrid model harm-weighted impact, see the harm-weighted percent change column, Table 14. For the Hybrid murder impact, see the murder impact column, Table 13. For the dummy model murder impact, see the murder impact column, Table 12. The dummy model harm-weighted impact is not included in the tables but is available upon request.

** Density is expressed in units of population in thousands per square mile.

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If one accepts the accuracy of the jurisdiction-specific estimates, one might interpret this cohort result as a kind of "Peter Principle" applied to law. As a pop theory of job advancement, the Peter Principle asserts that employees are promoted to jobs requiring successively higher skills until they reach a job where they are relatively incompetent.[124] Analogously, the data indicates that the law has performed less well—rising to the level of its incompetence—as successive states have chosen to adopt it. On the other hand, the temporal pattern, that states adopting shall-issue laws in the late 1980s did better while those adopting in the 1990s did worse, may simply reflect the influence of a time-varying factor (the crack trade?) that caused sharp rises in crime for many states in the late 1980s, and then greater-than-average crime declines in the 1990s.[125]

The regressions, however, do at least suggest some nontemporal traits that are associated with the estimated impact. Passing jurisdictions with larger populations had more beneficial impacts, and this correlation was strongly significant. Less dense jurisdictions and jurisdictions starting with higher base levels of violent crime had more pernicious impacts (and these effects were significant at the ten-percent level).[126] The negative association between state population and the estimated dollar impacts (estimated in Table 14) is dramatically illustrated in Figure 7, with the two largest states (Texas and Florida) having by far the largest harm-weighted dollar drops in crime. This should not be surprising, though, since a given percentage change in crime will have a bigger dollar impact in a larger (or higher-crime) state. Finally, western states tended to be associated with better crime outcomes, and this effect was statistically significant.[127] While there are always some "cultural" rationales that we could offer ex post for these effects, we should emphasize that these results are suspect as they are the byproduct of reduced-form regressions rather than growing out of *a priori* theory.

B. *The Resulting State-Specific Predictions*

Having derived the regression estimates of the factors that influence the impact of the law, we are at least conceptually able to use the results of these regressions to predict out-of-sample the expected impact of the law on the

124. LAURENCE J. PETER & RAYMOND HULL, THE PETER PRINCIPLE: WHY THINGS ALWAYS GO WRONG (1969); *see also* James A. Fairburn & James M. Malcomson, *Performance, Promotion, and the Peter Principle*, 68 REV. ECON. STUD. 45 (2001).

125. *See supra* text accompanying notes 34-38 and 110-11 (discussing the omitted variable problem represented by the upturn in crime following the rise of the crack trade).

126. While this empirical finding is potentially interesting, it is not evident that any theory could support it.

127. In the remaining panels of Table 16, we replicate the regression of the first row by regressing alternative measures for the impact of crime coming from the murder category and from the dummy-variable regressions. One sees that the patterns of sign and statistical significance are generally consistent.

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jurisdictions that either have never passed a shall-issue statute or had already passed such a statute before our data began in 1977.[128] To continue our methodological roadmap for how to generate these estimates (for cases in which the regression results were both stronger and based on a stronger foundation), Appendix Table 8 reports the predicted impact (as well as the standard deviation of the prediction) of passing the law today in all fifty-two jurisdictions (including Philadelphia and the District of Columbia), given the jurisdiction's underlying characteristics. While this predictive process is based on extremely limited data, it may be useful to illustrate the possibility of more nuanced state-specific policy recommendations in contrast to the across-the-board recommendations to adopt or repeal that are the only possible product of the type of aggregated analysis that Lott championed.

As one might suspect, the predictions produced heterogeneous results, which we break into four categories:

1. *Jurisdictions in which adoption is strongly predicted to increase crime.*

We found thirty-one jurisdictions in which adoption of the law predicted an annual percentage increase in the harm-weighted measure of crime that was more than twice the prediction's estimated standard deviation and, therefore, statistically significant. Fifteen of these jurisdictions had already adopted the law and thus would need to repeal the law to avoid the deleterious effect, while sixteen of these states would merely need to refrain from adopting the law in the future.

2. *Jurisdictions in which adoption is strongly predicted to decrease crime.*

We found just two states (Texas and California) in which adoption of the law predicted an annual percentage *decrease* in the harm-weighted measure of crime that was more than twice the prediction's estimated standard deviation. One of these jurisdictions (California) would need to adopt the law to secure its benefits while the other (Texas) would merely need to retain the law that it has previously adopted.[129]

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128. Lott undertook an analogous prediction procedure when he used a first-stage regression run on 10 states to predict how many permits would be issued in other states. LOTT, *supra* note 2, at 176.

129. While crime did drop as the Texas shall-issue law went into effect on January 1, 1996, California has never adopted a shall-issue law, so the prediction is based on our weakly predictive model with 24 data points (largely because California has a large population and is a western state). But while California is shown by these tentative impact regressions to be a possible candidate for enactment of a shall-issue law, its experience of showing dramatic crime declines in the 1990s, both absolutely and vis-à-vis shall-issue states, suggests another reason for caution before accepting the predictions of these

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3. *Jurisdictions in which adoption is weakly predicted to increase crime.*

We identified fourteen jurisdictions in which adoption of the law predicted annual percentage increases in crime but were not statistically significant at the five-percent level. Risk-aversion would probably counsel against passing (or retaining) shall-issue laws in these jurisdictions. Under this normative reckoning, eleven of these jurisdictions had previously passed the law and thus would be targets for repeal while three should simply continue to refrain from adopting the law.

4. *Jurisdictions in which adoption is weakly predicted to decrease crime.*

Finally, we identified five jurisdictions in which the predicted impact of the law on a harm-weighted measure of crime was beneficial but not statistically so. A libertarian would argue that these five states—which all previously passed the law—should retain them. However it should be noted that in four of these five states (Virginia, West Virginia, Pennsylvania, and Montana) the shall-issue law was estimated in Table 14 to have a deleterious (but statistically insignificant) impact on crime so that the more direct measure of impact would weakly counterindicate passage of the statute.[130]

In sum, this analysis suggests the following legislative (in)action: Six states should retain their current shall-issue laws; one state (California) should adopt the law for the first time; twenty-six states should repeal the law; and nineteen states should continue to refrain from adopting.[131] For all of the reasons set forth above, this analysis can only be taken as suggestive of the type of nuanced policy recommendations that are possible from statistical analysis of state-specific estimates of the impact of the law. Nonetheless, we think it can provide a useful blueprint for researchers who have rich enough data and well-specified state-specific regression results to be able to make more finely

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regressions.

130. Conversely, in two states (Georgia and Oregon) the law was predicted (in Appendix Table 8) to have a deleterious impact while the more direct estimate (from Table 14) was beneficial (but statistically insignificant). Accordingly these two states might be deemed to be reclassified as weakly indicated for passage (although a risk averse analyst might think that a state showing either a deleterious predicted or actual estimate was contraindicated). These discrepancies between the predicted effects from the Table 16 regressions and the more direct estimated effects from the full regressions that generated Table 14 underscores again the fragility of these results.

131. Lott's own analysis suggests that issuing an unlimited number of permits may produce a pernicious impact on crime. In his second edition, Lott estimated, in an aggregate specification, that when concealed handgun permits exceeded a maximum percentage of the population, additional permits would be predicted to increase crime. LOTT, *supra* note 2, at 178-80. If Lott is right on this point, a state that passed the law under the foregoing normative account might be advised to cap the maximum number of permits issued.

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grained policy recommendations than are possible from the typical aggregated analysis.

CONCLUSION

Judge Richard Posner has recently criticized moral philosophy for failing to persuade on any contentious issue.[132] A similar criticism might be made of quantitative empiricism. Readers might tend to accept only those quantitative analyses that resonate with their prior normative beliefs. Indeed, Judge Stephen Reinhardt famously proclaimed at a Yale seminar that social science had never affected his judicial decisionmaking.[133] And Donald Braman and Dan Kahan have recently called upon econometricians like Lott and us to put away our statistical packages. In a piece provocatively titled, "More Statistics, Less Persuasion," Braman and Kahan argue that rather than "quantifying the impact of gun control laws on crime," academics "should dedicate themselves to constructing a new expressive idiom that will allow citizens to debate the cultural issues that divide them."[134]

We disagree. Over time, a body of empirical research can disentangle thorny issues of causation and lead toward consensus. We view this Article as playing a role in this process (not in ending the conversation). On net, we believe that Lott and Mustard's efforts made an important contribution to the literature. They asked the initial question, amassed an important new panel dataset, and then energetically and creatively analyzed it. (Indeed, their dataset, which we know from experience was quite costly to construct, has been used by many researchers to explore this and other questions about crime.) Nevertheless, their results have not withstood the test of time. When we added five years of county data and seven years of state data, allowing us to test an additional fourteen jurisdictions that adopted shall-issue laws, the previous Lott and Mustard findings proved not to be robust. Importantly, we showed that the Lott and Mustard results collapse when the more complete county data is subjected to less-constrained jurisdiction-specific specifications or when the more-complete state data is tweaked in plausible ways. No longer can any plausible case be made on statistical grounds that shall-issue laws are likely to reduce crime for all or even most states. How much further one can go in arguing that shall-issue laws likely increase crime across the board or have heterogeneous effects across states (albeit most commonly pernicious) will be matters about which various analysts will differ. We conclude with Learned Hand's advice that, unlike a policy advocate, an academic must "keep an open

132. RICHARD A. POSNER, THE PROBLEMATICS OF MORAL AND LEGAL THEORY (1999).

133. Professor Ayres was at the seminar, which occurred, to the best of his memory, in the latter half of the 1990s.

134. Donald Braman & Dan M. Kahan, More Statistics, Less Persuasion: A Cultural Theory of Gun-Risk Perceptions (2002) (unpublished manuscript, on file with authors).

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mind to every disconcerting fact, [and] an open ear to the cold voice of doubt." Hand admonished: "You may not carry a sword beneath a scholar's gown."[135]

APPENDIX I: CODING THE TIMING AND STATUS OF SHALL-ISSUE LAWS

The dataset that Lott and Mustard initially constructed covered the years 1977-1992. Because of their fixed-effects estimation technique, their analysis is able to measure the effect of the law only for those states that changed their legal status over this period. Hence, the coding of any state as either a shall-issue or non-shall-issue state will not influence the estimated effect on crime as long as the legal status persisted over the entire sample time period.[136] Perhaps surprisingly, there are conflicts among the supporters and opponents of gun control legislation about whether various jurisdictions even have a shall-issue law or not. For example, the National Rifle Association characterizes Alabama and Connecticut as having shall-issue laws while the Brady Campaign to Prevent Gun Violence treat both states as having a more discretionary system of providing permits to carry concealed weapons (a so-called "may-issue" law).[137] Since neither of these states' laws officially changed status after 1977 (although query whether administrative enforcement patterns, as well as citizen behavior concerning the purchase and carrying of handguns, may have changed over this period), this dispute will have no bearing on the estimated effect of

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135. Louis L. Jaffe, *Profesors and Judges as Advisors to Government: Reflections on the Roosevelt-Frankfurter Relationship*, 83 HARV. L. REV. 366 (1969) (quoting LEARNED HAND, THE SPIRIT OF LIBERTY 138 (1952)).

136. Since the data from these states will influence the year effects and the estimated coefficients for the various explanatory variables, their inclusion in the analysis—as opposed to the coding of their shall-issue laws—will have an indirect influence on the estimated effect of shall-issue laws.

137. *See* http://www.bradycampaign.org/facts/gunlaws/ccw.asp. The NRA's description of the relevant Alabama law suggests that demonstrating need is in fact a requirement for obtaining a concealed-carry permit (http://www.nraila.org/GunLaws.asp?FormMode=Detail&R=AL), which would seemingly support the coding advanced by the Brady Campaign. On the other hand, it might well be the case that a law that, on the surface, seems to be a "may issue" law was always, or came to be, administered as a shall-issue law. Indeed, between 1985 and the present, every southern state from Texas to Virginia—with the single exception of Alabama—adopted a shall-issue law, and it is possible that this lone exception to a universal southern trend of adoption reflects the NRA's recognition that Alabama had become a shall-issue state in practice even if it had not originally been one by virtue of statutory language.

The case of Connecticut is more complicated, and since no state bordering on Connecticut has enacted the law, one cannot draw inferences about the enforcement of the law from geography as we suggested for Alabama. In January 1978, there was debate over whether local police chiefs (who first need to approve any application to carry a pistol before state-level approval can be sought) were being too stringent in rejecting the applications. At that point, nearly 50,000 Connecticut residents held state pistol permits, up from 27,628 in 1973. Apparently, concerns by the police about who was getting gun permits led local police chiefs to begin setting their own ground rules for determining who should get permits. *See* Millstein, *supra* note 20, at 1.

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shall-issue laws. Nonetheless, there are numerous disagreements among different scholars about the timing of adoption of shall-issue laws that can influence these estimates.[138] For purposes of replication, we generally tried to use shall-issue codings that were closer to those employed by Lott and Mustard. In light of our need to update the data beyond what Lott and Mustard's coding sources provided, we relied heavily on Vernick's coding, as this was an up-to-date coding of shall-issue laws by independent researchers. Appendix Table 1 sets forth the list of states that have shall-issue laws under the coding of Lott and Vernick (and underscores some discrepancies in Lott's coding).

Choices have to be made not only about identifying when and if a state adopts a shall-issue law but also how to begin modeling its effect. Lott states that he assumes the effect of the shall-issue law would emerge in the first year after the law takes effect. However, Lott coded the shall-issue law dummy in that fashion only for Florida and Georgia, with all other states being coded so that the effect of the law begins in the year of passage.[139] Appendix Table 1 shows the passage dates of the various shall-issue laws we employ when attempting to adhere to Lott's coding, our differences with Lott and Mustard, and their own inconsistencies across models. The coding used by Vernick, which we also employ in our regressions, is set forth in column D. Our postpassage dummy, as well as our postpassage trends, are coded to begin in the year following the passage dates indicated in either column A or D of Appendix Table 1, depending on whether we are trying to follow Lott or Vernick's coding.

Note that there is imprecision in these dates both because the statutes are not entirely clear about the precise legal status and because adoption of a shall-issue law does not perfectly equate with the actual enforcement of the law either within the state or over time since enforcement could be quite different county-by-county and year-by-year. This problem will exist whenever one must characterize imprecisely defined statutes into sharply delineated discrete categories so that one can say when a state changes from one category to

138. While conceding that there are different interpretations of which states have shall-issue laws, Lott and Mustard indicate that they follow the shall-issue law classification found in Clayton E. Cramer & David B. Kopel, *"Shall-issue": The New Wave of Concealed Handgun Permit Laws*, 62 TENN. L. REV. 679, 691 (1995). *See* Lott & Mustard, *supra* note 1, at 12 n.33. Lott and Mustard cite two states in particular—Maine and Virginia—as potentially not "true" shall-issue states though they state that their results are not affected by either redefining or dropping these states altogether. *See id.* at 12 nn.33-35, 23 n.49. Indeed, other scholars provide different dates of passage of the shall-issue laws for these two states than those offered by Cramer and Kopel. *See* Vernick & Hepburn, *supra* note 28, tbl.9A-5, at 387, 390.

139. Ayres & Donohue, *supra* note 15, at 449 n.21. Lott made different coding choices in his linear trend analysis, coding the enactment dates in Oregon, Pennsylvania, Virginia, and Philadelphia earlier than was proper. Lott's coding can be found in state datasets downloadable from http://www.law.yale.edu/ayres/.

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another. The difficulty is greatest when the statutory language of the shall-issue law invokes the command "shall" but then includes inherently discretionary criteria (such as a requirement for the "good moral character" of the permit recipient), or where the law says "may," but some counties or issuing authorities make it quite easy to obtain a permit. In either of these circumstances, there may be important differences between the law in practice and the law on the books; yet none of those nuances will be captured in our statistical analysis.[140]

APPENDIX II: DATA SOURCES

The following describes the source of the data used in the state dataset.

Variable	Source	Description
Crime Rates	Bureau of Justice Statistics	Per 100,000 population
Arrest Rates	Lott	Considered states with 0 arrest rate to be missing observations (24 total changes made over 9 crime categories)
Incarceration Rates	Sourcebook of Criminal Justice Statistics	Lagged one year, sentenced prisoners per 100,000 resident population
State Population	US Census Bureau	
Demographic Groups	US Census Bureau	Converted into percentage of total state population
Income (income, income maintenance, and unemployment insurance)	Regional Economic Information System	Adjusted to 1983 dollars
Density	Land Area in Square Miles obtained from Statistical Abstract of the United States	Density = Population per square mile

140. *See* Vernick & Hepburn, *supra* note 138, at 29. While there are enough classification discrepancies among the different authors that have tried to determine the presence of shall-issue laws that it becomes burdensome to probe all of the possible permutations, our efforts suggested that the aggregated results (which are weighted by state population, thereby sharply reducing the impact of small states on the analysis) were not highly sensitive to these classification issues. Of course, any estimated effects of the shall-issue laws for individual states will be far more sensitive to the classification issues for these small states.

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APPENDIX TABLE 1: DIFFERENCES IN CODING DATES OF PASSAGE OF SHALL-ISSUE LAWS

	(A)	(B)	(C)	(D)
		Implied Date of Passage		
State	Ayres & Donohue	Lott & Mustard Dummy*	Lott & Mustard Trend	Date of Passage Vernick's Coding
Maine	1985	1984	1985	1981
Florida	1987	1987	1987	1987
Virginia	1988	1987	1987	1995
Georgia	1989	1989	1989	1989
Pennsylvania**	1989	1988	1988	1989
West Virginia	1989	1988	1989	1989
Idaho	1990	1989	1990	1990
Mississippi	1990	1989	1990	1990
Oregon	1990	1989	1989	1990
Montana	1991	1990	1991	1991
Alaska	1994			1994
Arizona	1994			1994
Tennessee	1994			1994
Wyoming	1994			1994
Arkansas	1995			1995
North Carolina	1995			1995
Nevada	1995			1995
Oklahoma	1995			1995
Texas	1995			1996
Utah	1995			1986
Philadelphia**	1995			
Kentucky	1996			1996
Louisiana	1996			1991
South Carolina	1996			1996

Both Lott/Mustard and Vernick considered the following states to have adopted shall issue laws prior to 1977: Indiana, New Hampshire, and Washington. Lott and Mustard also considered Alabama, Connecticut, North Dakota, South Dakota, and Vermont as having adopted shall issue laws prior to 1977. Of those states, Vernick listed both North Dakota and South Dakota as states adopting shall issue laws in 1985, while he considered Alabama and Connecticut as states that never adopted shall issue laws. Finally, Vernick pointed out that in Vermont, no permit is necessary to carry a concealed weapon.

* We followed the protocol of beginning the first year of the post-passage dummy in the first full year after passage, which we understood to be the protocol of Lott and Mustard. On the assumption that they did follow that protocol, we list the dates of passage of shall issue laws that would be implied in their analysis. The data supplied to us by Lott and Mustard went through 1992, so we don't show dates for them after that year.

** Pennsylvania initially excluded Philadelphia from its 1989 shall-issue law. In 1995, the law was extended to include Philadelphia.

Note: The shaded lines highlight instances of conflicts between the Lott coding and the Vernick coding.

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APPENDIX TABLE 2A: AYRES AND DONOHUE'S EXPANDED LOTT DATASET: LIST OF VARIABLES AND SUMMARY STATISTICS

Variable	Description	Obs	Mean	Std. Dev.	Min	Max
lvio	ln(violent crime rate (per 100,000))	1428	5.971531	0.6543401	3.826465	7.979955
lmur	ln(murder rate (per 100,000))	1428	1.805917	0.703457	-1.609438	4.389499
lrap	ln(rape rate (per 100,000))	1428	3.391876	0.4712118	1.589235	4.626932
laga	ln (aggravated assault rate (per 100,000))	1428	5.440435	0.6506613	3.328627	7.350902
lrob	ln(robbery rate (per 100,000))	1428	4.668568	0.9453066	1.856298	7.399459
lpro	ln(property crime rate (per 100,000))	1428	8.356463	0.2943492	7.175796	9.16032
lbur	ln(burglary rate (per 100,000))	1428	6.98	0.3846931	5.729775	7.974774
llar	ln(larceny rate (per 100,000))	1428	7.920612	0.2970966	6.665301	8.671424
laut	ln(auto crime rate (per 100,000))	1428	5.8785	0.5515542	4.507557	7.517467
v_shall	Vernick's coding of the shall dummy	1428			0	1
stpop	State population	1428	4703487	5117964	324465	3.31×10^7
rpcpi	Real per capita personal income	1428	13280.69	2618.36	7644.664	23646.71
rpcui	Real per capita unempl insurance payments	1428	71.72929	47.68851	9.967369	411.6423
rpcim	Real per capita income maintenance	1428	170.4768	68.18757	41.25565	494.4506
densitym	Population per square mile of land area	1428	356.6089	1407.823	0.5696366	12167.85
incarc_rate	Incarceration Rate per 100,000, lagged 1 yr	1428	201.4424	172.2202	19	1913
aovio	Arrest Rate, from Lott's original state dataset	1041	39.68218	20.69746	0	558.81
aopro	Arrest Rate, from Lott's original state dataset	1048	16.47071	4.961436	0	58.56
aomur	Arrest Rate, from Lott's original state dataset	1046	88.02721	52.75273	0	1363.16
aorap	Arrest Rate, from Lott's original state dataset	1041	38.04754	17.26306	0	310.63
aorob	Arrest Rate, from Lott's original state dataset	1047	30.07347	13.36918	0	195.2
aoaga	Arrest Rate, from Lott's original state dataset	1048	43.58897	17.30412	0	190.93
aobur	Arrest Rate, from Lott's original state dataset	1048	13.26896	4.67076	0	44.73
aolar	Arrest Rate, from Lott's original state dataset	1048	18.01515	5.475389	0	77.27
aoaut	Arrest Rate, from Lott's original state dataset	1048	20.08427	33.46592	0	394.28
fipsstat	Numerical state identifier	1428			1	56
year	Numerical year identifier	1428			72	99
vernick_shall	Equivalent to Zheng's yradopt	1428			0	96
pbm1019	% Pop Black Male Aged 10-19	1428	1.034473	1.133384	0.0220235	7.135754
pbm2029	% Pop Black Male Aged 20-29	1428	0.9025913	0.9811033	0.0308761	6.571098
pbm3039	% Pop Black Male Aged 30-39	1428	0.7074403	0.8214441	0.0131709	5.368865
pbm4049	% Pop Black Male Aged 40-49	1428	0.5113697	0.619647	0.0072583	4.44528
pbm5064	% Pop Black Male Aged 50-64	1428	0.5243105	0.6860058	0.0012312	4.82312
pbmo65	% Pop Black Male Aged over 65	1428	0.3556466	0.4857088	0.0014183	3.556977
pbf1019	% Pop Black Female Aged 10-19	1428	1.02034	1.147568	0.0159746	7.390942
pbf2029	% Pop Black Female Aged 20-29	1428	0.9774034	1.157651	0.0169809	7.729461
pbf3039	% Pop Black Female Aged 30-39	1428	0.8107628	0.9713302	0.0092844	6.112494
pbf4049	% Pop Black Female Aged 40-49	1428	0.6017382	0.7530134	0.0050729	5.446821
pbf5064	% Pop Black Female Aged 50-64	1428	0.6540152	0.8833761	0.0019866	6.116782
pbfo65	% Pop Black Female Aged over 65	1428	0.5415593	0.7896529	0.0013851	6.115532
pwm1019	% Pop White Male Aged 10-19	1428	6.885923	1.729288	1.157911	10.75591
pwm2029	% Pop White Male Aged 20-29	1428	6.968634	1.334905	3.05121	10.84577
pwm3039	% Pop White Male Aged 30-39	1428	6.387067	1.254675	1.804001	9.731769
pwm4049	% Pop White Male Aged 40-49	1428	5.151871	1.166269	1.346922	8.659682
pwm5064	% Pop White Male Aged 50-64	1428	5.879721	1.062361	1.783073	8.037685
pwmo65	% Pop White Male Aged over 65	1428	4.288742	1.158931	0.6743689	7.505016
pwf1019	% Pop White Female Aged 10-19	1428	6.565628	1.699978	1.184778	10.59967
pwf2029	% Pop White Female Aged 20-29	1428	6.829447	1.34522	2.216033	9.662052
pwf3039	% Pop White Female Aged 30-39	1428	6.349452	1.240235	1.5848	9.385077
pwf4049	% Pop White Female Aged 40-49	1428	5.179891	1.135794	1.200093	8.411426
pwf5064	% Pop White Female Aged 50-64	1428	6.896208	1.479002	1.649705	11.36171
pwfo65	% Pop White Female Aged over 65	1428	6.182254	1.679061	0.7476923	9.902048
pnm1019	% Pop Neither W nor B Male Aged 10-19	1428	0.3777065	0.7904053	0.0145746	6.702039
pnm2029	% Pop Neither W nor B Male Aged 20-29	1428	0.3475048	0.7212771	0.0170222	6.465612
pnm3039	% Pop Neither W nor B Male Aged 30-39	1428	0.3007067	0.6514341	0.0139999	5.061728
pnm4049	% Pop Neither W nor B Male Aged 40-49	1428	0.2186409	0.5309817	0.008304	4.95868
pnm5064	% Pop Neither W nor B Male Aged 50-64	1428	0.2060169	0.6201685	0.007015	4.910496
pnmo65	% Pop Neither W nor B Male Aged over 65	1428	0.1288453	0.4792777	0.0050317	4.280399
pnf1019	% Pop Neither W nor B Female Aged 10-19	1428	0.3672072	0.7710804	0.0154089	6.687914
pnf2029	% Pop Neither W nor B Female Aged 20-29	1428	0.3546779	0.7204218	0.0222513	6.3433
pnf3039	% Pop Neither W nor B Female Aged 30-39	1428	0.3277798	0.6839861	0.0188864	5.324602
pnf4049	% Pop Neither W nor B Female Aged 40-49	1428	0.2432578	0.5715415	0.0110117	5.08445
pnf5064	% Pop Neither W nor B Female Aged 50-64	1428	0.2346599	0.6891328	0.0092633	5.365767
pnfo65	% Pop Neither W nor B Female Aged over 65	1428	0.2026374	0.8711339	0.0067783	10.9263

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APPENDIX TABLE 2B: ZHENG'S DATASET: LIST OF VARIABLES AND SUMMARY STATISTICS

Variable	Description	Obs	Mean	Std. Dev.	Min	Max
lvio	ln(violent crime rate (per 100,000))	1400	5.939708	0.6197534	3.826465	7.126328
lmur	ln(murder rate (per 100,000))	1400	1.766978	0.6511872	-1.609438	3.010621
lrap	ln(rape rate (per 100,000))	1400	3.380057	0.4663134	1.589235	4.626932
laga	ln (aggravated assault rate (per 100,000))	1400	5.417609	0.6333402	3.328627	6.666575
lrob	ln(robbery rate (per 100,000))	1400	4.622909	0.8967052	1.856298	6.527958
lpro	ln(property crime rate (per 100,000))	1400	8.345676	0.2862991	7.175796	8.986696
lbur	ln(burglary rate (per 100,000))	1400	6.969619	0.3803424	5.729775	7.974774
llar	ln(larceny rate (per 100,000))	1400	7.910755	0.2908949	6.665301	8.538191
laut	ln(auto crime rate (per 100,000))	1400	5.860307	0.5374967	4.507557	7.359531
shalll	Vernick's coding of the shall dummy	1400			0	1
pop	State population	1400	4785007	5136519	326494	3.31×10^7
rincome	Real per capita income	1400	19335.41	4340.893	8824.639	36795.03
unemp	Unemployment rate	1400	6.213143	2.10274	2.2	18
poverty	% Persons below poverty line	1400	12.82964	3.991182	2.9	30.7
police_per1	% Police, lagged 1 year	1400	196.6087	48.98285	61.89	400.5193
prison_per1	Incarceration rate, lagged 1 year	1400	190.0616	126.9586	20.3402	752.3188
alcohol_per	Average per capita alcohol consumption	1400	1.986453	0.5379952	0.4770634	4.999822
fipsstat	Numerical state indicator	1400			1	56
year	Numerical year indicator	1400			1972	1999
yradopt	Vernick's coding of the year of adoption	1400			0	96
black_per	% Black population	1400	9.454077	9.254046	0.1748652	36.66171
metro_per	% Metropolitan population	1400	62.9396	22.86231	0	100.3756
age1_per	% Population Aged 15-17	1400	4.904386	0.8245765	3.403702	6.905295
age2_per	% Population Aged 18-24	1400	11.63615	1.673176	7.800048	16.10618
age3_per	% Population Aged 25-34	1400	15.77754	1.865325	10.64819	23.5762

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APPENDIX TABLE 3A: THE ESTIMATED IMPACT OF SHALL-ISSUE LAWS ON CRIME
STATE DATA, 1977-1999 (LOTT'S CODING), USING INCARCERATION RATES
(ANALOGOUS TO TABLE 3A, BUT WITH LOTT'S CODING)

Time Period (1977-1999)	Violent Crime	Murder	Rape	Aggravated Assault	Robbery	Property Crime	Auto Theft	Burglary	Larceny
1. Dummy variable model:	**-3.3%**	-1.2%	**-6.7%**	**-4.5%**	0.3%	**2.2%**	**7.6%**	**-3.2%**	**3.2%**
Robust standard error:	(1.6%)	(2.0%)	(1.6%)	(2.2%)	(2.2%)	(1.0%)	(2.0%)	(1.4%)	(1.0%)
2. Lott spline model:	-0.8%	**-1.1%**	**-1.1%**	-0.8%	**-1.4%**	-0.2%	0.2%	**-1.0%**	-0.2%
Robust standard error:	(0.4%)	(0.5%)	(0.4%)	(0.5%)	(0.5%)	(0.2%)	(0.4%)	(0.3%)	(0.2%)
3. Hybrid model:									
Postpassage dummy	-2.4%	1.7%	**-4.4%**	**-5.7%**	**5.5%**	2.4%	**7.1%**	0.6%	2.4%
Robust standard error:	(1.9%)	(2.4%)	(2.2%)	(2.4%)	(2.7%)	(1.3%)	(2.5%)	(1.7%)	(1.3%)
Trend effect	-0.6%	**-1.3%**	-0.8%	-0.5%	**-1.8%**	-0.3%	-0.3%	**-1.1%**	-0.3%
Robust standard error:	(0.4%)	(0.5%)	(0.4%)	(0.6%)	(0.5%)	(0.2%)	(0.4%)	(0.3%)	(0.2%)

Notes: The dependent variable is the ln(crime rate) named at the top of each column. The data set is comprised of annual state-level observations (including the District of Columbia). 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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APPENDIX TABLE 3B: THE ESTIMATED IMPACT OF SHALL-ISSUE LAWS ON CRIME CONTROLLING FOR STATE TRENDS STATE DATA, 1977-1999 (LOTT'S CODING), USING INCARCERATION RATES (ANALOGOUS TO TABLE 3B, BUT WITH LOTT'S CODING)

Time Period (1977-1999)	Violent Crime	Murder	Rape	Aggravated Assault	Robbery	Property Crime	Auto Theft	Burglary	Larceny
1. Dummy variable model:	-0.3%	1.9%	**-3.7%**	-3.1%	**5.2%**	**3.3%**	**5.0%**	1.8%	**3.7%**
Robust standard error:	(1.6%)	(2.4%)	(1.6%)	(1.9%)	(2.5%)	(1.3%)	(2.2%)	(1.6%)	(1.2%)
2. Hybrid model:									
Postpassage dummy	-1.9%	4.2%	**-3.8%**	**-6.1%**	**6.2%**	**3.6%**	**7.7%**	2.6%	**3.5%**
Robust standard error:	(1.7%)	(2.3%)	(1.7%)	(2.2%)	(2.6%)	(1.3%)	(2.1%)	(1.6%)	(1.3%)
Trend effect	**1.5%**	**-2.2%**	0.1%	**2.8%**	-0.9%	-0.3%	**-2.5%**	-0.7%	0.1%
Robust standard error:	(0.4%)	(0.7%)	(0.5%)	(0.6%)	(0.7%)	(0.3%)	(0.6%)	(0.4%)	(0.3%)

Notes: The dependent variable is the ln(crime rate) named at the top of each column. The data set is comprised of annual state-level observations (including the District of Columbia). 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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APPENDIX TABLE 4A: THE ESTIMATED IMPACT OF SHALL-ISSUE LAWS ON CRIME STATE DATA, 1991-1999 (LOTT'S CODING), USING INCARCERATION RATES (ANALOGOUS TO TABLE 4A, BUT WITH LOTT'S CODING)

Time Period (1991-1999)	Violent Crime	Murder	Rape	Aggravated Assault	Robbery	Property Crime	Auto Theft	Burglary	Larceny
1. Dummy variable model:	2.7%	7.2%	-1.0%	-1.0%	**11.8%**	**6.3%**	**11.4%**	**6.0%**	**5.4%**
Robust standard error:	(2.3%)	(3.7%)	(2.0%)	(3.2%)	(2.3%)	(1.4%)	(3.0%)	(1.5%)	(1.4%)
2. Lott spline model:	-1.3%	-1.6%	**-3.0%**	-1.6%	0.0%	0.4%	0.1%	1.0%	-0.3%
Robust standard error:	(1.3%)	(2.2%)	(1.4%)	(1.8%)	(1.7%)	(1.0%)	(2.0%)	(1.1%)	(0.9%)
3. Hybrid model:									
Postpassage dummy	1.7%	3.9%	2.6%	-2.0%	**7.8%**	**5.3%**	**13.8%**	**4.5%**	**4.5%**
Robust standard error:	(2.7%)	(4.2%)	(2.2%)	(3.9%)	(2.9%)	(1.8%)	(3.6%)	(2.0%)	(1.7%)
Trend effect	-1.6%	-2.1%	**-3.4%**	-1.3%	-1.1%	-0.4%	-1.9%	0.4%	-0.9%
Robust standard error:	(1.4%)	(2.2%)	(1.5%)	(1.9%)	(1.6%)	(0.9%)	(1.8%)	(1.1%)	(0.9%)

Notes: The dependent variable is the ln(crime rate) named at the top of each column. The data set is comprised of annual state-level observations (including the District of Columbia). 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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APPENDIX TABLE 4B: THE ESTIMATED IMPACT OF SHALL-ISSUE LAWS ON CRIME CONTROLLING FOR STATE TRENDS
STATE DATA, 1991-1999 (LOTT'S CODING), USING INCARCERATION RATES
(ANALOGOUS TO TABLE 4B, BUT WITH LOTT'S CODING)

Time Period (1991-1999)	Violent Crime	Murder	Rape	Aggravated Assault	Robbery	Property Crime	Auto Theft	Burglary	Larceny
1. Dummy variable model:	0.8%	-3.1%	-0.2%	-0.5%	3.3%	**4.0%**	**<u>9.3%</u>**	2.1%	**3.6%**
Robust standard error:	(2.0%)	(4.3%)	(1.9%)	(3.0%)	(2.3%)	(1.8%)	(3.2%)	(1.8%)	(1.7%)
2. Hybrid model:									
Postpassage dummy	0.6%	-2.0%	<u>3.9%</u>	-0.9%	2.2%	**4.6%**	**<u>10.4%</u>**	1.9%	**4.6%**
Robust standard error:	(2.2%)	(4.5%)	(2.1%)	(3.1%)	(2.5%)	(1.9%)	(3.5%)	(2.0%)	(1.9%)
Trend effect	0.3%	-1.7%	**<u>-6.1%</u>**	0.7%	1.7%	-1.0%	-1.7%	0.3%	-1.5%
Robust standard error:	(1.4%)	(2.6%)	(1.4%)	(2.0%)	(1.5%)	(1.0%)	(2.0%)	(1.1%)	(1.0%)

Notes: The dependent variable is the ln(crime rate) named at the top of each column. The data set is comprised of annual state-level observations (including the District of Columbia). 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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APPENDIX TABLE 5A: THE ESTIMATED IMPACT OF SHALL-ISSUE LAWS ON CRIME
STATE DATA, 1977-1999 (LOTT'S CODING), USING INCARCERATION RATES
ADDING IN DUMMIES FOR YEARS MORE THAN 8 YEARS BEFORE OR 3 YEARS AFTER SHALL-ISSUE LAW ADOPTION
(ANALOGOUS TO TABLE 8A, BUT WITH LOTT'S CODING)

Time Period (1977-1999)	Violent Crime	Murder	Rape	Aggravated Assault	Robbery	Property Crime	Auto Theft	Burglary	Larceny
1. Dummy variable model:	-2.1%	1.4%	**-4.2%**	-4.2%	3.5%	**2.6%**	**8.5%**	-0.8%	**3.0%**
Robust standard error:	(1.7%)	(2.0%)	(1.9%)	(2.1%)	(2.4%)	(1.1%)	(2.1%)	(1.5%)	(1.1%)
2. Lott spline model:	-0.2%	-0.6%	-0.1%	-0.5%	-0.6%	0.0%	0.4%	-0.2%	-0.3%
Robust standard error:	(0.6%)	(0.7%)	(0.6%)	(0.8%)	(0.8%)	(0.4%)	(0.7%)	(0.5%)	(0.4%)
3. Hybrid model:									
Postpassage dummy	-2.2%	1.3%	**-4.8%**	**-5.7%**	**6.0%**	2.6%	**6.5%**	1.0%	2.5%
Robust standard error:	(2.0%)	(2.4%)	(2.2%)	(2.5%)	(2.8%)	(1.3%)	(2.6%)	(1.8%)	(1.3%)
Trend effect	-0.1%	-0.6%	0.1%	-0.2%	-0.9%	-0.2%	0.1%	-0.2%	-0.4%
Robust standard error:	(0.6%)	(0.7%)	(0.6%)	(0.8%)	(0.8%)	(0.4%)	(0.7%)	(0.5%)	(0.4%)

Notes: The dependent variable is the ln(crime rate) named at the top of each column. The data set is comprised of annual state-level observations (including the District of Columbia). 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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APPENDIX TABLE 5B: THE ESTIMATED IMPACT OF SHALL-ISSUE LAWS ON CRIME CONTROLLING FOR STATE TRENDS
STATE DATA, 1977-1999 (LOTT'S CODING), USING INCARCERATION RATES
ADDING IN DUMMIES FOR YEARS MORE THAN 8 YEARS BEFORE OR 3 YEARS AFTER SHALL-ISSUE LAW ADOPTION
(ANALOGOUS TO TABLE 8B, BUT WITH LOTT'S CODING)

Time Period (1977-1999)	Violent Crime	Murder	Rape	Aggravated Assault	Robbery	Property Crime	Auto Theft	Burglary	Larceny
1. Dummy variable model:	-0.1%	2.8%	**-3.7%**	-3.6%	**6.9%**	**3.9%**	**5.6%**	**3.4%**	**3.8%**
Robust standard error:	(1.6%)	(2.4%)	(1.6%)	(2.0%)	(2.6%)	(1.3%)	(2.3%)	(1.6%)	(1.3%)
2. Hybrid model:									
Postpassage dummy	-2.0%	3.8%	**-4.4%**	**-6.5%**	**6.8%**	**3.9%**	**7.3%**	3.1%	**3.8%**
Robust standard error:	(1.8%)	(2.4%)	(1.7%)	(2.2%)	(2.6%)	(1.3%)	(2.2%)	(1.7%)	(1.3%)
Trend effect	**2.6%**	-1.3%	1.0%	**4.0%**	0.1%	0.0%	**-2.4%**	0.3%	0.1%
Robust standard error:	(0.5%)	(0.9%)	(0.7%)	(0.7%)	(0.9%)	(0.4%)	(0.8%)	(0.5%)	(0.4%)

Notes: The dependent variable is the ln(crime rate) named at the top of each column. The data set is comprised of annual state-level observations (including the District of Columbia). 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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APPENDIX TABLE 6: ESTIMATING THE EFFECT OF SHALL-ISSUE LAWS USING LOTT'S TIME TREND SPECIFICATION FOR SELECTED YEARS

	Violent Crime	Murder	Rape	Aggravated Assault	Robbery	Property Crime	Auto Theft	Burglary	Larceny
Panel A1:Lott's Results, 1977-1992 (Table 4.8)									
After - before	-0.9%	-3.0%	-1.4%	-0.5%	-2.7%	-0.6%	-0.3%	-1.5%	-0.1%
Significance level	0.01	0.01	0.01	0.01	0.01	0.01	0.05	0.01	not sig
Panel A2: Reproduction of Lott's Trend Analysis (1977-1992)									
Shall trend before	-0.0048	0.0061	-0.0032	-0.0125	0.0103	0.00828	0.01292	0.0076	0.0068
Shall trend after	-0.0098	-0.036	-0.017	-0.008	-0.0168	0.00143	0.00995	-0.0082	0.0057
After - before	-0.50%	-4.21%	-1.38%	0.45%	-2.71%	-0.69%	-0.30%	-1.58%	-0.11%
P-value	0.222	0	0.0073	0.3377	0	0.0234	0.5341	0	0.7661
Panel A3: Correcting Lott's Trends (1977-1992)									
Shall trend before	-0.0058	0.0054	-0.0026	-0.0129	0.0073	0.00642	0.0113	0.0048	0.00491
Shall trend after	-0.0104	-0.0442	-0.0207	-0.0089	-0.0121	0.00628	0.0111	-0.00034	0.0124
After - before	-0.46%	-4.96%	-1.81%	0.40%	-1.94%	-0.01%	-0.02%	-0.51%	0.75%
P-value	0.2961	0	0.0009	0.4312	0.0011	0.9657	0.9778	0.1285	0.0593
Panel B1: Lott's Results, 1977-1996 (Table 9.1)									
After - before	-2.3%	-1.5%	-3.2%	-3.0%	-1.6%	-2.5%	-2.1%	-2.5%	-0.9%
Significance level	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01
Panel B2: Reproduction of Lott's Trend Analysis (1977-1996)*									
Shall trend before	0.0022	0.0057	0.0096	0.0042	0.0100	0.0050	0.0130	0.0062	0.0082
Shall trend after	-0.0229	-0.0281	-0.0256	-0.0325	-0.0372	-0.0004	0.0089	-0.0211	-0.0040
After - before	-2.53%	-3.38%	-3.52%	-3.67%	-4.72%	-0.54%	-0.41%	-2.73%	-1.22%
P-value	0	0	0	0	0	0.0886	0.3946	0	0.0212
Panel B3: Correcting Lott's Trends (1977-1996)									
Shall trend before	0.0014	0.0058	0.0097	0.0036	0.0089	0.0045	0.0132	0.0055	0.0081
Shall trend after	-0.0117	-0.0238	-0.0246	-0.0229	-0.0262	0.0030	0.0026	-0.0167	-0.0041
After - before	-1.31%	-2.96%	-3.43%	-2.65%	-3.51%	-0.15%	-1.06%	-2.22%	-1.22%
P-value	0	0	0	0	0	0.56	0.0124	0	0.0042

* Following Lott's Table 9.1, year*region dummies were included in this model as well as a RPCRPO*(YEAR>92) dummy.

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APPENDIX TABLE 7: THE ESTIMATED IMPACT OF SHALL ISSUE LAWS ON CRIME, JURISDICTION-SPECIFIC HYBRID MODEL WITH STATE TRENDS

Time Period (1977-1997)	Violent Crime	Murder	Rape	Aggravated Assault	Robbery	Property Crime	Auto Theft	Burglary	Larceny
Maine									
Postpassage dummy	**-13.0%**	15.6%	12.9%	**-21.6%**	-7.7%	-5.2%	-4.9%	**-12.3%**	-6.1%
	(5.8%)	(17.9%)	(8.6%)	(7.2%)	(9.3%)	(3.5%)	(5.0%)	(3.5%)	(4.7%)
Trend effect	**2.7%**	-2.8%	2.3%	2.1%	2.6%	**1.9%**	0.1%	**4.5%**	**1.5%**
	(1.1%)	(3.0%)	(1.6%)	(1.5%)	(1.6%)	(0.6%)	(0.9%)	(0.7%)	(0.7%)
Florida									
Postpassage dummy	-2.0%	17.0%	21.2%	**45.1%**	**49.5%**	2.7%	**56.6%**	**53.0%**	**54.5%**
	(4.3%)	(12.9%)	(12.2%)	(11.9%)	(11.7%)	(6.4%)	(12.6%)	(9.9%)	(16.9%)
Trend effect	**-2.9%**	**-14.3%**	**-9.0%**	**-10.1%**	**-19.5%**	**-2.6%**	**-14.2%**	**-15.7%**	**-10.9%**
	(0.8%)	(2.7%)	(2.5%)	(2.5%)	(2.5%)	(0.8%)	(2.5%)	(2.1%)	(2.5%)
Virginia									
Postpassage dummy	-3.2%	10.2%	6.2%	-4.6%	-5.2%	2.4%	2.1%	0.1%	1.7%
	(3.0%)	(8.4%)	(4.4%)	(3.6%)	(3.9%)	(2.8%)	(4.2%)	(2.4%)	(4.1%)
Trend effect	0.8%	-0.5%	0.6%	**2.0%**	0.5%	0.5%	1.3%	0.2%	0.2%
	(0.6%)	(1.4%)	(0.8%)	(0.7%)	(0.7%)	(0.4%)	(0.8%)	(0.5%)	(0.5%)
Georgia									
Postpassage dummy	3.4%	10.7%	-1.8%	3.5%	2.8%	-1.9%	-3.3%	4.1%	-6.6%
	(3.8%)	(7.2%)	(5.3%)	(4.4%)	(4.4%)	(3.1%)	(4.4%)	(3.1%)	(4.1%)
Trend effect	**-3.5%**	**-4.5%**	**-6.0%**	-0.7%	**-6.8%**	**-3.2%**	-1.3%	**-6.1%**	**-2.2%**
	(0.8%)	(1.3%)	(1.0%)	(0.9%)	(1.0%)	(0.6%)	(1.0%)	(0.7%)	(0.7%)
Pennsylvania									
Postpassage dummy	-2.2%	-8.0%	3.5%	-0.6%	-2.4%	-1.4%	2.4%	1.3%	-4.4%
	(3.5%)	(7.3%)	(3.7%)	(4.3%)	(4.3%)	(2.3%)	(4.6%)	(2.3%)	(2.9%)
Trend effect	1.3%	2.1%	-0.3%	0.8%	**2.7%**	**3.1%**	-0.8%	**1.3%**	**4.3%**
	(0.8%)	(1.3%)	(0.8%)	(0.9%)	(1.0%)	(0.5%)	(1.1%)	(0.6%)	(0.5%)
Philadelphia									
Postpassage dummy	**13.9%**	5.5%	-6.8%	-9.7%	**24.8%**	**-30.1%**	**-16.3%**	**-17.7%**	-0.6%
	(6.6%)	(10.2%)	(6.6%)	(7.1%)	(8.1%)	(4.8%)	(8.2%)	(7.3%)	(5.6%)
Trend effect	-1.7%	5.5%	4.9%	**7.2%**	-6.4%	**20.8%**	**16.7%**	**5.7%**	0.0%
	(3.3%)	(3.6%)	(2.6%)	(3.0%)	(3.8%)	(2.0%)	(4.6%)	(2.3%)	(2.1%)
West virginia									
Postpassage dummy	-1.9%	11.2%	4.8%	0.8%	-13.8%	**10.4%**	**-17.9%**	**11.3%**	**9.0%**
	(6.2%)	(10.5%)	(8.9%)	(8.3%)	(7.8%)	(3.4%)	(5.6%)	(3.7%)	(4.3%)
Trend effect	**7.1%**	**-4.5%**	0.9%	**10.5%**	2.1%	-0.6%	0.3%	-0.3%	0.7%
	(1.3%)	(2.0%)	(1.7%)	(1.7%)	(1.7%)	(0.7%)	(1.4%)	(0.9%)	(0.8%)
Idaho									
Postpassage dummy	-4.3%	-26.9%	**18.6%**	-7.9%	2.1%	0.7%	1.0%	**-11.6%**	-0.7%
	(6.6%)	(20.2%)	(7.6%)	(7.6%)	(9.5%)	(3.7%)	(6.2%)	(5.2%)	(4.0%)
Trend effect	**3.9%**	**8.3%**	1.9%	**5.5%**	**4.7%**	1.4%	**3.8%**	**3.0%**	1.2%
	(1.5%)	(3.8%)	(1.7%)	(1.7%)	(2.0%)	(0.9%)	(1.6%)	(1.2%)	(0.9%)
Mississippi									
Postpassage dummy	-2.1%	-3.7%	-1.8%	-15.3%	3.5%	-7.9%	2.1%	-4.9%	**-19.8%**
	(9.1%)	(10.1%)	(10.0%)	(10.8%)	(10.5%)	(8.6%)	(11.9%)	(8.8%)	(9.1%)
Trend effect	**13.3%**	**8.1%**	4.1%	**16.7%**	**12.0%**	**6.7%**	**11.7%**	**7.4%**	**8.8%**
	(2.1%)	(2.0%)	(2.1%)	(2.5%)	(2.2%)	(2.0%)	(2.5%)	(2.0%)	(2.2%)
Oregon									
Postpassage dummy	-6.8%	-11.5%	3.5%	-2.7%	**-30.9%**	**-18.0%**	**-21.1%**	**-29.5%**	**-17.3%**
	(4.7%)	(11.3%)	(5.8%)	(7.8%)	(6.1%)	(3.2%)	(6.1%)	(4.2%)	(3.8%)
Trend effect	**3.8%**	-4.5%	**-4.0%**	**9.5%**	-0.8%	**4.4%**	**4.0%**	**1.9%**	**5.1%**
	(1.2%)	(2.6%)	(1.2%)	(1.8%)	(1.6%)	(0.7%)	(1.3%)	(0.9%)	(0.8%)
Montana									
Postpassage dummy	-2.0%	**-93.0%**	-22.3%	0.4%	-34.3%	-9.1%	-22.2%	-11.1%	-12.6%
	(27.1%)	(27.7%)	(41.5%)	(27.6%)	(44.2%)	(19.5%)	(20.6%)	(18.2%)	(20.2%)
Trend effect	8.6%	**13.0%**	-2.9%	**12.1%**	13.0%	4.7%	9.4%	6.3%	5.0%
	(4.9%)	(4.8%)	(9.0%)	(5.0%)	(8.2%)	(3.8%)	(4.8%)	(3.6%)	(4.0%)
Alaska									
Postpassage dummy	-0.6%	8.5%	-21.3%	-8.1%	39.1%	-1.1%	3.3%	7.4%	-0.9%
	(40.2%)	(62.1%)	(43.2%)	(44.6%)	(26.1%)	(28.9%)	(41.2%)	(40.2%)	(23.1%)
Trend effect	1.5%	6.7%	-3.7%	11.0%	-17.1%	7.2%	-2.5%	-3.8%	5.0%
	(15.9%)	(24.3%)	(18.6%)	(17.4%)	(11.3%)	(12.3%)	(15.7%)	(17.9%)	(9.9%)

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APPENDIX TABLE 7 (CONTINUED)

Time Period (1977-1997)	Violent Crime	Murder	Rape	Aggravated Assault	Robbery	Property Crime	Auto Theft	Burglary	Larceny
Arizona									
Post-Passage Dummy	7.7%	17.2%	-7.4%	6.6%	4.5%	**15.3%**	30.6%	5.9%	7.6%
	(8.3%)	(12.1%)	(8.0%)	(8.7%)	(9.9%)	(6.8%)	(17.1%)	(8.8%)	(6.2%)
Trend Effect	1.4%	0.2%	**7.4%**	0.6%	8.1%	-2.2%	-4.3%	6.0%	1.4%
	(3.7%)	(4.8%)	(3.4%)	(3.9%)	(4.3%)	(2.8%)	(6.5%)	(3.4%)	(2.7%)
Tennessee									
Post-Passage Dummy	7.0%	-3.3%	-5.3%	13.1%	-0.3%	4.9%	-2.5%	-1.8%	3.2%
	(10.1%)	(10.7%)	(10.6%)	(12.2%)	(8.9%)	(8.0%)	(12.4%)	(8.7%)	(8.3%)
Trend Effect	6.5%	**11.1%**	9.0%	7.0%	6.7%	3.5%	6.4%	6.3%	5.6%
	(4.6%)	(5.5%)	(4.9%)	(5.3%)	(4.2%)	(4.0%)	(6.0%)	(4.4%)	(4.0%)
Wyoming									
Post-Passage Dummy	-14.3%	39.5%	27.9%	-25.0%	7.5%	11.1%	16.5%	8.2%	5.1%
	(12.6%)	(47.1%)	(19.8%)	(15.7%)	(24.1%)	(6.5%)	(11.0%)	(10.3%)	(7.3%)
Trend Effect	7.5%	-12.2%	-8.8%	12.1%	9.9%	-0.4%	-0.9%	4.9%	0.7%
	(5.8%)	(17.2%)	(8.8%)	(7.3%)	(10.9%)	(2.7%)	(5.2%)	(4.2%)	(3.2%)
Arkansas									
Post-Passage Dummy	-7.5%	-30.9%	-1.9%	10.1%	-12.0%	5.8%	-3.1%	-14.9%	8.9%
	(18.6%)	(19.9%)	(15.7%)	(24.1%)	(17.6%)	(7.0%)	(11.9%)	(11.6%)	(7.5%)
Trend Effect	16.9%	21.3%	8.4%	12.7%	10.9%	-6.3%	1.2%	5.5%	-1.8%
	(12.7%)	(13.1%)	(10.2%)	(16.4%)	(10.7%)	(4.4%)	(7.4%)	(6.8%)	(4.6%)
Nevada									
Post-Passage Dummy	22.9%	32.6%	-4.8%	33.6%	15.7%	14.3%	10.6%	15.4%	18.0%
	(14.7%)	(46.3%)	(21.3%)	(19.8%)	(12.9%)	(9.2%)	(18.2%)	(11.4%)	(9.6%)
Trend Effect	-2.5%	5.5%	9.8%	-5.5%	0.8%	-0.2%	4.9%	5.9%	-3.1%
	(8.1%)	(23.2%)	(15.3%)	(10.6%)	(7.7%)	(5.4%)	(12.1%)	(7.4%)	(6.3%)
North Carolina									
Post-Passage Dummy	-0.8%	-1.7%	12.0%	8.4%	-4.9%	**12.1%**	**17.8%**	-0.3%	11.4%
	(8.2%)	(14.0%)	(10.6%)	(10.1%)	(8.9%)	(6.1%)	(7.8%)	(7.2%)	(6.6%)
Trend Effect	5.4%	4.4%	-6.1%	4.3%	4.4%	**-11.0%**	0.5%	-5.0%	-2.8%
	(5.2%)	(9.2%)	(6.5%)	(6.2%)	(6.0%)	(3.6%)	(5.1%)	(4.4%)	(4.1%)
Oklahoma									
Post-Passage Dummy	12.6%	12.6%	10.7%	23.2%	-14.1%	8.2%	-5.3%	3.2%	7.9%
	(13.3%)	(19.1%)	(11.6%)	(14.8%)	(11.0%)	(9.8%)	(9.6%)	(8.8%)	(13.4%)
Trend Effect	-7.9%	-3.7%	0.0%	-10.6%	3.1%	-7.8%	-3.0%	-4.7%	-1.2%
	(8.1%)	(10.9%)	(8.4%)	(9.0%)	(6.5%)	(5.6%)	(6.4%)	(5.5%)	(7.8%)
Texas									
Post-Passage Dummy	-3.1%	-3.9%	9.0%	4.0%	-10.3%	-8.1%	-10.9%	-9.0%	-0.8%
	(8.7%)	(10.8%)	(9.6%)	(16.2%)	(10.4%)	(5.9%)	(11.2%)	(6.6%)	(8.7%)
Trend Effect	-4.7%	-10.5%	-6.2%	-6.0%	-1.5%	-2.3%	-3.6%	-0.3%	-1.1%
	(5.7%)	(6.5%)	(6.2%)	(10.2%)	(6.7%)	(3.6%)	(6.9%)	(4.3%)	(5.2%)
Utah									
Post-Passage Dummy	7.9%	**55.3%**	-2.3%	19.6%	-4.3%	6.8%	35.6%	15.1%	2.6%
	(11.5%)	(14.9%)	(17.4%)	(13.2%)	(16.5%)	(6.2%)	(23.9%)	(11.6%)	(6.6%)
Trend Effect	11.2%	-14.5%	12.7%	7.5%	18.5%	-1.2%	3.4%	6.2%	-2.3%
	(6.4%)	(8.4%)	(10.8%)	(7.4%)	(10.6%)	(4.3%)	(14.8%)	(7.8%)	(4.2%)
Kentucky									
Post-Passage Dummy	-1.0%	**41.9%**	-13.5%	0.2%	**29.5%**	-7.9%	12.4%	-12.4%	**-13.5%**
	(12.0%)	(13.8%)	(7.8%)	(20.6%)	(8.5%)	(4.4%)	(17.1%)	(8.4%)	(3.9%)
Louisiana									
Post-Passage Dummy	**16.1%**	**33.5%**	**20.3%**	16.9%	**32.1%**	**17.6%**	**33.0%**	**22.3%**	**14.6%**
	(7.9%)	(8.0%)	(7.2%)	(11.4%)	(11.2%)	(6.1%)	(8.8%)	(7.0%)	(7.0%)
South Carolina									
Post-Passage Dummy	8.7%	13.5%	-4.5%	**14.3%**	10.8%	**8.3%**	**21.4%**	4.5%	3.7%
	(5.6%)	(7.3%)	(5.0%)	(6.1%)	(6.3%)	(3.7%)	(7.2%)	(3.7%)	(3.9%)

Notes: The dependent variable is the ln(crime rate) named at the top of each column. The data set is comprised of annual state-level observations (including the District of Columbia) over the period 1977-1997. State- and year- fixed effects are included in all specifications. All regressions are weighted by state population. Standard errors (in parentheses) are computed using the Huber-White robust estimate of variance. Coefficients that are significant at the .10 level are underlined. Coefficients that are significant at the .05 level are displayed in bold. Coefficients that are significant at the .01 level are both underlined and displayed in bold. Post-Passage Trends for KY, LA and SC are dropped due to late passage date.

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APPENDIX TABLE 8: PREDICTION RESULTS

Jurisdiction	Predicted Impact	Standard Error	t-value
Nebraska	0.456	0.141	3.225
Kansas	0.421	0.139	3.022
Rhode Island	0.419	0.135	3.112
Delaware	0.404	0.101	4.002
South Dakota	0.389	0.145	2.675
Missouri	0.382	0.137	2.787
Iowa	0.381	0.139	2.741
Connecticut	0.381	0.133	2.855
Massachusetts	0.357	0.131	2.727
Indiana	0.353	0.135	2.617
Minnesota	0.340	0.137	2.482
New Mexico	0.334	0.154	2.164
Vermont	0.327	0.089	3.666
District of Columbia	0.324	0.070	4.645
New Hampshire	0.309	0.155	1.993
Maryland	0.304	0.089	3.419
Wisconsin	0.300	0.138	2.169
North Dakota	0.297	0.163	1.826
Alabama	0.290	0.095	3.044
South Carolina(1996)	0.276	0.065	4.276
New Jersey	0.264	0.127	2.080
Michigan	0.260	0.133	1.953
Illinois	0.249	0.060	4.172
Louisiana(1996)	0.244	0.135	1.808
Nevada(1995)	0.233	0.057	4.124
Kentucky(1996)	0.230	0.058	3.946
Arkansas(1995)	0.227	0.056	4.082
Oklahoma(1995)	0.227	0.054	4.171
Hawaii	0.207	0.105	1.969
Alaska(1994)	0.202	0.003	68.300
Ohio	0.194	0.048	4.078
Philadelphia(1995)	0.182	0.133	1.368
Colorado	0.166	0.100	1.668
Tennessee(1994)	0.166	0.045	3.705
Washington	0.140	0.097	1.442
Arizona(1994)	0.111	0.041	2.724
North Carolina(1995)	0.103	0.046	2.222
Wyoming(1994)	0.101	0.055	1.826
Utah(1995)	0.092	0.056	1.650
Maine(1985)	0.084	0.060	1.387
Mississippi(1990)	0.066	0.053	1.230
New York	0.052	0.051	1.022
Oregon(1990)	0.040	0.138	0.294
Georgia(1989)	0.040	0.049	0.825
Idaho(1990)	0.021	0.052	0.393
West Virginia(1989)	-0.003	0.076	-0.033
Pennsylvania(1989)	-0.056	0.060	-0.929
Montana(1991)	-0.059	0.055	-1.085
Virginia(1988)	-0.071	0.094	-0.757
Florida(1987)	-0.100	0.067	-1.492
Texas(1995)	-0.204	0.062	-3.312
California	-0.538	0.184	-2.933

Note: These predictions are based on the Hybrid Model Harm-Weighted impact regession of Table 15

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