# Violence Policy Center: Concealed Carry Killers

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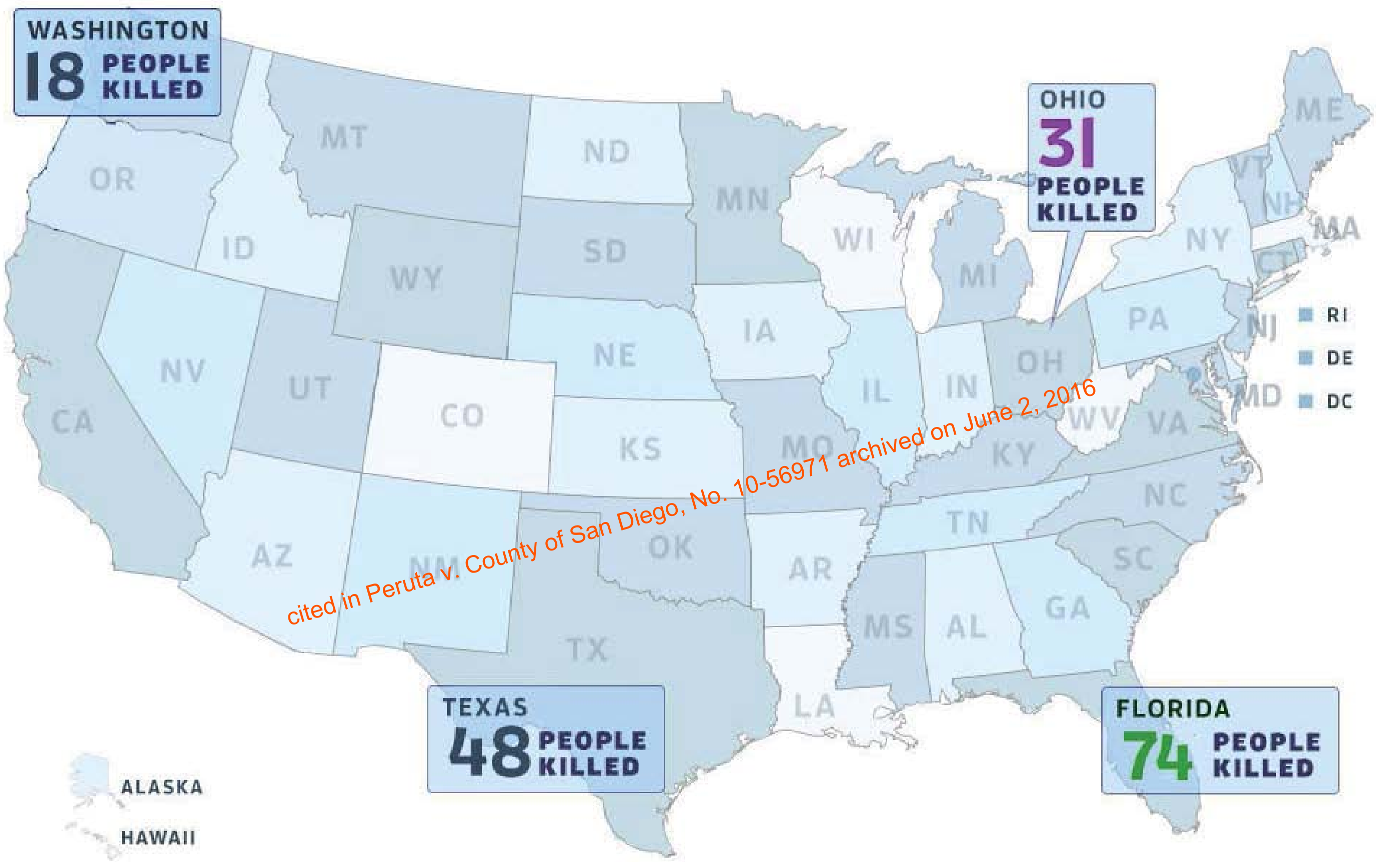






# Concealed Carry Killers

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

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

### Spotlight: Workplace Murder-Suicide in Indiana

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



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

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

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

*Last updated April 15, 2016*

**873**
**Total People Killed By Concealed Carry Killers**

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**29**
**Number of Mass Shootings Committed By Concealed Carry Killers**

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**53**
**Number of Murder-Suicides Committed By Concealed Carry Killers**

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**17**
**Law Enforcement Officers Killed By Concealed Carry Killers**



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

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

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

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JOHN J. DONOHUE

# 8 *The Impact of Concealed-Carry Laws*



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

This chapter draws freely on the work done in Ayres and Donohue (1999) and (forthcoming) and has profited from the outstanding research assistance of Matt Spiegelman, Emily Ryo, Melissa Ohsfeldt, Jennifer Chang, David Powell, and Nasser Zakariya. I am grateful for comments from John Lott, David Mustard, Willard Manning, and other participants in the Brookings Conference on Gun Violence.

I thank Christopher M. Cornwell, John R. Lott Jr., and the participants in the Brookings Conference on Gun Violence for their helpful comments.

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

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

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

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



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

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

2. Wilson (2000).

3. Wilson (2000).

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

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

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



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

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

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

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



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

## The Basic Methodology of Lott and Mustard

Lott and Mustard follow the basic contours of the current gold standard of microeconometric evaluation—a panel data model with fixed effects. That is, Lott and Mustard collect data over 1977–92 for individual states and counties across the United States, and then use panel data regression techniques to estimate the effect of the adoption of shall-issue laws, controlling for an array of social, economic, and demographic factors.[6] Essentially this approach determines for the ten states that adopted the shall-issue laws over this period how crime looks different after passage than it was before passage. In a study of this magnitude, the researcher must make many choices about data issues, model specification, and control variables, each of which has the potential to influence the outcome of the analysis in ways that are not often predictable.[7]

## *The Use of County Data*

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



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

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

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

9. Maltz and Targonski (2001).

### *Model Specification*

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

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

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



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

10. Ayres and Donohue (forthcoming).

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

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

## Lott and Mustard's Data

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



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

13. Ayres and Donohue (1999).

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

Percent

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

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

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

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



14. Lott and Mustard respond that the implications of the passage of a shall-issue law are uncertain since, for example, banks and businesses have always been protected by gun-toting personnel. Therefore, they contend, there may be substitution from highway robberies to robberies of banks and convenience stores, with uncertain implications for the overall number of robberies. I am not persuaded by this point. In 1999, 64.1 percent of robberies were either highway robberies (48.3 percent of the total) or robberies that occurred in churches, schools, trains, etc. (15.8 percent of the total)—the remainder being robberies in commercial firms including banks or in residences. FBI (1999, table 2.20). Thus the substantial majority of robberies are exactly the sort of crimes that Lott and Mustard argue should be deterred. In fact, the proportion of robberies that occur in public places is greater than the proportion of aggravated assaults occurring in public places. In 1999 aggravated assaults occurring in public places constituted 58.6 percent of the total. Bureau of Justice Statistics (1999, table 61). Moreover, even in the 8.2 percent of robberies that occur in convenience stores or gas stations, the armed citizenry are supposed to be protecting against crime (indeed, Mustard argues they even protect armed police officers! See Mustard (2001)).

15. Lott (2000, table 4-1); Ayres and Donohue (1999).

16. Ayres and Donohue (1999) replicate Lott precisely with the coding error and then show how the correction eliminates the robbery effect. The line 1 results in table 8-1 of this chapter are identical to the results in Lott's table 4-1 with three exceptions, which are maintained in all the regressions presented here: the coding error is corrected; standard errors are corrected to adjust for heterogeneity; and one explanatory variable—a measure of the real per capita income maintenance, SSI and other, for those over 65—was dropped. One can compare the results in table 1 of Ayres and Donohue (1999) with those of table 8-1 here to see that the only change that influences the basic story is the correction for the coding error. The explanatory variable of real per capita income maintenance for the elderly was omitted because, in expanding the data set to include the period 1993–97, we were unable to match the series for this variable with Lott's series through 1992. Since the omission had little impact on the pre-1993 results, and the theoretical argument for inclusion is not strong, we simply dropped the variable completely.

### *Lott's Spline Model*

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

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



### *The Hybrid Model Testing for Main and Trend Effects*

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

17. Lott (2000, table 4-8).

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

19. Ayres and Donohue (forthcoming).

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

## *Extending the County Data through 1997*



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

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

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

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

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



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

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

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

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

## *A State Data Analysis*

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



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

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

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

Percent

| *Item* | *Violent crime* | *Murder* | *Rape* | *Aggravated assault* | *Robbery* | *Property crime* | *Auto theft* | *Burglary* | *Larceny* |
|---|---|---|---|---|---|---|---|---|---|
| Lott's time period (1977–92) | | | | | | | | | |
| 1. Dummy variable model | **–8.3** | **–9.4** | –4.1 | **–9.3** | **–11.4** | –2.2 | –0.6 | **–6.0** | –1.1 |
| Robust std. error | (2.6) | (3.5) | (2.8) | (3.2) | (3.9) | (1.9) | (3.9) | (2.4) | (1.9) |
| 2. Lott-Spline model | –1.6 | **–5.4** | –0.8 | –1.9 | **–6.1** | –0.8 | **–3.3** | –2.0 | 0.0 |
| Robust std. error | (1.0) | (1.4) | (1.0) | (1.2) | (1.7) | (0.8) | (1.4) | (1.1) | (0.8) |
| 3. Hybrid model | | | | | | | | | |
| *Postpassage dummy* | 6.4 | 7.5 | –2.6 | 7.6 | 0.7 | 1.4 | **13.4** | 1.6 | –0.3 |
| Robust std. error | (3.7) | (5.6) | (4.4) | (4.8) | (5.6) | (2.7) | (5.0) | (3.3) | (2.7) |
| *Trend effect* | **–3.2** | **–7.3** | –0.2 | **–3.8** | **–6.2** | –1.2 | **–6.7** | –2.4 | 0.1 |
| Robust std. error | (1.3) | (2.0) | (1.5) | (1.6) | (1.8) | (0.9) | (1.8) | (1.3) | (0.9) |
| Entire period (1977–97) | | | | | | | | | |
| 4. Dummy variable model | **–7.0** | –4.5 | **–4.7** | **–5.9** | **–7.3** | 0.3 | 5.8 | **–4.2** | 0.7 |
| Robust std. error | (2.4) | (2.9) | (2.3) | (2.5) | (3.1) | (1.6) | (3.1) | (2.0) | (1.5) |
| 5. Lott-Spline model | **–2.3** | **–2.3** | **–1.8** | **–1.9** | **–3.0** | **–1.1** | **–1.7** | **–2.3** | **–0.9** |
| Robust std. error | (0.6) | (0.8) | (0.6) | (0.6) | (0.8) | (0.4) | (0.6) | (0.5) | (0.4) |
| 6. Hybrid model | | | | | | | | | |
| *Postpassage dummy* | –2.5 | –0.7 | –0.6 | –2.4 | 0.0 | 1.8 | **8.9** | 0.5 | 1.5 |
| Robust std. error | (2.7) | (3.8) | (2.9) | (3.2) | (3.7) | (1.7) | (3.6) | (2.3) | (1.6) |
| *Trend effect* | **–2.0** | **–2.2** | **–1.8** | **–1.6** | **–3.0** | **–1.3** | **–2.7** | **–2.3** | **–1.0** |
| Robust std. error | (0.7) | (0.9) | (0.7) | (0.8) | (0.9) | (0.4) | (0.7) | (0.5) | (0.4) |

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

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

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



Perhaps surprisingly, though, the state results—which Lott has tended to argue against—seem generally more supportive (table 8-2). First, robbery is always negative in table 8-2, as are most of the violent crime categories—although not always significantly. Second, the strange results of the county data set in the hybrid model is not repeated, as we generally do not see uniform large and positive main effects offset by negative trend effects for the full time period. While in table 8-1 the hybrid model rejected both the county dummy variable and spline models, the table 8-2 hybrid model, if anything, seems to reject the dummy variable model and support the spline model, particularly in the full data set. The inconsistency in the hybrid model across time periods (regressions 3 versus 6) is somewhat unsettling. Still, if one took regressions 5 and 6 in table 8-2 as perhaps the "best" regressions from these two tables, one might argue that shall-issue laws seem to be associated with drops of roughly 2 percent across all crime categories. Although this is perhaps a weaker story than Lott initially ventured, it has the virtue of not having the theoretically problematic result of no effect on robbery, even though it does stumble on two other anomalies: first, the peculiar finding that the estimated effects are virtually identical for both violent and property crime, and second, the problem that shall-issue laws are associated with *higher* crime in the regressions (both county and state) run over the 1991–97 period. The anomalies suggest that further exploration is needed before any conclusions on the impact of shall-issue laws can be drawn.

## Robustness and Endogeneity

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



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

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

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

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

Percent

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

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

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

Percent

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

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

### *Dropping the Arrest Rate and Including the Incarceration Rate*

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

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



24. Donohue and Levitt (2001).

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

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

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

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

## *Introducing Lead and Lag Dummies*

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

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

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



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

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

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

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

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

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



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

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

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

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

Percent

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

| Entire period (1977–97) | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| 5 or 6 years prior | 3.7 | 0.4 | 1.3 | 3.9 | 5.6 | 6.4 | 3.9 | 6.7 | 7.9 |
| Robust std. error | (1.1) | (1.7) | (1.3) | (1.2) | (1.5) | (1.1) | (1.7) | (1.0) | (1.9) |
| 3 or 4 years prior | 4.3 | 1.0 | 1.6 | 4.7 | 9.1 | 7.2 | 8.1 | 5.6 | 9.6 |
| Robust std. error | (1.2) | (2.0) | (1.3) | (1.5) | (1.6) | (1.1) | (1.8) | (1.0) | (1.9) |
| 1 or 2 years prior | 6.5 | –2.9 | 3.7 | 6.7 | 13.2 | 10.3 | 11.0 | 6.5 | 13.9 |
| Robust std. error | (1.7) | (2.2) | (1.5) | (1.7) | (1.9) | (1.2) | (1.9) | (1.2) | (2.0) |
| Year of or year after | 8.3 | –2.0 | 5.7 | 9.1 | 14.0 | 12.8 | 17.8 | 9.6 | 14.5 |
| Robust std. error | (1.9) | (2.5) | (1.6) | (1.9) | (2.1) | (1.6) | (2.3) | (1.3) | (3.3) |
| 2 or 3 years after | 6.1 | –4.2 | 2.6 | 6.3 | 10.7 | 13.3 | 17.7 | 9.0 | 17.4 |
| Robust std. error | (2.0) | (2.8) | (1.8) | (2.1) | (2.4) | (1.4) | (2.6) | (1.5) | (2.1) |
| 4 or 5 years after | 1.6 | –11.2 | –8.1 | 2.4 | 7.8 | 16.0 | 20.6 | 6.2 | 22.0 |
| Robust std. error | (2.1) | (3.2) | (2.1) | (2.3) | (2.5) | (1.4) | (2.6) | (1.6) | (2.1) |
| 6 or 7 years after | 2.6 | –14.4 | –3.0 | 6.2 | 6.1 | 19.0 | 25.6 | 8.8 | 26.9 |
| Robust std. error | (2.1) | (3.2) | (2.3) | (2.3) | (2.7) | (1.5) | (3.1) | (1.8) | (2.2) |
| 8 or more years after | 2.6 | –34.1 | –18.1 | –15.5 | –10.6 | 17.8 | 6.3 | –11.3 | 6.9 |
| Robust std. error | (2.6) | (5.8) | (5.0) | (5.0) | (5.1) | (1.8) | (5.2) | (4.1) | (4.6) |

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

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

Percent

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

| | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| Entire period (1977–97) | | | | | | | | | |
| 5 or 6 years prior | **3.4** | 3.9 | 1.3 | **4.2** | 3.4 | 1.8 | 4.4 | 2.0 | 2.3 |
| Robust std. error | (1.7) | (2.7) | (1.9) | (2.0) | (3.1) | (1.7) | (3.0) | (2.1) | (1.8) |
| 3 or 4 years prior | 0.8 | **6.8** | –0.9 | 2.6 | 0.7 | 0.8 | 3.5 | –0.8 | 1.7 |
| Robust std. error | (2.3) | (3.1) | (2.2) | (2.6) | (3.3) | (1.8) | (3.1) | (2.5) | (1.8) |
| 1 or 2 years prior | –2.0 | 5.6 | –0.4 | –1.5 | 2.4 | 1.2 | **9.5** | –0.5 | 1.5 |
| Robust std. error | (2.7) | (3.0) | (2.5) | (3.2) | (3.5) | (1.9) | (3.6) | (2.3) | (2.0) |
| Year of or year after | –4.1 | **7.8** | –1.6 | –3.6 | 1.1 | 2.7 | **16.9** | 0.3 | 2.0 |
| Robust std. error | (3.4) | (3.8) | (3.0) | (4.1) | (4.0) | (2.2) | (4.2) | (2.7) | (2.1) |
| 2 or 3 years after | –6.3 | 5.7 | –4.2 | –4.6 | –2.8 | 3.4 | 17.4 | –1.7 | 3.8 |
| Robust std. error | (4.2) | (4.6) | (3.9) | (4.5) | (5.0) | (2.4) | (5.2) | (3.2) | (2.4) |
| 4 or 5 years after | **–15.9** | –4.4 | **–11.7** | **–13.1** | **–14.9** | –1.6 | **12.0** | **–11.1** | 0.1 |
| Robust std. error | (4.6) | (5.7) | (4.9) | (4.8) | (5.5) | (2.8) | (5.8) | (3.8) | (2.8) |
| 6 or 7 years after | **–14.7** | –1.9 | **–12.8** | **–12.0** | **–14.8** | –0.8 | **11.7** | **–12.6** | 0.8 |
| Robust std. error | (5.5) | (5.8) | (5.0) | (5.7) | (6.7) | (3.2) | (5.1) | (4.3) | (3.3) |
| 8 or more years after | **–17.1** | –2.8 | –12.4 | –11.8 | **–20.5** | –3.8 | 4.3 | **–13.8** | –2.4 |
| Robust std. error | (7.9) | (9.2) | (7.3) | (8.9) | (8.0) | (4.1) | (5.3) | (5.0) | (4.5) |

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

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

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

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



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

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

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



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

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

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

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

## Disaggregating the Results by State

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



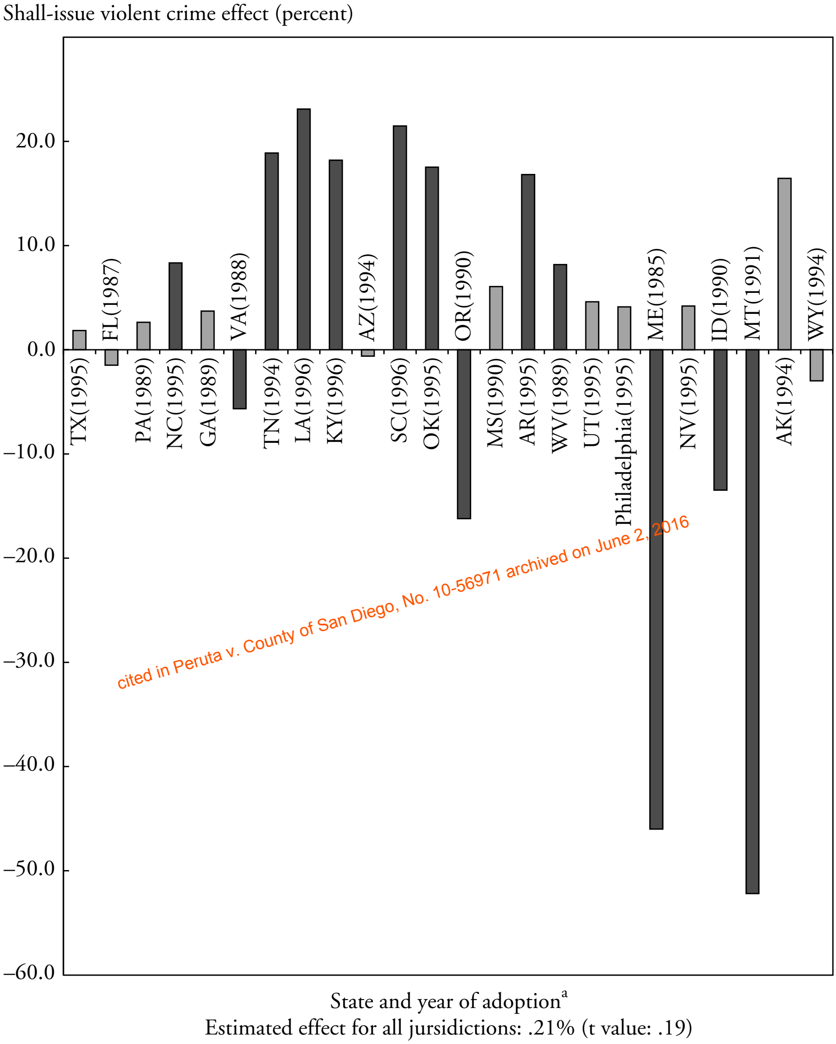
One way to explore the factors that drive the overall results in these aggregated analyses is to change the specification in both models to predict a state-specific effect from the passage of the law. This approach—that is, having a separate postpassage dummy in the dummy variable model for each adopting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

### *Disaggregating the Dummy Variable Model*

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

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

Figure 8-1. *Estimated Effect of Shall-Issue Laws on Violent Crime, Dummy Variable Model*

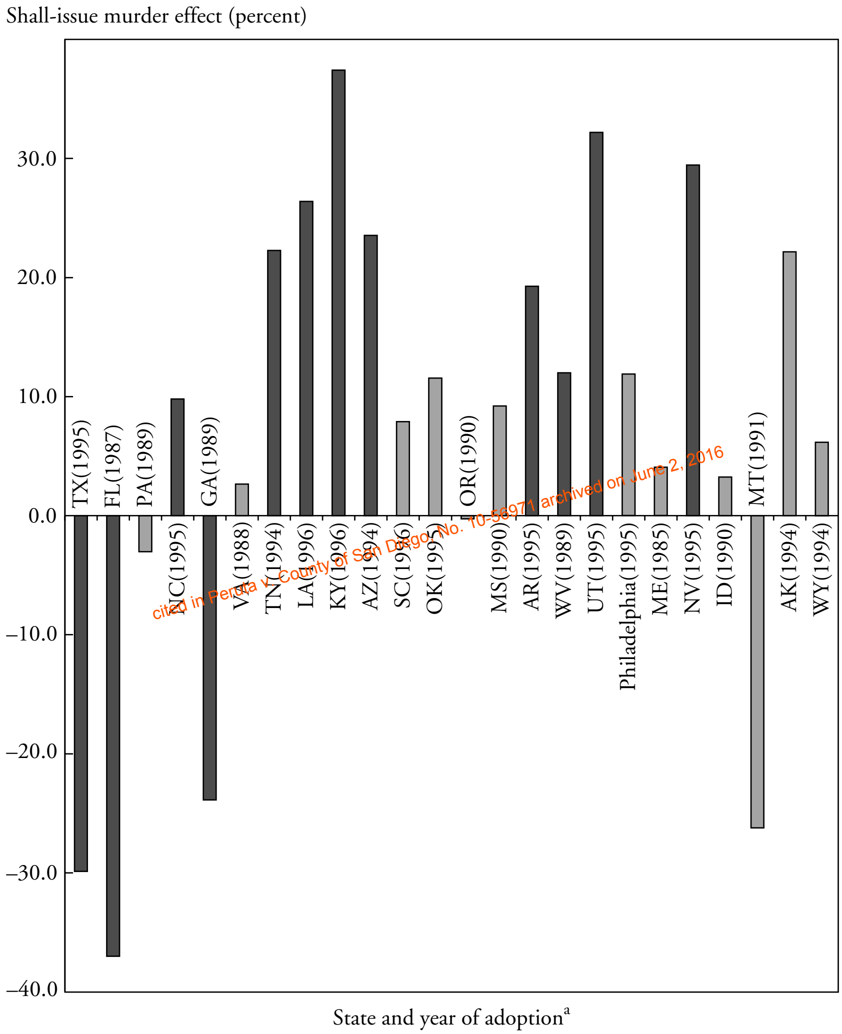





a. The dark shade means statistically significant.

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

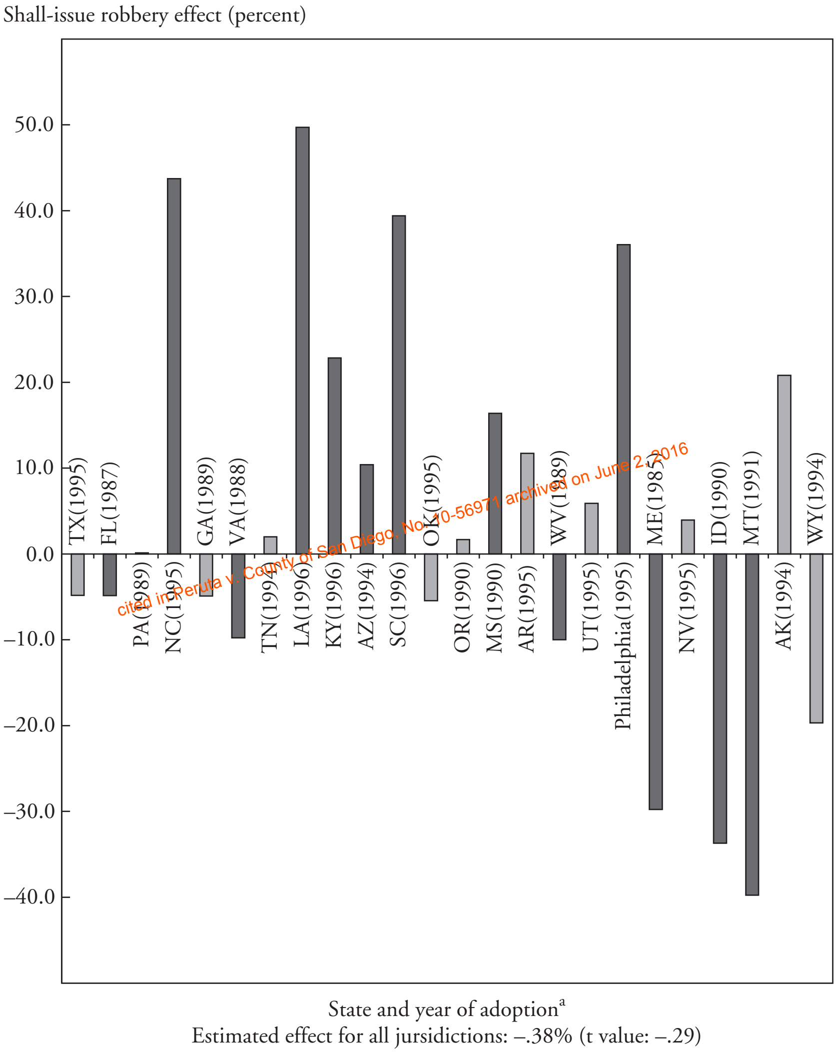
Figure 8-2. *Estimated Effect of Shall-Issue Laws on Murder, Dummy Variable Model*




Estimated effect for all jursidictions: –7.77% (t value: –4.57)

a. The dark shade means statistically significant.

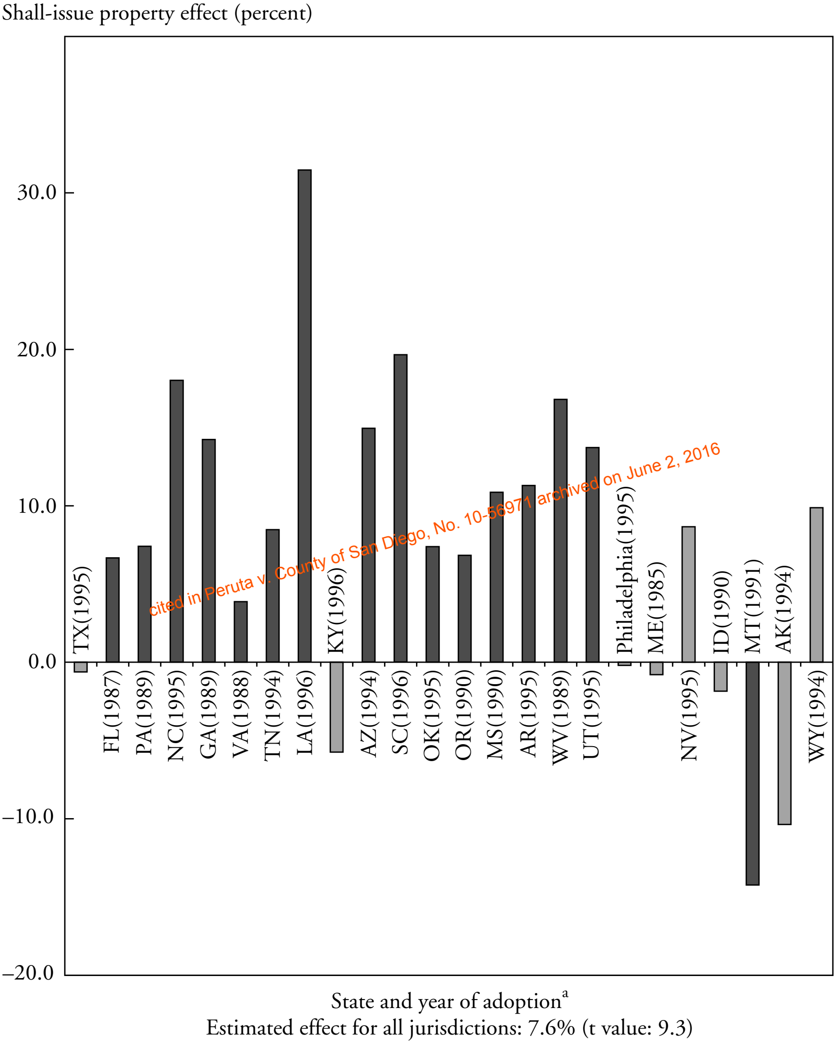
Figure 8-3. *Estimated Effect of Shall-Issue Laws on Robbery, Dummy Variable Model*






a. The dark shade means statistically significant.

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






a. The dark shade means statistically significant.

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



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

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

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

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

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



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

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

### *Disaggregating the Linear Trend Model*

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



## Summary

Lott and Mustard have clearly launched an enormous amount of scholarly work on the effect of laws enabling citizens to carry concealed handguns. It is not hard to see why they and others may have believed that these laws reduce crime, because simple panel data regression models for the 1977–92 data period that they first analyzed provided support for the view that some or most violent crime rates fell for the ten states that adopted shall-issue laws over that period. Indeed, some superficially supportive work—for example, a paper by Bronars and Lott (1998) arguing that the passage of a shall-issue law pushed criminals across the border into non-shall-issue states[39] and a paper by Lott and William M. Landes indicating the multiple victim homicides fell when shall-issue laws were adopted—might have been thought to buttress the more-guns, less-crime hypothesis.[40] Moreover, if the statistical evidence backed up the more-guns, less-crime hypothesis, the anecdotal evidence of cases in which guns were used defensively to thwart attacks and the overall estimates of the number of incidents of defensive gun might seem to provide some plausibility to the initial Lott and Mustard findings.[41]

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



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

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

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

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

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

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

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

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



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

44. Ludwig (1998).

45. Ayres and Donohue (1999); Duggan (2001).

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

47. Moody (1999); Benson and Mast (2001); and Plassman and Tideman (2000).

48. Ayres and Donohue (1999).

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

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

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



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

49. Ayres and Donohue (forthcoming).

50. Ayres and Donohue (forthcoming).

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

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

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

COMMENT BY

# David B. Mustard



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

## *Lott-Mustard and Subsequent Research*

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

52. Lott and Mustard (1997).

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

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



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

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

54. Black and Nagin (1998).

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

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



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

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

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

## *Post-1992 Analysis*

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



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

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

59. Plassman and Tideman (2001).

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

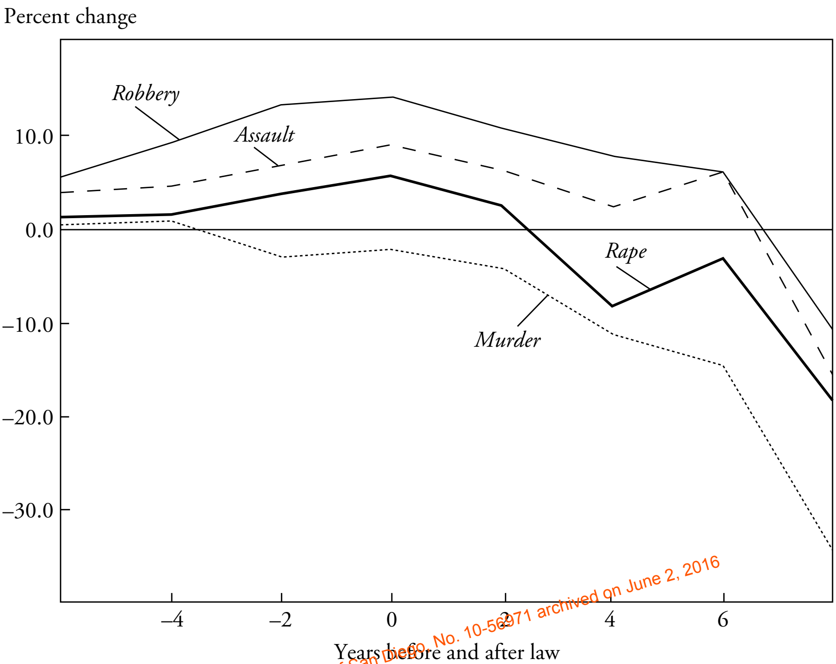
A second concern about the new empirical work is that although it is important to know whether the coefficient estimates in the postlaw years are positive or negative, it is also important to understand how they compare to the prelaw estimates. For example, if the prelaw coefficient estimate is 8.5,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



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

60. Bronars and Lott (1998).

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






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

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

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

61. Lott and Mustard (1997).

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



## COMMENT BY
# Willard Manning

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

62. Mustard (2001).

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

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

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

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



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

## *Correlated Errors*

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

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

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

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

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

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

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



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

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

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

69. Diggle and others (1994).

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

## *Multiple Comparisons*

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

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



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

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

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

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

71. Greene (2000, sec. 15.4).

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

## *Endogeneity*

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

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



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

72. Given the concerns discussed in note 4, the corrections for the county-level data reported in table 8-3 would be even larger.

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

74. In addition, Lott and his colleagues include a measure based on arrests and Donohue includes incarcerations as covariates; the main results are largely insensitive to the inclusion of the additional endogenous variables.

75. 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). See Angrist, Imbens, and Rubin (1996) for a fuller exposition of the requirements.

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

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

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



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

$$\begin{aligned} E(y) &= e^{x\beta}E(e^{\varepsilon}) \\ &\neq e^{x\beta} \end{aligned}$$

76. Manski (1990).

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

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

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

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

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

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

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

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



$$\frac{E(y_A)}{E(y_B)} = e^{(\mu_a - \mu_B) + 0.5\left(\sigma^2_A - \sigma^2_B\right)}$$

Under homoscedasticity ($\sigma^2 =$ a constant)

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

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

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

78. Manning (1998).

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

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

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

## *Conclusions*



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

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

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

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

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

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ELSEVIER

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

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



## I. Introduction

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

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

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

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

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



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

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

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

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

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

## II. Previous Research

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

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



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

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

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

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

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

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

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

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



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

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

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

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

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

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

## III. Empirical Methods and Results

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

### *Data*

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

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



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

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

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

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

TABLE 1. Descriptive statistics for state data

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

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

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

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



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

### *Estimation Strategy*

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

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

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

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

| | *Pre-shall-issue* | *Post-shall-issue* | *Difference* |
|---|---|---|---|
| Shall-issue states | | | |
| Adults ("treatment") | $b$ | $a$ | $(a\text{-}b)$ |
| Juveniles ("control") | $d$ | $c$ | $(c\text{-}d)$ |
| Difference in differences | | | $(a\text{-}b) - (c\text{-}d)$ |
| Non-shall-issue states | | | |
| Adults ("treatment") | $f$ | $e$ | $(e\text{-}f)$ |
| Juveniles ("control") | $h$ | $g$ | $(g\text{-}h)$ |
| Difference in differences | | | $(e\text{-}f) - (g\text{-}h)$ |
| DDD | | | $[(a\text{-}b) - (c\text{-}d)] - [(e\text{-}f) - (g\text{-}h)]$ |

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

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

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



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

$$y_{it} = \theta_0 + \theta_1(Exper_i) + \theta_2(Adult_i) + \theta_3(Post_t) + \theta_4(Exper_i{*}Adult_i) + \theta_5(Adult_i{*}Post_t) + \theta_6(Exper_i{*}Post_t) + \theta_7(Exper_i{*}Post_t{*}Adult_i) + v_{it} \qquad (1)$$

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

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

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

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

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

**Regression model*:

$$y_{it} = \theta_0 + \theta_1(Exper_i) + \theta_2(Adult_i) + \theta_3(Post_t) + \theta_4(Exper_i{*}Adult_i) + \theta_5(Adult_i{*}Post_t) + \theta_6(Exper_i{*}Post_t) + \theta_7(Exper_i{*}Post_t{*}Adult_i) + v_{it}$$

$y_{it}$ = homicide victimization rate for observation (either adult or juvenile) in state ($i$), period ($t$)
$Exper_i$ = 1 if state (i) enacts shall-issue law during sample period, 0 otherwise
$Adult_i$ = 1 if observation corresponds to adult victimization rate, 0 if juvenile rate
$Post_t$ = 1 if observation occurs in post-shall-issue law period, 0 if pre-shall-issue law period



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

The parameters in this regression model will capture fixed factors that reflect differences between shall-issue states and other states during the sample period ($\theta_1$), differences between adult and juvenile homicide rates ($\theta_2$), trends over time in homicide rates ($\theta_3$), differences in the effects of fixed-state factors on adults versus juveniles ($\theta_4$), differences in the trends of adult versus juvenile homicide rates over time ($\theta_5$), and differences in homicide trends over time between shall-issue states and other states ($\theta_6$). The key parameter of interest is $\theta_7$, which represents $[(a\text{-}b) - (c\text{-}d) - (e\text{-}f) - (g\text{-}h)]$, the effects of the shall-issue law on the difference between adult and juvenile homicide rates in states that do adopt a shall-issue law during this period versus those that do not.

That the estimate for $\theta_7$ from equation (1) represents an estimate for the quantity $[(a\text{-}b) - (c\text{-}d) - (e\text{-}f) - (g\text{-}h)]$ can be seen with the help of Table 3, which is identical to Table 2 except that the homicide rates are now expressed in terms of the parameters underlying equation (1). For example, the expected value of adult homicide victimization rates in shall-issue states after these laws are passed is given by [$a = (\theta_0 + \theta_1 + \theta_2 +$

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

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

### *Empirical Results*

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



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

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

[14] This can be seen by imagining two separate regression equations with adult and juvenile homicide rates as the dependent variables of interest and the various explanatory variables on the right-hand side. The regression equation with the ratio of adult to total homicides can be written as the ratio of the adult equation divided by the adult plus juvenile equations, with a residual term that still includes unobserved, time-varying state effects that influence adult and juvenile rates equally. These terms will be purged with my differencing strategy.

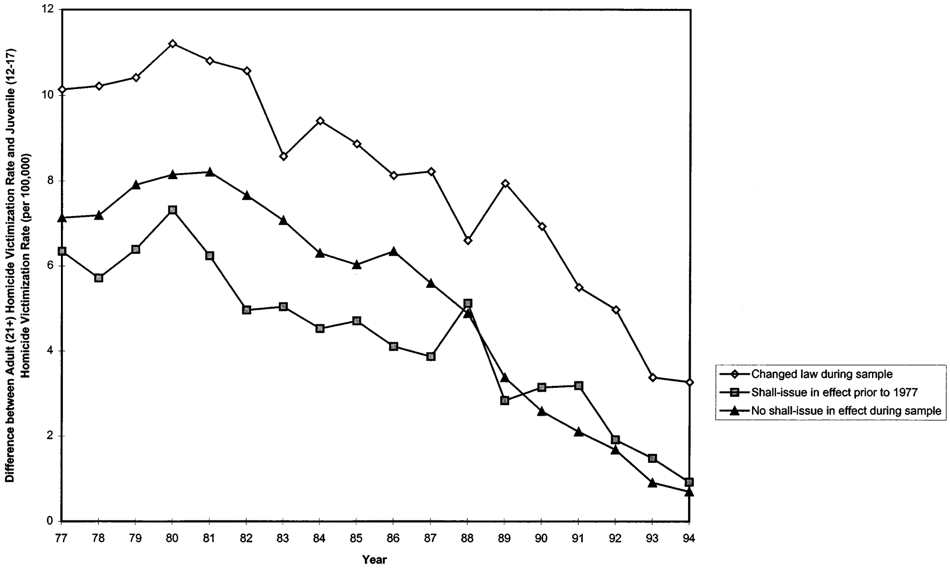


FIG. 1. Difference Between Adult (21+) and Juvenile (12–17) Homicide Victimization Rates, 1977–1994. States that enacted shall-issue laws during the sample period are as follows: Florida (1987), Georgia (1989), Idaho (1990), Mississippi (1990), Montana (1991), Oregon (1990), and West Virginia (1989). States with shall-issue laws in effect during entire sample period are: Alabama, Connecticut, Indiana, New Hampshire, North Dakota, South Dakota, Vermont, and Washington. Excluded from the sample are Maine, Virginia, and Pennsylvania (see text).



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

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

The regression results shown in Table 4 reveal that parameter $\theta_7$, which captures the effects of shall-issue laws on adult homicide rates, is slightly positive, implying an increase of around one-sixth of a homicide per 100,000 adults. With an average adult homicide victimization rate of 11.17 per 100,000 in the United States for 1977 through 1994, this implies an increase of 1.4%. Because the sample of states that change their laws from 1977 to 1994 is relatively small, the standard errors around this point estimate

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

| *Explanatory Variable†* | *Coeff.* | *Controls for* | *Estimate (standard error)* |
|---|---|---|---|
| *Exper* = 1 if state ever passes shall-issue law (=0 else) | $\theta_1$ | Fixed factors which differ between shall-issue and other states | −1.53 (0.53)* |
| *Adult* = 1 if observation is for adult homicide rates (=0 if observation is for juvenile homicide rate) | $\theta_2$ | Differences in levels between adult and juvenile homicide rates | 7.19 (0.26)* |
| *Post* = 1 if period is after implementation of shall-issue laws | $\theta_3$ | Trends over time in homicide rates | 4.80 (0.44)* |
| *Adult* × *Exper* | $\theta_4$ | Differences in shall-issue state fixed-effects on adult versus juvenile homicide rates | 2.62 (0.73)* |
| *Adult* × *Post* | $\theta_5$ | Differences in trends of adult versus juvenile homicide rates over time | −6.10 (0.52)* |
| *Exper* × *Post* | $\theta_6$ | Differences in homicide trends in shall-issue v. other states over time | −1.43 (1.01) |
| *Exper* × *Post* × *Adult* | $\theta_7$ | Effects of shall-issue laws on adult homicide rates relative to juvenile homicide rates | 0.16 (1.42) |
| *N* | | | 1,039 |
| Adjusted $R^2$ | | | 0.64 |



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

* = significant at 1%.

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

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

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

TABLE 5. Sensitivity analysis of DDD regression results

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



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

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

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

[15]Using the natural logarithm for the homicide victimization rates is complicated somewhat by the fact that several states reported no homicides to victims ages 12 to 17 for some of the years between 1977 and 1994. Because the logarithm of 0 is undefined, I substitute the logarithm of (0.1) in these cases. Substitution of the logarithm of yet smaller values will increase the implied difference between adult and juvenile homicides when there are no juvenile homicide cases and will serve to make the shall-issue coefficient more negative.

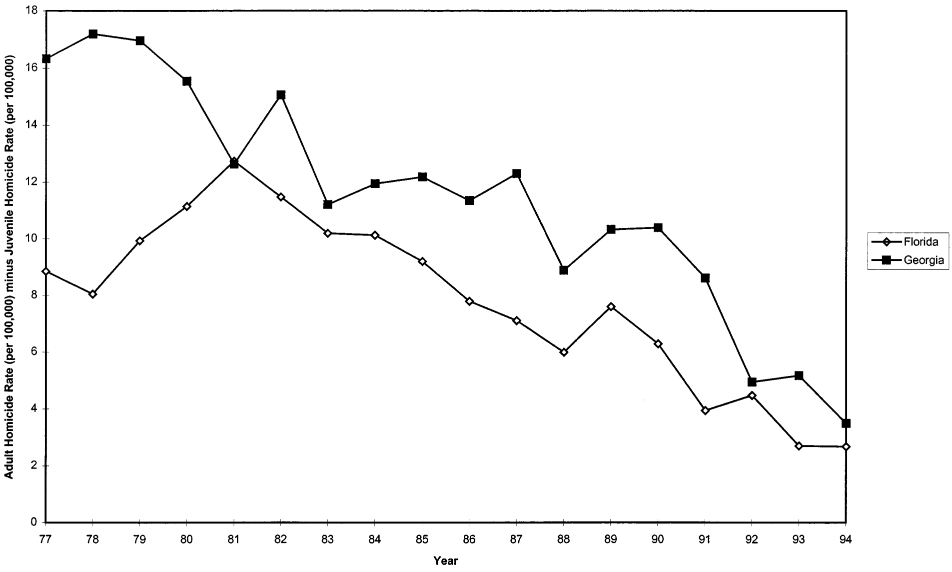


FIG. 2. Difference in Adult (21+) minus Juvenile (12–17) Homicide Victimization Rates in Florida and Georgia, 1977–1994. Florida enacted shall-issue law in 1987, while Georgia enacted shall-issue law in 1989.



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

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

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

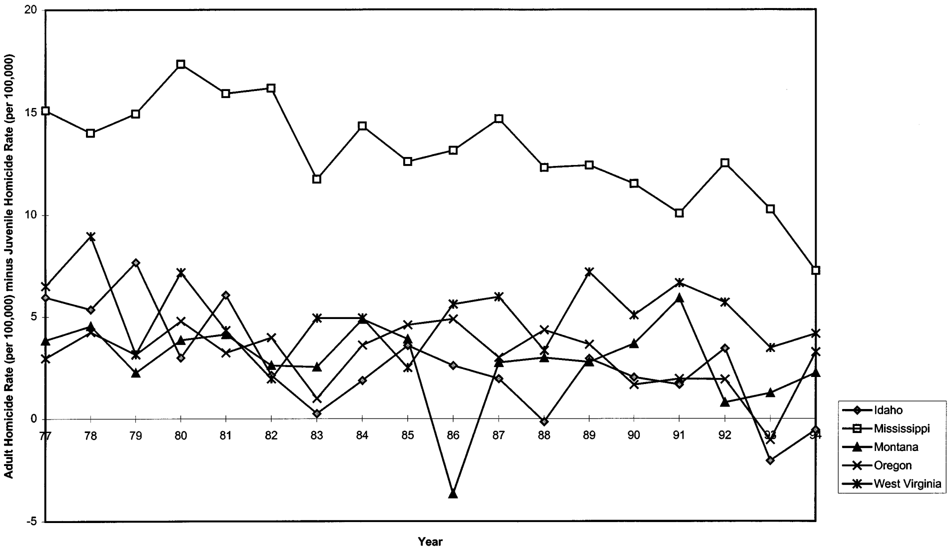


FIG. 3. Difference between Adult (21+) and Juvenile (12–17) Homicide Victimization Rates for Idaho, Mississippi, Montana, Oregon, and West Virginia, 1977–1994. Idaho, Mississippi, and Oregon enacted shall-issue laws in 1990, whereas Montana enacted a shall-issue law in 1991 and West Virginia in 1989.

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



## IV. Discussion

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

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

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

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

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



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# Easing Concealed Firearms Laws: Effects on Homicide in Three States

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

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## I. INTRODUCTION

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



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

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

In 1985, the National Rifle Association announced that it would

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

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

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

4 *Id.*

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

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

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

## II. The Laws

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

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



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

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

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

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

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

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

11 Miss. Code Ann. § 45-9-101 (1991).

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

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

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

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

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



## III. Possible Effects of Shall Issue Licensing on Crime

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

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

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

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

[14] Or. Rev. Stat. § 166.291-§166.295 (1991). *See also,* Rhonda Canby, *1989 Oregon Gun Control Legislation,* 26 Willamette L. Rev. 565 (1990).

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

[16] *Id.*

[17] *See, e.g.,* James D. Wright & Peter H. Rossi, Armed and Considered Dangerous: A Survey of Felons and Their Firearms 141-59 (1986).

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

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

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

## IV. Existing Evidence on the Effects of Shall Issue Licensing



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

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

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

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

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

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

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

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

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

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



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

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

## V. Research Design

### A. Study Design and Data

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

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

23 Fla. Stat. chs. 790.065, 790.0655 (1992).

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

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

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



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

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

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

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

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

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

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

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

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

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

### B. THREATS TO VALIDITY AND SUPPLEMENTARY ANALYSIS

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

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



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

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

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29 *See* David McDowall et al., Interrupted Time Series Analysis 83-85 (1980.

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

31 For a description of the FBI and NCHS data collection systems, see Marc Riedel, *Nationwide Homicide Data Sets: An Evaluation of the Uniform Crime Reports and the National Center for Health Statistics Data*, *in* Measuring Crime: Large-Scale, Long-Range Efforts 175 (Doris Layton MacKenzie et al. eds., 1990).

32 *See* Donald T. Campbell & Julian C. Stanley, Experimental and Quasi-Experimental Designs for Research 37-43 (1963).

33 Cook & Campbell, *supra* note 24, at 211.

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

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

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

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

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



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

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

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

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

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

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

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

## VI. Results

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



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

37 For 1973-1978 we used county-level population estimates from U.S. Department of Commerce, Bureau of the Census, Statistical Abstract of the United States (various years). For 1980-1992 we used unpublished Census Bureau estimates. The Census Bureau did not estimate county populations in 1979, and we interpolated values for that year.

38 *See* Cheng Hsiao, Analysis of Panel Data (1986).

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

40 Hsiao, *supra* note 38, at 138-40.

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

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

hypothesis of zero overall change.[43]

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

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

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



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

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

## VII. Discussion

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

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

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

allowed for population change.

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

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

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



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

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

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[45] Glenn L. Pierce & William J. Bowers, *The Bartley-Fox Gun Law's Short-Term Impact on Crime in Boston,* 455 Annals Am. Acad. Pol. & Soc. Sci. 120, 120-37 (1981); Patrick W. O'Carroll et al., *Preventing Homicide: An Evaluation of the Efficacy of a Detroit Gun Ordinance,* 81 Am. J. Pub. Health 576 (1991).

[46] Lawrence W. Sherman et al., *The Kansas City Gun Experiment,* National Institute of Justice Research in Brief (Jan. 1995). Sherman and associates note that about 20% of the seized firearms were legally carried.

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

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

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

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



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

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

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

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

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

## Table 1

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

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

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



## Table 2

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

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

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

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

### Table 3

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

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

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



### Table 4

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

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

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

# QuickFacts

## Yolo County, California



QuickFacts provides statistics for all states and counties, and for cities and towns with a population of 5,000 or more.



## Table



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

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

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

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

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

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

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

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

# QuickFacts

## Sacramento County, California

What's New & FAQs ›
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QuickFacts provides statistics for all states and counties, and for cities and towns with a population of 5,000 or more.



## Table

ALL TOPICS = Browse more datasets

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

| | | |
|---|---|---|
| 2010 (a) | | |
| Two or More Races, percent, July 1, 2014, (V2014) | 6.0% | 2.5% |

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

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

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

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