From the SelectedWorks of John Donohue

January 1999

Nondiscretionary Concealed Weapons Law: A Case Study of Statistics, Standards of Proof, and Public Policy

Contact Author

Start Your Own SelectedWorks

Notify Me of New Work

Available at: http://works.bepress.com/john_donohue/22

Nondiscretionary Concealed Weapons Laws:
A Case Study of Statistics, Standards of Proof, and Public Policy

Reviewing: John R. Lott, Jr., More Guns, Less Crime, University of Chicago Press, 1998

Ian Ayres
William K. Townsend Professor of Law
Yale Law School

and

John J. Donohue III
Professor of Law
Stanford Law School

We thank John Lott for generously sharing his data set and Craig Estes for superb research assistance. We also gratefully acknowledge the research support we have received from Yale and Stanford Universities.

Nondiscretionary Concealed Weapons Laws:
A Case Study of Statistics, Standards of Proof, and Public Policy

Ian Ayres & John J. Donohue III

Reviewing: John R. Lott, Jr., More Guns, Less Crime, University of Chicago Press, 1998

During the last 15 years, the National Rifle Association (NRA) has conducted a highly successful campaign to encourage states to enact legislation enabling citizens who meet modest requirements to carry concealed handguns. Many will be surprised – some even horrified -- to learn that 31 states have now adopted such laws, and the NRA campaign has been given a substantial academic boost from John Lott, who has become convinced that the passage of these so-called "shall issue" laws actually reduces violent crime.[1] Lott impressively marshals the evidence in support of his position in his best-selling (for an academic work) book More Guns, Less Crime. As a result, Lott has become one of the few members of the legal academy whose name is now bandied about on talk shows, in legislative sessions, and in the print media. His work has contributed to the increased pace of NRA successes in persuading state legislatures to allow more concealed handguns to be carried -- while only 10 states adopted shall issue laws between 1977 and 1992, 13 more have done so since 1993.[2] Readers will hardly need to be

[1] A law that allows a citizen to carry a concealed handgun if he or she can demonstrate a need to a government official is a discretionary, or "may issue," law. The shall issue laws are designed to eliminate discretion on the part of governmental officials, by requiring them to issue permit to carry concealed handguns unless specific and easily verifiable factors dictate otherwise.

[2] Eight states had shall issue laws prior to 1977: Alabama, Connecticut, Indiana, New Hampshire, North Dakota, South Dakota, Vermont, Washington. Ten states adopted shall issue laws between 1977 and 1992: Maine (1985), Florida (1987), Virginia (1988), Georgia (1989), Pennsylvania—excepting Philadelphia (1989), West Virginia (1989), Idaho (1990), Mississippi (1990), Oregon (1990), and Montana (1991). John R. Lott, Jr. and David B. Mustard, Crime, Deterrence, and Right-to-Carry Concealed Handguns, 26 Journal of Legal Studies 1 (1997). While conceding that there are different interpretations of which states have shall issue laws, Lott and Mustard state in footnote 32 that they follow the shall issue law classification found in Clayton E. Cramer & David B. Kopel, "Shall Issue": The New Wave of Concealed Handgun Permit Laws, 62 Tenn. L. Rev. 679, 691 (Spring 1995). Lott and Mustard cite two states in particular—Maine and Virginia—as potentially not "true" shall issue states, though they argue that their results are not affected by either redefining or dropping these states altogether (see Lott and Mustard, footnotes 33, 34, 35 and 49). It is also worth noting that Handgun Control, Inc. lists Alabama and Connecticut as may issue states, while Lott and the NRA list them as shall issue states (see http://www.handguncontrol.org/b-main.htm). The NRA's description of the relevant Alabama law suggests that demonstrating need is in fact a requirement for obtaining a concealed-carry permit (http://www.nraila.org/research/riflaws.html). Interestingly, the Cramer & Kopel paper, which is published online by the Independence Institute (http://www.ggnra.org/cramer//shall-issue.html), a conservative think tank in which Kopel is the Research Director for Criminal Justice Issues, undertakes a substantial biblical exegesis in support of the morality of carrying concealed handguns.

advised of the challenge that his work poses to the conventional wisdom that more guns leads to more crime – or at least more deaths and serious injuries. Lott's conclusions may have provoked the sharpest attacks against any study emanating from the University of Chicago since Isaac Ehrlich's work on the death penalty first surfaced almost 25 years ago.[3]

Since many find the book's conclusion so distasteful or obviously wrong, there has been a tendency by some to dismiss it. But for those who argue that the serious evaluation of data is an important and underutilized guide to good public policy, this book poses a real challenge. For make no mistake, John Lott has painstakingly constructed a massive data set, analyzed it exhaustively, and commendably shared it with scholars across the country. In these respects, Lott deserves high praise for following the scientific protocol so faithfully. Of course, those familiar with the statistical analysis of large data bases that are designed to test the effects of public policy initiatives on complex social phenomena such as rates of crime will recognize that even well-designed and ostensibly unassailable studies can reach the wrong conclusions. Indeed, it is distressingly easy to do so, even with the best of intentions. Consequently, final resolution of such matters often involves a complex and protracted process of verification, extension, and replication. But those who launch the enterprise play an important role even if their findings are ultimately supplanted. The injection of this work into the political domain before the research process has been exhausted poses special challenges for academics and policymakers, and John Lott's influential book provides an interesting case study of the substance and methodology of statistical evaluation of public policy.

Part I of this review describes Lott's basic argument that shall issue laws prevent violent crime but, in so doing, shift criminals towards committing more property crime. The theory behind this finding and the empirical evidence in support thereof are set forth in some detail. Part II replicates Lott's core findings and examines their sensitivity to various changes in specification, definition and inclusion of key variables, and geographic sample. Part III discusses an array of criticisms of Lott's work and his answers to the critics. Part IV examines the uncertain implications of Lott's work, and concludes that further research that builds on Lott's efforts while examining more states over a longer period of time may resolve some of the questions about the robustness and reliability of his findings.

I. Lott's Affirmative Case

A. The Theory of Deterrence From Increased Gun Possession

Over the last decade, a number of scholars have offered theoretical and empirical support for the notion that allowing law-abiding citizens to carry handguns can deter criminal

---

[3] Isaac Ehrlich, "The Deterrent Effect of Capital Punishment: A Matter of Life and Death," 65 Am. Econ. Rev. 397, 414-15 (1975). The academic reaction to Lott and Ehrlich raises the question whether well-publicized, empirical studies that reach "conservative" conclusions might be subjected to more searching scrutiny than similarly controversial studies reaching "liberal" conclusions. Such scrutiny could conceivably be more justified if "conservative" studies are likely to translate into unwise social policy, while "liberal" studies are likely to be ignored. Cf. David C. Baldus et al., Equal Justice and the Death Penalty 22-39 (1990) (detailing racial discrimination in imposition of Georgia death penalty) and Stanley Rothman & Stephen Powers, Execution By Quota?, The Public Interest 3 (Summer 1994).

behavior.[4] The implicit model has two premises: 1) the world can be divided into those who intend to do bad things with guns and those whose motives for gun possession are pure, and 2) the bad folk will be able to get their hands on guns in any event, so that restrictions on guns will largely limit the gun access of the good citizens. These premises suggest that arming the law abiding may tip the balance of power away from the criminals, thereby reducing crime by elevating the probability that a criminal would face an armed potential victim or passerby. In other words, since criminals are naturally concerned about their own safety, allowing more people to carry concealed weapons is one way to raise the costs of those crimes involving direct contact with potential victims. Moreover, concealed weapons can generate a general deterrence effect that potentially benefits all citizens - not just those who carry concealed weapons. Because criminals cannot know in advance who is armed with a concealed weapon, their risk goes up in an encounter with any potential victim. Thus, while the open carrying of handguns would only divert criminals from potential victims with guns to those without them, legalizing the concealed carrying of weapons holds out the promise of reducing crime rather than just shifting its incidence.

While theory is often neat, the world is generally messy. The categories of the criminal and the law abiding are not sharply drawn, and the presence of a gun might cause someone on the border between them to move squarely into the criminal camp -- for example, if angry disputes over small matters evolve into criminal homicides instead of mere black eyes. Moreover, it is conceivable that arming the citizenry can encourage an arms race leading more criminals to carry even higher-powered weapons, which they are more quick to discharge when threatened, given the increased cost of hesitation in choosing whether to fire.[5] Finally, accidental deaths and suicides are obviously aided by the presence of guns, and these costs could conceivably outweigh any benefits of shall issue laws in reducing crime. Extensive empirical study is needed to assess the relative magnitudes of the likely conflicting effects.

B. Some Evidence on Gun Ownership and The Enactment of Shall Issue Laws

Lott offers some background statistics to support the claim that simply arming law-abiding citizens will not cause them to harm one another. He cites the high number of permits issued in those states where such records are kept, and the low number of permit holders who were involved in shootings or whose licenses were revoked. This finding is not surprising. Most violent gun crimes are committed by a relatively small segment of the total population. Shall issue laws typically do not allow permits to be issued to individuals below age 18 or 21, or to those who have a criminal record or a history of mental illness. Since those most likely to commit crimes often already have a criminal record, they are unlikely to be given a permit to carry a concealed weapon even in shall issue states.

Lott is sensitive to the fact that states without shall issue laws vary in the ease with which residents can acquire concealed weapons permits. He claims that even within the same

---

[4] Kleck, Gary, Point Blank: Guns and Violence in America, A. de Gruyter, NY, 1991; Polsby, Daniel D., "The False Promise of Gun Control", The Atlantic Monthly, Vol. 273, No. 3, pg. 57 (March 1994).

[5] John Donohue and Steven Levitt, "Guns, Violence, and the Efficiency of Illegal Markets," 88 American Economic Review 463 (May 1998).

state, the level of demonstrable "need" for a concealed weapon that is necessary to receive a permit varies tremendously from county to county. Typically, he notes, the more populous a county, the more reluctant law enforcement officials are to issue permits.

To answer the question of how many and which people own guns, Lott examines data from exit polls from general elections. Looking at the results of the 1988 and 1996 presidential election polls, Lott estimates that gun ownership rose from 26 percent of the population in 1988 to 39 percent in 1996.[6] His estimate of a 50 percent increase in gun ownership in eight years might seem alarming, but it is worth noting that crime was substantially lower in 1996 than in 1988.[7]

Lott points out that the average gun owner is "rural, white male, middle-aged or older, who is a conservative Republican earning between $30,000 and $75,000," although he claims that the number of women gun owners is growing rapidly (p. 38). Middle and upper income individuals own guns at a significantly higher rate than individuals earning less than $15,000. Moreover, he points out that while whites are much more likely to own a gun, blacks are much more likely to kill or be killed with one. This last distinction is especially important to Lott, who wants to emphasize – as the title of his book suggests -- that gun ownership does not correlate positively with violent crime. It should be noted, though, that this data refers to gun ownership generally, and not to the carrying of concealed weapons. What, if any, effect the passage of shall issue laws has in increasing gun ownership is an interesting but uncertain question, and not one that Lott directly confronts. Presumably, however, this could be done by comparing changes in gun ownership in shall issue states with changes in non-shall issue states.[8]

In turning to the crime data itself, Lott initially offers a comparison between the crime rates of those states with shall issue laws and those without them, which are further divided into those with may issue laws and those prohibiting concealed weapons outright. The violent crime rates in shall issue states are substantially lower – although it must be emphasized that these crime rates were substantially lower even before the laws were adopted. Lott shows that the states adopting shall issue laws tend to be Republican and have high NRA membership and low but rising crime rates.

C. The Core of Lott's Case That Shall Issue Laws Reduce Violent Crime

In the end, only empirical research can hope to resolve which of the many possible effects of introducing more concealed handguns will dominate, and to that end Lott has created a massive data set in order to provide "the first systematic national evidence for all 3054

---

[6] To control for the fact that voters are not a random sample of the general population, Lott adjusted the data by age and race to weight groups over-represented as voters in accordance with their proportion of the total population.

[7] The national murder rate dropped from 8.4 in 1988 to 7.4 in 1996. Federal Bureau of Investigation, Uniform Crime Reports for the United States, 1988, 1996.

[8] Alternately, one could use permit data from Oregon, Pennsylvania, and Arizona to get at the relationship between gun ownership and the number of people carrying concealed weapons by comparing the change in gun owners by county to the number of permits issued.

counties in the United States over the sixteen years from 1977 to 1992." (p. 25).[9] This combination of cross-section data from each county with time-series data over a sixteen-year period enables Lott to conduct "panel-data" analysis, which is the preferred statistical approach to addressing the effect of shall issue laws. Since, as noted above, the states that have adopted these laws were traditionally low-crime states, simply running a cross-section regression would incorrectly appear to suggest that concealed gun laws reduce crime. By using a "fixed effects" model, Lott essentially tests how the passage of a shall issue law affects crime *within that state*. Consequently, Lott cannot address the impact of the shall issue laws in the eight states that adopted them prior to the start of his data set (1977) because his fixed effects panel-data analysis requires a *change* in the law during the period under study in order to identify its effect. This is not a criticism of Lott's fixed effects model, which is the appropriate statistical tool, but the reader should keep in mind that Lott has analyzed the impact of only 10 of the 18 shall issue laws that had been adopted by the last year of Lott's data set (1992).

Lott attempts to control for an array of factors in trying to explain crime rates for a variety of crime categories in each county in the country. His explanatory variables include the county arrest rate for the particular crime category, the county's population density, and various demographic breakdowns by race, sex, and age, as well as real per capita income and certain measures of transfer payments – such as average unemployment benefits by county. (p. 176, Appendix 3).

Lott offers three types of evidence in support of his conclusion that shall issue laws reduce violent crime. First, he provides evidence from the national panel-data analysis that the estimated coefficient identifying the existence of a shall issue law (a so-called "dummy variable") is negative and significant. This finding implies that, controlling for an array of explanatory variables, on average crime is lower after a state passes a shall issue law than it was before. We refer to this as his "shall issue dummy variable model." Second, Lott presents evidence from the same national panel-data analysis that tests whether the passage of the shall issue law alters the trend in crime in a state. This "time-trend model" is implemented by interacting the shall issue dummy variable with a time trend. Third, Lott narrows his focus to two states (Pennsylvania and Oregon) for which he has data on the number of permits issued in response to the shall issue laws, and provides evidence from regressing crime rates on the number of permits. While we discuss each of these three forms of evidence below, our primary focus will be on the first.

1. The Shall Issue Dummy Variable Model

The first line of Table 1 shows the coefficient estimates for the shall issue dummy for nine different crime regressions that Lott reports in his book.[10] These are essentially Lott's core results, from which he derives his bottom-line estimate that if all states passed concealed handgun laws, *annual* victim losses would drop by $5.7 billion dollars. Note that Lott estimates that nondiscretionary right-to-carry laws are responsible for *reducing* violent crime by

[9] Lott also discusses the relative merits of state versus county-level data, and concludes that viewing states as homogenous entities obfuscates the relationship between deterrence and crime. Thus, while he presents some results from statewide analyses, he relies more heavily on his county-level analysis.

[10] See Lott's Table 4.1.

almost 5 percent but *increasing* property crime by 2.7 percent and both of these effects are significant at the 1 percent level. Lott interprets these findings to mean that criminals are deterred from personal attacks such as murder, robbery, rape, and assault, and that some criminals shift their criminal impulses to auto theft and larceny, where they are less likely to confront an armed victim or passerby.

Lott seriously considers an array of such possible substitution effects -- that is, how concealed handgun laws might cause actors to substitute one behavior for another. He argues not only that criminals on the margin may have substituted away from violent crime and toward property crime,[11] but also considers whether concealed weapons laws may have caused criminals to substitute toward other locales (without concealed handgun laws),[12] toward younger victims (who cannot carry concealed weapons),[13] toward earlier crime (before potential victims arm themselves),[14] and toward acquaintance crime (against those known not to carry concealed

---

[11]On p. 95, Lott claims that "two of these three sets of estimates imply that concealed handgun laws also result in lower property crime rates." This seems to be flatly wrong. Lott's Table 4.1 (whose first line is replicated in our Table 1) and his analysis of shall issue dummies interacted with county population and county density in his Figures 4.1 and 4.2 indicate that shall issue laws *increase* property crime. The only analysis indicating that property crime decreases (by 4.19%) is the state-wide regression, which Lott correctly concludes should be given relatively less weight.

[12] Lott finds that "deterrent effects do spill over into neighboring areas . . .. Criminals who commit murder, rape and robbery apparently move to adjacent states without the laws." (p. 92.) There are several discrepancies between Lott's textual and tabular discussion. Lott claims textually that "adopting a non-discretionary concealed-handgun law lowers the number of aggravated assaults in neighboring counties" but his table indicates that adopting such a law increases aggravated assaults in neighboring counties without the law. Lott also fails to relate these estimates to his earlier analysis of the national effects of passing nondiscretionary concealed handgun laws. Part of the benefit that Lott initially finds from concealed handgun laws is due to the exportation of crime to neighboring states. If all states adopt the law, criminals may be less likely to move to other nations. At various points in the book, Lott seems to imply that causing criminals to leave the area is an unmitigated benefit, but from a national perspective exporting crime may be seen as a net harm.

[13] Lott concludes that shall issue laws deter "crime against adults more than against young people—because only adults can legally carry concealed handguns—but the effect is statistically insignificant." (p. 98.) Lott also claims, however, that raising the age limit for concealed handgun permits to age 21 "appears to lower murder rates, but it tends to reduce the decline in rape and overall violent crime rates that is normally associated with non-discretionary handgun laws." (p. 86.)

[14] 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." p. 102. This explanation may reflect an excessive tendency to interpret the data from the perspective of one convinced that shall issue laws have potent effects.

Another example of this tendency is found in Lott's analysis that shall issue laws appear to dampen rape rates, despite the fact that relatively few women admit to carrying concealed weapons. This, he concludes, demonstrates that the deterrent effect of women carrying guns is greater then that of men, simply because the gun augments a woman's ability to defend herself more so than a man's. This sounds reasonable enough, if you fully accept the potent deterrent effect resulting from shall issue laws. On the other hand, if one's prior belief is that shall issue laws do not directly reduce crime rates, one might interpret the same evidence differently: that rape rates were dropping despite the small number of armed women

weapons).[15] Lott also considers whether concealed handgun laws cause potential victims to substitute toward more reckless behavior[16] -- with particular emphasis on accidental death and suicide. While much is made in the press and the popular debate about the effect of handguns on accidental shootings, Lott provides regression evidence that the passage of concealed handgun laws has had little effect on either accidental deaths or suicide.[17] (p.112.)

2. The Trend Analysis

For the second prong of his argument that shall issue laws reduce violent crime, Lott attempts to disaggregate the effect of such laws across time. In a variety of different specifications, Lott tests to see whether there are different time trends in the crime rates before and after the passage of the shall issue laws. Lott finds that for most violent crimes, the time trend prior to the passage of the law indicates that crime was rising," (p. 73) but that after the implementation of the law the crime trends become more negative to a degree that is statistically significant. (Id.) When Lott interacts a linear time trend with the shall issue dummy, he finds not only that the violent crime rate is expected to drop .9 percent per year in counties that passed the shall issue laws, but that property crime is also expected to drop .6 percent per year. (p. 76.) Lott also produces figures based on regressions with both linear and squared time trends

---

breaks the correlation between high numbers of concealed weapons permits among potential victims and falling crime, thereby undermining the core causal dimension of Lott's theory.

[15] Lott finds that "in states with concealed handgun laws victims know their non-family offender 2.6 percentage points more frequently than not" but know their family offender a quarter of one percentage point less frequently. (p. 98.)

[16] Lott emphasizes that his estimates of the benefits of concealed handgun laws are conservative because of the potential substitution of victims toward riskier, but valuable activities. "To the extent that people are taking greater risks regarding crime because of any increased sense of safety produced by concealed-handgun laws, the preceding numbers underestimate the total savings from allowing concealed handguns." (p. 86.) Following Sam Peltzman, Lott sees that it might even be possible for "concealed firearm law [to] both make individuals safer and increase crime rates at the same time . . . [A]llowing citizens to carry concealed firearms may encourage them to risk entering more dangerous neighborhoods or to begin traveling during times they previously avoided." (pp. 33-34.)

[17] Of course, Lott's finding that shall issue laws had little impact on accidental deaths and suicides may also be the result of the difficulty of finding a model that satisfactorily explains these events. Lott makes the point that the number of accidental deaths attributed to handguns is roughly 200 per year, but this is likely to be a substantial underestimate. While 400 accidental deaths result from hunting rifles or shotguns, roughly another 1000 accidental deaths are reported every year as resulting from "other and unspecified firearm missile." Since these roughly 1000 firearm deaths are not attributed to either handguns or long-guns, it seems likely that at least a third of deaths caused by an unknown type of gun were caused by handguns, raising to approximately 550 the number of annual accidental deaths resulting from handguns. In any event, ignoring the unspecified category will certainly tend to understate the actual number of handgun fatalities.

Surprisingly, the total number of accidental firearm related deaths has declined from 2380 in 1975 to 1225 in 1995. Meanwhile, the number of handgun suicides has increased between from 2535 in 1982 to 3700 in 1995. The precisely offsetting nature of these two trends raises the question of whether some cases formerly deemed to be "accidental" deaths are now being categorized as suicides. Accident Facts, Editions 1982-1998, National Safety Council, Chicago, IL.

before and after the adoption of concealed-hand gun laws to show the allegedly dramatic effect of such laws on predicted crime rates. (p. 77.)

3. Evidence from Permit Data

Given the lack of data concerning the number of concealed-carry permits over time and in all counties, it is not possible to directly run a panel-data regression to determine the link between the number of permits and crime rates. To deal with this data shortcoming, Lott explores whether a county's population density can be a proxy for increases in the number of gun permits. (p. 63.) Based on his conversations with local law enforcement agents, Lott concluded that, in states where concealed weapon permits were legal but issued pursuant to official discretion, the counties that were least likely to issue them were the heavily populated, urban counties. In other words, in may issue states, rural counties required much less demonstrated evidence of "need" for a concealed weapon, and the law enforcement agencies were much more likely to grant permits. In contrast, agencies in cities and densely populated counties held applicants to much higher standards of need, resulting in far fewer permits being granted. Thus, when states passed shall issue laws, the counties that most likely experienced large increases in the number of permits were the densely populated ones. Therefore, Lott concludes, it is actually the urban counties that will benefit most from the passage of shall issue laws. He cites a regression where "passing a concealed-handgun law lowers the murder rate in counties with about 3,000 people per square mile...by 8.5%, 12 times more than it lowers murders in the average county."[18] (p. 63.)

Since the links between the issuance of permits and the crime reduction that Lott attributes to the shall issue laws is so crucial to establishing causality, more research on this issue is needed. Lott's county population proxies rely on his assumption that population density is a good predictor of the difficulty in obtaining permits under discretionary laws. However, if many states went directly from prohibiting concealed weapons to a nondiscretionary law (like Arizona), Lott's assumed relationship between permits and density would break down. Likewise, other state gun laws might confound this relationship; for example, Arizona had laws allowing guns to be carried openly before passing its shall issue law.[19] (pp. 63-65.)

---

[18] Note that in Lott's data set (1977-92), there were only 560 counties with density greater than 2900 people per square mile, or roughly 1 percent of the total 50,023 observations.

[19] Lott also finds that the violent crime reductions (and property crime increases) are systematically larger in counties with crime rates above the median rate than in counties with crime rates below the median rate. (p. 61.) This would make sense under Lott's theory if we suspected that there was a higher demand for permits in high-crime counties.

Lott also examines the data in numerous other ways. He compares the results of regressions run on high-crime areas with those run on low-crime areas (as measured by falling above or below the median crime rate). He drops out various demographic and economic independent variables, and examines the resulting shall issue law variable coefficients. He tries controlling for state trends, similar to the way in which he used year dummy variables to control for national trends. Although he doesn't show his results, he says the results were similar except that aggravated assault and larceny were no longer statistically significant. And although he discusses why a county-level analysis is superior, he also runs the regressions at the state level. He describes the results as similar to the county-level regressions, except that a) passage of shall issue laws lowered all crimes, not just violent, and b) the shall issue dummy explained more of the variation

Indeed, Lott's simple theory that "[s]tates with the largest increases in permits should show the largest decreases in crime rates" (p. 81) can also be undermined by differences in how various jurisdictions enforce the prohibition against carrying a concealed gun without a license. States that lack enforcement of such laws -- for example, by failing to prosecute citizens who illegally use a concealed weapon defensively -- may have a large class of individuals who carry concealed weapons illegally before a shall issue law is passed. When the shall issue law takes effect, these illegal concealed handgun carriers may rush to obtain more permits than we observe in a state that has a tradition of strictly enforcing its handgun laws. Consequently, the number of additional permits issued may not correlate across state (or even across counties within a state) with how many more people are actually carrying concealed weapons.

Lott's commendable efforts to ferret out data on the actual number of concealed weapons permits succeeded in securing current data in six states (Arizona, California, Florida, Oregon, Pennsylvania, and Washington), and time-series data for Arizona (94-96), Oregon (90-92) and Pennsylvania (86-92).[20] Regressions based on state data in Pennsylvania and Oregon counties are offered in support of Lott's thesis that nondiscretionary concealed handgun laws reduce violent crime. However, the size of the estimated coefficients in the murder regressions is so large as to cast doubt on their validity. Lott finds that a 1 percentage point increase in the proportion of the population with concealed weapons permits leads to a 26.7 and 37 percent reduction in the murder rates of Pennsylvania and Oregon, respectively.[21] Due to the confounding factors (such as, passage of other anti-crime measures and different rates across counties in the enforcement of pre-shall issue prohibitions on carrying concealed handguns), these regressions are not as clean as Lott would have liked. Perhaps for that reason, neither Lott nor his critics have put much weight on these regressions.

4. Summary

On the whole, we find that the combined weight of these three types of evidence require that Lott's thesis that concealed weapons laws reduce violent crime be taken seriously. While Lott's methodology has been criticized for not meeting the generally accepted standards of social science, we disagree. This book displays the indicia of high quality social science—Lott creates and analyzes a massive statistical data set on an important public policy issue, which he freely shares with scholars across the country, while checking the robustness of its core conclusions through a large number of alternative specifications. While we would have preferred a greater attentiveness to potentially disconfirming evidence, it would be unfair to hold Lott alone to the standards of Charles Darwin.[22] Because Lott's book has had such a high-

---

while arrest rates explained less. Also of interest, Lott adds county-level data from 1993 and 1994. While he was not able to include all the independent variables that went into the earlier regressions, he argues that the addition of those years supports his results.

[20] Florida numbers were also available, but only at the state level.

[21] The Pennsylvania (Lott, Table 5.4, p.104) and Oregon (Table 5.5, p.104) coefficients in the murder rate regressions are statistically significant respectively at the 1 and 11 percent levels.

[22] Darwin was relentless – virtually in a class by himself -- in seeking out every shred of evidence that could disconfirm his theories and prior beliefs. Stephen Jay Gould, "Why Darwin?" The New York

profile and controversial impact on this important policy issue, and because Lott has graciously shared his data, critics have subjected his work to a type of strict scrutiny that is rarely found in social science. We will first discuss our efforts at replication and testing of Lott's findings and then turn to these criticisms.

II. Replicating Lott's Findings and Testing Their Robustness

A. Correcting the Shall Issue Dummy and Adjusting Standard Errors

The second line of Table 1 presents our (almost perfect) replication of Lott's core findings.[23] We had a quibble with Lott's definition of his shall issue dummy in that he claimed that he started this variable in the first full year after the law's passage, when in fact he did not use that precise definition.[24] The third line of Table 1 shows that making this correction slightly weakens Lott's finding that violent crime will be reduced and strengthens the claim that property crime will go up. Thus, we see that the estimated coefficient on the shall issue dummy drops from –4.9 to only –3.6 percent for violent crime and from -2.2 to -.5 percent for robbery, while rising from 2.7 to 4.6 percent for property crime. Furthermore, calculating the robust standard errors for the panel-data analysis tends to increase the standard errors and consequently lower the t-statistics, as reflected in the adjusted t-statistics that we present beginning in line 3 of Table 1. On the surface, this evidence weakens Lott's analysis and would obviously substantially dampen the alleged monetary savings from shall issue laws, but it does not really change his basic story that violent crime seems to go down while property crime goes up. In fact, reducing some of the estimated effects, such as the reduction in the magnitude of the coefficient in the murder regression from almost -8 to about -5, might even be viewed as making the numbers more plausible. The primary problem that emerges from the line 3 results is that the effect of shall issue laws on robbery virtually disappears – and if the Lott effect is real, one expects to see a decline in that particular crime.[25]

B. The Incarceration Rate and The Arrest Rate

Of potential interest is Lott's failure to include in his regressions a fairly standard crime regression explanatory variable -- the state incarceration rate. Line 4 of Table 1 builds on the

---

Review of Books, April 4, 1996, p.10.

[23] The single exception was that we had a slightly lower estimate for the coefficient on the shall issue dummy in the murder regression.

[24] Inspection of Lott's Shall Issue Dummy revealed that only Florida and Georgia were lagged one year after the adoption of their shall issue laws; all other relevant states were counted as shall issue states starting with the year such a law was passed. "Because of delays in implementing the laws even after they go into effect, we also used a dummy variable that equals one starting the first full year that the law is in effect. The following tables report this second measure, though both measures produced similar results." Lott and Mustard, "Crime, Deterrence, and Right-to-Carry Concealed Handguns," 26 Journal of Legal Studies, 1, 13 (1997). All of our Table 1 regressions – except the line 2 replication of Lott's results – use the corrected shall issue dummy.

[25] We further discuss the expected effect on robbery and Lott's response on this issue, below. See TAN 40.

model of line 3 and simply adds the incarceration rate to the crime regressions. Comparing these two models, we see that introducing the incarceration rate tends to move the coefficient estimates in Lott's direction: the crime reducing effect of the shall issue law on murder increases and is restored for robbery. Clearly, Lott did not omit the incarceration rate because it harmed his analysis.

The inclusion of the incarceration rate did have the effect of weakening one of Lott's major explanatory variables – the arrest rate. While the arrest rate remains statistically significant when the state incarceration rate is included in the regression, its magnitude is dramatically reduced. This is not surprising since the arrest rate and the incarceration rate are likely to be correlated. One possible advantage of using the incarceration rate, though, is that it may enable us to dispense with the arrest rate, which in theory is a useful explanatory variable, but in practice is problematic on two grounds. First, and most importantly, using the arrest rate substantially reduces the size and composition of the data set because any county with no crimes of a certain type in a particular year would have a zero denominator for the arrest rate and therefore would be dropped from the analysis. Excluding observations based on the realization of the dependent variable (crime) is particularly likely to bias the results, and Black and Nagin have criticized the Lott analysis on this basis.[26] Second, since the dependent variable and the denominator of the arrest rate will both be derived from the identical crime number, the coefficient for the arrest rate will be infected with ratio bias whenever crime is measured with error – as it certainly is.[27] Accordingly, we dropped the arrest rate explanatory variable and added in the incarceration rate, which enabled a more complete data set to be used.

The results of this new specification are presented in line 5 of Table 1, which shows two important effects from this change. First, the estimated effect of shall issue laws on murder dropped sharply and became statistically insignificant (compare lines 4 and 5 in Table 1). Second, the effect on robbery is completely eradicated. While I imagine that Lott would still be happy with the idea that shall issue laws substantially reduce rapes and aggravated assault even if they do not reduce murder and robbery and do spur various property crimes, we argue below that the failure to see a crime-reducing effect of the law for robbery undermines Lott's theory.

C. Are There Yearly Uniform National Influences on Crime?

The overall picture that emerges from the first five lines of Table 1 is that shall issue laws have no effect on robbery or burglary, but reduce total violent crime while increasing total property crime. Lott's conclusion that violent crime drops and property crime rises still has support after the minor tinkering described above – although the particular coefficients and their statistical significance can bounce around more than one would like. Nonetheless, a bigger problem may be lurking undetected. Essentially, Lott knows that there are unexplained variations each year in the overall crime rate, and he controls for these effects by including a

---

[26] Dan Black and Daniel Nagin, "Do 'Right-to-Carry' Laws Deter Violent Crime?" 27 Journal of Legal Studies 209, 211 (1998).

[27] Put simply, if one year the number of reported crimes drops for some reason even though actual crimes remain constant, the calculated arrest rate will be elevated and will incorrectly seem to explain the lower reported crime rate (the dependent variable).

year dummy for each year of his model. The idea behind this explanatory variable is that if crime rises X percent in every county in the country owing to some factor that is not picked up by the other explanatory factors, we don't want to attribute this effect to the presence (or absence) of a shall issue law. Instead, we want to find the effect of a shall issue law net of the national influence. But is there a constant national influence on crime? Figure 1(f) compares the pattern of murder rates for all 20 states who have never had a shall issue law with that of the eight states who had such laws before 1977. Prior to 1985, the two patterns move in lock step, even though the shall issue states have an overall lower level of crime. This portion of the graph supports Lott's use of the year dummies. But after 1985, note how crime rose in the 20 non-shall issue states at a far more precipitous rate than in the shall issue states. The assumption of the constant national effect on crime can cause problems if two conditions apply: 1) the mean national effect varies substantially across counties, and 2) the shall issue laws tend to get passed in states that are less effected by the national upturn in crime that began around 1985.

Figure 1(f) suggests that both of these factors may hold true.[28] The late 1980s and early 1990s were a time in which crime rose dramatically in areas in which the crack trade proliferated. Other areas where crack did not penetrate saw no such surge in violent crime. One can get Lott-like results in estimating a crime regression if the states that passed the shall issue laws once the crack epidemic got underway were not the states that had the severe crack-induced problem of increased violence. Since crack is an omitted variable that affects crime, its omission from the crime regression will bias the estimated effect of shall issue laws if states where crack was prevalent shunned the shall issue laws. Recall Lott's assertion that the states adopting shall issue laws tend to be Republican and have high NRA membership and low crime rates. That doesn't sound like the sort of place where one finds the worst problem with crack. Since Lott's year dummy posits an identical nationwide effect on crime in any year and he has no explanatory variable to control for the violence-inducing influence of the local crack trade (no one else does either), there may well be a serious omitted variable bias problem in his regressions.

Figures 1(a) – 1(e) depict the murder rates for all 10 states that adopted shall issue laws during the period from 1977 through 1992, starting with Maine in 1985 and Florida in 1987. Maine probably makes the point well as it is exactly the sort of Republican, low-crime state to which we have just alluded. On the whole, it would be hard to argue from the visual inspection of the Maine time series that the 1985 law caused a drop in crime. But if crack caused large crime increases in many populous states after 1985, the year dummies would be substantially elevated and Maine would be *perceived* to be a state in which the 1985 shall issue law *depressed* crime relative to the national trend. Indeed, in only two of the ten states depicted in Figure 1 do we see anything that looks like a sustained drop in crime following – although not

[28] The graph is merely suggestive since we are really interested in comparing the crime pattern of the non-shall issue states with that of the 10 states that Lott studied under the counterfactual that they had not adopted a shall issue law. Moreover, even if we could do the relevant counter-factual comparison, which we can't, there would be nothing wrong with Lott's regression if any different observed crime pattern could be explained by the included explanatory variables.

necessarily because of -- the passage of the shall issue law.[29] The whole effect of Lott's regression thus comes from the fact that the national time dummies are high for the late 1980s and early 1990s and the shall issue dummy reveals that violent crime did not rise as substantially in the shall issue states. If the shall issue law was what restrained crime from growing in those 10 states, then Lott is right that the laws reduced violent crime. Conversely, if crack caused crime to grow in selected areas of the country and legislatures in states that did not have the crack problem passed shall issue laws (while those with the problem did not pass such laws), then Lott's results could simply be the product of omitted variable bias.

D. Exploring the Influence of Maine and Florida

To explore how sensitive Lott's results are to the experiences in Maine and Florida, we tried a number of tests that are reported in lines 6 through 8 of Table 1. First, we simply drop these two states from the analysis.[30] The Lott critic would note that all of the Lott findings that shall issue laws cause violent crime to fall become statistically insignificant. (See Table 1, line 6.) The Lott supporter could reply, though, that the basic pattern of falling violent crime and increasing property crime still persists and all five of the violent crime figures still are negative. Indeed, if it is appropriate to include the incarceration rate as an explanatory variable, then doing so again helps Lott's cause – even when Maine and Florida are dropped. (See Table 1, line 7.) Note that in this regression the estimated effect on robbery becomes statistically significant, and the other estimates for violent crime all move in the Lott direction (albeit still not to the point of statistical significance).

The last line of Table 1 includes Maine and Florida in the estimation but provides a separate dummy for those two states as opposed to the eight other shall issue states that adopted laws after 1987 (and before 1992). The first effect in regression 8 is essentially the pure Maine and Florida effects, which are extremely large. In fact, they are too large to be believed. A drop in murders of over 20 percent from the passage of a shall issue law strains credulity.[31] This result provides a strong indication that the model is over-attributing deviations from national trends in crime to the shall issue laws for these two states. For the other eight states – those that adopted shall issue laws after 1987 – the effects are far smaller, and, while negative for all violent crimes, are always statistically insignificant. Again, one can rehearse the pro-Lott and anti-Lott positions.

---

[29] The states are Florida and Georgia.

[30] There are a number of possible rationales for dropping these two states. First, Florida almost certainly experienced a massive upturn in crime from the Mariel boat lift of 1980 and from its role as a major center of the illegal drug trade. Thus, one might well have expected drops in crime in Florida when the effects of these factors subsided, yet the Lott regression will attribute this decline to the passage of the 1987 shall issue law. Second, as noted in footnote 2, above, there is some dispute as to whether Maine should be regarded as a shall issue state. Third, one might imagine that the initial states to adopt the shall issue law might differ systematically from subsequent states, and therefore it is useful to test whether states that are later to adopt shall issue laws get different and smaller benefits.

[31] Note also the anomalous (albeit statistically insignificant) finding that robberies in Maine and Florida *rose* by 5.3 percent.

III. Criticism and Lott's Response

Because the basic findings of Lott's book had already been published in his provocative article co-authored with Mustard,[32] a substantial body of criticism of Lott's thesis had appeared by the time his book went to press. This enabled Lott to respond, and his book addresses 23 specific criticisms. Once again, Lott deserves credit for his energy and willingness to engage in this extended academic debate, but as noted below some of the more salient criticisms have to date not been satisfactorily answered. For expositional convenience, we group the pre-existing criticisms into two categories: inconsistencies with theoretical predictions; and weaknesses in the estimation methodology.

A. Inconsistencies with Theoretical Predictions

We have already alluded to the problem that the types of crime reduction reported in Table 1 may not be consistent with the theoretically plausible effects of a shall issue law. Serious theoretical inconsistencies can lead us to reject altogether the reliability of the Lott regression model. This section will examine a number of such potential inconsistencies.

1. Unrealistically Large Reductions in Crime

Frank Zimring and Gordon Hawkins have suggested that the estimated size of crime reduction is unrealistic because it is "out of proportion to the small scale of the change in carrying firearms that the legislation produced."[33] Is it plausible that issuing concealed gun permits to 2 percent of the population (as in Florida) could generate an 8 percent reduction in murders or a 5 percent reduction in rapes? Lott claims the answer is "yes": "Assuming that just 2 percent of the population carries concealed handguns, the drop in the murder rate only requires that 0.025 percent of those with concealed-handgun permits successfully ward off a life-threatening attack to achieve the 0.0005 percent[age point] reduction in the murder rate." (p. 130.)[34] Since people who are more at risk are more likely to seek concealed weapons permits, Lott argues this calculus is plausible.

Zimring and Hawkins observe that there are two potential transmission mechanisms by which potential criminals respond to the passage of a shall issue law. The first, which they term the announcement effect, changes the conduct of potential criminals because the publicity attendant to the enactment of the law makes them fear the prospect of encountering an armed victim. The second, which they call the crime hazard model, implies that potential criminals will

---

[32] John R. Lott, Jr. and David B. Mustard, "Crime, Deterrence, and Right-to-Carry Concealed Handguns," 26 Journal of Legal Studies 1 (1997).

[33] Zimring and Hawkins, "Concealed Handgun Permits: The Case of the Counterfeit Deterrent," 59 The Responsive Community (Spring 1997).

[34] Note that the 8 percent reduction in murder to which Lott alludes comes from line 1 of Table 1. But recall from regression 8 in Table 1 that the Florida effect was a 21 percent drop in murders and a 15 percent drop in rapes. Moreover, Black and Nagin estimate the effect in Florida alone to be a 28 percent drop in murders and a 17 percent drop in rapes. (See their Table 1 on p. 212.) These do seem implausibly large. Still, even if Lott's model generates over-estimates of the effect of shall issue laws in some states owing to random influences, the overall estimates could still be sound if random influences lead to offsetting under-estimates in other states.

respond to the actual increased risk they face from the increased arming of the citizenry.[35] Lott adheres to the standard economist's view that the latter mechanism is the more important of the two – but he doesn't fully probe its implications. Recidivists and individuals closely tied to criminal enterprises are likely to learn more quickly than non-repeat criminals about the actual probability of encountering a concealed weapon in a particular situations. Therefore, we suspect that shall issue laws are more likely to deter recidivists. If 2 percent of the population carries concealed weapons, then a criminal who robs 100 people a year faces an 86.7 percent chance of encountering a concealed weapon over the course of the year.[36] A 2 percent chance of encountering an armed victim may not be sufficient to deter a one-time criminal, but it may be sufficient to deter someone from making a profession out of robbery.

Appreciating the role of recidivism may also provide an additional reason (beyond the announcement effect) for why the concealed gun laws could have an immediate crime-reducing effect, since recidivists can be adversely impacted by the presence of even relatively small proportions of concealed handgun carrying. Thus, if Lott's theory were true, we would also suspect that the proportion of crime committed by recidivists should be decreasing and that crime categories with higher proportions of recidivism – and robbery is likely in this category -- should exhibit the highest reductions. Once again, though, the lack of a strong observed effect for robbery raises tensions between the theoretical predictions and Lott's evidence.

2. Unpredicted Effects on Particular Crime or Victim Types

As we have noted, the deterrence theory generates predictions that certain types of crime and certain types of victims will be disparately effected by the expanded prevalence of concealed handguns. Specifically, crimes in which criminals are more likely to confront their victims should show greater reductions than crimes of stealth where the victim (and her concealed gun) is not likely to be present. Similarly, victims who are less likely to carry concealed weapons are more likely to be victimized than victims who have a higher propensity to pack concealed heat. These theories seem to suggest that the enactment of shall issue laws should cause: (1) the ratio of stranger to acquaintance killings to decrease; (2) the amount of the confrontational and economically motivated crime of robbery to go down; and (3) the ratio of

---

[35] Zimring and Hawkins, ibid, p.53.

[36] The calculation assumes that each of the 100 victims has a two percent chance of having a gun. For a similar argument of how the Lojack anti-auto theft device might deter serious car thieves, see Ian Ayres and Steven Levitt, "Measuring Positive Externalities from Unobservable Victim Precaution: An Empirical Analysis of Lojack," 113 Quarterly Journal of Economics 43 (1998). In fact, Lott states, "[T]he theoretical deterrent effect of these devices is the same as that of concealed handguns: because the device is small and easy to hide, a criminal cannot easily know whether a car has the tracking device until the police arrive." (p. 147). However, in footnote 31 on page 212, he advises caution in accepting the claim that Lojack substantially reduces auto theft: "The main issue with their empirical estimates … is whether they might be overestimating the impact from Lojack because they do not control for any other responses to higher auto-theft rates. For example, while higher auto theft rates might trigger implementation of Lojack, they might also increase purchases of other anti-theft devices like The Club. In addition, the political support for altering the distribution of police resources among different types of crimes might also change." Of course, these cautions would also apply to Lott's work, which does nothing to control for other victim precautions or for increases in policing activity over time.

young to old victims to increase (because youths cannot legally carry concealed weapons). Critics have charged that Lott's data is inconsistent with each of these predictions.[37]

Let's start with the first point. As Al Alschuler observes, "[Lott and Mustard] report (astonishingly) that the proportion of stranger killings increases following the enactment of right-to-carry laws, while the proportion of intra-family killings declines. That right-to-carry laws deter intrafamily homicides more than they deter stranger homicides is inconceivable."[38] Lott claims, however, that this finding is not worrisome because murder victims are often deemed to be acquaintances of their murderers, even though they are really like strangers.[39] Therefore, he concludes, "the diverse breakdown of these groupings makes it difficult to predict on theoretical grounds how the number of murders among family members, acquaintances, strangers, or unknown cases should necessarily change relative to each other." Lott is certainly right on the last point. While most simple theories would predict that stranger murders would be deterred more than acquaintance murders, a slightly more complicated model can yield the opposite conclusion. For example, if potential murderers would not seek to kill those whom they knew to be armed, then getting a gun would protect you from murder by your acquaintances who would know you possess a firearm. If individuals were good at knowing when their friends and acquaintances might harbor serious hostility towards them, they might disproportionally be the ones to buy and carry arms, thereby substantially reducing acquaintance-homicides. Since the theory, as is so often the case, is indeterminate, Lott's finding on this issue cannot be dismissed as in conflict with theory.

But the theory seems stronger on the expected effect of shall issue laws on the confrontational and economically motivated crime of robbery, yet we find little or no effect in many of the regressions of Table 1.[40] Lott responds that robbery does exhibit a large and statistically significant reduction in the linear time trend tests that he performed. He also claims that robbery includes many crimes that are not street robberies such as bank or residential robberies. This latter point is theoretically true but empirically insignificant. Certainly, bank robberies make up only a small faction of overall reported robberies. Moreover, if the Lott

---

[37]Zimring and Hawkins have also suggested another inconsistency with the theoretical predictions of Lott model. To wit, if an increase in concealed weapons were responsible for deterring crime we might expect to see an increase in the number of justified homicides in jurisdiction that pass shall issue statutes. However, Zimring and Hawkins find no such increase.

[38]See Albert W. Alschuler, "Two Guns, Four Guns, Six Guns, More Guns: Does Arming The Public Reduce Crime?" 31 Val. U. L. Rev. 365 (1997).

[39] Lott observes that the "category of acquaintance murder is extremely broad (encompassing shootings of cab drivers, gang members, drug dealers or buyers, and prostitutes or their clients)." Moreover, "[f]amily members may also find that concealed handguns protect them from other estranged family members." (p. 148).

[40]This criticism was first raised by Daniel W. Webster, "The Claims That Right-to-Carry Laws Reduce Violent Crime Are Unsubstantiated," The Johns Hopkins Center for Gun Policy and Research, http://infosys.jhsph.edu/centers/gunpolicy/research.cfm, which states on p. 3 that Lott's "results indicate that shall-issue laws had little or no effect on robbery rates."

hypothesis is true, we might have expected to see a stronger crime-reducing effect for robbery than, say, for murder, since carrying concealed guns can potentially stimulate some murders even if it deters others. But while law abiding citizens with concealed weapons may get angry and commit murder, it is less likely that they will get angry and commit robbery, which is primarily an economically motivated crime. Indeed, the whole thrust of the claimed shift from violent to property crime hinges on robberies being deterred by concealed weapons. The notion is that if you can't rob for money, you may shift to stealing cars and burglarizing houses. But why would someone shift to property crime if his or her plan to assault someone was deterred by the fear of the victim's response? Lott's entire explanatory edifice is quite shaky if robbery is not deterred by shall issue laws.[41]

Finally, Lott finds no statistically significant difference in the way that concealed weapon laws deter crime against adults and youth victims. (p. 98.) Lott claims that the laws have a spillover effect—in that gun-toting adults may be "able to protect some youngsters in threatening situations." (p. 147.) Again, it is plausible to argue that there will be some spillover but it is surprising for criminals on the margin not to shift toward victims who by law cannot carry concealed weapons.[42]

3. Varying Degrees of Reduction Across States

Dan Black and Daniel Nagin have criticized Lott's findings in part because concealed weapons laws have not produced similar effects across different states and counties.[43] Lott responds that we should not expect the same deterrence effects in different jurisdictions because counties that were permissive in granting concealed hand gun permits under the discretionary regime should, ceteris paribus, have a smaller increase in the usage of concealed handgun when a shall issue law is adopted. Lott claims to have shown that "the magnitude of the drops, both across counties and states and over time, corresponds to the number of permits issued," (p. 132) but in fact he has only shown that there are larger violent crime reductions in the more populated and more densely populated counties of shall issue states vis-à-vis non-shall issue states. Lott assumes—based on discussions with government officials—that such counties are

---

[41] Nor can Lott's invocation of a Peltzman effect – that we don't perceive the reduction in robbery because people feel safer because of the crime drops induced from shall issue laws and therefore expose themselves to, and thus experience, more robberies -- alleviate the problem. (See note 16, supra.) Even if such an effect did operate, it would still keep the criminals in the robbery business, not cause them to shift away.

[42] Jens Ludwig has published a sophisticated empirical test of whether adopting states have a higher adult to youth victim ratio than non-adopting states. Jens Ludwig, "Concealed-Gun-Carrying Laws and Violent Crime: Evidence from State Panel Data,"18 Int'l Rev. L. & Econ. 239 (1998). Using a "difference-in-differences" approach, Ludwig finds that there is no statistically significant difference in the relative adult and juvenile homicide victimization rates of adopting and non-adopting states. This result is not consistent with a strong-form version of deterrence theory, but as noted above there are theoretical reasons to expect some deterrence spillover. Ludwig does not directly address the question of whether violent crime has systematically declined in adopting states.

[43] See also Webster, "Claims," p. 2.

likely to have a larger increase in the number of concealed permits granted.

Black and Nagin show that the estimated effect of the shall issue law varies substantially across the 10 states that adopted them during the period of Lott's data set, and they even find several violent crime categories for individual states where the adoptions of concealed handgun laws is associated with a positive and statistically significant increase. Overall, Black and Nagin argue that for the four crimes of murder, rape, assault, and robbery for the 10 shall issue states, only 15 of the 40 possible coefficients are significant and three of these 15 are *positive* – suggesting higher crime resulted from the law. It may be too much to expect the individual state coefficients to be statistically significant, but in only one of the four categories – aggravated assault – do the number of negative coefficients substantially outweigh the positive coefficients.[44] This evidence would not necessarily undermine Lott's findings, but it does raise some concern. Moreover, Lott does not respond to Black and Nagin's finding that excluding Florida and small counties (with population less than 100,000) from his samples destroys the statistical significance of all of the violent crime categories except assault.[45] This suggests that Lott's results are not as robust as he claims. True, Lott's thesis is not embarrassed by varying degrees of deterrence across state (especially since he shows that this variance may be related to the number of permits issued). However, his thesis is embarrassed by the considerable number of state specific crime categories where concealed handgun laws are associated with an increase in crime and where the overall significance of his results is undermined by the exclusion of Florida and small counties.[46]

B. Weaknesses in the Estimation Methodology

It is natural to ask whether the coefficients in a regression analysis suffer from either endogeneity or omitted variable bias. As to endogeneity, there is the dual concern that the

---

[44]For assault, 9 out of 10 estimated coefficients were negative (thus supporting Lott's thesis) and 6 of these 9 were statistically significant at the 5 percent level (the outlier was Pennsylvania showing a statistically significant 7 percent *increase* in assaults).

[45]Black & Nagin removed small counties to respond to the problem of omitting all observations with a zero crime rate (where the arrest rate variable was undefined), which generally was found in smaller counties for murder and rape. Dan Black and Daniel Nagin, "Do 'Right-to-Carry' Laws Deter Violent Crime?" 27 Journal of Legal Studies 209, 213 (1998). As discussed, the selection effect induced by using the arrest rate explanatory variable seemed to bias the results in Lott's favor. (Compare lines 4 and 5 of Table 1.)

[46]Lott's basic regression has also been criticized for having arguably anomalous signs on some of the demographic coefficients. In particular, Zimring and Hawkins have questioned the finding that "the concentration of older black women in a population predicts higher crime rates . . . but not the increased concentration of young men, aged 20 to 29." Zimring and Hawkins, at 56. See also Alschuler, at 368 ("the proportion of the population that is black, male and between ten and nineteen is less significantly correlated with the murder rate than is the proportion that is black female and over sixty five.") Lott, however, emphasizes that the concentration of older black women may indicate that the county has a larger population of susceptible victims, and given the high multicollinearity between the many different race/age demographic variables for which Lott controls, we do not find this isolated result particularly troubling.

crime rate in the states that adopted nondiscretionary laws might have gone down anyway -- either because (1) the same concerns that caused states to pass the nondiscretionary laws also caused them to pursue other anti-crime measures or prompted citizens to stay home or take other precautions to avoid being victimized, or (2) the crime rates generally are mean reverting. Lott acknowledges each of these and has taken some reasonable steps to respond to them. For example, he addresses the first concern by seeking to ascertain whether other legal reforms were simultaneously passed in such states, and by controlling for them when possible.[47] Lott also runs alternative regressions controlling for both national and state time trends in an effort to address the issue of mean reversion in crime rates being attributed to shall issue laws, although this important issue could probably by probed further. All in all, the endogeneity of Lott's right-hand side variables seems to be less severe than in many other regression contexts.

Omitted variable bias, on the other hand, is a more troubling concern. Seven of the ten shall issue states that Lott studied – the exceptions being Florida, Georgia, and Mississippi -- had systematically lower murder rates than those states that chose not to pass the laws, as Figure 1 clearly shows. Even though Lott's fixed effects regression will correct for some of the unobserved differences between the two groups of states,[48] we worry in particular that the crack-induced crime jump in the mid-1980's in the states that did not pass shall issue laws may account for the apparent crime-reducing effects of the concealed handgun laws.[49] The omission of crack-related explanatory variables may have lead Lott's regression to under-predict the crime rates in states that failed to pass shall issue laws. The adoption of shall issue laws by six states in the 1980s may be associated with an unexpected crime increase in state's that did not pass the laws rather than an unexpected concealed-gun-induced crime decrease in states that did. Two testable conclusions flow from our crack hypothesis: (1) Lott's results may not be robust to changes in specification that more fully capture differences in states that adopt or shun shall issue laws;[50] and (2) Lott's results may become weaker as additional years of data are

---

[47]Lott runs regressions with variables that control for two types of additional laws: "(1) increased sentencing penalties for crimes involving the use of a gun and (2) waiting periods required before a citizen can obtain a permit for a gun." (p. 81.) He concludes that "one other finding is clear: laws involving sentence length and waiting periods do not alter my earlier findings with respect to nondiscretionary laws; that is, the earlier results for nondiscretionary laws cannot merely be reflecting the impact of other gun laws." (p. 83, see Table 4.11 on p 84-85).

[48] Lott also ran alternative regressions allowing for state specific time trends (p. 58). Given, however, Lott's claim that there is more variation within states than across states, it might have made more sense to interact the time trend with county population or to calculate separate time trends for large and small counties.

[49]This possibility was also seen by Zimring and Hawkins: "These differences between states may influence trends over time, as when crack cocaine impacted on very large eastern cities in the mid-1980's with a substantial impact on homicide levels. Do we want to compare Idaho, West Virginia, and Mississippi trends to Washington, D.C. and New York City trends over time?" (p. 51.)

[50] Indeed, Dezhbakhsh and Rubin have shown that splitting Lott's sample and running separate regressions – "one for counties in states with a concealed handgun law and the other for the remaining counties" -- leads to reductions in crime that are much smaller and "by no means negative across all crime categories." Hashem Dezhbakhsh & Paul H. Rubin, Lives Saved or Lives Lost?: The Effects of Concealed-

added (because crack-related crime seems to have been declining sharply, giving the non-adopting states a relatively better crime performance in the last five years).

In fact, Black and Nagin have shown that Lott's analysis of trends is not very robust to alternative specifications. When these authors first difference the left-hand side variable (to explain changes in, rather than levels of, crime) and restrict the sample to large counties, they find no statistically significant change in trends from the passage of shall issue laws for any of the major violent crime categories (homicides, rapes, assaults, and robberies).[51] Black and Nagin also show that merely adding a quadratic time trend variable to Lott's linear trend analysis destroys the statistical significance of Lott's basic conclusion (and even suggests that the adoption of right-to-carry laws caused a statistically significant *increase* in the number of assaults).[52] At the end of the day, we are concerned that Lott's estimated coefficients for adopting states are not as robust as he claims and may be seriously biased because of omitted explanatory variables. While virtually every respectable regression omits some potentially relevant variables, in future work, we hope to explore whether the crack hypothesis can provide a more compelling explanation for Lott's results than his proposed deterrence hypothesis.

IV. Conclusion

John Lott's work has given great prominence to the important substantive issue of ascertaining the effect on crime of the passage of laws enabling citizens to carry concealed handguns. His work has raised important methodological issues of how one best resolves that question and what standards of proof are needed before policy recommendations are warranted. Our guess is that many of Lott's strongest critics believe that his work has stepped over the line of academic research into policy advocacy, which may have dulled his attentiveness to disconfirming evidence.

But if Lott is wrong, his book provides a cautionary tale for other empirical researchers – since many less compelling studies have been relied upon uncritically. One might respond to this point by arguing that the conclusions of other studies that are quickly accepted have not departed so sharply from academic consensus, but we certainly don't want to fall into the position of saying that empirical research is only useful when it supports our current beliefs. Still, if intuitions are counter to an empirical finding and if enough anomalies are found in the statistical results, it is probably wise to push for more study than for more laws.

Furthermore, even if Lott were correct that the ten shall issue laws that he has studied reduced violent crime, the appropriate policy advice that follows is not an easy question. First, the effect may be true on average for the ten states in question, but further adoptions may bring in states for which the effect of shall issue laws is more pernicious. Certainly, as Figure 1(f) shows, the early adopters of shall issue laws are very different than the states that have never

---

Handgun Laws on Crime, 88 Am. Econ. Rev. 468 (Papers and Proceedings, 1998).

[51]Black and Nagin at 216.

[52]Id. Lott does note, though, that the inclusion of the quadratic term may not be appropriate if it improperly attributes a crime reduction caused by a shall issue law to the general pre-shall issue time trend.

adopted such laws, with the latter having on average roughly a 50 percent higher murder rate than the former over the course of two decades.[53]

Second, the issue of citizen fear needs to be explored in evaluating shall issue laws. This factor could cut either way: the populace at large may feel less safe knowing that more individuals walking in their midst are armed. This factor might be outweighed, though, by the fear reductions caused by reduced crime (if Lott is right) and by the decreased fear that the gun toters will have. In general, the evidence suggests that the type of modest reductions in crime that Lott posits are probably below the perceptual threshold of most citizens, and thus unable to affect the overall level of apprehension over crime. Thus, in the end, the fear issue pits the gains of the small percentage of gun toters against the losses to those in the broader population who fear the increased presence of guns.

Third, as Zimring and Hawkins emphasize, what sets America apart from the rest of the industrialized world is our degree of lethal gun violence, and therefore one has to think deeply about the full array of consequences stemming from marginal interventions into such a major problem area.[54] Even if Lott were right that marginal tinkering by adding more guns into the mix through the passage of shall issue laws does reduce some violent crime, one must ask whether this step serves to entrench a bad situation. Moving from 200 million guns to 210 million guns might have some benefits if the right folks get the last 10 million, but moving from 200 million down to zero may be far better. Thus, reaching the global and local maxima may require heading in different directions. Nonetheless, one cannot by Pollyanna-ish on this question. The bad situation may already be fully entrenched – some even argue, by constitutional edict.[55] In this event, marginal improvement may be all one can hope for.

But is Lott right? We have noted the issues of robustness that have been raised about his findings, which have generally involved showing that the alleged reduction in violent crime is statistically insignificant. Assuming that the entrenchment and fear issues discussed above were resolved in his direction, Lott could still live with a finding of small but not statistically significant crime reductions and advocate for shall issue laws on liberty grounds.

Therefore, the ultimate criticism of Lott will be that the model is too flawed to provide any information on the effect of the law. This is the strong conclusion of Zimring and Hawkins,

---

[53] The drop in crime in the last few years in the states that have never adopted a shall issue law has been so great that, conceivably, the crime rate in these non-shall states has now fallen to the level of the very low-crime early adopters of shall issue laws. With such a powerfully benign trend at work in these 20 states, the argument for changing policy sharply to allow more concealed handguns is weakened.

[54] Zimring and Hawkins, at 59.

[55] The argument stems from the Second Amendment, which reads: "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." It is much debated whether this amendment refers to the rights of well-regulated state militias or to some personal right to carry guns. If one really believed the latter position, then it would seem that shall issue laws themselves would be constitutionally mandated. Since no one makes that claim, it must be conceded that the right to bear arms is not an unqualified personal right. Indeed, no federal court has ever invalidated a law regulating the private ownership of guns on Second Amendment grounds.

who state that "as soon as we find flaws in the major conclusions, the regression analyses tell us nothing."[56] Clearly, there are concerns about model reliability. One of the strongest results to emerge from Lott's book is that shall issue laws, as he models them, lead to higher property crime. If you don't believe this, then you can't endorse any of Lott's findings. But, to believe that property crime rose you must also believe that the rate of robbery fell, because the only reason that more concealed handguns would cause property crime to go up is that some other money-generating activity became less available or less attractive. One would hardly expect that someone desiring to beat up an individual would instead decide to steal a car if the assaultive option were foreclosed. But since the robbery results are very weak, it is hard to tell a convincing story that would explain the alleged shift from violent crime to property crime that the Lott model attributes to shall issue laws.

At this point, Zimring and Hawkins want to throw out, not just Lott's study, but all econometric evaluations of public policy. They say that a good evaluation of shall issue legislation "will focus on the experience of one or two jurisdictions, not ten at a time."[57] While the detailed examination that a focused inquiry can provide is extremely valuable, we would not dispense with the large statistical study, but rather would supplement it. As this review has discussed, there are many powerful random influences on crime that impact on states at particular points in time. A study that focuses on one jurisdiction can provide valuable evidence of the transmission mechanisms of alleged impacts on crime, but it will have a hard time establishing that unusual positive or negative random influences are not operating at the moment in question.

A casual glance at the pattern of murders in Georgia (Figure 1(c)) and Mississippi (Figure 1(d)) following the adoption of their shall issue laws reveals that very different stories would likely emerge from a study based on either one of these shall issue adoptions. Perhaps that would be appropriate, or perhaps there were unusual influences operating at the time of adoption that obscure the true impact of the shall issue law. The strength of the statistical study is in enabling the researcher to see beyond the random influences, and Lott has started us on the journey and substantially advanced the process of data creation and analysis. Now that more years have transpired and 13 more states have passed these laws since Lott's data period ended, there are likely to be considerable benefits from further study that can explore the implications of the potentially serious omitted variable problem that the crack hypothesis poses and the difficult issue of heterogeneity among states enacting shall issue laws. This effort to determine the true effect of these laws will not only inform the substantive policy debate, it can also illuminate the circumstances under which we can rely on statistical studies of crime and other public policy matters. Such insight may be an even more important contribution, since it would have broad implications for scholarly inquiry and policy evaluation.

---

[56] Zimring and Hawkins, at 59.

[57] Id. at 58.

Figure 1(a)
Murder Rates: Florida and Maine and Date of Adoption of Shall-Issue Law

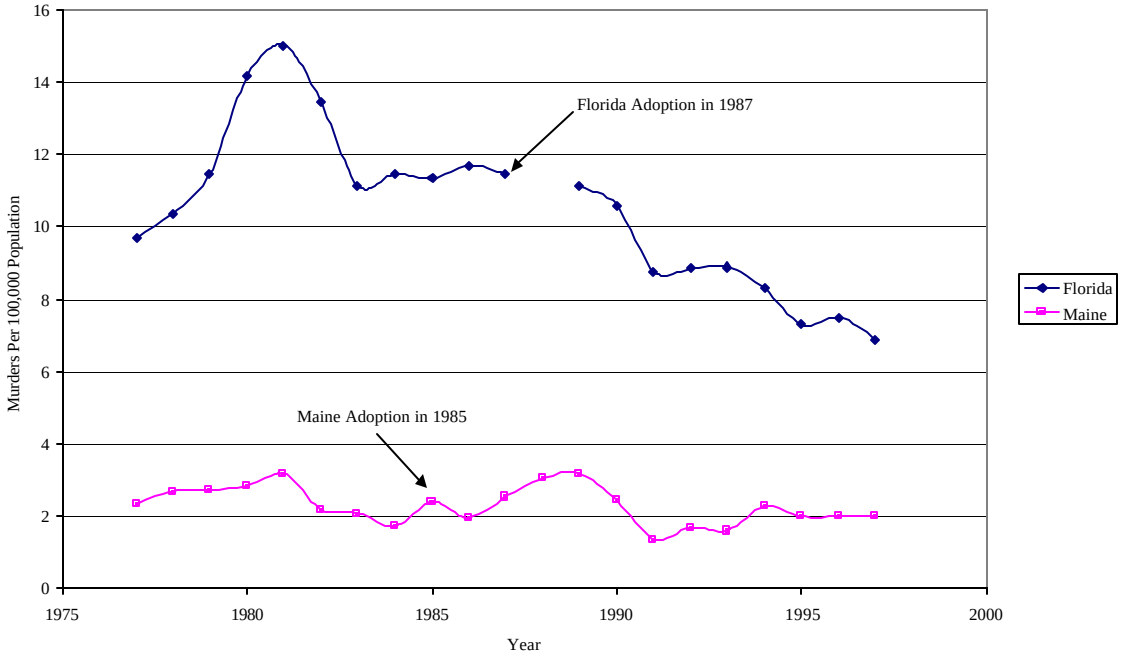

Figure 1(b)
Murder Rates: Virginia and West Virginia and Date of Adoption of Shall-Issue Law

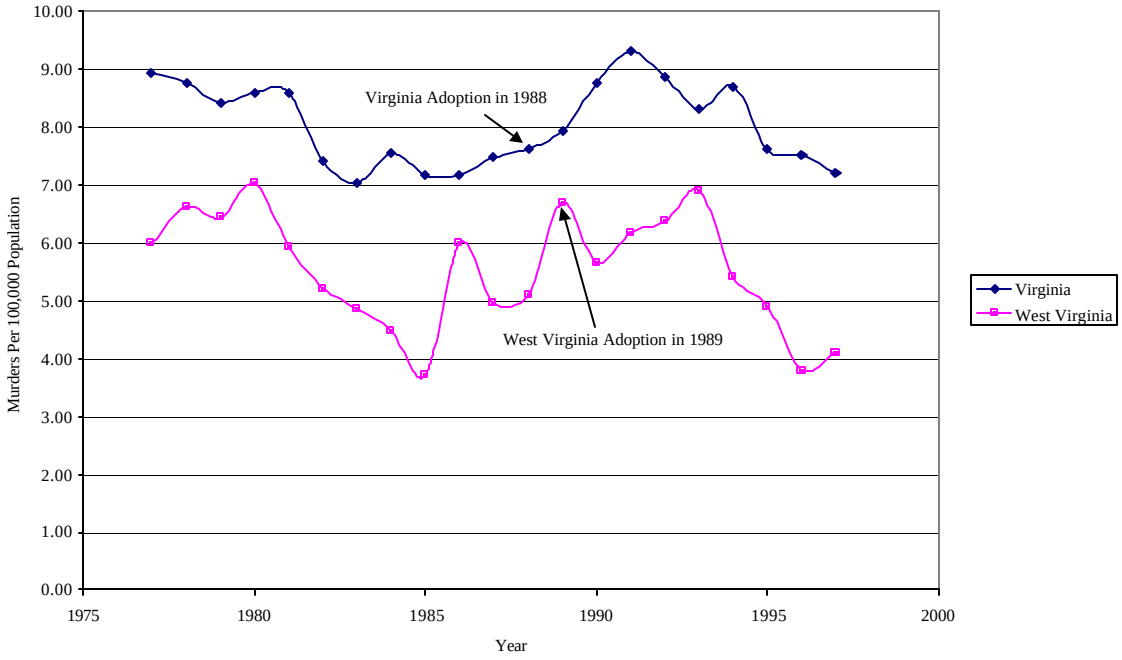

Figure 1(c)
Murder Rates: Georgia and Pennsylvania and Date of Adoption of Shall-Issue Law

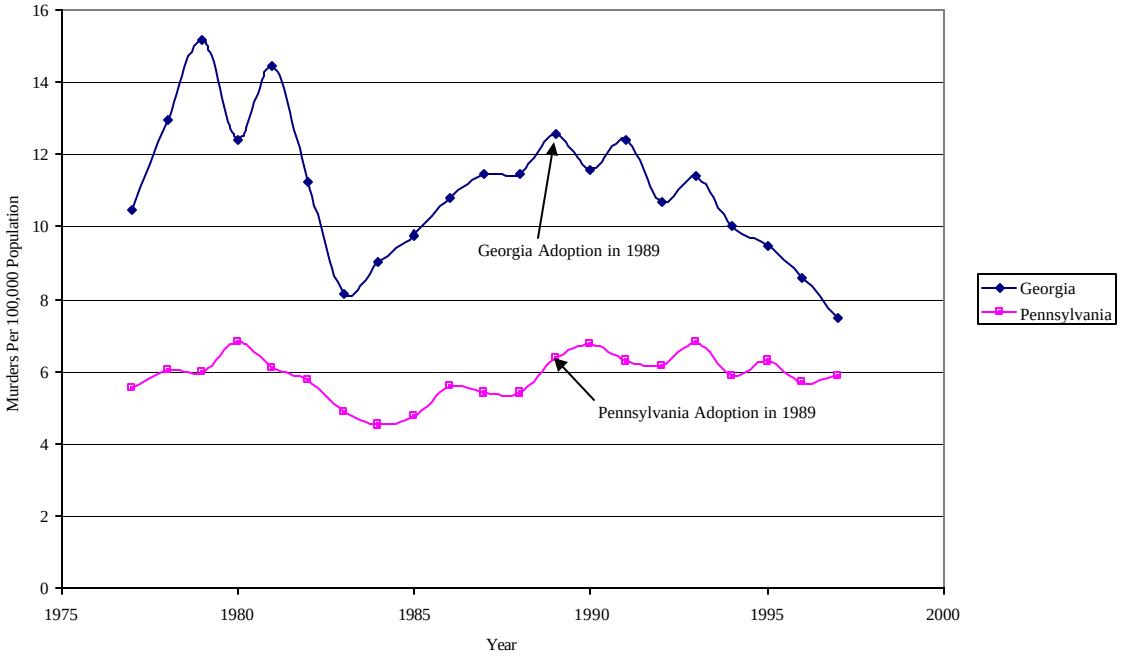

Figure 1(d)
Murder Rates: Mississippi and Idaho and Date of Adoption of Shall-Issue Law

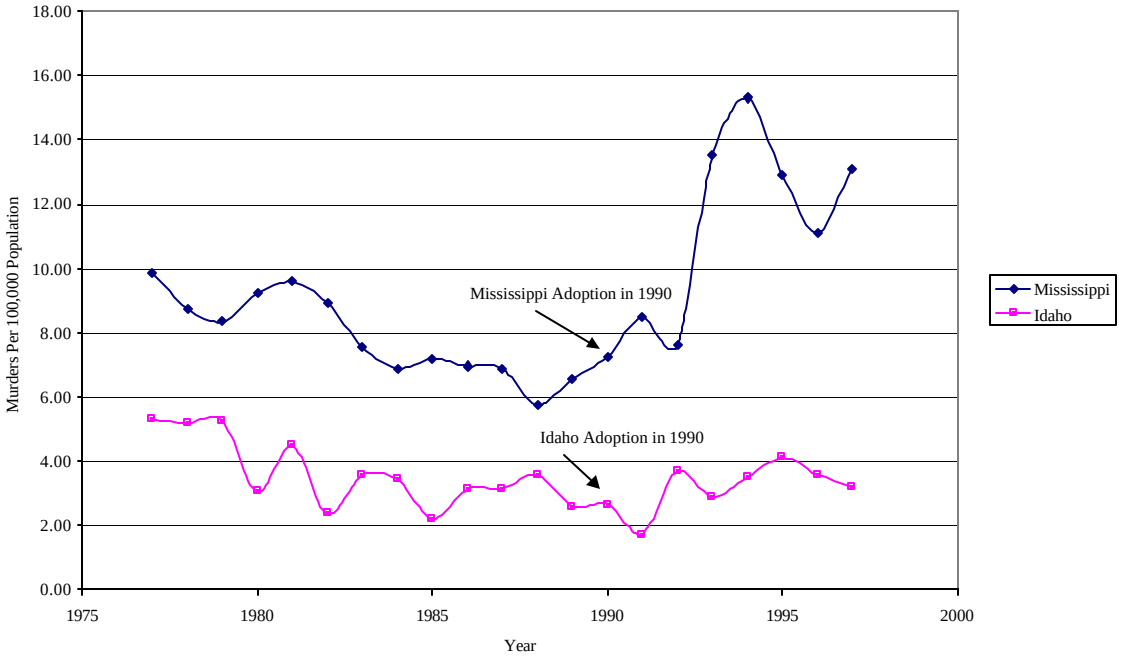

Figure 1(f)
Murder Rates for States by Passage of Shall Issue Law, Weighted by State Population

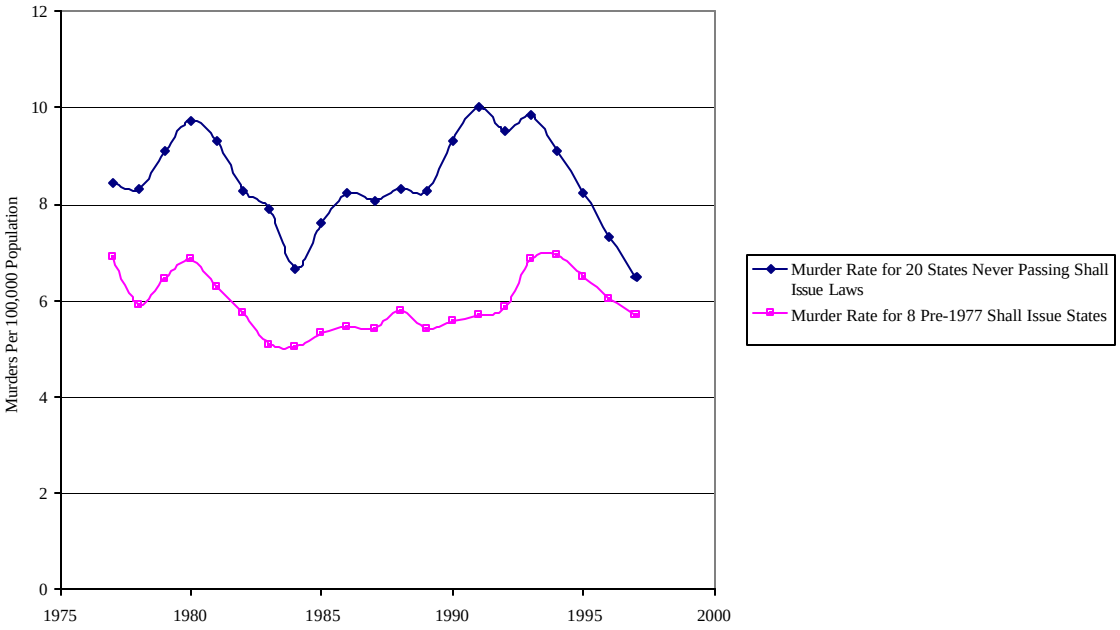

Figure 1(e)
Murder Rates: Oregon and Montana and Date of Adoption of Shall-Issue Law

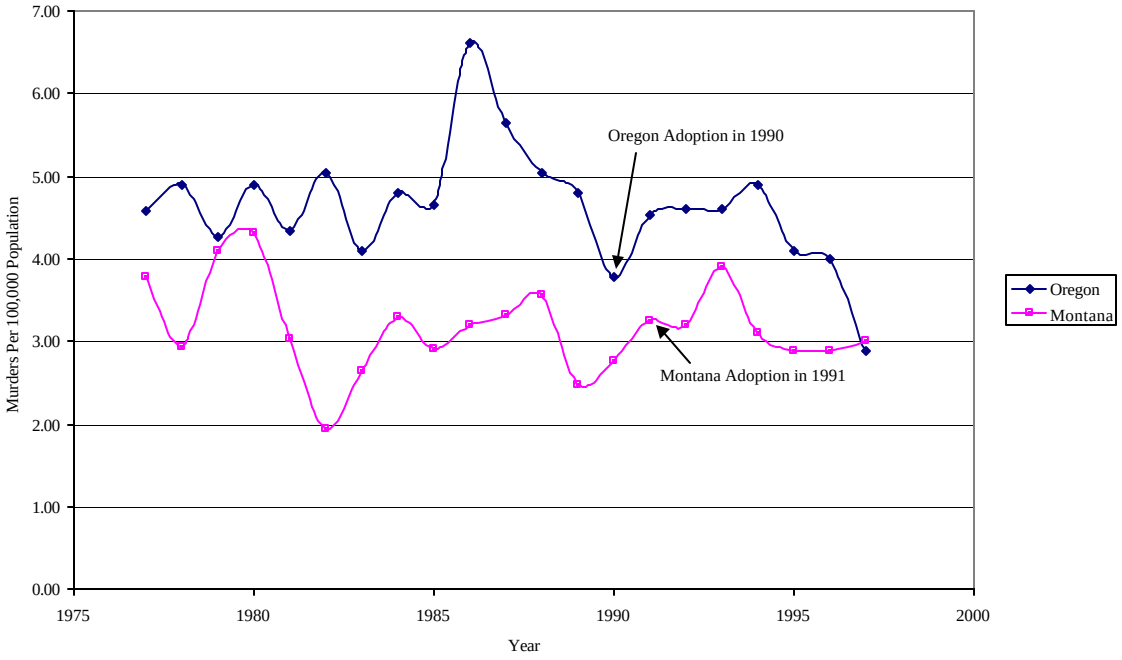

Table 1

The Effect on Crime of a State's Adoption of a Shall Issue Law

	Percent change in various crime rates for changes in explanatory variables--County Data Set, 1977-1992								
	Violent crime	Murder	Rape	Aggravated Assault	Robbery	Property crime	Auto Theft	Burglary	Larceny
1. Lott's Results (Table 4.1)									
Shall Issue Dummy	-4.9%	-7.7%	-5.3%	-7.0%	-2.2%	2.7%	7.1%	0.05%	3.3%
2. Reproduction of Lott*									
Shall Issue Dummy	-4.9%	-7.3%	-5.3%	-7.0%	-2.2%	2.7%	7.1%	0.05%	3.3%
t statistic	-5.0	-4.7	-4.3	-6.1	-1.7	3.7	6.3	0.1	3.8
3. Corrected Shall Issue Dummy**									
Shall Issue Dummy	-3.6%	-8.4%	-5.0%	-5.8%	-0.5%	4.6%	8.2%	1.5%	5.5%
t statistic	-3.5	-5.1	-3.9	-4.8	-0.4	6.0	6.8	1.8	5.9
adjusted t statistic***	-3.0	-3.3	-3.3	-3.5	-0.3	4.0	4.2	1.3	2.8
4. Including Incarceration Rate									
Shall Issue Dummy	-4.7%	-9.4%	-6.1%	-6.1%	-3.5%	2.8%	6.5%	-0.9%	4.0%
adjusted t statistic***	-3.7	-3.5	-3.9	-3.5	-1.7	2.3	3.3	-0.8	2.0
5. Including Incarceration Rate but Dropping Arrest Rate									
Shall Issue Dummy	-3.9%	-3.7%	-7.6%	-4.9%	-0.1%	3.3%	8.1%	0.5%	4.1%
adjusted t statistic***	-3.0	-1.2	-4.0	-2.7	-0.1	2.6	4.0	0.4	1.9
6. Dropping Maine and Florida									
Shall Issue Dummy	-1.4%	-2.7%	-1.1%	-2.4%	-1.8%	5.2%	5.2%	0.9%	6.2%
adjusted t statistic***	-1.0	-0.9	-0.7	-1.3	-1.0	4.5	2.8	0.8	3.6
7. Including Incarceration Rate and Dropping Maine and Florida									
Shall Issue Dummy	-2.3%	-3.5%	-2.1%	-2.6%	-4.5%	3.6%	3.8%	-1.3%	4.7%
adjusted t statistic***	-1.6	-1.2	-1.3	-1.4	-2.3	3.0	2.1	-1.0	2.8
8. Two Shall Issue Dummies, for States Passing Shall Issue Laws Before and After 1987									
Before 1987 Shall Issue Dummy	-7.9%	-20.9%	-15.1%	-12.7%	5.3%	4.1%	16.5%	4.3%	4.9%
adjusted t statistic***	-3.8	-5.0	-5.2	-4.7	1.4	1.9	4.2	2.2	1.1
After 1987 Shall Issue Dummy	-1.8%	-2.7%	-0.6%	-2.8%	-3.0%	4.8%	4.7%	0.3%	5.8%
adjusted t statistic***	-1.3	-0.1	-0.4	-1.6	-1.6	4.2	2.6	0.2	3.3

* We replicate Lott's figures perfectly, except for Murder.

** Correcting all the Shall Issue Dummies to begin in the first full year after the adoption of the law.

*** Using robust standard errors corrected for panel data.