

EXHIBIT 1

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Evaluating Gun Policy

*Effects on Crime
and Violence*

Jens Ludwig
Philip J. Cook
editors

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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.

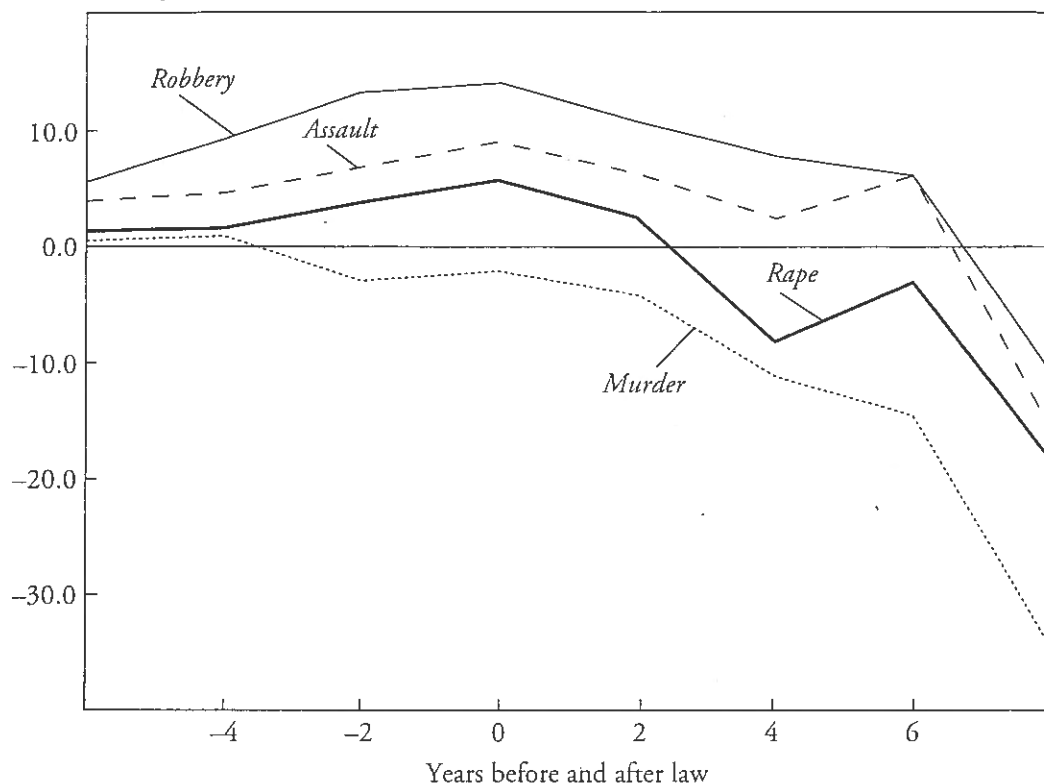
A second concern about the new empirical work is that although it is important to know whether the coefficient estimates in the postlaw years are positive or negative, it is also important to understand how they compare to the prelaw estimates. For example, if the prelaw coefficient estimate is 8.5, 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

Percent change



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."