

individual dummy variables for each county and controlling for individual time trends in each county in difference estimates were among the many ways that we controlled for initial legal differences across counties. Changes in other gun laws were also accounted for, such as the existence of waiting periods (which are synonymous with background checks), the length of waiting periods, and penalties for using guns during the commission of a crime.

"The authors identify Virginia as having adopted a new shall-issue law in 1988. . . Virginia did not enact a real shall-issue law until 1995." Virginia's 1988 law explicitly uses the term "shall-issue,"⁷ and, with the exception of 3 counties, the state has followed that rule. More important, as Ludwig was reminded in the public debates we had, Mustard and I examined^{2(pp12,19)} whether redefining Virginia as a non-shall-issue state would alter the results, and it did not.

Conclusion

The largest previous gun control study examined only 170 cities within a single

year. None of the previous studies even accounted for such obvious variables as arrest or conviction rates. My work examined crime rates over 16 years across 3054 US counties. It accounted for changes in arrests and convictions, detailed county demographics, drug prices, and general variables such as unemployment, income, and poverty. Strong evidence links the number of concealed handguns to lower violent crime rates and fewer deaths. Others have confirmed the insensitivity of our results.^{1,8} The additional 2 years of data examined in my book³ provided even stronger evidence than that reported here.

The Webster et al. discussion contains many inconsistencies. For example, they accept the conclusion of McDowall et al. that Florida's shall-issue law increased homicides but at the same time they accept Black and Nagin's claim that the drop in Florida's homicides after this law was so large that including that state unduly influenced the rest of the sample.

Unfortunately, because I have been allowed only a quarter the number of words allotted to Webster et al., it is impossible to correct most of their claims. These inaccuracies

are clearly refuted by the original study as well as by my book.^{2,3} □

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Webster and Colleagues Respond: No Proof That Shall-Issue Laws Reduce Violence

Daniel W. Webster, ScD, MPH, Jon S. Vernick, JD, MPH, and Jens Ludwig, PhD

Introduction

Lott's response¹ to our criticisms² of his and Mustard's study on the effects of "shall-issue" (right-to-carry) laws³ generally falls into one of two categories: (1) some attempt to fix the problem was made, or (2) the results are not sensitive to how the problem is addressed. While Lott and Mustard attempted to address some of the problems we identified, there is strong evidence that their efforts were inadequate. There is also considerable evidence that their findings are quite sensitive to how these problems are addressed. As we indicate below, Lott's own data support our view.

Crime Cycles and Unmeasured Factors Affecting Crime

As we previously discussed,² our concern about inadequate control for crime cycles is part of a larger concern that omitted variables biased Lott and Mustard's estimates

Daniel W. Webster and Jon S. Vernick are with the Center for Gun Policy and Research, School of Public Health, Johns Hopkins University, Baltimore, Md. Jens Ludwig is with the Graduate Public Policy Program, Georgetown University, Washington, DC.

Requests for reprints should be sent to Daniel W. Webster, ScD, MPH, Center for Gun Policy and Research, Johns Hopkins University, School of Public Health, 624 N Broadway, Baltimore, MD 21205.

of the effects of shall-issue laws. For example, they inadequately controlled for poverty and did not control for changes in local drug markets or police practices. Their attempts to correct this problem did not adequately control for nonlinear temporal patterns and the factors that influence such patterns.

Reanalysis of Lott and Mustard's data supports our skepticism of their models. Black and Nagin⁴ performed commonly used model specification tests⁵ on Lott and Mustard's primary regression models and found consistent evidence of systematic error in the prediction of crime trends. Lott and Mustard's models that use robbery and burglary rates to control for unmeasured confounders also failed this model specification test (D. Black, University of Kentucky, written communication, October 1997). When Black and Nagin included state-specific nonlinear trend parameters in the models, model fit improved, but the only significant effect of shall-issue laws was an increase in assaults. Ludwig addressed the omitted variable problem in his study of shall-issue law effects on adult homicides by using homicide victimization rates for juveniles (who are ineligible for concealed carry permits in all states) as a control. He found no deterrent effects of the laws.⁶

Figure 1 in Lott's rebuttal actually illustrates the results of Black and Nagin's model misspecification tests. The prelaw data points in Lott's Figure 1 (top) represent the prediction errors from Lott and Mustard's regressions and portray a clear upward trend.¹ If their models included no systematic prediction errors, these prelaw values should center around zero. This clearly is not the case. Thus, Lott provides evidence to refute his own model.

In both graphs in Lott's Figure 1, the dramatic dropoff occurs 4 to 7 years after the shall-issue laws were implemented. Lott fails to acknowledge that the data points for postlaw years 4 to 7 represent only 3 (Maine, Florida, and Virginia) of the 10 states represented by other points on the graph. The lack of consistency between prelaw and postlaw data points makes the comparisons very misleading.

Errors in Characterizing Concealed-Carry Laws

The data points for postlaw years 4 to 7 in Lott's Figure 1 (bottom) are even more problematic because Lott and Mustard make questionable decisions about how to classify Virginia and Maine. Virginia's 1988 law required concealed-carry appli-

cants to be of "good character" and to show a "demonstrated need." Legislation was enacted in 1995 to remove this discretionary language because many counties continued to deny carry permit requests after the 1988 legislation. Lott and Mustard's errors in the classification of Maine's concealed-carry law have been described elsewhere.²

Lott claims that redefining Virginia as a non-shall-issue state does not alter the results, but his data again prove him wrong. With Virginia included as a shall-issue state as of 1988, Lott and Mustard's model estimates that shall-issue laws reduce murders by 7%. When Virginia is not counted as a shall-issue state, the estimated reduction in murders is 58%,^{3(p 19)} an effect so implausible that it casts further doubt on the estimation methods.

Florida's Influence on the Results

Black and Nagin found that estimates of shall-issue law effects based on Lott and Mustard's primary model are extremely sensitive to the inclusion of Florida and that the models for Florida produce systematic prediction errors.⁴ Lott now claims that only 8 of 1000 regressions he reestimated for his forthcoming book were sensitive to the exclusion of Florida.⁷ Regardless of the sheer quantity of regressions Lott performs, he has yet to produce evidence that regression models that pass standard specification tests produce shall-issue deterrent effects, with or without Florida's inclusion.

No Consistent Shall-Issue Law Effects on Robbery

Robberies are more likely than other violent crimes to be committed by a stranger in a public place.⁸ Therefore, any deterrent effects from shall-issue laws should be strongest for robberies. But, as Lott and Mustard state, "robbery and burglary are the violent and property crime categories that are *least* [emphasis added] related to changes in concealed-carry laws."^{3(p34)} When they correct for their mischaracterization of Maine and Virginia's concealed-carry laws, their models show no law effects on robberies.^{3(p19)} Lott uses Figure 2 to support his claim that shall-issue laws have reduced robberies.¹ But the problems described for Figure 1 also apply to Figure 2.

Prevalence of Concealed-Carry Permits

Lott states that he interviewed law enforcement officials in all 50 states to obtain permit issuance information. But he was able to obtain sufficient historical data on permit issuance for only 2 states. Evidence from these 2 states that more carry permits lead to less violence is weak: only 1 statistically significant deterrent effect from increasing carry permits in 8 regressions on violent crimes.

Conclusion

We have highlighted only some of the instances of unreliable findings from Lott and Mustard's study. Lott has also reported that shall-issue laws reduced multiple-victim shootings, such as the recent Jonesboro, Ark, incident, by 84%. Readers should conclude for themselves whether the plausibility of this finding is sufficient to advocate, as Lott does, encouraging teachers to bring guns into schools to prevent mass shootings.⁹

Space limitations prevent a complete refutation of all of Lott's claims. But a review of the available evidence does not support Lott's assertion that shall-issue laws reduce violent crime. Subsequent research correcting for several of the problems with Lott and Mustard's study shows no evidence that these laws reduce violent crime. □

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