DO RIGHT-TO-CARRY LAWS DETER VIOLENT CRIME?

*DAN A. BLACK and DANIEL S. NAGIN**

Abstract

John R. Lott and David B. Mustard conclude that right-to-carry laws deter violent crime. Our reanalysis of Lott and Mustard's data provides no basis for drawing confident conclusions about the impact of right-to-carry laws on violent crime. We document that their results are highly sensitive to small changes in their model and sample. Without Florida in the sample, there is no detectable impact of right-to-carry laws on the rate of murder and rape, the two crimes that by the calculations of Lott and Mustard account for 80 percent of the social benefit of right-to-carry laws. A more general model based on year-to-year differences yields no evidence of significant impact for any type of violent crime. As a result, inference based on the Lott and Mustard model is inappropriate, and their results cannot be used responsibly to formulate public policy.

I. Introduction

By 1992, 18 states had enacted laws creating a presumptive right to carry a concealed handgun.[1] Such laws require that an adult applicant be granted a concealed-weapon permit unless the individual is a felon or has a history of serious mental illness. In a highly publicized article, John R. Lott and David B. Mustard conclude that right-to-carry laws deter violent crimes, increase crimes of stealth, and have no effect on the number of accidental deaths. They argue that rational criminals substitute away from violent crimes and instead engage in property crimes, such as burglary and larceny,

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[1] John R. Lott and David B. Mustard, *Crime, Deterrence, and Right-to-Carry Concealed Handguns,* 26 J Leg Stud 1, 12 (1997).

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in which there is less chance of confrontation with an armed victim. Lott and Mustard estimate that with nationwide adoption of right-to-carry (RTC) laws in 1992 there would have been 1,414 fewer homicides; 4,177 fewer rapes; and over 60,000 fewer aggravated assaults.[2] By their calculation, the social benefit of avoiding these violent crimes is $6.6 billion, while the social cost of increased nonviolent monetary crimes is only $417 million. The findings of Lott and Mustard have generated considerable controversy because of their manifest implications for the regulation of access to firearms.[3]

In this article, we reanalyze Lott and Mustard's data, which they graciously shared with us. Our purpose is to test the robustness of their results by varying the model specification and the sample used in estimation. In Section II, we describe our specification and sample. In Section III, we demonstrate the sensitivity of their estimates to minor changes in the model or sample. In Section IV, we offer concluding comments.

II. Specification

Lott and Mustard analyze county-level crime data for the years 1977 to 1992. These data were assembled from the Federal Bureau of Investigation's Uniform Crime Reports and from various Census Bureau reports. The crime data include reports on the number of homicides, rapes, aggravated assaults, robberies, burglaries, larcenies, and automobile thefts as well as data on the number of arrests for each of these crimes by county and year. In this article, we focus on the four categories of violent crime: homicides, rapes, aggravated assaults, and robberies.[4]

Lott and Mustard estimate the impact of RTC laws with the following model:

$$y_{it} = \alpha year_t + \beta x_{it} + \delta Shall_{it} + \phi_i + \epsilon_{it}, \tag{1}$$

where y_{it} is the logarithm of the number of crimes per 100,000 people in county i in year t, $year_t$ is a vector of year dummies, x_{it} is a vector of demographic and economic controls, ϕ_i is a county fixed effect, and ϵ_{it} is an error

[2] Id at 29.

[3] Albert W. Alschuler, *Two Guns, Four Guns, Six Guns, More Guns: Does Arming the Public Reduce Crime,* 31 Valp U L Rev 1–9 (1997). Marion P. Hammer, *The Sheriffs' Revenge,* Economist (December 7–13, 1996), at 26–27. Marion P. Hammer, *The President's Column,* Am Rifleman (October 1996), at 10. Jens Ludwig, *The Effects of Concealed-Handgun Laws Revisited? A Critique of John Lott and David Mustard* (unpublished manuscript, Georgetown Univ, October 1996).

[4] In addition, Lott and Mustard use the aggregated category of violent crime. They note, however, that this broad category is somewhat problematic in that all crimes are given the same weight (for example, one murder equals one aggravated assault). See Lott and Mustard, at 8 (cited at note 1). We agree and limit our attention to the narrower crime categories.

term with the standard properties. The variable $Shall_{it}$ is a dichotomous variable equal to one when the county has an RTC law in effect and equal to zero when there is no such law in effect; δ is the parameter of interest. Lott and Mustard use county populations as weights in each regression.

The first step in our analysis is to reproduce the estimates of Lott and Mustard. The focus is on replication of the model specification reported in Lott and Mustard's table 3,[5] which forms the basis for the crimes-averted estimates cited above.[6] Our results, which we report in the first row of Table 1, are virtually identical to those reported by Lott and Mustard.

To control for variation in the probability of apprehension, the Lott and Mustard model specification includes the arrest ratio, which is the number of arrests per reported crime. Our replication analysis shows that the inclusion of this variable in the model specification materially affects the size and composition of the estimation data set. Specifically, division by zero forces all counties with no reported crimes of a particular type in a given year to be dropped from the sample for that year. Lott and Mustard's sample contains all counties, regardless of population, and this problem of dropping counties with no reported crimes is particularly severe in small population counties with few crimes. The frequencies of missing data are 46.6 percent for homicide, 30.5 percent for rape, 12.2 percent for aggravated assault, and 29.5 percent for robbery. Thus, the Lott and Mustard model excludes observations based on the realization of the dependent variable, potentially creating a substantial selection bias.

Our strategy for finessing the missing data problem is to analyze only counties maintaining populations of at least 100,000 during the period 1977 to 1992. It is important to note that when we eliminate observations from small counties we are selecting a sample based on the realization of an exogenous variable (population), not on the realization of the dependent variable. This strategy limits the sample to 393 counties, 86 of which have enacted RTC laws. Among the 10 states that adopt RTC laws during this period, the 86 counties are distributed as follows: 28 counties in Pennsylvania, 20 counties in Florida, 13 counties in Virginia, 9 counties in Georgia, 6 counties in Oregon, 4 counties in Maine, 3 counties in Mississippi, and 1 each in West Virginia, Idaho, and Montana. Compared with the sample composed of all counties, the missing data rate in the large-county sample

[5] Lott and Mustard, at 20–23 (cited at note 1).

[6] Lott and Mustard, at 44 (cited at note 1). We also reproduced Lott and Mustard's two-stage least squares estimates (2SLS) reported in panel A of Lott and Mustard's table 11. The 2SLS estimates imply that RTC laws reduce homicides by 67 percent and rapes by 65 percent. Impacts of this magnitude should be trivial to spot in the data. Because we detect no such impacts, we conclude that the 2SLS estimates are not credible and focus our attention on the ordinary least squares estimates.

TABLE 1

State-Specific Impact of Right-to-Carry Laws, Large-County Sample, 1977–92

	Homicides	Rapes	Assaults	Robberies
Lott and Mustard sample and specification	**−.071***	**−.052***	**−.072***	−.022
	(2.94)	(3.53)	(4.53)	(1.19)
	[26,458]	[33,865]	[43,445]	[34,949]
Large-county, Lott and Mustard specification	**−.090***	−.035	**−.068***	−.029
	(2.78)	(1.81)	(3.06)	(1.13)
	[6,009]	[6,036]	[6,109]	[6,173]
State-specific impacts:				
Maine	.072	.036	**−.515***	**−.333***
	(.51)	(.58)	(7.26)	(4.12)
Florida	**−.277***	**−.170***	−.066	.073
	(5.01)	(4.11)	(1.75)	(1.37)
Virginia	.039	−.076	**−.161***	−.121
	(.47)	(1.82)	(3.39)	(1.91)
Georgia	−.052	.045	−.041	.077
	(.70)	(.74)	(.61)	(.97)
Pennsylvania	−.059	.044	**.068***	−.053
	(1.13)	(1.54)	(2.19)	(1.57)
West Virginia	**.718***	**−.285***	−.029	.094
	(4.23)	(2.77)	(.17)	(.85)
Idaho	−.210	−.097	**−.306**	**−.643***
	(.73)	(1.25)	(4.25)	(5.46)
Mississippi	.054	**.320***	**−.450***	.103
	(.38)	(4.08)	(4.35)	(.81)
Oregon	−.089	.035	**−.172***	−.035
	(1.01)	(.68)	(2.27)	(.58)
Montana	−.367	**−.972***	**−.707***	−.139
	(1.55)	(2.35)	(2.16)	(.41)
p-value for the *F*-test of the Lott and Mustard model	0.0000	0.0000	0.0000	0.0000
Summary of coefficient estimates:				
No. positive (no. significant at the 5% level for a two-tailed test)	4 (1)	5 (1)	1 (1)	4 (0)
No. negative (no. significant at the 5% level for a two-tailed test)	6 (1)	5 (3)	9 (6)	6 (2)
N	6,009	6,036	6,109	6,173
Large-county, Lott and Mustard specification without Florida	−.013	.012	**−.063***	−.046
	(.37)	(.59)	(2.72)	(1.90)
	[5,730]	[5,756]	[5,829]	[5,893]

Note.—Specifications of the equations are the same as those in Lott and Mustard's table 3, in their *Crime, Deterrence, and Right-to-Carry Concealed Handguns,* 26 J Leg Stud 1 (1997), except where noted. Absolute values of *t*-statistics, given in parentheses, are calculated using White standard errors. Regressions use the county's mean population between 1977 and 1992 as weights. Sample sizes are given in brackets.

* Coefficient estimates in bold are significant at the 5% level for a two-tailed test.

is low: 3.82 percent for homicide, 1.08 percent for rape, 1.18 percent for assault, and 1.09 percent for robberies. Moreover, Lott and Mustard argue that the impacts of RTC laws are greater in more populous areas, arguing that ''larger counties have a much greater response . . . to changes in the [RTC] laws.''[7]

Therefore, in what follows, we generally limit our analysis to the large-county sample using the same specification as Lott and Mustard's table 3. In 1990, the large-county sample contains about 69 percent of the population contained in Lott and Mustard's samples, but it is more important that the large-county sample contains at least 80 percent of the crimes contained in Lott and Mustard's samples. In Table 1, we report the results for this sample. The estimates using this large-county sample are reasonably similar to those reported in Lott and Mustard's table 3, although the homicide effect is somewhat larger and the rape effect somewhat smaller in magnitude.

III. Sensitivity of the Right-to-Carry Estimates

The Lott and Mustard model makes two restrictive identification assumptions. First, the model assumes the impact is the same across all 10 states that passed RTC laws in the period from 1977 to 1992. We refer to this as the geographic aggregation assumption. Second, the model assumes that RTC laws have an impact on crime rates that is constant over time, which we label the intertemporal aggregation assumption.

To relax the geographic aggregation assumption, we estimate a modest extension of the Lott and Mustard model:

$$y_{it} = \alpha year_t + \beta x_{it} + \sum_{i=1}^{10} \delta_j Shall_{jit} + \phi_i + \epsilon_{it}, \tag{2}$$

where $Shall_{jit}$ is equal to one when the jth state has enacted a RTC law and is zero otherwise. The parameter δ_j is simply the state-specific estimate of the impact of the RTC laws on crime. If the Lott and Mustard model is properly specified, we should be able to restrict δ_j's to be the same for each of the 10 states, thus reducing equation (2) to the Lott and Mustard model given in equation (1).

In Table 1, we report the estimates of state-specific impacts, or δ_j, for each state that adopts an RTC law during the observation period. For every crime, we strongly reject the Lott and Mustard model's assumption of a

[7] Lott and Mustard, at 31 (cited at note 1).

uniform impact across states. In three of the four crime equations, there are simultaneously significant positive and negative coefficients.[8]

The estimates are disparate. Murders decline in Florida but increase in West Virginia. Assaults fall in Maine but increase in Pennsylvania. Nor are the estimates consistent within states. Murders increase, but rapes decrease in West Virginia. Moreover, the magnitudes of the estimates are often implausibly large. The parameter estimates imply that RTC laws increased murders 105 percent in West Virginia but reduced aggravated assaults by 67 percent in Maine.[9] While one could ascribe the effects to the RTC laws themselves, we doubt that any model of criminal behavior could account for the variation we observe in the signs and magnitudes of these parameters. Widely varying estimates such as these are classic evidence that, even beyond the assumption of homogeneous impacts across states, the model is misspecified.

The large variations in state-specific estimates of RTC impacts cause concern that the Lott and Mustard results could be driven by a single state for which their model does a particularly poor job of fitting the data. As it turns out, one such state is Florida. With the Mariel boat lift of 1980 and South Florida's thriving drug trade, Florida's crime rates are quite volatile. Further, 4 years after its 1987 passage of the RTC law, Florida passed several other gun-related measures, including background checks of handgun buyers and a waiting period for handgun purchases.[10] We reestimated the model given in equation (1) without any observations from Florida. We report the results in the last row of Table 1. While the estimated impact of RTC laws on assaults is relatively unaffected, without Florida there is no evidence of any impact on homicides or rapes.[11] Thus, for these two crimes—the two crimes that account for 80 percent of the total social benefit of RTC laws that Lott and Mustard quantify in their table 5—the evidence of a deterrent effect vanishes with the removal of a single state from the analysis.

As another check of the sensitivity of the estimates to model specification, we relax the intertemporal aggregation assumption of the Lott and Mustard model:

[8] Using Lott and Mustard's sample from their table 3, we also reject the hypothesis of a uniform impact across states for each crime.

[9] To calculate these percentages, we use the approximation $100 \times [\exp(\delta) - 1]$.

[10] David McDowall, Colin Loftin, and Brian Wiersema, *Easing Concealed Firearms Laws: Effects on Homicide in Three States,* 86 J Crim L & Criminol 193 (1995).

[11] Nor is this result a function of our use of the large-county sample. Without Florida in the sample, the estimation of Lott and Mustard's model, which is given by equation (1), for all counties provides no evidence of an impact of RTC laws on homicide and rape.

$$\Delta y_{it} = \alpha year_t + \beta \Delta x_{it} + \sum_{j=-5}^{-1} \delta_j Shall_{jit} + \sum_{j=1}^{5} \delta_j Shall_{jit} + \Delta \epsilon_{it}, \quad (3)$$

where the Δ's indicate that variables are first differenced. The advantage of first-differencing is that it eliminates the county fixed effect. The variable $Shall_{jit}$ is a dummy variable indicating the number of years since or until enactment of the RTC law. This specification replaces Lott and Mustard's $Shall_{it}$ variable that denotes the mere existence of an RTC law in county i at time t with a series of dummy variables capturing the temporal relationship between year t and the year of enactment. For positive values of j, the variable indicates the number of years since enactment of the law.[12] For example, consider a county located in Maine, which passed its RTC law in 1985. In this case, in 1987, the $Shall_{jit}$ is equal to one for $j = 3$, with the other nine temporal dummy variables equal to zero.

Table 2 reports the results for this model. For positive values of j, the coefficients, δ_j, estimate the change in the annual impact of the law in the jth year of the law being in effect. For negative values of j, the coefficient estimates estimate changes in the crime rate in the jth year prior to enactment, conceivably in anticipation of the law becoming effective. Intuitively, if RTC laws deter crime, the RTC laws' coefficients after enactment should be smaller (or more negative) than those preceding enactment. Because the δ_j's estimate annual change, the total impact equals the sum of the annual changes. Figure 1 depicts these cumulative impacts. Specifically, it graphs the deviations from the national trend implied by the estimates in Table 2. For homicide, rape, and assault, crime rates were declining in these counties prior to the adoption of an RTC law, and the decline continued after the passage of the RTC law. For robbery, crime rates were increasing prior to adoption. The increase continued after adoption, albeit at a somewhat slower rate, but the difference is not statistically significant. In summary, Figure 1 indicates no apparent shift in the time series after passage of the RTC laws for any type of crime.

To test formally for such shifts, we test whether the sum of the coefficients prior to adoption is significantly different from the sum of the coefficients following adoption. This test provides a more general test of the impact of RTC laws because Lott and Mustard's model assumes that the coefficients before adoption are zero and all but the first coefficient after

[12] To limit the number of parameters to be estimated, $Shall_{5it}$ is equal to one whenever the law was passed 5 or more years from time t. For negative values of j, $Shall_{jit}$ represents the number of years until enactment of the law.

TABLE 2

DYNAMIC ANALYSIS OF RIGHT-TO-CARRY LAWS, LARGE-COUNTY SAMPLE, 1977–92

	Homicides	Rapes	Assaults	Robberies
A. Large counties, difference specification:				
Years before adoption of right-to-carry laws:				
5	−.015	**−.057***	−.024	.023
	(.33)	(2.13)	(.91)	(.71)
4	−.039	−.073	**−.067***	−.002
	(.64)	(1.85)	(2.09)	(.06)
3	.018	−.067	**−.089***	.062
	(.27)	(1.84)	(2.22)	(1.42)
2	−.094	**−.095***	−.047	.039
	(1.31)	(2.31)	(1.21)	(.91)
1	−.022	−.053	−.012	**.096***
	(.29)	(1.51)	(.34)	(2.57)
Years after adoption of right-to-carry laws:				
1	−.089	−.053	**−.061***	.048
	(1.35)	(1.60)	(2.01)	(1.33)
2	.076	−.038	.011	−.032
	(.84)	(.95)	(.25)	(.70)
3	−.088	**−.087***	−.024	.029
	(−1.19)	(2.11)	(.67)	(.67)
4	.080	−.066	−.54	.040
	(1.01)	(1.80)	(1.35)	(.98)
5	−.133	**−.123***	**−.101***	−.018
	(1.66)	(2.75)	(2.77)	(.39)
Sum of coefficients prior to adoption	−.152	**−.346***	−.239	.219
	[.5277]	[.0160]	[.0740]	[.1780]
Sum of coefficients after adoption	−.154	**−.367***	−.228	.067
	[.5403]	[.0089]	[.0956]	[.6895]
p-value of the test that the sum of coefficients years prior to and after adoption are equal	.9834	.7450	.9009	.1507
N	5,449	5,587	5,664	5,725
B. Quadratic state-specific time-trend specification:				
Right-to-carry coefficient, δ	**−.090***	−.035	**−.068***	−.029
	(2.78)	(1.81)	(3.06)	(1.13)
Right-to-carry coefficient, δ, with state-specific, quadratic time trend	.038	.051	**.084***	−.068
	(.60)	(1.48)	**(2.06)***	(1.33)
N	6,009	6,036	6,109	6,173

NOTE.—Specifications of the equations are the same as those in Lott and Mustard's table 3, in their *Crime, Deterrence, and Right-to-Carry Concealed Handguns,* 26 J Leg Stud 1 (1977), except for the addition of the time trends in Panel B. Absolute values of *t*-statistics, given in parentheses, are calculated using White standard errors. Regressions use the county's mean population between 1977 and 1992 as weights. *p*-values are given in brackets.

* Coefficient estimates in bold are significant at the 5% level for a two-tailed test.

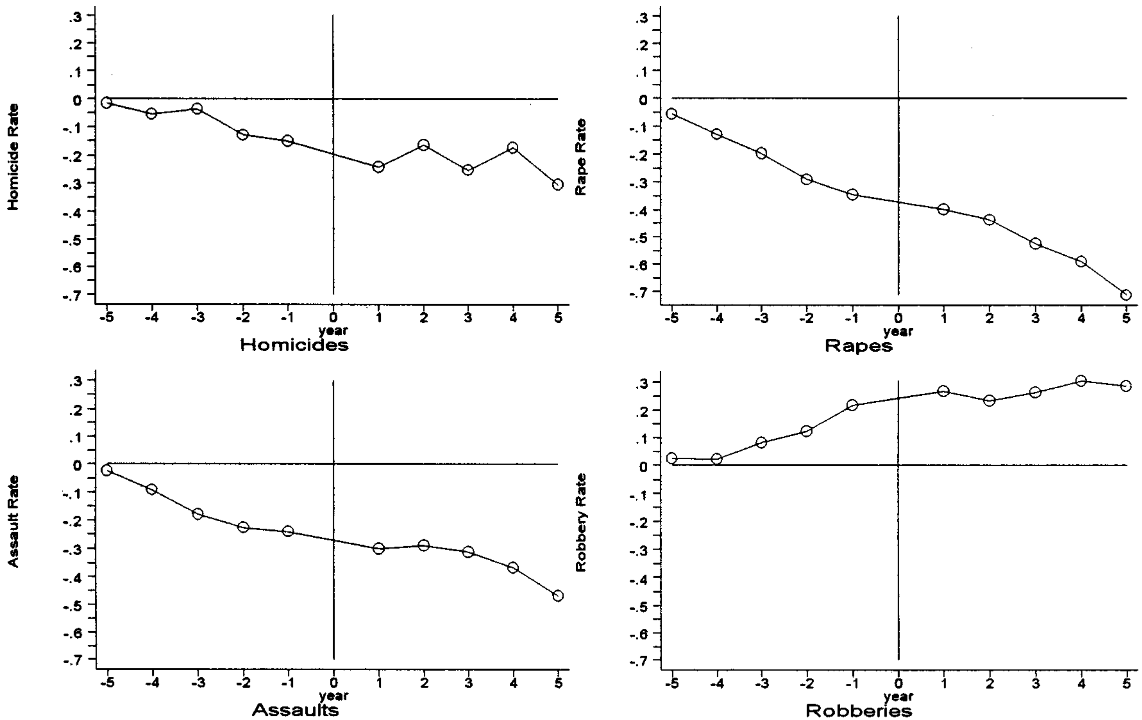

FIGURE 1.—Time-varying impacts of right-to-carry laws on crime rates, large-county sample

adoption are zero, whereas this model does not.[13] Table 2 reports the results of this more general testing method. We find no statistically significant evidence that RTC laws have an impact on any of the crime rates.

Why is the Lott and Mustard model so sensitive to model specification? To gain insight into this issue, we applied a number of specification tests suggested by James J. Heckman and V. Joseph Hotz.[14] The results of the tests are available from us on request. The specifics of the findings, however, are less important than the overall conclusion that is implied. The results show that commonly the model either overestimates or underestimates the crime rate of adopting states in the years prior to adoption. For instance, we have seen that the models are sensitive to the inclusion of Florida. The tests show that for Florida the model consistently underestimates the level of homicides and rapes prior to adoption. In contrast, the model tends to overestimate murders in Georgia. Thus, there must exist systematic factors, not yet modeled, that account for these differences.

The results suggest that the Lott and Mustard model, which includes only a single national trend, does not adequately capture local time trends in crime rates. To test for this possibility, we generalized the Lott and Mustard model to include state-specific trends in an effort to control for these unobserved factors. For instance, for the quadratic time-trend model, we estimate

$$y_{it} = \alpha year_t + \sum_{i=2}^{50} (\kappa_{1j} D_j T + \kappa_{2j} D_j T^2) + \beta x_{it} + \delta Shall_{it} + \phi_i + \epsilon_{it}, \quad (4)$$

where T is the time variable, D_j is a dummy variable indicating that the county is in state j, and κ_{1j}'s and κ_{2j}'s are the time-trend parameters for each state. In Table 2, we report the results for models with a quadratic time trend.[15] The only significant impact estimate is for assaults, and its sign is positive, not negative.

IV. Conclusions

Our reanalysis of Lott and Mustard's data provides no basis for drawing confident conclusions about the impact of RTC laws on violent crimes. We document that their estimated impacts of RTC laws are highly sensitive to

[13] While these estimates improve on Lott and Mustard's estimates, we feel that it is ill-advised to use these for policy recommendations. Our purpose in reporting them is only to demonstrate the deficiencies in the Lott and Mustard estimates.

[14] James J. Heckman and V. Joseph Hotz, *Choosing among Alternative Nonexperimental Methods for Estimating the Impact of Social Programs: The Case of Manpower Training*, 84 J Am Stat Assoc 862–74 (1989).

[15] A linear time trend proved inadequate; the model with linear state-specific time trends repeatedly failed the Heckman and Hotz specification test.

small changes in the sample and the model: for instance, the seemingly salutary impacts of RTC laws on murder and rape depend entirely on the data for Florida. Without Florida in the sample, there is no detectable impact for these two crimes that, by the calculations of Lott and Mustard, account for 80 percent of the social benefit of RTC laws. State-specific impact estimates are commonly implausible and vary widely—some are negative, but others are positive. Finally, a more general model based on year-to-year differences yields no evidence of significant impact. As a result, inference that is based on the Lott and Mustard models is inappropriate, and their results cannot be used responsibly to formulate public policy.