TESTING FOR THE EFFECTS OF CONCEALED WEAPONS LAWS: SPECIFICATION ERRORS AND ROBUSTNESS*

CARLISLE E. MOODY
College of William and Mary

Abstract

In 1997, John Lott and David Mustard published an important paper in which they found that right-to-carry concealed weapons laws reduce violent crime. Although Lott and Mustard appear to do all possible variations of the analysis, a closer reading reveals that the study might suffer from several possibly important errors. I reestimate the model and check for incorrect functional form, omitted variables, and possible second-order bias in the *t*-ratios. Lott and Mustard's basic conclusions are generally robust with respect to these potential econometric problems. Overall, right-to-carry concealed weapons laws tend to reduce violent crime. The effect on property crime is more uncertain. I find evidence that these laws also reduce burglary.

I. Introduction

In their landmark paper, "Crime, Deterrence, and Right-to-Carry Concealed Handguns," John Lott and David Mustard[1] found that allowing citizens to carry concealed weapons deters violent crime. The theory is straightforward. In those states that have "shall-issue" laws, concealed weapons permits are granted unless there is a good reason to deny them. In the remaining states, concealed weapons permits are issued only if the applicant can show that he or she needs to carry such a weapon. As a result, in shall-issue states, more ordinary citizens carry concealed handguns. Potential criminals are deterred because the probability of effective resistance is higher. Unarmed citizens in those states are free riders who benefit at no cost from the actions of the ones who actually carry weapons. Consequently, it might be expected that violent crime rates will decline in those states that pass shall-issue laws

* Paper presented at the conference Guns, Crime, and Safety, December 10–11, 1999, at the American Enterprise Institute. The conference was sponsored by the American Enterprise Institute and the Center for Studies in Law at the Yale School of Law. I would like to thank Thomas B. Marvell, John R. Lott, and an anonymous referee for suggesting substantial improvements. I am solely responsible for all remaining deficiencies.

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

[*Journal of Law and Economics,* vol. XLIV (October 2001)]

relative to those states that do not. At the same time, the rate of property crime might rise if criminals substitute the now relatively lower cost property crime for violent crime. These conclusions are apparently counterintuitive to gun control advocates, who generally believe that more guns lead to more violent crime.

The actual impact of shall-issue laws is an empirical question. Lott and Mustard[2] and Lott[3] examine this issue using a data set consisting of county-level crime, economic, and demographic information for over 3,000 counties for the years 1977–92. The data set contains over 50,000 observations on almost 200 variables.

With these data, Lott and Mustard perform what appear to be innumerable regressions of various crime rates on the shall-issue dummy variable, arrest rates, and a host of control variables. The fundamental results are presented in table 3 in Lott and Mustard.[4] The dependent variables are the natural log of nine major crime categories (violent crime, murder, rape, robbery, assault, property crime, burglary, larceny, and auto theft). The independent variables are the shall-issue dummy; the arrest rate for the particular crime in question; population; population density; real per capita income in the form of personal income, unemployment insurance, income maintenance, and retirement payments; 36 gender, race, and age variables (male, female, black, white, other, ages 10–19, 20–29, 30–39, 40–49, 50–64, and 65 and over); county dummies; and year dummies. They replicate the results from this basic set of regressions using a wide variety of alternative models. The results are apparently extremely robust.

It might appear that nearly all possible regressions have been done and that no other conclusions are possible. I consider even more possibilities below and find that Lott and Mustard's conclusions, at least with respect to violent crime, are in fact very robust.

II. Possible Specification Error: Functional Form

One of the fundamental theorems of econometrics is that if one or more of the variables on the right-hand side of a regression is correlated with the error term, the resulting ordinary least squares estimates are biased and inconsistent. One of the ways this can happen is to choose the incorrect functional form.[5] Lott and Mustard exclusively utilize the semilog function. In this model, the log of the dependent variable is regressed on a set of linear explanatory variables. While this model has been commonly used in labor

[2] *Id.*

[3] John R. Lott, Jr., More Guns, Less Crime: Understanding Crime and Gun Control Laws (1998; 2d ed. 2000).

[4] Lott & Mustard, *supra* note 1, at 20–23.

[5] See, for example, Jeffrey M. Wooldridge, Introductory Econometrics: A Modern Approach 278–84 (2000).

TABLE 1

COEFFICIENTS ON THE SHALL-ISSUE LAW DUMMY USING VARIOUS FUNCTIONAL FORMS

Functional Form	Violent Crime	Mrder	Rape	Robbery	Assault	Property Crime	Burglary	Larceny	Auto Theft
Semilog	−.039*	−.074*	−.038*	−.012	−.063*	.033*	.003	.040*	.075*
	(3.94)	(4.77)	(3.13)	(.88)	(5.56)	(4.53)	(.39)	(4.49)	(6.56)
Linear	−17.15*	−1.09*	−3.30*	3.70	−21.95*	123.0*	25.37	71.9*	19.0*
	(3.23)	(6.96)	(7.82)	(1.11)	(6.98)	(3.75)	(1.91)	(4.49)	(3.64)
Log-log	−.012*	−.005	−.006	−.009*	−.013*	.004*	.000	.006*	.003
	(4.35)	(.61)	(1.16)	(2.13)	(4.02)	(2.80)	(.07)	(3.45)	(1.07)

NOTE.—In this table, I report only the estimated coefficients on the shall-issue dummy. However, all models include all the control variables used by Lott and Mustard in their basic regressions reported in table 3 (John R. Lott, Jr., & David B. Mustard, Crime, Deterrence, and Right-to-Carry Concealed Handguns, 26 J. Legal Stud. 1, 20–23 (1997)), including county and year dummies. The *t*-ratios are reported in parentheses. The semilog regressions replicate the Lott and Mustard basic model. All regressions are weighted by county population.

* Significant at the .05 level, two-tailed test.

economics, it is relatively unusual for a crime study. More common are the linear model and the double-log model. In the latter specification, both the dependent variable and all continuous explanatory variables are expressed in logarithms. Since these two specifications are far more common than the semilog model, I reestimated the Lott and Mustard basic equations reported in their table 3, using the Lott and Mustard data set, under all three functional forms. The results are reported in Table 1.

The results indicate that there is really little to choose from between the linear and semilog functional forms. However, unlike the other two models, the log-log model indicates no significant effect on murder and rape and a significant and negative effect on robbery.

It is of some interest to find a way to choose among the three specifications. Two tests are available to help make this choice. The first is the extended projection (PE) test for nonnested hypotheses created by Russell Davidson and James MacKinnon.[6] The basic idea of this test is to construct an overall model that contains the two different models as nested hypotheses. Unfortunately, it is possible under this test either to fail to reject or to reject both models. In the present case, the PE test generated indeterminate results. An alternative test is the regression specification error test created by James Ramsey.[7] In this test, predicted values of the dependent variable are raised to the powers of 2, 3, and 4 and added to the original model. If these variables are found to be significant, this indicates significant nonlinearity. The coefficients were significant in all three test equations. These results may indicate the presence of some unresolved nonlinearities in the analysis. They

[6] Russell Davidson & James G. MacKinnon, Several Tests for Model Specification in the Presence of Alternative Hypotheses, 40 Econometrica 781 (1981).

[7] James B. Ramsey, Tests for Specification Error in Classical Linear Least-Squares Analysis, 71 J. Royal Stat. Ass'n, Series B, 350 (1969).

TABLE 2

COEFFICIENTS ON THE SHALL-ISSUE LAW DUMMY: UNWEIGHTED LEAST SQUARES

Functional Form	Violent Crime	Murder	Rape	Robbery	Assault	Property Crime	Burglary	Larceny	Auto Theft
Semilog	−.062*	−.039*	−.052*	−.012	−.088*	.034*	.005	.041*	.026
	(4.07)	(2.21)	(3.10)	(.73)	(5.23)	(3.10)	(.45)	(3.49)	(1.95)
Linear	−54.9*	−.720*	−.352*	−2.81	−37.6*	−92.7	−39.0	−51.2	−9.15
	(5.52)	(2.40)	(4.48)	(.71)	(7.70)	(1.33)	(1.17)	(1.20)	(1.53)
Log-log	−.012*	−.004	−.006	−.009*	−.014*	.004*	.001	.006*	.002
	(4.34)	(.55)	(1.23)	(1.96)	(4.03)	(2.83)	(.32)	(3.35)	(.89)

NOTE.—Each regression contains all the control variables used by Lott and Mustard (John R. Lott, Jr., & David B. Mustard, Crime, Deterrence, and Right-to-Carry Concealed Handguns, 26 J. Legal Stud. 1, 20–23 (1997)), including arrest rates, county dummies, and year dummies.

* Significant at the .05 level, two-tailed test.

also leave us no good way to distinguish among the three models. However, since the results are reasonably consistent across all three models and I cannot find a good reason to reject the original semilog specification, I will use this specification in all further analyses.

Another choice for investigators is whether to use weighted least squares or ordinary least squares. Lott and Mustard weigh by population, which is reasonable and sensible and gives the largest counties the most weight. Nevertheless, it would be unfortunate if the results were sensitive to the weighting scheme. In Table 2, I show the estimated coefficients on the shall-issue dummy for the three model specifications using unweighted ordinary least squares.

The results are again similar to Lott and Mustard's original analysis and very close to the weighted least squares estimate. However, there is no significant relationship between the shall-issue law and property crime in the unweighted linear model. Nevertheless, Lott and Mustard's results are generally robust with respect to the weighting scheme.

III. POSSIBLE SPECIFICATION ERROR: OMITTED VARIABLES

Over the last 25 years, the prison population has exploded in the United States. There is now a considerable body of research showing that incarcerating criminals reduces crime.[8] Perhaps because they use county data and states have the responsibility of the prison population, Lott and Mustard omit the level of the prison population in all of their regressions, although they do incorporate sentence length in the Oregon and Pennsylvania subsamples. While they include arrest rates and number of police, these variables do not

[8] Thomas B. Marvell & Carlisle E. Moody, The Impact of Out-of-State Prison Population on State Homicide Rates: Displacement and Free-Rider Effects, 36 Criminology 513 (1998); Age Structure, Trends, and Prison Populations, 25 J. Crim. Just. 114 (1997); Steven D. Levitt, The Effect of Prison Population Size on Crime Rates: Evidence from Prison Overcrowding Litigation, 111 Q. J. Econ 319 (1996).

capture the incapacitation effects of prison. Those states that passed shall-issue concealed weapons laws may be the very ones that engaged in a "tough on crime" campaign overall so that these states also incarcerated a larger number of violent criminals. If this is the case, then the Lott and Mustard result may be due to the combined deterrence and incapacitation effect of a larger prison population. To test for this possibility, I repeated the basic Lott and Mustard regressions including the per capita state prison population.

In addition, I investigated the effect of the potential biases associated with the use of the arrest rate (clearance rate) as an explanatory variable. Using the arrest rate for a particular crime category, defined as the number of arrests divided by the number of crimes of that category (for example, arrests for assault divided by the number of assaults), could cause bias for two reasons. First, the same variable appears in the numerator of the dependent variable (assaults per capita) as in the denominator in the explanatory variable (arrests per assault). If there are measurement errors in the arrest rate (which can be considerable especially in county data because of random nonreporting by local law enforcement agencies), then the same errors appear in both the dependent and independent variable and cause the estimates to be biased. One way to avoid this problem is to use the overall arrest rate for all crime categories instead of the arrest rate for the particular crime category.

The arrest rate and crime rate may be simultaneously determined, in which case ordinary least squares estimates are biased and inconsistent. In this case, the problem can be solved by estimating the model using instrumental variables or by estimating the reduced-form equation. Instrumental variable estimation is relatively inefficient compared with ordinary least squares, while estimating the reduced-form equation with ordinary least squares yields unbiased, consistent, and efficient estimates. Reduced-form equations can be derived by dropping the arrest rate from the equation.[9] The downside to this strategy is that if the arrest rate is not simultaneously determined, the reduced-form equation suffers from omitted-variable bias. Consequently, it makes sense to estimate both versions of the model.

The results are presented in Table 3. The regressions are semilog, in levels, weighted by county population.

Examination of Table 3 reveals that the inclusion of prison population does not reduce the impact of the shall-issue law. However, there is no significant effect on property crime because burglary now appears to be negatively associated with the shall-issue law while the other property crimes remain positively associated. The results are generally robust with respect to the presence or absence of the arrest rate variable. However, the effect of

[9] I should add exogenous variables that determine arrests but are independent of crime. However, such variables are notoriously hard to find. See Franklin M. Fisher & Daniel Nagin, On the Feasibility of Identifying the Crime Function in a Simultaneous Model of Crime Rates and Sanction Levels, in Deterrence and Incapacitation: Estimating the Effects of Criminal Sanctions on Crime Rates (1978).

TABLE 3

Coefficients on the Shall-Issue Dummy Variable with Prison Population

Crime Category	Clearance Rate of Individual Crime	Clearance Rate of All Crime	Omitting Clearance Rate
Violent crime	−.054*	−.046*	−.042*
	(5.45)	(3.56)	(3.21)
Murder	−.090*	−.035	−.043
	(5.63)	(1.25)	(1.59)
Rape	−.065*	−.075*	−.079*
	(5.27)	(3.21)	(3.43)
Robbery	−.053*	−.024	−.106*
	(3.93)	(1.11)	(7.75)
Assault	−.065*	−.049*	−.053*
	(5.61)	(3.12)	(3.35)
Property crime	.008	.017*	.015
	(1.13)	(2.24)	(1.88)
Burglary	−.029*	−.019*	−.012
	(3.80)	(2.13)	(1.26)
Larceny	.022*	.027*	.022*
	(2.37)	(2.76)	(2.13)
Auto theft	.060*	.065*	.075*
	(5.22)	(4.60)	(5.13)

Note.—The *t*-ratios are in parentheses. Each regression contains all the control variables used by Lott and Mustard (John R. Lott, Jr., & David B. Mustard, Crime, Deterrence, and Right-to-Carry Concealed Handguns, 26 J. Legal Stud. 1, 20–23 (1997)), including county and year dummies and prison population. In every case, the coefficient on prison population is negative and significant. The coefficient on arrest rate is also negative and significant where used.

* Significant at the .05 level, two-tailed test.

shall-issue laws on the murder rate appears to depend crucially on the particular treatment of the arrest rate.

Overall, the major conclusion of the Lott and Mustard study, namely, that violent crime is reduced and property crime may be increased as a result of the passage of a shall-issue law, is confirmed by these further robustness checks. However, for the individual crime results, the effect of the shall-issue law on murder seems to be the most fragile. This is important in that the cost-benefit analysis of the law is sensitive to the inclusion or exclusion of a homicide effect because of the enormous costs associated with murder.

IV. Possible Specification Error: Misspecified Dynamics

Since a pooled time-series cross-section analysis could suffer from econometric problems associated with time series, analysts should investigate the time-series properties of the data. While in some analyses Lott and Mustard allow for linear trends and in one version of the basic regression they take first differences, they otherwise ignore the time-series aspects of the data.

TABLE 4

AUGMENTED DICKEY-FULLER UNIT ROOT TEST STATISTICS

	Without Year Dummies			With Year Dummies		
	$p = 2$	$p = 1$	$p = 0$	$p = 2$	$p = 1$	$p = 0$
Violent crime	−.156	−.197	−.304	−.158	−.199	−.321
	(38.3)	(49.3)	(78.4)	(38.6)	(49.8)	(88.7)
Murder	−.335	−.426	−.586	−.335	−.426	−.586
	(57.1)	(76.5)	(120.2)	(57.1)	(76.5)	(120.2)
Rape	−.234	−.309	−.454	−.237	−.312	−.458
	(46.5)	(63.4)	(100.7)	(47.0)	(63.9)	(101.5)
Robbery	−.145	−.200	−.329	−.144	−.199	−.330
	(35.0)	(48.7)	(81.8)	(34.8)	(48.6)	(81.9)
Assault	−.169	−.211	−.312	−.171	−.213	−.314
	(40.7)	(51.8)	(79.3)	(41.1)	(52.2)	(79.8)
Property crime	−.071	−.080	−.109	−.069	−.078	−.108
	(26.9)	(30.7)	(41.8)	(26.5)	(30.2)	(41.6)
Burglary	−.143	−.165	−.252	−.141	−.164	−.252
	(37.4)	(44.2)	(68.6)	(36.8)	(43.9)	(68.5)
Larceny	−.091	−.102	−.149	−.090	−.102	−.149
	(29.6)	(33.8)	(48.7)	(29.5)	(33.6)	(48.7)
Auto theft	−.212	−.274	−.398	−.210	−.273	−.398
	(44.0)	(58.5)	(90.5)	(43.7)	(58.2)	(90.4)

NOTE.—The *t*-ratios (distributed asymptotically normal) are in parentheses; $p =$ lag length. All coefficents are significant at the .05 level, two-tailed test.

I test for unit roots in an attempt to discover if the variables are nonstationary random walks. The test equation is as follows:[10]

$$\Delta Y_{it} = \alpha + (\rho - 1)Y_{i,t-1} + \sum_{j=1}^{p} \gamma_j \Delta Y_{i,t-j} + \varepsilon_{it}.$$

The null hypothesis of a unit root ($\rho = 1$) is tested with a standard significance test of the coefficient on $Y_{i,t-1}$.[11]

There are some advantages to testing for unit roots in panel data in that there is often much more variation in such samples than in a single time series. The drawback to this approach is that it cannot test for the possibility that some counties' crime rates are nonstationary while others are stationary. The results of the unit root tests for the dependent variables are presented in Table 4.

Judging from the *t*-ratios presented in Table 4, there seems to be no question that these series are stationary. Consequently, there is no advantage to be gained from reestimating the equations in first differences. In fact, there may

[10] Jorg Breitung & Wolfgang Meyer, Testing for Unit Roots in Panel Data: Are Wages on Different Bargaining Levels Cointegrated? 26 Applied Econ. 353 (1994).

[11] *Id.*

be disadvantages. Such regressions are strictly short run in that changes of crime are related to changes in the shall-issue law, arrests, and so on. In this case, information is lost concerning the long-run equilibrium relationship among these variables. Economic theory typically deals with long-run equilibria. Economists seldom know very much about short-run dynamics.

Although the data appear to be stationary, there is still an important time-series dimension to the analysis that Lott and Mustard neglect to explore. All of their basic equations are static in the sense that lagged variables are omitted from the analysis. This can create a kind of omitted variable bias if the lag effects are significant. Given the very real possibility that crime causes crime, it would appear to be necessary to incorporate momentum, lags, and other dynamics into the analysis.[12] An increase in crime can cause the law enforcement sector to be overwhelmed. The probability of arrest and conviction declines. Everyone seems to be committing crime and getting away with it. The result is a multiple increase in the crime rate. Similarly, a policy that successfully decreases crime reduces the burden on law enforcement. Arrests and convictions go up. The probability of avoiding arrest and conviction declines. The result is a multiple decrease in crime. These dynamic effects can be captured with difference equations, the discrete analog of differential equations, using lagged variables.

Also, for annual time-series regressions estimated in levels, it is necessary that the investigator include a time trend in order to avoid possible spurious correlation due to omitted time-dependent variables. Since many phenomena grow according to a trend, investigators must at least include a trend in a preliminary analysis and drop it only if it is insignificant. Lott and Mustard include individual year dummies, which help to avoid the worst effects of omitting the trend, but they are better used to control for unobserved common factors and events that affect all counties and can result in both positive and negative effects on crime. It is often the case that both the trend and the year effects are significant. Lott and Mustard mention a set of regressions that include a time trend and a trend-squared term split at the year of passage of the shall-issue law. However, the estimated equations are not reported.[13]

In an attempt to determine if the inclusion of dynamic effects alters the fundamental results of the Lott and Mustard study, I reestimate the basic equations below. To avoid possible omitted variable bias arising from omitted dynamic effects, all regressions include two lags of the dependent variable, the contemporaneous arrest rate, the contemporaneous prison population, two lags of the arrest rate, two lags of the prison population, and a linear time trend.

[12] Edward L. Glaeser, Bruce Sacerdote, & Jose A. Scheinkman, Crime and Social Interactions, 111 Q. J. Econ. 507 (1996).

[13] Lott & Mustard, *supra* note 1, at 35.

V. Possible Underestimated Standard Errors[14]

While most of the variables used by Lott and Mustard vary from county to county, the shall-issue dummy is a statewide variable. It is an aggregate variable that takes the same value for all the counties in the state.[15] Counties in the same state may share unobservable characteristics that could lead to correlation among the error terms. If this is the case, then ordinary least squares estimates of the standard errors will be underestimated, leading to possibly spurious findings of significance on the aggregate variable.[16] The data are said to be clustered by state. There are two approaches to this problem. The first, suggested by the referee, is to do the analysis at the state level, which might be a more natural level of analysis for a state-level variable like the shall-issue dummy. The disadvantage is that this approach throws away potentially useful variance on crime rates across counties. The second approach is to correct the standard errors for clustering. This is an application of the robust standard errors first suggested by P. J. Huber.[17] Another way to avoid some of the problems of clustering is to cause the shall-issue dummy to interact with the county population to yield the approximate number of people who are potentially able to effectively defend themselves against crime. This variable was used in some of the original Lott and Mustard regressions. The prison population variable is also an aggregate variable that takes the same value for all counties in each state. The *t*-ratios for this variable suffer from the same potential bias. The possibility of spurious significance due to clustering is critical. Future analyses based on these data should include this correction.[18]

One advantage of aggregating is that a longer time series is available. I have data at the state level from 1971–98. In the first set of regressions, I use the shall-issue dummy as the target variable. In the second set, I use the shall-issue dummy interacted with state population. The coefficients and *t*-ratios of the target variable are reported in Table 5.

The results are similar to the previous analyses. The Lott and Mustard results are obtained, although somewhat muted. Violent crime, especially murder and rape, is significantly reduced in several models, while some kinds of property crime are increased.

[14] I would like to thank an anonymous referee for suggesting this possibility.

[15] The exception is Pennsylvania, which exempted Philadelphia for several years.

[16] Brent R. Moulton, An Illustration of a Pitfall in Estimating the Effects of Aggregate Variables on Micro Units, 72 Rev. Econ. & Stat. 334 (1990).

[17] P. J. Huber, The Behavior of Maximum Likelihood Estimates under Non-standard Conditions, in Proceedings of the Fifth Berkeley Symposium in Mathematical Statistics and Probability 221 (1967).

[18] Robust standard errors corrected for clustering are conveniently available in STATA, STATA Users Guide, Release 6 (1999).

TABLE 5

EFFECT OF SHALL-ISSUE LAWS: STATE-LEVEL ANALYSES

Model	Violent Crime	Murder	Rape	Robbery	Assault	Property Crime	Burglary	Larceny	Auto Theft
1	−.113^{+}	.146	−.217*	.103	−.175*	.013	.041	.028	−.087
	(1.69)	(1.28)	(2.69)	(1.07)	(2.00)	(.30)	(.68)	(.64)	(.82)
2	.001	−.026*	.009	−.003	.006	.008^{+}	−.001	.012*	.007
	(.16)	(2.18)	(.1.02)	(.25)	(.63)	(1.67)	(.15)	(2.31)	(.65)
3	−.001	−.027^{+}	−.011	−.002	.001	.008	−.001	.012	.005*
	(.15)	(1.78)	(1.20)	(.18)	(.09)	(1.64)	(.24)	(.24)	(2.30)
4	.310	.000	−.036*	.004	.001	.004	−.004	.007	−.001
	(.06)	(.02)	(3.00)	(.25)	(.07)	(.61)	(.46)	(1.04)	(.10)
5	−.074*	.072	−.010*	.042	−.106*	.006	.011	.016	−.025
	(2.09)	(1.23)	(2.40)	(.84)	(2.33)	(.25)	(.34)	(.73)	(.49)
6	.001	−.004*	−.000	−.000	.002^{+}	.001	−.000	.001*	.001
	(1.29)	(3.45)	(.48)	(.06)	(1.76)	(1.31)	(.28)	(1.98)	(.68)
7	.001	−.005 *	−.000	−.000	.001	.001	−.000	.001*	.000
	(1.18)	(3.15)	(.52)	(.03)	(1.56)	(1.30)	(.31)	(1.97)	(.65)
8	.000	−.001	−.004*	−.000	.001	−.000	−.001	.000	−.001
	(.15)	(.72)	(3.10)	(.07)	(.43)	(.56)	(1.32)	(.06)	(.91)

NOTE.—Model 1: shall-issue dummy, with arrests; model 2, shall-issue dummy, no arrests; model 3, shall-issue dummy, no arrests, with trend; model 4, shall-issue dummy, no arrests, individual state trends; model 5, shall-issue dummy interacted with population, with arrests; model 6, shall-issue dummy interacted with population, no arrests; model 7, shall-issue dummy interacted with population, no arrests, with trend; model 8, shall-issue dummy interacted with population, no arrests, individual state trends.

$^{+}$ Significant at the .10 level, two-tailed test.

* Significant at the .05 level, two-tailed test.

The results derived from replicating this analysis at the county level, but with robust standard errors corrected for clustering, are reported in Table 6.

Examination of Table 6 reveals that the shall-issue law significantly reduces violent crime, but it also seems to reduce property crime, especially burglary. Robbery seems particularly susceptible to shall-issue laws, and murder is significantly reduced in those models using the shall-issue dummy interacted with county population. Burglary is also significantly reduced in all four dynamic models. It is possible that easy access to permits to carry concealed weapons may encourage more people to keep the weapons in their residences or make them more readily accessible at home. This could have a deterrent effect on potential burglars who might seek to avoid encounters with armed residents.

In a dynamic model, the coefficient on the shall-issue variable yields the impact of that variable in the current year. The long-run impact is estimated by dividing the coefficient by one minus the sum of the coefficients on the lagged dependent variables. For the model to be stable, the coefficients on the lagged dependent variable must sum to a number less than one in absolute value. All the estimated models are stable. The long-run effects of the shall-issue laws are presented in Table 7, corresponding to the models reported in Table 6.

TABLE 6

EFFECT OF SHALL-ISSUE LAWS: COUNTY-LEVEL ANALYSIS

Model	Violent Crime	Murder	Rape	Robbery	Assault	Property Crime	Burglary	Larceny	Auto Theft
	−.051*	−.041	−.042	−.074*	−.032	−.016	−.053*	−.005	.006
1	(2.29)	(.89)	(1.48)	(2.59)	(1.02)	(1.03)	(2.14)	(.30)	(.28)
	−.056*	−.040	−.071	−.068⁺	−.030	−.017	−.053*	−.004	.030
2	(2.46)	(.59)	(1.61)	(1.69)	(1.05)	(1.10)	(2.27)	(.20)	(.85)
	−.009*	−.014	−.008*	−.014*	−.003	−.000	−.009*	.004	.003
3	(3.64)	(1.54)	(2.88)	(3.23)	(1.02)	(.15)	(2.98)	(.84)	(.89)
	−.012*	−.023*	−.015*	−.015*	−.007⁺	−.000	−.001*	.005	.004
4	(5.05)	(2.80)	(4.11)	(3.07)	(1.90)	(.12)	(3.99)	(1.16)	(.70)

NOTE.—The *t*-ratios are in parentheses. Standard errors are corrected for clustering. All models are dynamic with two lags of the dependent variable, prison population, prison population lagged twice, an overall trend, and all the control variables used by Lott and Mustard (John R. Lott, Jr., & David B. Mustard, Crime, Deterrence, and Right-to-Carry Concealed Handguns, 26 J. Legal Stud. 1, 20–23 (1997)). Model 1, shall-issue dummy, with arrests and arrests lagged twice; model 2, shall-issue dummy, no arrest variables; model 3, shall-issue dummy interacted with county population, with arrest variables; model 4, shall-issue dummy interacted with county population, no arrest variables.

⁺ Significant at the .10 level, two-tailed test.

* Significant at the .05 level, two-tailed test.

The coefficients in the first two rows can be interpreted as the long-run percentage change in crime due to the passage of a shall-issue law. The coefficients in the second two rows estimate the long-run effects on crime of a 1 percent increase in the population of a county with a shall-issue law. Again, the overall Lott and Mustard results that right-to-carry concealed weapons laws tend to reduce violent crime are confirmed by the implied long-run effects. The dynamic model using the shall-issue dummy variable seems to indicate that both violent crime and property crime are reduced by the passage of right-to-carry concealed weapons laws. The effects can be substantial. The results from model 1 indicate that both robbery and burglary can be expected to eventually decline by 10 percent following the passage of a shall-issue law. In any case, the fact that the lagged dependent variables are highly significant means that future analyses of these data should include lags.

VI. MISSPECIFIED DYNAMICS: LEADS AND LAGS OF THE SHALL-ISSUE VARIABLE[19]

One problem that could arise from using a dummy variable to indicate a policy change is that the timing of the policy change could simply coincide with an exogenous change in the dependent variable, leading to a spurious correlation between the dependent variable and the policy dummy. For example, crime rates might have begun to decline before the passage of the

[19] This section was suggested by an anonymous referee.

TABLE 7

LONG-RUN EFFECTS OF THE SHALL-ISSUE LAW

Model	Violent Crime	Murder	Rape	Robbery	Assault	Property Crime	Burglary	Larceny	Auto Theft
1	−.093*	−.043	−.058	−.100*	−.061	−.028	−.100*	−.008	.010
2	−.083*	−.043	−.077	−.071⁺	−.047	−.028	−.085*	−.006	.036
3	−.016*	−.015	−.011*	−.019*	−.006	−.000	−.017*	.006	.005
4	−.018*	−.025*	−.016*	−.015*	−.011⁺	−.000	−.002*	.007	.005

NOTE.—The *t*-ratios are in parentheses. Standard errors are corrected for clustering. The models are the same as reported in Table 6.

⁺ Significant at the .10 level, two-tailed test.

* Significant at the .05 level, two-tailed test.

shall-issue laws so that the shall-issue dummy captures effects on crime that would have happened anyway. One way to test for this possibility is to include leads and lags of the shall-issue dummy. If the leads are not significant, or of the sign opposite to the coefficient on the shall-issue variable, then the analysis does not suffer from this potential source of error. To test this hypothesis, I add two leads and two lags of the shall-issue dummy variable to the regression model reported in Table 6. The results are reported in Table 8.

Examination of Table 8 reveals only two significant coefficients associated with leads of the shall-issue dummy, both of which have positive signs. While some of the estimated coefficients on the leads are negative, none of them are close to being significant. I can find no evidence that the shall-issue variable captures the effects of declines in the crime rates that began before the passage of the shall-issue law.

VII. SUMMARY AND CONCLUSIONS

The publication of Lott and Mustard's path-breaking article in 1997, of Lott's book, *More Guns, Less Crime*, in 1998, and of the second edition in 2000 has refocused the debate on handguns. Lott and Mustard make the credible suggestion that relatively easy access to handguns may deter crime by allowing people to effectively defend themselves. This suggestion has the weight of economic theory behind it. It incorporates the theory of public goods and externalities and recognizes that citizens who are not armed can benefit from the fact that others may be carrying a concealed weapon.[20]

Despite the fact that Lott and Mustard seem to do all possible variations of the analysis, a closer reading finds that several possibly important specification errors could have been made in the original paper. Incorrect functional form, omitted variables, and simultaneity are all sources of specification

[20] For another example, see Ian Ayres & Steven D. Levitt, Measuring Positive Externalities from Unobservable Victim Precaution: An Empirical Analysis of Lojack, 113 Q. J. Econ. 43 (1998).

TABLE 8

LEADS AND LAGS OF THE SHALL-ISSUE DUMMY

Model	Violent Crime	Murder	Rape	Robbery	Assault	Property Crime	Burglary	Larceny	Auto Theft
Shall_{t+2}	−.009	−.021	−.017	.057	−.029	.004	.018	−.009	.054*
	(.47)	(.65)	(.64)	(1.11)	(1.16)	(.16)	(.59)	(.32)	(2.72)
Shall_{t+1}	.016	.054	−.028	−.009	.037*	−.002	−.001	−.044	.026
	(1.00)	(1.41)	(1.48)	(.44)	(1.96)	(.07)	(.06)	(.94)	(.80)
Shall	−.045*	−.121*	−.048	−.095	−.029	−.016	−.063*	.038	−.042
	(1.97)	(2.10)	(1.60)	(1.51)	(1.20)	(.70)	(2.24)	(1.00)	(1.16)
Shall_{t-1}	.008	.024⁺	.013	.018⁺	.000	−.003	−.000	−.017	.007
	(1.18)	(1.89)	(1.52)	(1.86)	(.00)	(.42)	(.08)	(.92)	(.78)
Shall_{t-2}	−.002	.013	.009	.007	−.005	.004	.004	.001	.004
	(.21)	(.83)	(.77)	(.69)	(.53)	(.89)	(.84)	(.08)	(.50)

NOTE.—The *t*-ratios are in parentheses. Standard errors are corrected for clustering. All models are dynamic with two lags of the dependent variable, prison population, prison population lagged twice, arrests, arrests lagged twice, an overall trend, and all the control variables used by Lott and Mustard (John R. Lott, Jr., & David B. Mustard, Crime, Deterrence, and Right-to-Carry Concealed Handguns, 26 J. Legal Stud. 1, 20–23 (1997)). The subscript on Shall indicates lead (+) or lag (−)

⁺ Significant at the .10 level, two-tailed test.

* Significant at the .05 level, two-tailed test.

bias that can give rise to biased and inconsistent estimates. I find that their original conclusions are robust with respect to these potential problems. I also find that while the lags of the shall-issue dummy are not significant, other lagged variables have significant coefficients in most of the crime equations. I conclude that there are important dynamic effects in these data that should be incorporated into future analyses.

There is also an important source of spurious significance in these data. The shall-issue variable is constant across all counties in the same state (as is prison population). Merging an aggregate variable with microlevel variables causes ordinary least squares formulas to severely overestimate the *t*-ratios associated with the aggregate variables.[21] In one attempt to avoid this problem, I estimated a version of the Lott and Mustard model using a pooled cross section of 50 states over the years 1972–98. As a further check, I reestimated the model using the original county-level data set but adjusted the standard errors for clustering within states. The results were somewhat different from the original Lott and Mustard findings. While violent crime, especially rape and murder, seems to be reduced in the presence of shall-issue laws in the state-level analysis, the results are less powerful than in the original study. The results of the dynamic analysis of the county-level data with standard errors adjusted for clustering are also different. While shall-issue laws reduce violent crime in general in all models, the effects seem to be concentrated in robbery. Murder and rape are significantly reduced in only one version of the model. The new result is that burglary also appears

[21] Moulton, *supra* note 17.

to be significantly reduced by the passage of these laws, while no other property crimes are significantly increased. I find that the long-run effects of the laws can be substantial. Finally, an examination of the effects of leads and lags of the shall-issue dummy indicates that the variable is not capturing declines in crime rates that began before the passage of the law.

The results of the above analyses confirm and reinforce the basic findings of the original Lott and Mustard study. Passage of a right-to-carry concealed weapons law tends to reduce violent crime. The effect on property crime is more problematic. According to some of the analyses, property crime is increased. However, the preferred county-level model shows that burglary is also deterred by shall-issue laws. In any case, the Lott and Mustard analysis represents a complete reversal of the received wisdom that making guns more accessible increases violent crime. In none of my analyses do I find a positive and significant coefficient relating shall-issue laws to any category of violent crime.

Increasing arrests and incarcerating felons also reduces crime. However, those policies are much more expensive than simply allowing citizens to defend themselves.

Bibliography

Ayres, Ian, and Levitt, Steven D. "Measuring Positive Externalities from Unobservable Victim Precaution: An Empirical Analysis of Lojack." *Quarterly Journal of Economics* 113 (1998): 43–77.

Breitung, Jorg, and Meyer, Wolfgang. "Testing for Unit Roots in Panel Data: Are Wages on Different Bargaining Levels Cointegrated?" *Applied Economics* 26 (1994): 353–61.

Davidson, Russell, and MacKinnon, James G. "Several Tests for Model Specification in the Presence of Alternative Hypotheses." *Econometrica* 49 (1981): 781–93.

Fisher, Franklin M., and Nagin, Daniel. "On the Feasibility of Identifying the Crime Function in a Simultaneous Model of Crime Rates and Sanction Levels." In *Deterrence and Incapacitation: Estimating the Effects of Criminal Sanctions on Crime Rates*, edited by Alfred Blumstein, Jacqueline Cohen, and Daniel Nagin. Washington, D.C.: National Academy of Sciences, 1978.

Glaeser, Edward L.; Sacerdote, Bruce; and Scheinkman, Jose A. "Crime and Social Interactions." *Quarterly Journal of Economics* 111 (1996): 507–48.

Granger, C. W. J., and Newbold, Paul. "Spurious Regressions in Econometrics." *Journal of Econometrics* 2 (1974): 111–20.

Hamilton, James D. *Time Series Analysis*. Princeton, N.J.: Princeton University Press, 1994.

Huber, P. J. "The Behavior of Maximum Likelihood Estimates under Non-Standard Conditions." In *Proceedings of the Fifth Berkeley Symposium in*

Mathematical Statistics and Probability, pp. 221–33. Berkeley: University of California Press, 1967.

Levitt, Steven D. "The Effect of Prison Population Size on Crime Rates: Evidence from Prison Overcrowding Litigation." *Quarterly Journal of Economics* 111 (1996): 319–51.

Lott, John R., Jr. *More Guns, Less Crime: Understanding Crime and Gun Control Laws.* Chicago: University of Chicago Press, 1998.

Lott, John R., Jr. *More Guns, Less Crime: Understanding Crime and Gun Control Laws.* 2d ed. Chicago: University of Chicago Press, 2000.

Lott, John R., Jr., and Mustard, David B. "Crime, Deterrence, and Right-to-Carry Concealed Handguns." *Journal of Legal Studies* 26 (1997): 1–65.

Marvell, Thomas B., and Moody, Carlisle E. "The Impact of Out-of-State Prison Population on State Homicide Rates: Displacement and Free-Rider Effects." *Criminology* 36 (1998): 513–35.

Marvell, Thomas B., and Moody, Carlisle E. "Age Structure, Trends, and Prison Populations." *Journal of Criminal Justice* 25 (1997): 114–24.

Moulton, Brent R. "An Illustration of a Pitfall in Estimating the Effects of Aggregate Variables on Micro Units." *Review of Economics and Statistics* 72 (1990): 334–38.

Ramsey, James B. "Tests for Specification Error in Classical Linear Least-Squares Analysis." *Journal of the Royal Statistical Association.* Series B 71 (1969): 350–71.

STATA Users Guide, Release 6. College Station, Tex.: Stata Press, 1999.

Woodridge, Jeffrey M. *Introductory Econometrics: A Modern Approach*. Cincinnati: South-Western College Publishing, 2000.