The Impact of Right-to-Carry Concealed Firearm Laws on Mass Public Shootings

Grant Duwe, Tomislav Kovandzic and Carlisle E. Moody

Homicide Studies 2002; 6; 271
DOI: 10.1177/108876702237341

The Impact of Right-to-Carry Concealed Firearm Laws on Mass Public Shootings

GRANT DUWE
Florida State University
TOMISLAV KOVANDZIC
University of Alabama at Birmingham
CARLISLE E. MOODY
College of William and Mary

Right-to-carry (RTC) laws mandate that concealed weapon permits be granted to qualified applicants. Such laws could reduce the number of mass public shootings as prospective shooters consider the possibility of encountering armed civilians. However, these laws might increase the number of shootings by making it easier for prospective shooters to acquire guns. We evaluate 25 RTC laws using state panel data for 1977 through 1999. We estimate numerous Poisson and negative binomial models and find virtually no support for the hypothesis that the laws increase or reduce the number of mass public shootings.

Beginning with the mass murder carried out by Charles Whitman in 1966, mass public shootings have sparked a great deal of debate over gun control. Proponents of gun control have adduced such incidents as evidence that the United States needs stricter laws regulating access to guns, such as mandatory waiting periods for handgun purchases, gun registration, purchase permits, owner licenses, and restrictions on gun-carrying outside the home. More recently, gun control advocates have responded to a number of high-profile mass shootings by calling for a ban on assault weapons and large ammunition clips. These efforts ultimately led to the passage of a federal assault weapon ban in 1994.

AUTHORS' NOTE: The authors would like to thank two anonymous reviewers and the editor for their helpful suggestions.

HOMICIDE STUDIES, Vol. 6 No. 4, November 2002 271-296
DOI: 10.1177/108876702237341

Although highly publicized mass public-shooting incidents have undoubtedly led many states (such as California) and the federal government to pass more restrictive gun laws, such incidents may also explain why many states are making it easier for private citizens to arm themselves in public. Commonly referred to as "shall issue" or "right-to-carry" (hereafter, RTC laws) concealed firearms laws, these laws mandate that local authorities issue a permit to carry a concealed handgun to anyone who satisfies certain objective criteria. Those favoring RTC laws maintain that gun-carrying among prospective victims may exert a deterrent effect on mass public shootings by raising prospective shooters' perception of risk from armed victims (Lott, 2000; Lott & Landes, 2000). Results from the National Self-Defense Survey (NSDS), which indicate that private firearms may be used in self-defense up to two and a half million times each year, provide some support for this argument (Kleck & Gertz, 1995). In addition, because the laws presumably increase gun-carrying and defensive use of guns by prospective victims in public places, one might expect RTC laws to increase the rate at which armed victims disrupt shootings, and thus the severity of the attacks, even if they have no effect on the number of shootings attempted (Lott & Landes, 2000). Indeed, this point was raised by Suzanna Gratia Hupp after George Hennard gunned down her parents and 21 other people at a Luby's cafeteria in Killeen, Texas, in 1991. Gratia Hupp was inside Luby's during the attack, but she left her gun in her car because of the state's gun-carrying laws at the time (see Lott, 2000, for a discussion of several cases in which citizens used guns to stop armed attackers). In response to the mass shooting, Gratia Hupp launched a crusade to permit licensed gun owners to carry concealed handguns in public places. After winning a seat in the Texas legislature, she helped persuade lawmakers to pass an RTC law in Texas in 1995.

Those opposed to RTC laws argue that the prospect of coming into contact with an armed citizen is unlikely to deter prospective shooters, as it is not clear how many prospective shooters are aware of the existence of RTC laws or the number of people with carry permits. Even if prospective shooters became aware of the existence and application of such laws, it is not clear that such knowledge would deter many from going on shooting sprees because many are thought to be mentally ill and/or suicidal

(Levin & Fox, 1985). They are, therefore, unlikely to take into consideration the increased costs of coming into contact with an armed citizen during a shooting attack. Of course, none of this proves that there are no prospective shooters who could be deterred by the passage of an RTC law. Prospective shooters could fall along a continuum of willingness and motivation to go on a shooting spree, and a number of them may fall far enough toward the low end of the scale. RTC laws could work by increasing the risk associated with such sprees.

Nevertheless, the additional costs to potential offenders for going on a shooting spree in a public place would probably be small because the number of people carrying guns with permits is small compared to overall gun-carrying, especially in urban areas where illegal gun-carrying is common (Kleck & Gertz, 1998). Also, if permit holders were carrying illegally before the laws, it would mean there was actually no change in carrying or in actual costs to prospective shooters (Kleck & Gertz, 1998; Ludwig, 1998). Opponents of RTC laws argue that the laws might actually increase the number of mass public shootings by making it easier for prospective shooters to gain access to firearms, or at least increase the lethality of such attacks because gunshot wounds are more likely to result in death than wounds inflicted by other weapons (Cook, 1991; Kleck, 1997; Zimring, 1968).

Whether RTC laws deter potential shooters from going on shooting sprees in public places (or the severity of the attacks) or actually have the unintended consequence of increasing the number of mass public shootings is an empirical question and is the main focus of this article. We estimate numerous Poisson and negative binomial fixed-effect models on state panel data for more than 20 years, ending in 1999. In keeping with previous mass murder research we define mass public shootings as incidents in which four or more people are fatally shot in a public place. We use Supplementary Homicide Report (SHR) data from the Federal Bureau of Investigation (FBI) to help locate newspaper accounts of mass public shootings because these data provide important information on almost every mass shooting reported to the police. As a result, we were able to locate virtually all of the mass public shootings involving four or more fatal victims that occurred between 1976 and 1999. The following section discusses the only study, Lott and Landes (2000), to evaluate the impact of

RTC laws on mass public shootings. The next section explains at length the data and regression procedures used. The last two sections present and discuss the results.

PREVIOUS RESEARCH

Although a number of studies have examined the impact of RTC laws on homicide (e.g., Bartley & Cohen, 1998; Black & Nagin, 1998; Dezhbaksh & Rubin, 1998; Lott, 2000; Lott & Mustard, 1997; Ludwig, 1998; Marvell, 1999; McDowall, Loftin, & Wiersema, 1995), we are aware of only one study that has examined their impact on multiple homicides (Lott & Landes, 2000). Lott and Landes (2000) evaluated the impact of RTC laws on multiple-victim public shootings in 23 states using Poisson regression models with fixed effects on pooled state-level data for 1977 to 1997. The RTC laws were entered as binary dummy variables and trends for years before the RTC law and years after the RTC law went into effect. The authors used the newspaper database in Lexis-Nexis to locate 931 instances of mass public shootings, which they defined as incidents in which two or more people were killed or wounded in a public place. Lott and Landes excluded shootings committed in connection with robberies or burglaries, organized crime (e.g., contract killings), and gang-related activity on the theory that people involved in these activities would probably carry guns regardless of RTC laws. Control variables included the arrest rate for murder, economic trends, and demographic variables pertaining to age, sex, and race. Results of the analysis indicated that RTC laws significantly reduced the number of multiple-victim shootings and the number of people killed and wounded. The authors found similar results when the victim threshold was increased from two to four.

As we discuss below in the section on analytic methods, the Poisson regression model is appropriate for variables whose means and variances are equal (the *equidispersion* assumption). We find below that, especially for the number killed and wounded, the mean and variances are very different. Therefore, the appropriate method is the negative binomial, not the Poisson. If the Poisson is used in cases where the equidispersion assumption is violated, the *t* ratios are overestimated (Gourieroux, Monfort, & Trognon, 1984). This could explain the differences between our results and those of Lott and Landes.

DATA AND METHOD

We use a multiple time series design (MTS) with state panel data from 1976 to 1999. The MTS design has long been considered an excellent evaluation design because of its feasibility and its ability to mitigate internal validity threats (e.g., Campbell & Stanley, 1963). It has several advantages compared to the more commonly used time series or cross-sectional designs. First, the design provides for a very large sample size, allowing one to enter numerous control variables while still retaining a large number of degrees of freedom. Second, the design allows one to correct for unobserved heterogeneity across states. Finally, it allows one to enter proxy variables for unknown factors that cause the dependent variable to vary over time. The proxy variables are discussed more fully below. We used a fixed-effects model, which is the standard method for panel data (Hsiao, 1986; Mundlak, 1978). This procedure eliminates cross-sectional variation between states and controls for unobserved factors (or difficult to measure factors) that cause incidences of mass public shootings to differ from state to state. Year dummies control for unobserved factors that might affect mass public shootings in a given year across all states. For example, this would include any impact of the 1994 federal assault weapons ban, which prohibited the possession, manufacture, and sale of about 19 types of assault weapons. Finally, we include an overall national trend variable to capture any omitted trending variables that might raise or lower the incidence of mass public shootings nationwide. The overall national trend variable is necessary because without it, the coefficient on the RTC variables would simply measure whether mass public shootings are higher or lower for the years after the law (relative to national trends captured by the year dummies), even if the increase occurred before or well after the law went into effect. The overall time trend variable is coded as 0 in 1977, the first year of our sample.

Mass Public Shootings

Determining the number of victims necessary for classification as a mass public shooting is an admittedly arbitrary decision. As noted above, Lott and Landes (2000) use a liberal definition in that they adopt a threshold of two wounded victims. Previous research on mass murder, however, has placed the fatal victim threshold at either three (Dietz, 1986; Holmes & Holmes, 1992; Petee, Padgett, & York, 1997) or four (Duwe, 2000; Fox & Levin, 1994, 1998; Levin & Fox, 1985, 1996; Ressler, Burgess, & Douglas, 1988). We selected a criterion of four fatal victims because, compared to a two- or three-victim requirement, it greatly reduces the potential for measurement error in the identification of mass public shootings.[1] Nevertheless, some may think the definition we use is too narrow. As discussed later, however, the four-victim threshold produces enough cases to determine whether RTC laws have a statistically significant impact on mass public shootings. Moreover, when Lott and Landes (2000) confined their sample to cases involving four or more fatal victims, they found that RTC laws still had a deterrent effect on mass public shootings.

Although Lott and Landes (2000) removed every felony-related shooting, we excluded only incidents where both the victims and offender(s) were involved in unlawful activities, such as organized crime, gang activity, and drug deals. Of the 52 felony-related mass shootings from 1976 to 1999, 36 (69%) involved offenders who committed a robbery in a public location (usually a convenience store or fast-food restaurant) and then shot the victims who were present to eliminate eyewitnesses. However, the victims in these incidents were not engaged in criminal activities; rather, they were innocent, law-abiding citizens who were simply in the wrong place at the wrong time. We included the 36 robbery/mass shootings because economic theory predicts that RTC laws should have an impact on these cases. As shown later, excluding felony-related shootings from our counts does not alter the results.

Most of the data on mass public shootings were derived from a previous study that examined the media's presentation of mass murder (Duwe, 2000). Using SHR data to identify when and where mass murders occurred between 1976 and 1996, Duwe (2000) found 30,027 newspaper articles on 495 mass killings by searching the newspaper databases in Lexis-Nexis, Dialog@ CARL, and CD Newsbank (see Duwe, 2000, for a detailed discussion of the search methodology used). The articles were then examined to record additional information not provided by the SHR, such as the location where the murders took place (e.g., residential or public setting) and the number of wounded victims. Of the 495 cases, 102 met our criteria for classification as mass public shootings.

A similar search methodology was used to collect data on mass public shootings that occurred between 1997 and 1999. The SHR indicated that 55 gun-related incidents involving four or more victims took place during this time period. After determining when and where these shootings occurred, we searched the newspaper database in Lexis-Nexis and found articles on all 55 shooting incidents.[2] The news accounts revealed that 14 cases met our definitional requirements. Overall, we identified 116 mass public shootings that took place between 1976 and 1999. Of the 116 shootings, 61 took place in states that enacted RTC laws or their equivalents in the 1976-to-1999 period.

Right-to-Carry Law Variables

Between 1976 and 1999, 25 states implemented nondiscretionary permit systems allowing applicants who meet certain objective criteria to obtain a permit to carry a concealed handgun. The 25 states and the dates they began issuing permits on a nondiscretionary basis were obtained through extensive statutory research conducted by Marvell (1999).[3] Because the deterrent impacts of the laws might occur immediately, if prospective shooters quickly learn about and fear coming into contact with armed bystanders, or increase over time as more people get carry permits (or decline over time as publicity surrounding passage of the laws fades), we decided to represent the RTC laws using two separate measures to account for both of these possibilities. The first variable is a step dummy variable scored 1 the year after a law went into effect and 0 otherwise.[4] The step dummy variable enables us to capture any immediate and constant impacts of the law on mass public shootings due to their initial passage. This specification assumes that prospective shooters generally know when the laws are passed, do not forget about the laws, and believe that the chance of encountering armed victims and bystanders does not change over time. The second RTC variable is a time trend variable interacted with the RTC step dummy. The RTC time trend variable is coded 0 for all the years up to and including the year the RTC law was passed in each particular state and the digits 1, 2, and so on for the following years. This representation allows us to test whether the deterrent impact of the laws is more closely linked to the number of people carrying guns in public, which is likely to grow over time as more people obtain permits (or decrease as publicity surrounding passage of the law fades). As Lott (2000) notes, estimates derived from step dummies probably underestimate the full impacts of RTC laws because not everyone who wants a handgun permit gets one right away.[5] As a result, one might expect any subjective shift in potential perpetrators' perceived risk of going on a shooting spree in a public place to increase over time as the number of people with permits reaches levels high enough to produce sharp changes in prospective shooters' awareness of gun-carrying among potential victims and bystanders (see also Black & Nagin, 1998; Lott, 2000; Ludwig, 1998). Indeed, research by Lott and Mustard (1997) revealed that the number of permits in the states studied (Arizona, Pennsylvania, and Oregon) increased substantially over time.

Specific Control Variables

In addition to the proxy variables for unknown factors, we include a variety of specific control variables that prior research and theory suggest are important correlates of homicide and mass murder. The decision of which control variables to include in the mass murder models was based on a review of previous macro-level studies linking homicide rates to the structural characteristics of ecological units (see Kovandzic, Vieraitis, & Yeisley, 1998; Land, McCall, & Cohen, 1990; and the studies reviewed therein). Previous research finds a strong link between economic conditions and homicide rates. In addition, Fox and Levin (1994) have noted that employment problems frequently precipitate mass killings. Therefore, we include four economic variables: unemployment rate, employment rate, real per capita income, and the poverty rate. See Marvell and Moody (1996) for sources. Homicide rates tend to vary with age groups. Therefore, we enter the percentage of population falling into eight different age groups (15 to 17, 18 to 19, 20 to 24, 25 to 29, 30 to 34, 35 to 44, 45 to 54, and 55 to 64) provided by the U.S. Bureau of the Census on computer disk. Some researchers suggest that prison population and the death penalty are important determinants of homicide (e.g., Ehrlich, 1975; Layson, 1985; Marvell & Moody, 1997; Phillips, 1980). The prison population variable is the number of inmates sentenced to state institutions for more than a year (year-end estimates), available annually at the state level. State-level prison population data were obtained from the Bureau of Justice Statistics website. As in Marvell and Moody (1997), the prison population value is an average of the current year and the prior year. Data for executions in each year and each state through 1990 are from the Espy and Smykla (1994) data set, and later data are from Stephan and Snell (1996) and earlier reports. Two additional variables are included because recent research suggests they may be important correlates of mass murder: percentage of people living alone and residential mobility (defined as the percentage of the population age 5 years and older who reported living in a different state or abroad 5 years earlier). Data for these two measures come from the decennial census and are interpolated for intercensal years. The variable names, definitions, and means are presented in Table 1 below.

ANALYTIC METHOD

The annual number of mass public shootings involving four or more fatal victims in a given state is very small; frequently, it is 0. It is a count variable that can only take nonnegative integer values. Although it is possible to estimate a count model using standard regression techniques, the results are biased, inefficient, and inconsistent (Long, 1997).

If an event can occur in any of a large number of trials but the probability of the event is small, then according to the "law of rare events," the number of events will follow, approximately, a Poisson distribution (Cameron & Trivedi, 1998). The histogram of the number of mass public shootings in Figure 1 below has the classic Poisson shape.

The distribution has the form

TABLE 1
Variable Definitions and Means

Variable Definition	*Mean*
Number of incidents	0.098
Number of people killed	0.557
Number of people wounded	0.343
Right-to-carry step dummy	0.173
Right-to-carry time trend	0.778
Linear trend	11.00
Number of legal executions	0.509
State prison population per capita	2.342
Population	4816.
Percentage of people who moved in past 5 years	13.14
Percentage of single-person households	23.56
Percentage age 15 to 17	4.698
Percentage age 18 to 19	3.249
Percentage age 20 to 24	8.126
Percentage age 25 to 29	8.127
Percentage age 30 to 34	8.079
Percentage age 35 to 44	14.06
Percentage age 45 to 54	10.54
Percentage age 55 to 64	8.719
Unemployment rate	6.209
Total employment	260.0
Per capita income, thousands	4.494

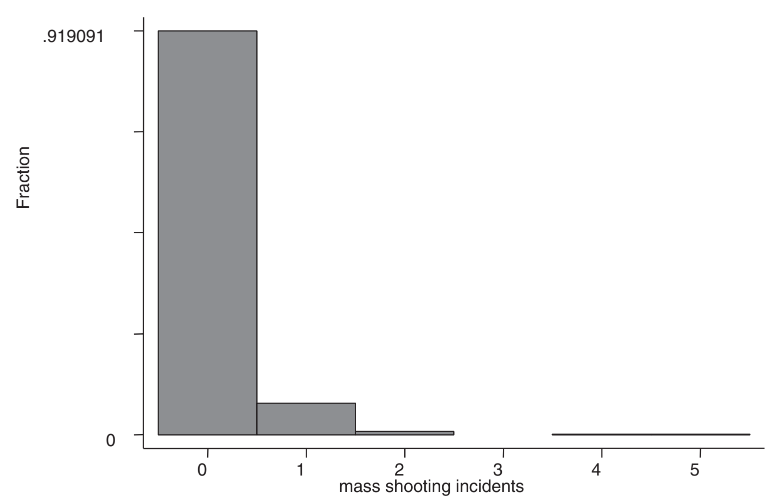

FIGURE 1: Histogram of the Number of Mass Public Shooting Incidents

$$\Pr(y|\mu) = \frac{\exp(-\mu)\mu^{y}}{y!} \tag{1}$$

for y = 1, 2, 3, . . . The random variable y is the number of times an event occurs over a fixed time interval, and the parameter μ is strictly positive. The mean of the distribution is μ, and the variance is equal to the mean (equidispersion).

The Poisson assumes that observations are independent. This means that the probability of a mass public shooting should be the same whether or not a shooting has already taken place. Several scholars have speculated that the extensive publicity given to mass killings encourages individuals predisposed to violence to commit similar acts of bloodshed (Fox & Levin, 1994; Levin & Fox, 1985; Rappaport, 1988). As a result, mass public shootings may appear in clusters if a public shooting leads others to imitate or mimic the behavior of the first perpetrator. Given that our data show little sign of clustering, the Poisson remains the model of choice.

To extend the analysis to a multivariable context, we use the Poisson regression model where the mean is taken to be a linear function of a vector of explanatory variables

$$\mu_i = E\,(y_i \mid x_i) = \exp\,(x_i\beta) \tag{2}$$

where x_i is a vector of observations on the explanatory observations and β is the corresponding vector of regression parameters. This model implies a particular form of heteroscedasticity because the assumption of equidispersion implies that the variance equals the mean, or

$$V(y_i \mid x_i) = \exp\,(x_i\beta) \tag{3}$$

The model is estimated using maximum likelihood. If the true data-generating process is Poisson, then maximum likelihood methods will yield a consistent estimate of the parameters, the variance-covariance matrix, and the resulting standard errors and *t* ratios.

For our purposes, we need to extend these regression models to pooled time series and cross-section data. Because it is very likely that there is unobserved heterogeneity among the states and that this heterogeneity is correlated with one or more of the explanatory variables, our basic model is the fixed effects model (Hausman, Hall, & Griliches, 1984). We assume multiplicative individual fixed effects because the explanatory variables occur in an exponential function.

$$y_{it} \sim \Pr(\mu_{it} = \alpha_i \exp(x_{it}\beta)), i = 1, \ldots, n, t = 1, \ldots, T \tag{4}$$

The multiplicative individual effects can be interpreted as an intercept shift.

$$E[y_{it} \mid x_{it}, \alpha_i] = \mu_{it} = \alpha_i \exp(x_{it}\beta) = \exp(\gamma_i + x_{it}\beta) \tag{5}$$

where $\gamma_i = \exp(\alpha_i)$ (Cameron & Trivedi, 1998).

If the regressors include lagged dependent variables (dynamic panels), then the fixed effects model is no longer consistent. This is similar to the problem in linear panel models (Nickell, 1981). In linear models, these biases have been shown to be quite large for very short panels. However, the bias is reduced dramatically as the number of time periods increases. Because we have more than 20 years of data, the bias is likely to be considerably reduced.

There are two ways to treat the lagged dependent variable in the dynamic panel model. The first is to simply add y_{t-1} to the explanatory variables (exponential feedback), so that the conditional mean becomes

$$\mu_i = \exp(x_i\beta + \rho y_{t-1}) \tag{6}$$

The second approach adds the lag of the log of the dependent variable as an explanatory variable. The conditional mean becomes

$$\mu_i = \exp(x_i\beta + \rho 1n y^*_{i,t-1}) = \exp(x_i\beta)(y^*_{i,t-1})^\rho \tag{7}$$

where $y_i^* = y_i + c$ where c is some positive constant to avoid taking the log of 0 (we set $c = 1$). This is a more natural model because the dependent variable is already logged (Cameron & Trivedi, 1998).

Models with lagged dependent variables, however, are seldom used in cases like ours with many zeroes (Cameron & Trivedi, 1998, p. 240). In our data, the typical state has several years of zeroes followed by one or two incidents, followed by several years of zeroes. Thus, each zero appears to cause the next. There is also no a priori reason to suspect a dynamic relationship between incidents in one year and the next. Nevertheless, we present results for both static and dynamic models using both exponential feedback and log feedback. As seen in Table 4, the results remain virtually unchanged when we add lagged dependent variables.

The Poisson model also has an important robustness property. Even when the Poisson distribution does not hold, the maximum likelihood estimates of the regression parameters are consistent and asymptotically normal (Wooldridge, 2000). If the equidispersion assumption of the Poisson model is violated, the parameter estimates remain consistent but are inefficient, and the standard errors are underestimated (Gourieroux et al., 1984). In this case, the negative binomial model is appropriate. The negative binomial has the same mean as the Poisson, but the variance is no longer required to be equal to the mean.

To generate the negative binomial, we assume that the Poisson parameter μ_{it} is distributed according to gamma with parameters η_{it},δ with $\eta_{it} = \exp(\gamma_i + x_{it}\beta)$ and δ constant across states and years (Hausman et al., 1984, p. 922). The mean is $E(\mu_{it}) = \eta_{it} \mid \delta$, and the variance is $V(\mu_{it}) = \eta_{it} \mid \delta^2$. Taking the gamma distribution for μ_{it} and integrating by parts yields the negative binomial distribution with parameters η_{it} and δ.

$$pr(y_{it})\frac{\Gamma(\eta_{it} + y_{it})}{\Gamma(\eta_{it})\Gamma(y_{it} + 1)}\left(\frac{\delta}{1+\delta}\right)^{\eta_{it}}(1+\delta)^{-y_{it}} \tag{8}$$

Examination of the state-level means and variances, which are more relevant here because the fixed effects model ignores variation across states and uses only within-state variation to estimate the parameters, reveals that the number of shooting incidents variable satisfies the equidispersion property almost perfectly. See Table 2.

On the other hand, in many states, the numbers of people killed and wounded have variances much larger than the means. In fact, the variances for the number killed and wounded average 10 times the means for these two variables. For this reason, we use the negative binomial as our basic regression model and use the Poisson model as a robustness check, especially for the incidents variable. We estimate all models using Stata 7.0 for Windows (Stata Corporation, 2001).

TABLE 2
Means and Variances of the Dependent Variables, by State

State	*Mean Incidents*	*Variance Incidents*	*Mean Killed*	*Variance Killed*	*Mean Wounded*	*Variance Wounded*
Alabama	0.304	0.209	0.565	4.075	0.000	0.000
Alaska	0.174	0.241	1.043	8.680	0.087	0.174
Arizona	0.174	0.150	0.913	4.992	0.000	0.000
Arkansas	0.087	0.083	0.391	1.704	0.565	4.621
California	0.739	1.292	6.087	112.0	3.565	65.98
Colorado	0.174	0.150	1.130	8.937	1.130	27.11
Connecticut	0.043	0.043	0.174	0.696	0.000	0.000
Delaware	0.000	0.000	0.000	0.000	0.000	0.000
Florida	0.348	0.237	2.000	8.909	1.739	17.74
Georgia	0.043	0.043	0.522	6.261	0.522	6.261
Hawaii	0.043	0.043	0.304	2.130	0.000	0.000
Idaho	0.043	0.043	0.174	0.696	0.043	0.044
Illinois	0.087	0.083	0.478	2.715	0.087	0.174
Indiana	0.043	0.043	0.174	0.696	0.000	0.000
Iowa	0.043	0.043	0.217	1.087	0.043	0.044
Kansas	0.000	0.000	0.000	0.000	0.000	0.000
Kentucky	0.130	0.119	0.739	4.202	0.739	6.565
Louisiana	0.043	0.043	0.174	0.696	0.174	0.696
Maine	0.000	0.000	0.000	0.000	0.000	0.000
Maryland	0.000	0.000	0.000	0.000	0.000	0.000
Massachusetts	0.130	0.119	0.565	2.257	0.130	0.210
Michigan	0.130	0.119	0.522	1.897	0.435	2.893
Minnesota	0.043	0.043	0.174	0.696	0.000	0.000
Mississippi	0.087	0.174	0.391	3.522	0.130	0.391
Missouri	0.174	0.150	0.739	2.747	0.130	0.210
Montana	0.000	0.000	0.000	0.000	0.000	0.000
Nebraska	0.000	0.000	0.000	0.000	0.000	0.000
Nevada	0.043	0.043	0.174	0.696	0.000	0.000
New Hampshire	0.043	0.043	0.174	0.696	0.174	0.696
New Jersey	0.087	0.083	0.348	1.328	0.043	0.044
New Mexico	0.087	0.083	0.391	1.704	0.000	0.000
New York	0.304	0.221	1.391	4.794	1.087	13.44
North Carolina	0.130	0.119	0.522	1.897	0.609	3.704
North Dakota	0.000	0.000	0.000	0.000	0.000	0.000
Ohio	0.043	0.043	0.174	0.696	0.000	0.000
Oklahoma	0.174	0.150	1.217	10.45	0.261	1.565
Oregon	0.130	0.119	0.609	2.704	2.000	40.81
Pennsylvania	0.087	0.083	0.348	1.328	0.087	0.083
Rhode Island	0.043	0.043	0.174	0.696	0.000	0.000
South Carolina	0.043	0.043	0.174	0.696	0.130	0.391
South Dakota	0.000	0.000	0.000	0.000	0.000	0.000
Tennessee	0.043	0.043	0.174	0.696	0.000	0.000
Texas	0.696	1.130	4.217	61.08	2.304	28.31
Utah	0.000	0.000	0.000	0.000	0.000	0.000
Vermont	0.000	0.000	0.000	0.000	0.000	0.000
Virginia	0.043	0.043	0.174	0.696	0.000	0.000

TABLE 2 Continued

State	*Mean Incidents*	*Variance Incidents*	*Mean Killed*	*Variance Killed*	*Mean Wounded*	*Variance Wounded*
Washington	0.043	0.043	0.174	0.696	1.000	23.00
West Virginia	0.000	0.000	0.000	0.000	0.000	0.000
Wisconsin	0.000	0.000	0.000	0.000	0.000	0.000
Wyoming	0.000	0.000	0.000	0.000	0.000	0.000
Mean	0.099	0.116	0.558	5.409	0.344	4.904

RESULTS

Negative binomial regression estimates of the impact of RTC laws on mass public shootings appear in Tables 3 and 4. We estimated six separate negative binomial models for each dependent variable: two static models (with and without year dummies); two dynamic models with lagged, logged dependent variables included as regressors (with and without year dummies); and two dynamic models with lagged, unlogged dependent variables included as regressors (with and without year dummies). Because the results from these six separate equations were almost identical, we only present the full set of results for the static fixed-effects negative binomial model. We consider this model specification to be most appropriate because the year dummies tended to be significant as a group. Table 4 includes only the coefficients for the RTC variables obtained in the static models and the remaining dynamic models.

The negative binomial regression estimates in Tables 3 and 4 provide little evidence that RTC laws increase or reduce the number of mass public shootings (Table 3, column 1, Table 4, panel A). Although the coefficients on the RTC step dummy variables are generally in the negative direction, consistent with the hypothesis that passage of an RTC law immediately reduces mass public shootings, none of the point estimates are even close to being significant at the .05 level or the more generous .10 level. In addition, the coefficients on the RTC time trend variable give no indication that the deterrent impact of RTC laws on mass public shootings grows stronger over time as more people obtain carry permits. The coefficients are far from significant at the .05 level, and they do not always have the expected negative sign.

(text continues on pg. 289)

TABLE 3
The Impact of Right-to-Carry (RTC) Handgun Laws on Mass Public Shootings, 50 States, 1976 to 1999

	Static Fixed-Effects Negative Binomial Model								
	Mass Public-Shooting Incidents			*People Killed During Mass Public Shootings*			*People Wounded During Mass Public Shootings*		
Independent Variable	*Coefficient*	*Incident Rate Ratio*	z	*Coefficient*	*Incident Rate Ratio*	z	*Coefficient*	*Incident Rate Ratio*	z
RTC step dummy variable	–.401	.670	–0.78	–.418	.658	–0.87	–.279	.756	–0.47
RTC time trend variable	.021	1.02	0.22	–.038	.963	–0.46	.055	1.06	0.51
Overall time trend variable	–.356*	.701	–2.07	–.117	.889	–0.98	–.045	.956	–0.29
State population	.001*	5.20[a]	2.63	.000	3.77	0.43	.000	4.23	0.32
Percentage of population age 15 to 17	–18.32	.833	–0.16	58.66	1.80	0.58	–25.68	.774	–0.17
Percentage of population age 18 to 19	49.83	1.65	0.35	36.70	1.44	0.26	–217.30	.114	–1.09
Percentage of population age 20 to 24	–74.50	.475	–1.45	–108.83*	.337	–2.40	–59.14	.553	–0.88
Percentage of population age 25 to 29	10.19	1.11	0.21	55.38	1.74	1.23	82.52	2.28	1.26
Percentage of population age 30 to 34	77.34	2.17	1.25	52.96	1.70	1.04	43.58	1.55	0.58
Percentage of population age 35 to 44	–38.24	.682	–0.84	–84.80*	.428	–2.36	–92.30	.397	–1.81
Percentage of population age 45 to 54	114.19*	3.13	2.08	147.29*	4.36	3.06	97.46	2.65	1.65
Percentage of population age 55 to 64	69.36	2.00	1.27	–39.09	.676	–1.06	40.15	1.49	0.74
Percentage of population who live alone	.331	1.39	1.07	.198	1.22	1.61	–.345	.708	–1.32
Percentage of population who moved in last 5 years	.098	1.10	0.94	.098	1.10	1.95	–.099	.906	–1.65
Unemployment rate	–.095	.909	–0.68	–.198	.820	–1.67	–.251	.778	–1.59
Total employment	–.012	.988	–1.73	.000	1.00	–0.05	–.001	.999	–0.17
Per capita income, thousands	19.05*	1.873[b]	2.57	1.41	4.09	0.36	–.054	.947	–0.01

Poverty rate	.000	1.00	0.01	.002	1.00	0.03	–.072	.930	–0.86
Prison population	.824*	2.28	2.76	.878*	2.41	3.71	.608	1.84	1.92
Number of executions	–.127*	.881	–2.57	–.048	.953	–1.14	–.011	.989	–0.25
Model chi-square		71.30			101.93			42.05	
Log likelihood		–223.05			–423.93			–228.17	
Number of observations		888			888			624	

NOTE: These are static negative binomial fixed-effect regression estimates. The negative binomial model allows for extra-Poisson variation (i.e., overdispersion) in the dependent variables across states. The three columns between each dependent variable are the regression coefficients, incident rate ratios, and absolute *z* statistics. Due to space limitations, results for the year dummies are not presented.

a. Multiply coefficient by 100,000.

b. Multiply coefficient by 100,000,000.

$*p < .05$.

TABLE 4
Alternate Negative Binomial Fixed-Effect Model Specifications

	Right-to-Carry Law Step Dummy Variable			*Right-to-Carry Law Trend Variable*					
	Coefficient	*IRR*	*z*	*Coefficient*	*IRR*	*z*	*Dynamic/ Static*	*Feedback*[a]	*Year Dummies*
Panel A: Mass Public Shooting Incidents	–.551	.576	–1.13	.032	1.03	0.37	Static	no	no
	–.339	.712	–0.68	.078	1.08	0.83	Dynamic	log	no
	–.162	.850	–0.31	.075	1.08	0.74	Dynamic	log	yes
	–.377	.686	–0.75	.081	1.08	0.86	Dynamic	linear	no
	–.184	.832	–0.35	.078	1.08	0.76	Dynamic	linear	yes
Panel B: Number of People Killed During Mass Public Shootings	–.332	.717	–0.73	–.011	.989	–0.15	Static	no	no
	–.325	.723	–0.70	–.004	.996	–0.05	Dynamic	log	no
	–.407	.666	–0.84	–.031	.970	–0.36	Dynamic	log	yes
	–.326	.722	–0.71	–.001	.999	–0.02	Dynamic	linear	no
	–.405	.667	–0.84	–.024	.976	–0.28	Dynamic	linear	yes
Panel C: Number of People Injured During Mass Public Shootings	–.203	.816	–0.36	.051	1.05	0.49	Static	no	no
	–.175	.840	–0.29	.096	1.10	0.87	Dynamic	log	no
	–.169	.844	–0.26	.124	1.13	1.03	Dynamic	log	yes
	–.178	.837	–0.29	.111	1.12	1.01	Dynamic	linear	no
	–.216	.806	–0.34	.156	1.17	1.31	Dynamic	linear	yes

NOTE: These are alternate negative binomial fixed-effect regression estimates. They are similar to the negative binomial model reported in Table 3, except as noted.

a. The feedback column refers to the treatment of the lagged dependent variables. *No* means a static model with no lagged dependent variables; *log* means that mass public-shooting incidents, killings, and injuries are logged before being lagged; and *linear* refers to the exponential feedback model in which the lagged dependent variable is kept in its natural units. In all cases, we use two lags of the dependent variable.

*$p < .05$.

Although Tables 3 and 4 suggest that RTC laws do not deter mass shootings, it is still possible that the laws reduce the number of people killed and injured during these incidents. It is possible, for example, that perpetrators of mass public shootings choose smaller public venues where the probability of coming into contact with armed citizens is lower. That is, whereas RTC laws do not appear to deter shooters from acting, they may discourage offenders from committing their attacks in larger public venues where the likelihood that one or more potential victims are armed is very high. Negative binomial regression estimates with the number of people killed and wounded in mass public shootings as dependent variables are presented in Table 3, columns 2 and 3, and Table 4, panels B and C. There is no evidence that the presence of an RTC law or the number of people with carry permits (as measured by the RTC time trend variable) reduces the number of people killed and injured during shooting attacks. The signs of the coefficients on the RTC law variables are about evenly divided between positive and negative, and none are statistically significant at the .05 level. In all, the results in Tables 3 and 4 provide little support for Lott and Landes's (2000) hypothesis that RTC laws deter prospective shooters from going on shooting sprees in public places. There is also no evidence that RTC laws increase the number of mass public shootings by making it easier for prospective shooters to carry guns in public places.

Some readers might find the results in Tables 3 and 4 unconvincing because of the rarity of mass public-shooting incidents involving four or more deaths, resulting in insufficient power to detect an impact of RTC laws on mass public shootings, even if one existed. However, negative binomial regression is designed specifically for low probability events, and all three of our dependent variables are obvious candidates for the negative binomial distribution. Also, results for many of the control variables are highly significant in Table 3, which would have been unlikely if our sample did not have enough statistical power to detect significant relationships.

Robustness Checks

We believe the negative binomial fixed-effect model is the most appropriate model for the mass public-shooting data because the dependent variables are clearly overdispersed. Nevertheless, we present results of Poisson fixed-effect models for comparative purposes in Table 5. To simplify the table and to ease interpretation, we only present the coefficients, incident rate ratios, and *z* statistics for the key RTC law variables. As seen in Table 5, the regressions with Poisson models produced results similar to the negative binomial models, except that the coefficients on the RTC time trend variable are all significant and positive in the killing and wounded models. These results are very different from those reported in Table 4. This may be the result of modeling overdispersed data with Poisson regression models producing biased standard errors and inflated *t* statistics.[6]

Finally, we examined whether differences in conceptual definitions of mass public shootings are largely responsible for the disparate results between our study and those obtained by Lott and Landes (2000) for four or more victims. To examine this possibility, we reran the model specifications estimated in Tables 3 and 4 but excluded felony-related shootings involving innocent victims and bystanders. As seen in Table 6, excluding felony-related shootings from our definition has little impact on the results. None of the coefficients on the RTC variables are significant at the .05 level, and many are actually smaller in magnitude than those obtained with felony-related shootings included (see Tables 3 and 4).

CONCLUSION

Using pooled cross-section and time-series data across states for 1976 to 1999, as well as numerous model specifications for count data, we find, at best, weak evidence that RTC laws increase or decrease the number of mass public shootings. Although there is some support for the hypothesis that the number of people killed and wounded in such incidents increases over time, these increases occurred only for the Poisson regression model specifications. Because the number of people killed and wounded in mass public shootings does not satisfy the Poisson model assumption of equidispersion, which leads to incorrect standard errors and inflated *t* statistics, one might dismiss these results

TABLE 5
Poisson Fixed-Effect Model Specifications

	Right-to-Carry Law Step Dummy Variable			*Right-to-Carry Law Trend Variable*					
	Coefficient	*IRR*	*z*	*Coefficient*	*IRR*	*z*	*Dynamic/ Static*	*Feedback*[a]	*Year Dummies*
Panel A: Mass Public-Shooting Incidents	–.560	.590	–1.17	.032	1.04	0.38	Static	no	no
	–.409	.667	–0.82	.029	1.03	0.32	Static	no	yes
	–.351	.739	–0.61	.073	1.08	0.87	Dynamic	log	no
	–.176	.845	–0.33	.082	1.09	0.87	Dynamic	log	yes
	–.388	.713	–0.69	.076	1.09	0.90	Dynamic	linear	no
	–.199	.827	–0.37	.084	1.09	0.85	Dynamic	linear	yes
Panel B: Number of People Killed During Mass Public Shootings	–.720*	.485*	–3.34	.091*	1.10*	2.54	Static	no	no
	–.420	.637	–1.85	.085*	1.09*	2.13	Static	no	yes
	–.441*	.658*	–1.95	.121*	1.13*	3.17	Dynamic	log	no
	–.130	.857*	–0.66	.135*	1.14*	3.15	Dynamic	log	yes
	–.567*	.574*	–2.53	.130*	1.14*	3.40	Dynamic	linear	no
	–.258	.755*	–1.21	.144*	1.15*	3.38	Dynamic	linear	yes
Panel C: Number of People Injured During Mass Public Shootings	–.479	.604	–1.74	.100*	1.11*	2.12	Static	no	no
	–.383	.608	–1.22	.186*	1.25*	3.46	Static	no	yes
	–.347	.732	–1.01	.125*	1.12*	2.20	Dynamic	log	no
	.394	1.47	1.02	.346*	1.42*	4.73	Dynamic	linear	yes
	–.446	.661	–1.45	.187*	1.20*	3.24	Dynamic	linear	no
	.119	1.09	0.24	.404*	1.51*	5.46	Dynamic	linear	yes

NOTE: These are Poisson fixed-effects regression estimates. They are similar to the negative binomial regression estimates reported in Tables 3 and 4, except they assume equality between the mean and the variance for each dependent variable (i.e., equidispersion property of the Poisson).
a. The feedback column refers to the treatment of the lagged dependent variables. *No* means a static model with no lagged dependent variables; *log* means that mass public-shooting incidents, killings, and injuries are logged before being lagged; and *linear* refers to the exponential feedback model in which the lagged dependent variable is kept in its natural units. In all cases, we use two lags of the dependent variable.
*$p < .05$.

TABLE 6
The Impact of Right-to-Carry (RTC) Concealed Handgun Laws on Non-Felony Related Mass Public Shootings

	Right-to-Carry Law Step Dummy Variable			*Right-to-Carry Law Trend Variable*					
	Coefficient	*IRR*	*z*	*Coefficient*	*IRR*	*z*	*Dynamic/ Static*	*Feedback*[a]	*Year Dummies*
Panel A: Mass Public-Shooting Incidents	–.726	.484	–1.16	.165	1.18	1.37	Static	no	no
	–.718	.488	–1.09	.191	1.21	1.48	Static	no	yes
	–.501	.606	–0.80	.191	1.21	1.54	Dynamic	log	no
	–.385	.681	–0.56	.225	1.25	1.47	Dynamic	log	yes
	–.529	.589	–0.67	.193	1.21	1.74	Dynamic	linear	no
	–.397	.672	–0.58	.227	1.25	1.66	Dynamic	linear	yes
Panel B: Number of People Killed During Mass Public Shootings	–.126	.881	–0.22	.130	1.14	1.24	Static	no	no
	–.077	.925	–0.12	.111	1.12	1.00	Static	no	yes
	–.074	.928	–0.13	.129	1.14	1.23	Dynamic	log	no
	–.026	.975	–0.04	.114	1.12	0.99	Dynamic	log	yes
	–.106	.900	–0.18	.127	1.14	1.22	Dynamic	linear	no
	–.033	.968	–0.05	.117	1.12	1.03	Dynamic	linear	Yes
Panel C: Number of People Injured During Mass Public Shootings	–.116	.890	–0.19	.138	1.15	1.25	Static	no	no
	–.273	.761	–0.42	.160	1.17	1.39	Static	no	yes
	.091	1.10	0.14	.120	1.13	1.07	Dynamic	log	no
	.103	1.11	0.15	.178	1.20	1.43	Dynamic	linear	yes
	.085	1.09	0.13	.137	1.15	1.22	Dynamic	linear	no
	.039	1.04	0.06	.212	1.24	1.71	Dynamic	linear	yes

NOTE: These are negative binomial fixed-effect regression estimates. They are similar to the negative binomial models reported in Tables 3 and 4 but exclude felony-related shootings.

a. The feedback column refers to the treatment of the lagged dependent variables. *No* means a static model with no lagged dependent variables; *log* means that mass public-shooting incidents, killings, and injuries are logged before being lagged; and *linear* refers to the exponential feedback model in which the lagged dependent variable is kept in its natural units. In all cases, we use two lags of the dependent variable.

altogether. Under this interpretation, RTC laws have no effect on mass public shootings at all.

We should note, however, that our study only estimated the average impact of RTC laws on mass public shootings across all states. Given the large differences in RTC laws across states with respect to where citizens can carry guns, training requirements required to obtain a permit, and permit fees, this average impact may have obscured or averaged out of existence major between-state variations in the effects of the laws. Indeed, recent research by Pesaran and Smith (1995) and Baltagi and Griffin (1997) suggests that the assumption in pooled regression models—that coefficients on key variables are the same for each ecological unit—is probably not justified. It is also possible, for example, that RTC laws can both increase and reduce the number of mass public shootings. The results for any one state might reflect the net effect of the opposing factors in that state. Finally, many of the laws analyzed here are relatively new, and analyses in the future after states have more experience with these laws might well find a significant and negative effect.

NOTES

1. Of the victim thresholds used in previous research, we believe that four allows us to virtually eliminate any measurement error for the mass public-shooting variables. Consider, for example, that a two-victim criterion would produce about 18 times as many cases as a four-victim threshold, whereas a three-victim requirement would yield roughly three times as many cases. However, a two- or three-victim criterion would increase the risk of underreporting because incidents with smaller body counts are not only more numerous, they are also less newsworthy, which is an important consideration given that this study relies on news coverage as a source of data. Indeed, previous research has shown that the number of victims killed has a significant positive effect on the newsworthiness of a homicide (Duwe, 2000; Johnstone, Hawkins, & Michener, 1994; Wilbanks, 1984). Therefore, even though two- or three-victim public shootings often receive media coverage, these cases are still more likely to go unreported than those involving four or more victims.

2. We began our search for news reports on the 55 cases by using connected search terms that included the name of the city or county in which the mass shooting occurred along with a descriptive word such as *murder*, *shot*, *shooting*, or *homicide*. To cut down on the number of articles returned, we narrowed the time frame of the search to cover only the month (as indicated by the SHR) in which the mass shooting took place. With a few incidents, however, we had to broaden the search to cover all of 1997 to 1999 to find newspaper articles on these cases. To account for the possibility that some cases were not reported to the SHR, we performed a search for these years, using the terms *mass shooting*, *public shooting*, and *mass public shooting*. This approach yielded an additional three mass public-shooting incidents that were not reported to the SHR.

3. The dates are as follows: Alaska (1994), Arizona (1995), Arkansas (1995), Florida (1987), Georgia (1989), New Hampshire (1994), Idaho (1990), Indiana (1980), Kentucky (1996), Louisiana (1996), Mississippi (1990), Maine (1980), Montana (1991), Nevada (1995), North Carolina (1995), Oklahoma (1996), Oregon (1990), Pennsylvania (1989), Tennessee (1994), Virginia (1983), South Carolina (1996), Texas (1995), Utah (1995), West Virginia (1988), and Wyoming (1994). Six states had RTC laws or their equivalent prior to 1976 (Alabama, Connecticut, North Dakota, South Dakota, Vermont, and Washington).

4. The six states that had RTC laws or their equivalent prior to 1976 (see note 3) were coded as zero because the effect of the law is captured by the state dummy variable.

5. Another reason why step dummies might underestimate the impacts of RTC laws is because of simultaneity bias; mass public shootings may partly explain why some states pass RTC laws. If, in fact, RTC laws are endogenous and determined at least in part by a state's experiences with mass public shootings, then any negative impacts of RTC laws on shooting attacks would be masked by the positive impacts of shooting attacks on the passage of RTC laws. In the present situation, however, it is unlikely that shooting attacks play any role in the passage of RTC laws because of the large lag between shooting attacks and the passage of RTC laws, which would be unlikely if shooting attacks influenced the passage of RTC laws.

6. The positive coefficient on the RTC time trend variable is probably nothing more than a statistical artifact because the number of incidents is already quite small and can never go below zero. It is highly unlikely that the increase in mass public shootings can be attributed to the increasing number of people with carry permits, given the infrequent use of firearms by permit holders in the commission of violent crime, especially for homicide (see Lott, 2000).

REFERENCES

Baltagi, B. H., & Griffin, J. M. (1997). Pooled estimators vs. their heterogeneous counterparts in the context of dynamic demand for gasoline. *Journal of Econometrics*, *77*, 303-327.

Bartley, W. A., & Cohen, M. A. (1998). The effect of concealed weapons laws: An extreme bound analysis. *Economic Inquiry*, *36*, 258-265.

Black, D. A., & Nagin, D. S. (1998). Do right-to-carry laws deter violent crime? *Journal of Legal Studies*, *27*, 209-219.

Cameron, A. C., & Trivedi, P. K. (1998). *Regression analysis of count data*. Cambridge, UK: Cambridge University Press.

Campbell, D., & Stanley, J. (1963). *Experimental and quasi-experimental designs for research*. Boston: Houghton Mifflin.

Cook, P. J. (1991). The technology of personal violence. In M. Tonry (Ed.), *Crime and justice* (pp. 1-71). Chicago: University of Chicago Press.

Dezhbaksh, H., & Rubin, P. H. (1998). Lives saved or lives lost? The effects of concealed-handgun laws on crime. *American Economic Review*, *88*, 468-474.

Dietz, P. E. (1986). Mass, serial, and sensational homicides. *Bulletin of the New York Academy of Medicine*, *62*, 477-490.

Duwe, G. (2000). Body-count journalism: The presentation of mass murder in the news media. *Homicide Studies*, *4*, 364-399.

Ehrlich, I. (1975). The deterrent effect on capital punishment: A question of life and death. *American Economic Review*, *65*, 397-417.

Espy, M. W., & Smykla, J. O. (1994). Executions in the United States, 1608-1991: The Espy file (3rd ICPSR ed.). In *Violence research data* (CD ROM). Ann Arbor, MI: Inter-University Consortium for Political and Social Research.

Fox, J. A., & Levin, J. (1994). *Overkill: Mass and serial killing exposed*. New York: Plenum.

Fox, J. A., & Levin, J. (1998). Multiple homicide: Patterns of serial and mass murder. In M. Tonry (Ed.), *Crime and justice: A review of research* (pp. 407-455). Chicago: University of Chicago Press.

Gourieroux, C., Monfort, A., & Trognon, A. (1984). Pseudo maximum likelihood methods: Applications to Poisson models. *Econometrica*, *52*, 701-720.

Hausman, J. B., Hall, H., & Griliches, Z. (1984). Econometric models for count data with an application to the patents-R&D relationship. *Econometrica*, *52*, 909-938.

Holmes, R. M., & Holmes, S. T. (1992). Understanding mass murder: A starting point. *Federal Probation*, *56*, 53-60.

Hsiao, C. (1986). *Analysis of panel data*. New York: Cambridge University Press.

Johnstone, J. W. C., Hawkins, D. F., & Michener A. (1994). Homicide reporting in Chicago dailies. *Journalism Quarterly*, *71*, 860-872.

Kleck, G. (1997). *Targeting guns: Firearms and their control*. New York: Aldine de Gruyter.

Kleck, G., & Gertz, M. (1995). Armed resistance to crime: The prevalence and nature of self-defense with a gun. *Journal of Criminal Law and Criminology*, *86*, 150-187.

Kleck, G., & Gertz, M. (1998). Carrying guns for protection: Results from the national self-defense survey. *Journal of Research in Crime and Delinquency*, *35*, 193-224.

Kovandzic, T. V., Vieraitis, L. M., & Yeisley, M. R. (1998). The structural covariates of urban homicide: Reassessing the impact of income inequality and poverty in the post-Reagan era. *Criminology*, *36*, 569-599.

Land, K. C., McCall, P. L., & Cohen, L. E. (1990). Structural covariates of homicide rates: Are there any invariances across time and social space? *American Journal of Sociology*, *95*, 922-963.

Layson, S. K. (1985). Homicide and deterrence: A reexamination of United States time-series evidence. *Southern Economic Journal*, *52*, 68-89.

Levin, J., & Fox, J. A. (1985). *Mass murder: America's growing menace*. New York: Berkley Books.

Levin, J., & Fox, J. A. (1996). A psycho-social analysis of mass murder. In T. O'Reilly-Fleming (Ed.), *Serial and mass murder: Theory, research, and policy* (pp. 55-76). Toronto: Canadian Scholars' Press.

Long, J. S. (1997). *Regression models for categorical and limited dependent variables*. Thousand Oaks, CA: Sage.

Lott, J. R., Jr. (2000). *More guns less crime*. Chicago: University of Chicago Press.

Lott, J. R., Jr., & Landes, W. M. (2000). *Multiple victim public shootings*. Unpublished paper, available from the Social Science Research Network at http://papers.ssrn.com/sol3/papers.cfm?abstract_id=272929

Lott, J. R., Jr., & Mustard, D. B. (1997). Crime, deterrence, and right-to-carry concealed handguns. *Journal of Legal Studies*, *26*, 1-68.

Ludwig, J. (1998). Concealed-gun-carrying laws and violent crime: Evidence from state panel data. *International Review of Law and Economics*, *18*, 239-254.

Marvell, T. B. (1999). *Outline of remarks concerning Lott and Mustard evaluation of ten "shall-issue" handgun permit laws*. Paper presented at the annual meeting of the American Society of Criminology, Toronto, Canada.

Marvell, T. B., & Moody, C. E. (1996). Specification problems, police levels, and crime rates. *Criminology*, *34*, 609-646.

Marvell, T. B., & Moody, C. E. (1997). The impact of prison growth on homicide. *Homicide Studies*, *1*, 205-233.

McDowall, D., Loftin, C., & Wiersema, B. (1995). Easing concealed firearms laws: Effects on homicide in three states. *Journal of Criminal Law and Criminology*, *86*, 193-206.
Mundlak, Y. (1978). On the pooling of time series and cross section data. *Econometrica*, *46*, 69-86.
Nickell, S. (1981). Biases in dynamic models with fixed effects. *Econometrica*, *49*, 1399-1416.
Pesaran, H. M., & Smith, R. (1995). Estimating long-run relationships from dynamic heterogeneous panels. *Journal of Econometrics*, *68*, 79-113.
Petee, T. A., Padgett, K. G., & York, T. S. (1997). Debunking the stereotype: An examination of mass murder in public places. *Homicide Studies*, *1*, 317-337.
Phillips, D. P. (1980). The deterrent effect of capital punishment: Evidence on an old controversy. *American Journal of Sociology*, *86, 139-148.*
Rappaport, R. G. (1988). The serial and mass murderer: Patterns, differentiation, pathology. *American Journal of Forensic Psychiatry*, *9, 39-48.*
Ressler, R. K., Burgess, A. W., & Douglas, J. E. (1988). *Sexual homicide: Patterns and motives*. New York: Lexington Books.
Stata Corporation. (2001). Stata statistical software (Release 6.0) [Computer software]. College Station, TX: Author.
Stephan, J. J., & Snell, T. L. (1996). *Capital punishment, 1994*. Washington, DC: Bureau of Justice Statistics.
Wilbanks, W. (1984). *Murder in Miami: An analysis of homicide patterns and trends in Dade County (Miami) Florida, 1917-1983*. Lanham, MD: University Press of America.
Wooldridge, J. M. (2000). *Introductory econometrics: A modern approach*. Mason, OH: South-Western College Publishing.
Zimring, F. E. (1968). Is gun control likely to reduce violent killings? *University of Chicago Law Review*, *35*, 721-737.

Grant Duwe *is a Ph.D. candidate in the School of Criminology and Criminal Justice at Florida State University. His interests include research methods, multiple murder, the media and crime, and the social construction of crime problems.*

Tomislav Kovandzic *is an assistant professor in the Department of Justice Sciences at the University of Alabama at Birmingham. His research interests include criminal justice policy and gun-related violence. His most recent articles have appeared in* Criminology *and* Journal of Criminal Justice. *He received the Ph.D. in Criminology from the Florida State University in 1999.*

Carlisle E. Moody *is professor of economics and chairman of the Economics Department at the College of William and Mary. He specializes in econometric studies of energy and crime.*