Multiple Victim Public Shootings, Bombings, and Right-to-Carry Concealed Handgun Laws: Contrasting Private and Public Law Enforcement

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I. Introduction

Few events obtain the same instant national and even worldwide news coverage as when several people are shot and killed in a public place. The worst examples in the United States come readily to mind: Colin Ferguson killed 6 people during his rampage on the Long Island (NY) Railroad in December 1993; 22 people were killed during the October 1991 shooting in Luby's Cafeteria in Killeen, Texas; 5 persons died at the Cleveland Elementary School in Stockton, California in January 1989; and 21 persons were killed at the San Ysidro, California McDonald's in July, 1984. Shootings by disgruntled post office employees, has led to the phrase "going postal" entering our language. And with the recent public school shootings, a sense of urgency has also been added to the discussion.

The most common suggestion for reducing the incidence of public shootings (the term we use to denote shootings in public places where two or more individuals are killed or injured) calls for greater regulation of guns. For example, recent public shootings in Tasmania, Australia and Dunblane, Scotland have lead to strict gun

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prohibitions in those countries. And after a 1997 shooting at the top of the Empire State Building in which one person was killed, the Mayor of New York called for national gun licensing laws. Other laws restricting access to guns, such as waiting periods, are often justified as producing a cooling off period to prevent shooting sprees. Yet, the response to these shootings has not been uniform. In Texas and other states, multiple shooting incidents may have helped pave the way for passage of concealed handgun laws permitting law-abiding citizens to carry handguns. Terrorist shootings in Israel have lead to wider licensing of citizens to carry concealed handguns. (In this paper, we use the term "shall issue law" or "right-to-carry" to denote a state law that sets up objective criteria for a law-abiding citizen to obtain a permit to carry a concealed handgun.)

Not surprisingly, those opposed to concealed handgun laws point to the loss of life and injuries that result from these shootings. Their argument is straightforward: "If you introduce a gun into a violent encounter, it increases the chance that someone will die."[2] Since a large number of murders may arise from fits of rage that are quickly regretted, keeping guns out of people's reach (even temporarily) might prevent deaths in many instances.[3] Shootings in public places may be the most visible manifestation of individuals who might have refrained from such acts but for having access to guns. For example, in the recent rash of school shootings in 1997 and 1998, the perpetrators obtained their guns from relatives or neighbors. Had no guns been accessible, the acts may not have been committed.

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[2] Philip Cook quoted in Editorial, *Cincinnati Enquirer*, Jan. 23, 1996, A8. Others share this belief. "It's common sense," says Doug Weil, research director at the Center to Prevent Handgun Violence, in Washington. "The more guns people are carrying, the more likely it is that ordinary confrontations will escalate into violent confrontations" (William Tucker, "Maybe You Should Carry A Handgun," *The Weekly Standard*, Dec. 16, 1996, p. 30).

[3] See P. J. Cook, "The Role of Firearms in Violent Crime," in M.E. Wolfgang and N.A. Werner, eds., *Criminal Violence*, Sage Publishers: Newbury, N.J.(1982) and Franklin Zimring, "The Medium is the Message: Firearm Caliber as a Determinant of Death from Assault," *Journal Legal Studies*, 1 (1972) for these arguments.

In contrast, proponents of concealed handgun laws point to the potential use of guns for defensive purposes. They argue that the prospect of encountering a victim who is armed may deter a criminal from an attack in the first place. National polls indicate that people use guns defensively against criminal attacks somewhere between 760,000 and 3.5 million times per year.[4] Data from the Department of Justice's National Crime Victimization Survey from 1979 to 1987 indicate that the risk of serious injury from a criminal attack is 2.5 times greater for women offering no resistance than for women resisting with a gun (Southwick, 1996).[5] The most comprehensive study of laws permitting individuals to carry concealed weapons indicates that these laws reduce murder rates by about 10 percent, with similar declines in other violent crimes (Lott 1998b but see related comments by Bartley et. al., 1998; Black and Nagin, 1998; Bronars and Lott, 1998; Plassman and Tideman, 1998; Lott and Mustard, 1997; and Lott, 1998a). And contrary to a popular misconception, the use of concealed handguns by permit holders are virtually never involved in the commission of crime, let alone murder (Lott, 1998b).[6]

Just as advocates of greater regulation of guns point to shooting sprees that kill or maim many individuals as evidence supporting their position, opponents point to anecdotal evidence supporting more permissive handgun laws. During the Luby's Cafeteria shooting one of the restaurant's patrons, an expert marksman, had left her handgun in her car to comply with the Texas state law existing at the time. In an incident in 1997, a gunman in a Florida restaurant was prevented from shooting people by the quick

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[4] Kleck, Gary, and Marc Gertz, "Armed Resistance to Crime: The Prevalence and Nature of Self-Defense with a Gun," 86 *Journal of Criminal Law and Criminology* 86 (Fall 1995).

[5] There are problems with the National Crime Victimization Survey both in terms of its nonrepresentative sample (for example, it weights too heavily urban and minority populations) and its failure to adjust for many people not admitting to a law enforcement agency that they used a gun, even defensively. Unfortunately, this survey provides the only available evidence how the probability of significant injury varies with level and type of resistance.

[6] We add that no data are available on whether handguns lawfully bought by permit holders are used in crimes by another party at a later date.

reactions of two people with concealed handguns.[7] (See Lott (1998b) for a more complete list of such cases.) Internationally, similar examples can be found. On March 13, 1997, a Jordanian soldier shot seven young Israeli girls to death while they were visiting Jordan's so-called "Island of Peace." According to newspaper reports, the Israelis had "complied with Jordanian requests to leave their weapons behind when they entered the border enclave. Otherwise, they might have been able to stop the shooting, several parents said."[8]

Referring to the July 1984 massacre at a San Ysidro, California, McDonald's restaurant, Israeli criminologist Abraham Tennenbaum wrote that:

> what occurred at a [crowded venue in] Jerusalem some weeks before the California McDonald's massacre: three terrorists who attempted to machine-gun the throng managed to kill only one victim before being shot down by handgun carrying Israelis. Presented to the press the next day, the surviving terrorist complained that his group had not realized that Israeli civilians were armed. The terrorists had planned to machine-gun a succession of crowd spots, thinking that they would be able to escape before the police or army could arrive to deal with them.[9]

Obviously allowing Israeli citizens to carry concealed handguns has not eliminated terrorist attacks. Indeed, terrorists may well have responded to the difficulty of successfully shooting civilians in public places by substituting bombings, which allow potential victims little chance to respond.

Anecdotal evidence cannot resolve the question whether allowing persons to carry concealed handguns will save or cost lives. In this study, we provide a systematic empirical analysis of the effects of different gun laws on multiple victim public shootings. We focus

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[7] Allison Thompson, Robber Gets Outgunned on Westside," *Florida Times-Union* (Jacksonville, FL),September 24, 1997, p. B1.

[8] Rebecca Trounson, "Anxiety, Anger Surround Return of Young Survivors," *Los Angeles Times,* March 14, 1997, p. A1

[9] *Baltimore Sun,* Oct. 26, 1991. As referenced in an article by Don Kates and Dan Polsby. "Of Genocide and Disarmament," *Journal of Criminal Law and Criminology,* 86 (Fall 1995): 252.

primarily on nondiscretionary (or "shall issue") concealed handgun laws although we also look at evidence on the role of waiting periods and additional penalties imposed on individuals who use guns in the commission of a crime.

We analyze multiple victim public shootings (hereafter, multiple shootings or killings) in the United States in the period 1977 to 1995.[10] The main advantage of restricting our study to U.S. data is that we can compare states with and without shall issue laws at different points in time (other things constant) and, therefore, can estimate the effects of a change in the law within a state during the sample period. In contrast, time series data for a single country faces the problem that many different events may occur at around the same time which makes it difficult to disentangle the impact of a change in the law from other factors. And an international study across different countries faces problems in finding comparable data on gun laws, crime rates, and gun ownership. Our study ends in 1995 because many of the variables we use are not available after than date.

A few words about why we study multiple shootings. Aside from the public attention these acts receive, multiple shootings allow us to test the applicability of economics to an area believed to be far outside the domain of economics. Perpetrators of these acts are often thought to be psychotic or irrational and hence not responsive to costs and benefits. Thus, legal sanctions or, as in this case, the prospect of encountering an armed individual during a shooting spree would have no deterrent effect on these individuals. Indeed, the act itself is cited as powerful evidence of irrational or psychotic behavior since a sane person would never kill helpless victims in a

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[10] While the recent rash of public school shootings during the 1997-98 school took place after the period of our study, these incidents raise questions about the unintentional consequences of laws. The five public school shootings took place after a 1995 federal law banned guns (including permitted concealed handguns) within a thousand feet of a school. The possibility exists that attempts to outlaw guns from schools, no matter how well meaning, may have produced perverse effects. It is interesting to note that during the 1977 to 1995 period, 15 shootings took place in schools in states without right-to-carry laws and only one took place in a state with this type of law. There were 19 deaths and 97 injuries in states without the law, while there was one death and two injuries in states with the law.

public place. The claim is then made that a law permitting individuals to carry concealed weapons couldn't possibly deter shooting sprees in public places (though it might reduce the number of people killed or wounded). And since these laws might well increase the availability of guns to potential perpetrators, the combination of criminal irrationality and greater availability of guns might lead to an increase in the number of incidents of multiple shootings.

In contrast, the economic model of crime predicts that a shall issue law will raise the potential perpetrator's cost of acting (e.g., he might be wounded or killed if he acts) and lower his expected benefit (e.g., he will do less damage if he encounters armed resistance). Although not all offenders will alter their behavior in response to a shall issue law, some individuals will be deterred from carrying out a shooting spree because the resulting changes in costs and benefits will be sufficiently large to make their net gain from acting negative. How large the deterrent effect is depends on how many potential offenders are close enough to the margin so that the passage of a shall issue law changes the net benefit from positive to negative. Economics predicts, therefore, that shall issue laws will reduce the number of mass shootings (subject to the "greater availability of guns" qualification noted above) though the magnitude of this response is uncertain.

A study of multiple shootings also allows us to compare whether a shall issue law will produce a bigger deterrent effect on multiple shootings than on ordinary murders and other crimes. This may appear surprising in light of the claimed irrationality of individuals who go on shooting sprees. But another consideration points in the opposite direction. Suppose a shall issue law deters crime primarily by raising the probability that a perpetrator will encounter a potential victim who is armed. In a single victim crime, this probability is likely to be very low. Hence the deterrent effect of the law—though negative—might be relatively small. Now consider a shooting spree in a public place. The likelihood that one or more potential victims or bystanders are armed would be very large even though the probability that any particular individual is armed is very

low.[11] In principle, this suggests a testable hypothesis: a shall issue law will have a bigger deterrent effect on shooting sprees in public places than on more conventional crimes. Finally, because the presence of citizens with concealed handguns may be able to stop attacks before the police are able to arrive, our data also allows us to provide the first evidence that we know of concerning whether the severity of those crimes that still take place can be mitigated.

II. Multiple Victim Public Shootings: A First Look

We define a multiple shooting as one in which two or more people are killed or wounded in a public place. "Public place" refers to a shooting that occurred in a church, business, bar, street, government building, public transit, place of employment, park, health care facility, mall or restaurant. We collected data on these shootings from articles reporting multiple shootings during the 1977 to 1995 period in the Lexis/Nexis computerized database. Our sample does not include all multiple shootings in the database. We exclude the following: those that were the byproduct of another crime (e.g., a robbery or drug deal); shootings that involved gang activity (e.g., drive by shootings); professional hits or shootings related to organized crime; and serial killings or killings that took place over the span of more than one day.[12] Although there is no bright line between different types of multiple shootings, there are several reasons for limiting our sample as we do.

1. One of us has already studied the effects of shall issue laws on murder rates in general. Hence by adding shootings from robberies,

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[11] To illustrate, let the probability (p) that a single individual is carrying a concealed handgun equal .10. Assume further that there are 10 individuals in a public place. Then the probability that at least one of them is armed is about .65 $(= 1 - (.9)^{10})$.

[12] In a recent paper (see T. Petee, K. Padgett and T. York, Debunking the Stereotype: An Examination of Mass Murder in Public Places, 1 Homicide Studies 317 (1997)) the authors find felony related mass murders account for 36 percent and gang motivated 5.8 percent of mass murder incidents over the 1965 to 1995 period. That study defines mass murders as the killing three or more persons (so it has much fewer incidents than our sample).

gang activity and so forth to our sample we risk duplicating earlier work.

2. Shall issue laws permit law-abiding citizens to carry guns so these laws should have little impact on killings related to gang activity, drug deals and organized crime. Individuals involved in these activities are already engaged in unlawful activities that often require them to carry guns. In short, these persons carry guns independent of whether the law permits them to do so. Hence a "shall issue" should have little effect on their behavior.

3. Restricting our study to shootings in public places allows us to concentrate on places where economic theory suggests that shall issue laws will have their greatest effect.[13] We expect this because more citizens will be carrying concealed handguns in public places after a law allowing them to carry a gun has been passed. And a shall issue law should also be a more effective deterrent if the potential perpetrator either knows or is uncertain whether the intended victim is armed. Conversely the law should have no effect if the offender knows in advance that the victim is not armed. But such knowledge is unlikely for public places. So unless there are explicit restrictions on carrying guns in certain places (e.g., near schools), a shall issue law should increase the likelihood that a potential victim or bystander is armed.[14]

4. One can also question our definition of multiple shootings as requiring two or more killings or injuries. Later in the paper we look

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[13] Alschuler (1997, p. 369) claims that concealed handguns should only deter crimes involving strangers. Our response is that concealed handguns can deter crimes involving acquaintances as well as strangers, though deterrence involving acquaintances might be more easily thought of as similar to open carrying of guns. The big effect of concealed handguns is that they may allow people to be able to now defend themselves outside of their home or business. The passage of the concealed handgun laws may deter crimes against acquaintances simply to the extent to which it increases gun ownership.

[14] Most states allow private businesses to decide whether permit holders are allowed to carry concealed handguns on their premises. State rules may also vary with regard to other places such as government buildings, churches, and bars.

at alternative definitions requiring greater number of deaths and injuries.

Tables 1 and 2 present data on multiple shootings for the United States as a whole, and for states with and without shall issue laws. We find that states without shall issue laws had more deaths and injuries from multiple shootings per year (both in absolute numbers and on a per capita basis) during the 1977 to 1995 period. Note also that the number of states without shall issue laws declined from 43 to 29 and the percentage of the U.S. population in these states fell from 91.5 to 68 percent in this period. Yet states without shall issue laws still account for the overwhelming majority (often over 90 percent) of deaths and injuries. The different rates of shootings, murders, or injuries are very consistent over time and do not arise from a few unusual years.

Tables 3 and 4 look more closely at the 14 states that adopted shall issue laws between 1977 and 1995.[15] (No state has ever repealed this law.) Table 3 shows a sharp drop in multiple murders and injuries per 100,000 persons after the passage of a shall issue law. Murders fell by 89 percent and injuries by 82 percent. Table 4 indicates that this drop occurred largely during the first full year after a state enacted its law (year "1" in the first column). Overall, the decline is so large that we observe zero multiple killings in three of the eight years after the passage of a law, an event that did not occur

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[15] The fourteen states that enacted "shall issue" or "right-to-carry" laws in the 1977 to 1995 period (dates in parentheses) are as follows: Alaska (1994), Arizona (1994), Florida (1987), Georgia (1989), Idaho (1990), Maine (1985), Mississippi (1990), Montana (1991), Oregon (1990), Pennsylvania (1989), Tennessee (1994), Virginia (1988), West Virginia (1989), and Wyoming (1994). The following eight states had "shall issue" laws over the entire period: Alabama, Connecticut, Indiana, New Hampshire, North Dakota, South Dakota, Vermont and Washington. Data on states having laws prior to 1993 are from Clayton E. Cramer and David B. Kopel, Shall Issue: The New Wave of Concealed Handgun Permit Laws, 62 Tennessee Law Review, 679 (1995). We used a Nexis search to determine the state and date for states passing laws between 1993 and 1995. These two sources were also used in Lott and Mustard (1997). Because of objections raised to the dates for "shall issue" laws in Maine and Virginia (see the discussion in Lott and Mustard), the regression analysis presented in part III examines the sensitivity of our findings to alternative dates for Maine and Virginia.

during any year before passage of the law.[16] Table 3 also presents data on a narrower sample consisting of multiple shootings that appeared in the first section of the *New York Times* at the time the shootings took place. We use this sample as an estimate of more serious or, at least, more notorious multiple shootings. Similar to the data on all multiple shootings, the *New York Times* data show a decline of 91 percent in the rate of multiple shootings after a state adopted a shall issue law. Finally, we consider the possibility that shall issue laws lead criminals to substitute bombings for shootings. Data on bombings (from the Bureau of Alcohol, Tobacco and Firearms annual publication Arson and Explosives: Incidents Report) suggest no systematic impact on the number of bombings. After the passage of shall issue laws, actual and attempted bombings increased slightly, incendiary bombings fell and other bomb-related incidents (involving stolen explosives, threats to treasury facilities, and hoax devices) declined compared to the before law period.

III. Accounting for Other Factors

Although the above tables suggest that shall issue laws reduce mass shootings, other factors may explain these changes. To take account of this possibility, we estimated regression equations with the following state specific variables: the arrest rate for murder; real per capita personal income; real per capita government payments for income maintenance, unemployment insurance and retirement; the unemployment rate; the poverty rate; state population and population squared; and a set of demographic variables that subdivide a state's population into 36 different race, sex, and age groups. We also include year and state specific dummy variables in the regression analysis. Table 5 lists the variables included in the regression analysis. Thus our results hold constant both the effects of any national trends and state-specific effects on multiple shootings which may coincide with the adoption of shall issue laws. So, for example, if the multiple shooting rate declines nationally between two years, the regression coefficient on the law variable tests for whether the decline is relatively greater in states that adopted shall issue laws

[16] Of course, there were zero mass shootings in individual states in particular years before the passage of concealed handgun laws.

during the two year period. This approach may actually understate the impact of shall issue laws since the year dummy variables may also pick up some of the changes attributed to the increasing number of states that passed these laws.

Table 6 presents regressions for twelve different dependent variables (six for multiple shootings and six for bombings) using the simplest specifications of the shall issue law variable—a dummy law variable which equals one if a state has a concealed handgun or "shall issue' law and zero otherwise. The regression analysis contains 953 observations (50 states and the District of Columbia for 19 years minus 16 observations for various states and years in which we lacked data on the arrest rate).[17] To simplify the table, we only present the regression coefficients (and t-statistics) on the dummy law variable.

The results of Table 6 indicate that concealed handguns laws significantly reduce multiple shootings in public places (but have no systematic effects on bombings). For example, shall issue laws appear to lower the combined number of killings and injuries (equation (3)) in a state by 11.1 per 10 million people per year, or by more than 80 percent of a one standard deviation change in the murder and injury rate from multiple shootings. Equations (4) and (5) imply that the average state passing these laws reduces the number of murders and injuries by 6.9 and 6.5 persons respectively. Indeed these estimated effects are so large that they often exceed the annual average number of murders and injuries from public shootings in a state (either absolutely or per 100,000 persons). To be sure, we expect large deterrent effects from these laws because of the high probability that one or more potential victim or bystander will be harmed. Still the drop in murders and injuries is surprisingly large. And as we shall see, alternative measures of shootings and adding other control variables do not seem to reduce the magnitude of the law's effect.

Turning to the other variables in the regressions in Table 6, we find that states with larger populations have more multiple shooting

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[17] The states and years of the missing observations are as follows: Florida (1988); Illinois (1993-95); Iowa (1991); Kansas (1993-95); Kentucky (1988); Montana (1994-95); New Hampshire (1984 and 1995); Pennsylvania (1995) and Vermont (1978-79). As a further check on our results, we reestimated the regressions in Tables 6 and 7 deleting the arrest variable and adding the 16 missing observations. The coefficients and levels of significance on the shall issue law variable were virtually unchanged.

deaths and injuries per 100,000 persons though the rate increases at a decreasing rate. We also find that personal income, poverty and population density are insignificant while retirement payments and unemployment have positive and significant (or marginally significant) effects on the murder rate. Higher arrest rates for murder are associated with fewer multiple murders and killings but these results are never statistically significant[18] (The full regressions are available from the authors on request.)

Finally, notice that the number of bombings in Table 6 (with the exception of "other bombing incidents" in eq. 12) are not related to shall issue laws. Some types of bombings appear to rise and others fall after the passage of a law, the signs often depend on whether bombings are expressed as a rate or an absolute number, and five of the six coefficients are not statistically significant. In short, there does not appear any significant substitution between shootings and bombings in states enacting "shall issue" laws. (In the remaining tables we do not report the results for the bombing regressions because, in almost all cases, bombings are not significantly related to shall issue laws.)

Table 7 replaces the simple dummy law variable with two time trend law variables for those states that passed laws between 1985 (the first year a state passed a law during the 1977 to 1995 sample period) and 1995. The first is a time trend variable before passage of the law that takes the value 0 in the year the law is passed (and all years following passage), -1 in the year before passage, -2 in the second year before passage and so forth. The second is a post law variable that takes the value 0 in the year the law is passed (and 0 in all years before passage), 1 in the first year after passage and so on. The main reason for this specification is that we expect the impact of shall issue laws to increase over time as more people obtain permits. It may take many years after enacting a handgun law for states to reach their long run level of permits. For states in which

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[18] We note that the arrest rate variable understates the actual (or expected) arrest rate of individuals who go on shooting sprees. More than 90 percent of these offenders are either arrested or killed, which is slightly greater than the overall arrest rate for murder. The 90 percent figure (which comes from a Nexis search) represents perpetrators who were immediately captured or killed. We do not know whether those who escaped were apprehended later.

data on permits are available the share of the population with permits is still increasing a decade after the passage of the law (Lott, 1998b, p. 75).[19]

In Table 7, we find that deaths or injuries from mass shootings are rising before the passage of the law and falling afterwards (though the before law trend is only marginally significant in most cases). The F-test for the differences in these time trends is always significant at least at the .02 level. As expected, therefore, the more years a shall issue law has been in effect in the 14 states that passed laws starting in 1985, the greater the decline in murders and injuries (both absolutely and per 100,000) from mass public shootings.

Because of the relatively large number of shootings that occurred in the year prior to enactment of the laws and the possibility that our results might be picking up a simple regression to the mean, we reestimated the regressions in Tables 6 and 7 after removing the observations for that year. All of the shall issue coefficients in the shooting regressions remained statistically significant, with the single exception of the injury rate in Table 6 which was negative but no longer statistically significant.[20]

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[19] We note three other points related to Table 7.

(1) Eight states in our sample had shall issue laws during the entire period. All eight passed their laws before 1960 and so should have reached their equilibrium level of permits before 1977 (the first year in our sample). These value assigned to two time trend variables for these states and states that never enacted laws is zero.

(2) A second reason for the split time trend specification is that if (relative to other states) shootings in states that pass shall issue laws are rising before the law goes into effect and falling thereafter, a dummy law variable would underestimate the law's impact (even though the regression contains year dummy variables). For example, imagine that the increase in shootings before the law is symmetrical with the decline after the law. A simple dummy variable for the presence or absence of the law would indicate that the law had no effect yet the law might well have caused a change in the trend from positive to negative.

(3) We also estimated regressions adding two time-squared variables for the law variables. Here we find the same pattern of declining murders and injuries after passage of the law with the decline flattening out by the sixth year after enactment of the law.

[20] Because of the relatively large number of shootings that occurred in the year prior to enactment of the laws and the possibility that our results might be picking up a simple regression to the mean, we reestimated the regressions in

Table 8 adds other law variables that may influence the number of mass shootings. The law variables include the following: a dummy variable if a state has a waiting period before an individual can obtain a gun and the length of this period in days and days-squared (see Lott and Mustard for a discussion of this variable); the probability of execution (equal to the number of executions per murder in a given year); and a dummy variable for whether a state imposes additional penalties for using a gun in the commission of a crime.[21]

Three conclusions emerge from Table 8. First, the statistically significant negative impact of shall issue laws on mass public shootings continues to hold. Second, the regression coefficients on the shall issues variables are of the same magnitude as in Table 7. Third, the other gun related law variables and the capital punishment variable have no significant impact on mass shootings. The point estimates on the waiting period variables sometimes imply that longer waiting periods increase the risk of mass public shootings and other times they imply the reverse. In no case is the waiting period variable statistically significant. Although the execution rate has a negative coefficient in the six regressions, it is never statistically significant. The execution variable itself may be only weakly related to the probability that a mass murderer will be executed given the

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Tables 6 and 7 after removing the observations for that year. All of the shall issue coefficients in the shooting regressions remained statistically significant, with the single exception of the injury rate in Table 6 which was negative but no longer statistically significant. For example, the t-statistics for the shall issue dummies in specifications 1 to 3 in Table 6 are -3.02, -1.456, and -2.295; and the F-statistics for the before and after trends corresponding to specifications 1 to 3 in Table 7 are 20.15, 6.67, and 13.99. Similar results were also obtained when the two years preceding a state's adoption of the law were deleted from the sample. The t-statistics for the shall issue dummies in specifications 1 to 3 in Table 6 are -2.732, -1.50, and -2.281; and the F-statistics for the before and after trends corresponding to specifications 1 to 3 in Table 7 are 20.96, 7.92, and 15.29.

[21] See the Tracy L. Snell, Prisoners executed under civil authority in the United States, by year, region, and jurisdiction, 1977-1995, Bureau of Justice Statistics, May 14, 1997. For the source of penalties imposed for when a gun is used in a commission of a crime see Thomas B. Marvell and Carl E. Moody, The Impact of Enhanced Priosn Terms for Felonies Committed with Guns," *Criminology* 33 (May 1995): 247, 258-61.

long delays before execution and its over-inclusiveness (i.e., the variable measures the execution rate for all murders not mass murders), so we also tried including a simple dummy variable for whether the dead penalty was in effect. However, the coefficient was never statistically significant and did not alter our other results. Finally, the law variable denoting the imposition of additional penalties for using a gun in a crime is insignificant.

The impact of the death penalty on public shootings stands in sharp contrast to evidence that we have put together on murder rates using county level data from 1977 to 1995. We find that a one percentage point increase in the execution rate is associated with a seven percent decline in the overall murder rate and the effect is statistically significant at better than .01 percent level. The question is why does the presence of concealed handgun laws deter multiple victim shootings when other penalties fail to have an effect. One possibility is that the execution rate does not deter mass public killers because they already die at such high rates from their crime, but concealed handgun laws deter attacks because the killers are committing the crime to kill or injure a large number of people and that their return from these attacks is reduced when citizens with concealed handguns can limit the carnage.

We also reestimated the regressions by including both the murder and total bombing rates as explanatory variables because there may be some overlap between factors that explain public shootings, overall murders and bombings that are not taken into account by the independent variables in the previous regressions. Adding these variables to the regressions in Tables 6, 7 and 8 produces little change from our previous results. In 17 of the 18 regressions, the shall issue variable has a negative and statistically significant effect on multiple shootings.[22]

IV. Alternative Measures of Multiple Shootings

[22] Even in the one case where the coefficient is no longer statistically significant it is still negative. The t-statistic is –1.63 in the regression on injuries per 100,000 persons using the specification in Table 6 with the addition of the murder and bombing variables. The murder rate is always positively related to mass shootings but statistically significant in only six of the 18 regression equations. Total bombings are never statistically significant in any of these regressions.

The dependent variable in Table 9 is the number of multiple shootings reported in the first section of the *New York Times*. Because the Tobit regressions with state specific effects did not converge, we substituted regional dummy variables in the Tobit regressions. We also present OLS estimates that include state fixed effects variables. Regional and even state fixed effects may be important if the *New York Times* has a regional or state bias in its coverage of shooting events. Overall, the results are consistent with our earlier findings. Both the dummy shall issue law variable and the split time trend variables indicate significant reductions in multiple shootings after the passage of the law. The OLS estimates also produce negative effects of the law but these estimates are less significant (i.e., they are generally significant at the .10 but not .05 level).

The only other study that we know of on multiple victim murders defines the offense more narrowly than we have, and it limits the definition to four or more people killed in the shooting (Petee et. al., 1997). This definition sharply lowers the number of public shootings to 36 incidents during the 1977 - 1995 period. We attempted to explain both the per capita and absolute number of people killed in these shootings using the same specifications as in Tables 6 and 7.[23] The results are similar to those reported earlier. We find that shall issue laws reduce the number of deaths, and that these deaths were increasing before passage of the law and falling thereafter.[24].

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[23] Again, the Tobit estimates do not converge when state fixed effects are used for there is not enough variation in the data to distinguish the law's impact on these shootings with state fixed effects. Consequently, the state fixed effects are replaced with regional dummies (Northeast, Midwest, South, and West (the left out region)).

[24] In explaining the per capita number of people killed, the shall issue concealed handgun dummy variable was -.1207502 (t-statistic = 1.966) and the difference in the before and after trends equaled -.057 (f-statistic = 6.96). For the regressions explaining the number of people killed in a state, the shall issue concealed handgun dummy variable was -15.4 (t-statistic = 2.363) and the difference in the before and after trends equaled -7.05 (f-statistic = 9.41). The mean number of deaths per 100,000 persons per state per year is .0037 (standard deviation = .0294) and the mean number of deaths per state per year is .249 (standard deviation = 1.52).

V. The Number of Shootings and the Number of People Killed or Injured Per Shooting

The presence of concealed handguns should reduce both the number of public shootings and the amount of harm caused by any one event. Consider the following examples. During a recent shooting spree at a public school in Pearl, Mississippi, an assistant principal retrieved his gun and physically immobilized the shooter before he caused additional harm (CNN, October 2, 1997, 2:40 PM EST). And in the public school related shooting in Edinboro, Pennsylvania, which left one teacher dead, a shot gun pointed at offender while he was reloading his gun prevented additional harm (Reuters Newswire, April 26, 1998). The police did not arrive for another ten minutes. The examples mentioned in the introduction also illustrate cases where shooters have been shot by citizens and thus presumably prevented from harming even more people. Although one can also imagine circumstances where shall issue laws increase the availability of guns to potential offenders or where guns used in self defense lead to more not less killings, our results so far strongly indicate that these effects, if they exist, are not sufficient to offset the overall negative impact of shall issue laws on multiple shootings.

Here we separate the effects these laws have on the number of shootings from the number of people harmed. Suppose, for example, perpetrators are undeterred by legal penalties or the prospect of encountering an armed defender. Then the number of persons harmed per shootings could still fall (as the two school shooting examples suggest) if concealed handguns interfered with the offender's ability to carry out his plans. Using either the dummy law variable or the before-and-after time trends, the coefficients in Table 10 indicate that concealed handguns reduce both the number of shootings and the number of people harmed.[25] The evidence on whether shall issue laws have a bigger impact on the number of

[25] Note that there are 234 observations in the deaths or injuries per shooting regressions although Table 1 indicates that there were 396 shootings in the sample period. The dependent variable in equations (1) – (3) in Table 10 equals the average number of deaths or injuries per shooting in a state in a year. Hence if there were two or more multiple shootings in a state in a year, this counted as one observation in the regression.

people harmed relative to the number of shootings is mixed. The absolute size of the coefficient in the regression on deaths and injuries (equation (3)) is three times greater than in the shooting regression (equation (4)) but the elasticity is greater in latter than in the former regression (the means are 4.52 and .41 for the number of persons harmed and the number of shootings respectively).[26]

VI. Do Shootings Produce More Shootings?

Does a public shooting lead others to imitate or mimic the behavior of the first gunman? One might reason that the attention and notoriety surrounding a shooting by A might encourage B to undertake a similar act, and B's act might encourage C and so on. The notion of crime "fad" or epidemic is not new. Landes (1978, pp. 16-18) investigated and rejected the hypothesis that the increase and subsequent decrease in airline hijackings in Europe and the United States over the 1961 to 1976 period could be explained by a fad rather than by the increase in apprehension rates and penalties.

To test for fads or imitative behavior, we calculate the number of mass shootings per month for the 228 months in the 1977 to 1995 period. We specified the dependent variable in two ways: the number of monthly shootings and the difference in the number between adjacent months. The regression includes dependent variables denoting various monthly lags in either the number of shootings (or number reported in the *New York Times)* or the change in the number of shootings. We control for the increase in the number of states with shall issue laws during this period by adding a variable denoting the percentage of the U.S. population covered by these laws. We also include month dummy variables and a time trend (in months). Table 11 reports both Tobit and OLS estimates of the regression equations.

In Table 11, only the percent of the U.S. population covered by shall issue laws and the time trend variables are consistently significant in all eight regressions. The positive coefficients on the

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[26] While individuals with permits produce a large social benefit, they risk being shot by the attacker. We have no instances where people with permits have indeed been shot, but this risk surely raises the prospects of whether citizens with permits should be compensated or at least not have to pay large fees for obtaining a permit.

lagged values of shootings in the first four regressions provide some weak evidence of faddish behavior. But the lagged values of the change in shootings in last four equations imply the opposite—i.e., the larger the increase in shootings during the prior months, the greater the decline in the current month. In short, the evidence on fads is mixed.[27]

One reason we may not find significant evidence of faddish behavior is that lagged shootings and lagged stories on shootings in the *New York Times* are highly collinear. To test this we reestimated the regressions in Table 11 using either lagged shootings or lagged stories by themselves. We still find no systematic evidence of faddish behavior. Lagged values of shootings are positively related to monthly shootings while lagged differences are negatively related to differences in monthly shootings. Again the percent of the population covered by shall issue laws continues to have negative and statistically significant effects on the number and change in the number of monthly shootings.

While the evidence provides little support for the copycat hypothesis, we note that our data contains shootings by adults. The recent public school shootings involving children might be different and more consistent with the imitative hypothesis. However, data on school shootings involve such a small sample that it is not possible to study these shootings separately.

VII. Explaining the Passage of Shall Issue Laws

The previous specifications assume that the passage of a shall issue law is an exogenous event. Following Lott and Mustard (1997, pp. 39-48), we assume that the likelihood a state will enact a law depends on political influence variables that include the percentage of its population that are members of the National Rifle Association, the percentage of votes received by the Republican presidential candidate in the state, lagged and changes in the rates of violent and property crime, and fixed regional effects. Since presidential elections occur every four years, we interacted the percentage voting

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[27] Note that October appears to be the most dangerous month although he number of shootings in October is only significantly greater than the number in January, September and November. Note, however, that the monthly dummy variables are not jointly significant.

Republican with dummy variables for the years adjacent to the relevant elections. Thus, the percentage of the vote obtained in 1980 is multiplied by a year dummy for the years 1979-82, and so on, through the 1996 election.

The first stage (see the bottom half of Table 12) implies that states adopting these laws are relatively Republican and have low violent crime but high property crime rates. Neither NRA membership nor rising violent and property crimes rates are statistically significant (though the NRA variable is marginally significant). The second stage regressions support our earlier results contained in Table 6. Adopting a shall issue law is associated with a significant decline in the combined number of multiple killings and injuries (both absolutely and per 100,000 persons). In the separate murder and injury regressions, the coefficients are always negative and either significant or marginally significant (a t-statistic greater that 1.65).[28]

VIII. Conclusion

The results of this paper support the hypothesis that concealed handgun or shall issue laws reduce the number of multiple victim public shootings. Attackers are deterred and the number of people injured or killed per attack is also reduced, thus for the first time providing evidence that the harm from crimes that still occur can be mitigated. The results are robust with respect to different specifications of the dependent variable, different specifications of the handgun law variable, and the inclusion of additional law variables (e.g., mandatory waiting periods and enhanced penalties for using a gun in the commission of a crime). Not only does the passage of a shall issue law have a significant impact on multiple shootings but it is the only law related variable that appears to have a significant impact. Other law enforcement efforts from the arrest rate for murder to the death penalty to waiting periods and background checks are not systematically related to multiple

[28] As a test of whether the shall issue laws were passed because of a shooting, we reestimated just first stage regression by itself after including the lagged murder or injury rate from the shootings to see if the law was adopted because of the shooting. While the coefficients on these lagged values were positive, neither variable was ever statistically significant.

shootings. We also find that shall issue laws deter both the number of multiple shootings and the amount of harm per shooting. Finally, because the presence of citizens with concealed handguns may be able to stop attacks before the police are able to arrive, our data also allows us to provide the first evidence on the reduction in severity of those crimes that still take place.

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Table 1
The Number of Multiple Victim Murders and Injuries in
Public Shootings by Year and by the Presence of a Concealed Handgun Law

Year	All States			States Without Nondiscretionary Handgun Law (Including the District of Columbia)						
	# of Murders in Public Shootings	# of Injuries in Public Shootings	# of Public Shootings	# of States w/out Non-discretionary Concealed Handgun Law	# of Murders in Public Shootings	# of Injuries in Public Shootings	# of Shootings	% of Total Deaths (Column 5/ Column 1)	% of Total Injuries (Column 6/ Column 2)	% of Total Deaths (Column 7/ Column 3)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
1977	19	46	7	43	19	46	7	100%	100%	100%
1978	14	12	8	43	14	12	8	100%	100%	100%
1979	23	77	13	43	20	74	12	87%	96%	92%
1980	30	51	11	43	22	46	8	73%	90%	73%
1981	44	60	30	43	37	50	27	84%	83%	90%
1982	32	92	20	43	28	92	19	87%	100%	95%
1983	19	36	18	43	16	22	14	84%	61%	78%
1984	56	76	26	43	53	73	24	95%	96%	92%
1985	38	45	24	43	34	37	21	89%	82%	88%

1986	41	54	21	42	41	52	20	100%	96%	95%
1987	44	73	36	42	41	69	34	93%	95%	94%
1988	49	90	35	41	47	85	32	96%	94%	91%
1989	49	84	31	40	39	79	24	80%	94%	77%
1990	29	53	22	37	20	43	20	69%	81%	91%
1991	58	68	22	34	53	58	18	91%	85%	82%
1992	31	55	18	33	29	54	17	94%	98%	94%
1993	87	83	33	33	83	76	30	95%	92%	91%
1994	15	20	10	33	13	19	9	87%	95%	90%
1995	26	11	11	29	23	11	10	88%	100%	91%

Table 1 (continued)

Year	States With Shall Issue Concealed Handgun Law						
	# of States w/ Law	# of Murders in Public Shootings	# of Injuries in Public Shootings	# of Shootings	% of Total Deaths (Column 12/ Column 1)	% of Total Injuries (Column 13/ Column 2)	% of Total Deaths (Column 14/ Column 3)
	(11)	(12)	(13)	(14)	(15)	(16)	(17)
1977	8	0	0	0	0%	0%	0%
1978	8	0	0	0	0%	0%	0%
1979	8	3	3	1	13%	4%	8%
1980	8	8	5	3	27%	10%	27%
1981	8	7	10	3	16%	17%	10%
1982	8	4	0	1	13%	0%	5%
1983	8	3	14	4	16%	39%	22%
1984	8	3	3	2	5%	4%	8%
1985	8	4	8	3	11%	18%	12%
1986	9	0	2	1	0%	4%	5%

1987	9	3	4	2	7%	5%	6%
1988	10	2	5	3	4%	6%	9%
1989	11	10	5	7	20%	6%	23%
1990	14	9	10	2	31%	19%	9%
1991	17	5	10	4	9%	15%	18%
1992	18	2	1	1	6%	2%	6%
1993	18	4	7	3	5%	8%	9%
1994	18	2	1	1	13%	5%	10%
1995	22	3	0	1	12%	0%	9%

Table 2
The Rate of Multiple Victim Murders and Injuries in
Public Shootings by Year and by the Presence of a Concealed Handgun Law

Year	States Without Shall Issue Law			States With Shall Issue Law			Comparison of Rates Between Two Types of States	
	# of States w/out Shall Issue Law (Including the District of Columbia)	Murders and Injuries in Public Shootings Per 100,000 People	# of Shootings Per 100,000 People	# of States w/ Shall Issue Law	Murders and Injuries in Public Shootings Per 100,000 People	# of Shootings Per 100,000 People	Does the Murder and Injury Rate in States w/out Laws Exceed the Rate in States w/ Laws	Does the Shooting Rate in States w/out Laws Exceed the Rate in States with Laws
1977	43	0.033	.004	8	0.000	0	Yes	Yes
1978	43	0.013	.007	8	0.000	0	Yes	Yes
1979	43	0.046	.011	8	0.031	.003	Yes	Yes
1980	43	0.033	.004	8	0.067	.010	No	No
1981	43	0.041	.012	8	0.087	.009	No	Yes
1982	43	0.057	.009	8	0.020	.004	Yes	Yes
1983	43	0.018	.014	8	0.086	.012	No	Yes
1984	43	0.058	.012	8	0.030	.021	Yes	No
1985	43	0.032	.010	8	0.060	.024	No	No
1986	42	0.042	.006	9	0.009	.002	Yes	Yes

1987	42	0.050	.014	9	0.033	.004	Yes	Yes
1988	41	0.063	.019	10	0.021	.004	Yes	Yes
1989	40	0.057	.010	11	0.037	.031	Yes	No
1990	37	0.034	.013	14	0.031	.002	Yes	Yes
1991	34	0.061	.005	17	0.022	.002	Yes	Yes
1992	33	0.045	.004	18	0.004	.001	Yes	Yes
1993	33	0.085	.013	18	0.015	.001	Yes	Yes
1994	33	0.017	.005	18	0.004	.0004	Yes	Yes
1995	29	0.019	.003	22	0.004	.0008	Yes	Yes
Ave.		0.0423	.009		0.0288	.0064	Yes	Yes

Table 3
The Fourteen States That Changed Their
Concealed Handgun Laws between 1977 and 1995
(Each cell shows the mean and the standard deviation in parentheses.)

	Fourteen States that Changed from Not Having to Having a Shall Issue Concealed Handgun Law	
	Years during Which These States Did Not Have Shall Issue Concealed Handgun Laws (Observations = 200)	Years During Which They Did Have Shall Issue Concealed Handgun Laws (Observations = 66)
Murders in Multiple Victim Public Shootings Per 100,000 People	.026 (.119)	.0028 (.0093)
Injuries in Multiple Victim Public Shootings Per 100,000 People	.025 (.099)	.0044 (.014)
Murders and Injuries in Multiple Victim Public Shootings Per 100,000 People	.051 (.190)	.007 (.021)
References to Multiple Victim Public Shootings in the First Section of the *New York Times* Per 100,000 People	.0033 (.023)	.0003 (.0016)
Number of Shootings Per 100,000 People	.0136 (.0499)	.002 (.0061)
Actual and Attempted Bombings Per 100,000 People	.600 (.632)	.6998 (.492)
Actual and Attempted Incendiary Bombings Per 100,000 People	.1468 (.206)	.1257 (.124)
Other Bomb Related Incidents Per 100,000 People	.863 (.833)	.757 (.799)
Total Explosive Incidents Per 100,000 People	1.610 (1.277)	1.571 (1.059)

Table 4
Examining the Means for States that Changed Their Concealed Handgun Laws During the 1977 to 1995 Period

		States that Changed Their Concealed Handgun Laws During the 1977-95 Period: Using State Avg's to Compute Rates						
Years Before and After the Adoption of the Law (Year 1 is the first full Year that the Law is in Effect)	Number of States that Fall into that Category	Murders in Multiple Victim Public Shootings Per 100,000 People	Injuries in Multiple Victim Public Shootings Per 100,000 People	Murders and Injuries in Multiple Victim Public Shootings Per 100,000 People	The Number of Shootings Per 100,000 People	Total Number of Murders in Multiple Victim Public Shootings for all States in this Category	Total Number of Injuries in Multiple Victim Public Shootings for all States in this Category	Total Number of Murders and Injuries in Multiple Victim Public Shootings for all States in this Category
-8	14	0.0033	0.0175	0.0208	0.0060	4	20	24
-7	14	0.0033	0.0145	0.0177	0.0116	2	7	9
-6	14	0.0593	0.0349	0.0942	0.0307	12	11	23
-5	14	0.0055	0.0032	0.0087	0.0013	8	5	13
-4	14	0.0151	0.0235	0.0386	0.0141	16	16	32
-3	14	0.0064	0.0159	0.0223	0.0045	5	12	17
-2	14	0.0188	0.0041	0.0229	0.0102	6	4	10
-1	14	0.0570	0.0896	0.1467	0.0460	13	17	30
0	14	0.0152	0.0277	0.0428	0.0154	20	25	45

1	14	0.0034	0.0097	0.0131	0.0046	4	10	14
2	10	0.0033	0.0050	0.0083	0.0033	4	6	10
3	10	0.0062	0.0046	0.0108	0.0008	8	6	14
4	10	0.0000	0.0166	0.0166	0.0003	0	2	2
5	9	0.0000	0.0000	0.0000	0.0000	0	0	0
6	6	0.0049	0.0061	0.0110	0.0024	4	5	9
7	3	0.0048	0.0024	0.0072	0.0024	2	1	3
8	2	0.0000	0.0000	0.0000	0.0000	0	0	0

Table 5
Means and Standard Deviation of Variables

	Obs.	Mean	*Standard Deviation*
Shall Issue Law Dummy	969	.22497	.41778
Arrest Rate for Murder	953	89.4275	53.9812
Murders in Multiple Victim Public Shootings Per 100,000 Persons	969	.01586	.066083
Injuries in Multiple Victim Public Shootings Per 100,000 Persons	969	.021886	.09133
Murders and Injuries in Multiple Victim Public Shootings Per 100,000 Persons	969	.03775	.13236
Murders in Multiple Victim Public Shootings	969	.72652	2.47185
Injuries in Multiple Victim Public Shootings	969	1.1207	3.850898
Murders and Injuries in Multiple Victim Public Shootings	969	1.84727	5.76515
Attempted or Actual Bombings Per 100,000 Persons	969	.568889	.488192
Attempted or Actual Incendiary Bombings Per 100,000 Persons	969	8.12461	19.16359
Attempted or Actual Other Bombing Incidents Per 100,000 Persons	969	.709398	.6875484
Attempted or Actual Bombings	969	26.20906	42.02015
Attempted or Actual Incendiary Bombings	969	8.12461	19.16359
Attempted or Actual Other Bombing Incidents	969	28.44376	41.44581
Deaths per shooting	239	1.707	1.54538
Injuries per Shooting	239	2.22	4.47260
Deaths or Injuries per Shooting	239	4.525	4.83836
Number of Shootings	969	.4087	1.01036
Number of Shootings per 100,000 Persons	969	.00888	0.0289133
Murders per 100,000 Persons	966	7.56856	7.41677
Death Penalty Execution Rate	966	.0008941	.0042598
Waiting Period Dummy	969	.3750877	.47944
NRA Members Per 100,000 Persons	969	1231.408	674.4301
State Population	969	4718730	5115287
State Population Squared	969	4.84e+13	1.20e+14
Real Per Capita Personal Income	969	12874.66	2293.915
Real Per Capita Income Maintenance	969	166.6172	66.8434
Real Per Capita Unemployment Insurance Payment	969	73.36105	44.88649

Real Retirement Payments Per Person Over 65	969	151.1192	55.0607
Population Density Per Square Mile	969	356.704	1399.525
Unemployment Rate	969	6.564	2.1023
Poverty Rate	969	13.55103	4.223485
Percent of the Population that is:			
Black Males 10 to 19 Years of Age	969	1.00559	1.08956
Black Females 10 to 19 Years of Age	969	.9921156	1.10503
White Males 10 to 19 Years of Age	969	6.56392	1.560617
White Females 10 to 19 Years of Age	969	6.25813	1.526072
Other Males 10 to 19 Years of Age	969	.368235	.732602
Other Females 10 to 19 Years of Age	969	.3557895	.707472
Black Males 20 to 29 Years of Age	969	.946677	1.021947
Black Females 20 to 29 Years of Age	969	1.024541	1.207095
White Males 20 to 29 Years of Age	969	7.205986	1.252604
White Females 20 to 29 Years of Age	969	7.05257	1.291831
Other Males 20 to 29 Years of Age	969	.3611558	.7046579
Other Females 20 to 29 Years of Age	969	.3647368	.7125971
Black Males 30 to 39 Years of Age	969	.7368627	.8415113
Black Females 30 to 39 Years of Age	969	.8434159	1.00203
White Males 30 to 39 Years of Age	969	6.74614	1.214384
White Females 30 to 39 Years of Age	969	6.693695	1.203899
Other Males 30 to 39 Years of Age	969	.3143137	.6723823
Other Females 30 to 39 Years of Age	969	.3460475	.7103821
Black Males 40 to 49 Years of Age	969	.4893395	.5828483
Black Females 40 to 49 Years of Age	969	.5763158	.7112123
White Males 40 to 49 Years of Age	969	5.019412	1.049877
White Females 40 to 49 Years of Age	969	5.03807	1.024572
Other Males 40 to 49 Years of Age	969	.2131166	.502547
Other Females 40 to 49 Years of Age	969	.2391022	.5474948
Black Males 50 to 64 Years of Age	969	.5142621	.6773694
Black Females 50 to 64 Years of Age	969	.6462745	.8779527
White Males 50 to 64 Years of Age	969	5.727946	1.03609
White Females 50 to 64 Years of Age	969	6.147668	1.225103

Other Males 50 to 64 Years of Age	969	.2020537	.6068594
Other Females 50 to 64 Years of Age	969	.2359133	.6975253
Black Males Over 64 Years of Age	969	.3600516	.4907798
Black Females Over 64 Years of Age	969	.5555728	.8039973
White Males Over 64 Years of Age	969	4.337441	1.157958
White Females Over 64 Years of Age	969	6.324757	1.685616
Other Males Over 64 Years of Age	969	.1291434	.4826889
Other Females Over 64 Years of Age	969	.1490402	.5136774
Violent Crime Rate Per 100,000 Persons	969	482.7601	338.3358
Murder Rate Per 100,000 Persons	969	7.56856	7.416777
Rape Rate Per 100,000 Persons	969	33.76551	16.04484
Aggravated Assault Rate Per 100,000 Persons	969	282.3533	177.0785
Robbery Rate Per 100,000 Persons	969	161.7178	176.6475

Appendix: Coefficient Estimates for Regression Explaining Murders from Multiple Victim Public Shootings
(Does not report the state and year fixed effects that were included in the regression.)

	Coefficient	t-statistics	Probability
Shall Issue Law Dummy	-.0608	2.766	0.006
Arrest Rate for Murder	-.00027	1.597	0.111
State Population	5.11e-08	2.167	0.031
State Population Squared	-7.95e-16	1.618	0.106
Real Per Capita Personal Income	4.74e-06	0.365	0.715
Real Per Capita Income Maintenance	.000078	0.317	0.751
Real Per Capita Unemployment Insurance Payment	-5.28e-06	0.028	0.977
Real Retirement Payments Per Person Over 65	.00109	1.795	0.073
Unemployment Rate	.0103159	1.999	0.046
Poverty Rate	-.00212	0.832	0.406
Percent of the Population that is:			
Black Males 10 to 19 Years of Age	-.518476	1.169	0.243
Black Females 10 to 19 Years of Age	.62572	1.416	0.157
White Males 10 to 19 Years of Age	.508906	2.169	0.030
White Females 10 to 19 Years of Age	-.55929	2.275	0.023
Other Males 10 to 19 Years of Age	.94188	1.994	0.046
Other Females 10 to 19 Years of Age	-1.12658	2.292	0.022
Black Males 20 to 29 Years of Age	.50902	1.739	0.082
Black Females 20 to 29 Years of Age	-.30357	1.069	0.285
White Males 20 to 29 Years of Age	-.310526	3.067	0.002
White Females 20 to 29 Years of Age	.3187005	2.815	0.005
Other Males 20 to 29 Years of Age	.3769835	0.797	0.425
Other Females 20 to 29 Years of Age	-.252396	0.514	0.607
Black Males 30 to 39 Years of Age	-.92964	2.593	0.010
Black Females 30 to 39 Years of Age	.60665	2.119	0.034

White Males 30 to 39 Years of Age	.31494	1.815	0.070
White Females 30 to 39 Years of Age	-.37101	2.178	0.030
Other Males 30 to 39 Years of Age	-1.3244	2.135	0.033
Other Females 30 to 39 Years of Age	.67917	1.402	0.161
Black Males 40 to 49 Years of Age	-.201499	0.482	0.630
Black Females 40 to 49 Years of Age	.20465	0.525	0.599
White Males 40 to 49 Years of Age	-.12779	0.721	0.471
White Females 40 to 49 Years of Age	.19292	1.081	0.280
Other Males 40 to 49 Years of Age	.1767997	0.287	0.774
Other Females 40 to 49 Years of Age	-.56335	1.037	0.300
Black Males 50 to 64 Years of Age	.11955	0.273	0.785
Black Females 50 to 64 Years of Age	-.72681	1.814	0.070
White Males 50 to 64 Years of Age	-.14827	0.889	0.374
White Females 50 to 64 Years of Age	.12739	0.929	0.353
Other Males 50 to 64 Years of Age	.92883	1.627	0.104
Other Females 50 to 64 Years of Age	1.0097	1.785	0.075
Black Males Over 64 Years of Age	-.29203	0.594	0.553
Black Females Over 64 Years of Age	.37786	1.115	0.265
White Males Over 64 Years of Age	.060696	0.420	0.675
White Females Over 64 Years of Age	-.02943	0.312	0.755
Other Males Over 64 Years of Age	1.1684	1.490	0.136
Other Females Over 64 Years of Age	-1.0168	1.528	0.127
Intercept	-.02097	-0.017	0.987
Chi Square	356.36		
Number of Observations	953		

Table 6
The Impact of Shall Issue Concealed Handgun Laws on Public Shootings and Bombings

(The regressions use the tobit procedure. The regressions include the following independent variables: detailed demographic information by sex, race, and age; population and population squared; state unemployment rate; state poverty rate; real per capita personal income, unemployment payments, income maintenance payments; retirement payments; arrest rate of murder; and state and year fixed effects. Absolute t-statistics are shown in parentheses.)

	Endogenous Variables					
Exogenous Variables	Murders in Multiple Victim Public Shootings Per 100,000 People	Injuries in Multiple Victim Public Shootings Per 100,000 People	Murders and Injuries in Multiple Victim Public Shootings Per 100,000 People	Murders in Multiple Victim Public Shootings	Injuries in Multiple Victim Public Shootings	Murders and Injuries in Multiple Victim Public Shootings
	(1)	(2)	(3)	(4)	(5)	(6)
Shall Issue Law Dummy Variable	-.061 (2.766)	-.0582 (1.658)	-.111 (2.467)	-6.887 (2.959)	-6.473 (2.208)	-13.38 (3.136)
Chi-Square	356.36	384.04	361.96	619.5	720.60	723.22
Number of Observations	953	953	953	953	953	953

	Endogenous Variables					
Exogenous Variables	Attempted or Actual Bombings Per 100,000 People	Attempted or Actual Incendiary Bombings Per 100,000 People	Other Bombing Incidents Per 100,000 People	Attempted or Actual Bombings	Attempted or Actual Incendiary	Other Bombing Incidents
	(7)	(8)	(9)	(10)	(11)	(12)
Shall Issue Law Dummy Variable	-.0488 (0.874)	.0018 (0.047)	.00046 (1.266)	.965 (0.215)	-2.9798 (0.762)	13.76 (2.034)
Chi-Square	1077.7	688.01	804.98	2688.8	1371.66	1820.26
Number of Observations	953	953	953	953	953	953

Table 7
The Time Trends in Public Shootings and Bombings
Before and After the Implementation of the Shall Issue Laws

(The regressions use the tobit procedure and include the following independent variables: detailed demographic information by sex, race, and age; population, population squared, and state unemployment rate; state poverty rate; real per capita personal income, unemployment payments, income maintenance payments, retirement payments; arrest rate for murder; and state and year fixed effects. Absolute t-statistics are shown in parentheses.)

	Endogenous Variables					
Exogenous Variables	Murders in Multiple Victim Public Shootings Per 100,000 People	Injuries in Multiple Victim Public Shootings Per 100,000 People	Total Murders and Injuries in Multiple Victim Public Shootings Per 100,000 People	Murders in Multiple Victim Public Shootings	Injuries in Multiple Victim Public Shootings	Total Murders and Injuries in Multiple Victim Public Shootings
	(1)	(2)	(3)	(4)	(5)	(6)
Time Trend for Years Before the Shall Issue Law Went into Effect	.0093 (2.321)	.0102 (1.630)	.0148 (1.900)	.8217 (1.867)	.7738 (1.402)	1.1283 (1.461)
Time Trend for Years After the Shall Issue Law Went into Effect	-.0222 (3.507)	-.01888 (1.879)	-.0384 (2.973)	-2.7703 (4.040)	-1.915 (2.229)	-4.606 (3.698)
F-test for Differences in Time trends (probability in parentheses)	18.37 (.0000)	6.49 (.0110)	13.00 (.0003)	20.61 (.0000)	7.57 (.0060)	16.24 (.0001)
Chi-Square	367.55	388.07	369.17	632.81	723.40	730.55
Number of Obs.	953	953	953	953	953	953

Table 8
Including Other Gun Control Laws and Death Penalty Execution Rates

(The regressions use the tobit procedure and include the following independent variables: detailed demographic information by sex, race, and age; population, population squared, and state unemployment rate; state poverty rate; real per capita personal income, unemployment payments, income maintenance payments, retirement payments; arrest rate for murder; and state and year fixed effects. Absolute t-statistics are shown in parentheses.)

	Endogenous Variables					
Exogenous Variables	Murders in Multiple Victim Public Shootings Per 100,000 People	Injuries in Multiple Victim Public Shootings Per 100,000 People	Total Murders and Injuries in Multiple Victim Public Shootings Per 100,000 People	Murders in Multiple Victim Public Shootings	Injuries in Multiple Victim Public Shootings	Total Murders and Injuries in Multiple Victim Public Shootings
	(1)	(2)	(3)	(4)	(5)	(6)
Time Trend for Years Before the Shall Issue Law Went into Effect	.0093 (2.270)	.0119 (1.842)	.0164 (2.047)	.7016 (1.556)	.7953 (1.392)	1.059 (1.325)
Time Trend for Years After the Shall Issue Law Went into Effect	-.0247 (3.260)	-.0218 (1.911)	-.042 (2.789)	-3.113 (3.713)	-2.1359 (2.213)	-5.07 (3.472)
Waiting Period Dummy	.025 (0.513)	.0558 (0.702)	.0501 (0.509)	-.7575 (0.127)	-1.23 (0.158)	-1.652 (0.151)
Length of Waiting Period in Days	.00017 (0.0111)	-.011 (0.427)	-.00084 (0.027)	3.7184 (1.199)	4.233 (0.969)	6.777 (1.148)

Length of Waiting Period Squared	-.00064 (0.474)	-.0007 (0.307)	-.0016 (0.614)	-.4137 (1.045)	-.6252 (1.101)	-.801 (1.058)
Additional Penalty for Using Gun in the Commission of a Crime Dummy	-.00048 (0.271)	.0179 (0.619)	.017 (0.485)	.0037 (0.002)	1.777 (0.711)	2.551 (0.755)
Death Penalty Execution Rate	-.8077 (0.431)	-1.91 (0.624)	-2.28 (0.593)	-187.02 (0.878)	-.81499 (0.003)	-1882.35 (0.469)
F-test for Differences in Time trends (probability in parentheses)	16.73 (.0000)	7.27 (.0071)	12.71 (.0004)	17.74 (.0000)	7.55 (.0061)	14.87 (.0001)
Chi-Square	369.75	393.76	373.65	642.40	728.87	739.63
Number of Observations	953	953	953	953	953	953

Table 9
News Stories on Multiple Victim Public Shootings in the First Section of the *New York Times*

(Because the Tobit estimates reported in the first two columns did not converge when we included all the control variables from the earlier regressions, we replaced the state fixed effects with regional dummy variables. The OLS regressions in the third and fourth columns control for all variables used earlier. Absolute t-statistics are shown in parentheses.)

	Endogenous Variables			
	Tobit Estimates		Ordinary Least Squares Estimates	
Exogenous Variables	Multiple Victim Public Shooting Stories Appearing in the First Section of the New York Times Per 100,000 People in a State	Multiple Victim Public Shooting Stories Appearing in the First Section of the New York Times	Multiple Victim Public Shooting Stories Appearing in the First Section of the New York Times Per 100,000 People in a State	Multiple Victim Public Shooting Stories Appearing in the First Section of the New York Times
A) Estimates Using Only a Simple Dummy Variable				
Shall Issue Law Dummy Variable	-.0305 (3.645)	-2.485 (3.530)	-.0025 (1.635)	-.2151 (1.684)
Chi-Square	239.06	363.31		
F-statistic			2.06	4.44
R^2			.1274	.3787
B) Estimates Using Before and After Time Trends				

Time Trend for Years Before the Shall Issue Law Went into Effect	.0023 (1.832)	.1913 (1.773)	.0001899 (0.823)	.0096 (0.502)
Time Trend for Years After the Shall Issue Law Went into Effect	-.0055 (2.66)	-.4126 (2.434)	-.00058 (1.556)	-.06212 (2.009)
F-test for Differences in Time trends (probability in parentheses)	10.48 (.0013)	9.63 (.0020)	2.86 (.0913)	3.61 (.0579)
Chi-Square	234.39	358.70		
F-statistic			2.05	4.46
R^2			.1277	.3797
Number of Observations	953	953	953	953

Table 10
The Impact of Shall Issue Concealed Handgun Laws on the Number of Shootings and the Number of Deaths or Injuries from each Shooting

(Equation (3) uses ordinary least squares while the other regressions use the tobit procedure. The regressions include demographic information by sex, race, and age; population, population squared, and state unemployment rate; state poverty rate; real per capita personal income, unemployment payments, income maintenance payments, and retirement payments; and state and year fixed effects. Absolute t-statistics are shown in parentheses.)

	Endogenous Variables				
Exogenous Variables	Deaths per Shooting	Injuries per Shooting	Deaths or Injuries per Shooting	Number of Shootings	Number of Shootings per 100,000 people
	(1)	(2)	(3)	(4)	(5)
Shall Issue Law Dummy Variable	-2.246 (2.749)	-4.315 (1.962)	-6.139 (2.020)	-1.405 (2.115)	-.0172 (2.145)
F-statistic			1.72		
R^2			.6067		
Chi-Square	182.01	214.90		758.63	355.67
	(6)	(7)	(8)	(9)	(10)
Time Trend for Years Before the Shall Issue Law Went into Effect	-.0237 (0.105)	.7029 (1.090)	.44796 (0.535)	.2140 (1.728)	.0030 (2.066)

Time Trend for Years After the Shall Issue Law Went into Effect	-.6726 (2.385)	-.9100 (1.187)	-1.635 (1.568)	-.5870 (3.069)	-.0053 (2.404)
F-test for Differences in Time trends (probability in parentheses)	3.81 (0.0534)	3.50 (0.0636)	2.5 (0.0885)	12.88 (0.0004)	9.92 (0.0017)
F-statistic			1.68		
R^2			.6039		
Chi-Square	180.23	214.57		767.41	361.23
Number of Observations	234	234	234	953	953

Table 11
Do Shootings Produce Yet More Shootings?

(Absolute t-statistics in parentheses. The regression also includes monthly dummy variables.)

	Endogenous Variable: Number of Shootings Per Month				Endogenous Variable: First Differences in the Number of Shootings Per Month (For these specifications the explanatory variables for the number of shootings and the number of *New York Times* articles are also differenced and then lagged)			
	Tobit			OLS	Tobit		Cochrane-Orcutt	
Exogenous Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Number of Shootings in Previous Month	.1690 (1.785)	.1526 (1.588)	.1264 (1.324)	.1550 (2.000)	-.5362 (5.136)	-.5072 (4.956)	-.5048 (4.881)	-.5227 (8.124)
Number of Shootings Two Months Ago	. . .	.1039 (1.084)	.07761 (0.812)		. . .	.0354 (0.415)	.0384 (0.443)	
Number of Shootings Three Months Ago	. . .	. . .	.1813 (1.880)		. . .	. . .	-.099 (1.057)	
Number of *New York Times*' Stories in the Front Section in Previous Month	-.2057 (1.116)	-.1825 (0.983)	-.1111 (0.601)	-.1367 (0.921)	.0411 (0.208)	-.313 (1.317)	-.320 (1.312)	0.0478 (0.373)
Number of *New York Times*' Stories in the Front Section Two Months Ago	. . .	-.09145 (0.498)	-.0634 (0.348)		. . .	-.551 (2.869)	-.519 (2.649)	
Number of *New York Times*' Stories in the Front Section Three Months Ago	. . .	. . .	.0428 (0.237)		. . .	. . .	.0132 (0.070)	

Percentage of the Nation's Population Covered by Shall Issue Laws	-9.432 (3.616)	-8.7729 (3.212)	-7.5203 (2.711)	-6.9996 (3.313)	-8.860 (2.241)	-9.201 (2.330)	-9.152 (2.266)	-8.174 (2.660)
Monthly Time Trend	.01449 (4.117)	0.139 (3.719)	.0113 (2.996)	.0107 (3.810)	.0133 (2.451)	.0136 (2.512)	.0143 (2.567)	.0118 (2.842)
Intercept	.6236 (1.431)	.5393 (1.221)	.2627 (0.571)	.9917 (2.851)	-.7686 (1.117)	-.5465 (0.769)	-1.165 (1.408)	-.0769 (0.159)
Chi Squared or F-statistic	35.36	36.68	40.00	2.47, DW=2.005	48.05	54.80	55.29	6.23, transformed DW=2.008
Number of Observations	227	226	225	227	226	224	222	225

Table 12
Simultaneous Logit-Tobit Estimates

(The regressions control for sex, race, age; population, population squared, state unemployment rate, state poverty rate, real per capita personal income, unemployment payments, income maintenance payments, retirement payments, arrest rate for murder and state and year fixed effects. The first stage estimates do not report the regional dummy variables that were in the regression. Absolute t-statistics are shown in parentheses.)

Second Stage Estimates	Endogenous Variables					
Exogenous Variables	Murders in Multiple Victim Public Shootings Per 100,000 People	Injuries in Multiple Victim Public Shootings Per 100,000 People	Murders and Injuries in Multiple Victim Public Shootings Per 100,000 People	Murders in Multiple Victim Public Shootings	Injuries in Multiple Victim Public Shootings	Murders and Injuries in Multiple Victim Public Shootings
Shall Issue Law Dummy Variable	-.023 (1.647)	-.0743 (3.110)	-.0921 (2.271)	-2.6746 (1.674)	-10.36 (4.371)	-13.166 (2.989)
Chi-Square	330.90	421.88	365.44	580.15	723.25	690.13
Number of Observations	892	892	892	892	892	892

First Stage Estimates	Exogenous Variables						
Endogenous Variable	NRA Membership Rate	Lagged Violent Crime Rate	Lagged Property Crime Rate	Change in Violent Crime Rate	Change in Property Crime Rate	% Rep. Pres. in State Vote * Year Dummy1977-78	% Rep. Pres. in State Vote * Year Dummy1979-82
Shall Issue Law Dummy Variable	.000302 (1.682)	-.00259 (5.004)	.00025 (2.642)	.00159 (1.354)	.00013 (.916)	.043 (2.558)	.038 (2.641)

First Stage Estimates (cont'd)	Exogenous Variables						
Endogenous Variable	% Rep. Pres. in State Vote * Year Dummy1983-86	% Rep. Pres. in State Vote * Year Dummy1987-90	% Rep. Pres. in State Vote * Year Dummy1991-94	% Rep. Pres. in State Vote * Year Dummy1995	% of the State that is White	% of the State that is Black	Chi-Square
Shall Issue Law Dummy Variable	.033 (2.646)	.047 (3.468)	.0942 (4.932)	.083 (5.116)	.0314 (2.226)	.031 (1.683)	161.55