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Original Paper

Is There a Policy That Reduces Mass Public Shooting Deaths?

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Abstract

The fact that an individual is willing to commit the most serious crime that carries with it the most serious punishment means that that person is unlikely to be deterred by laws with less serious consequences. This situation is compounded by the fact that many multiple victim public shooters are expecting, even planning, to die in the commission of their crimes. Combining newly developed and traditional difference-in-differences methodologies, we analyze several policies that have been suggested as possibly effective in reducing deaths due to mass public shootings. We find that none of the proposed policies significantly reduce such deaths. However, we find evidence that mass public shooting deaths are lower in places that allow the carrying of concealed firearms.

Keywords

mass public shootings, policy analysis, difference-in-differences, gun-free zones

1. Introduction

Mass public shooting incidents are rare but horrifying events. Such incidents receive intense media coverage and become political events leading to demands that we do something. The obvious question is what should we do? The fact that an individual is willing to commit the most serious crime that carries with it the most serious punishment, life in prison or execution, means that that person is unlikely to be deterred by laws with less serious consequences. This situation is compounded by the fact that many of the shooters are expecting, even planning, to die in the commission of their crimes (Note 1). We analyze several policies that have been found to be effective, or suggested as possibly effective, in reducing deaths due to mass public shootings.

Most of the studies of mass public shootings have employed state-level panel datasets using Two-Way Fixed-Effects (TWFE) regressions with state and year fixed effects. Such regressions have recently been criticized by econometricians studying difference-in-differences analysis. For example, deChaisemartin and D'Haultfoeuille (2020), henceforth CD, show that the sign of the difference-in-differences coefficient estimated using fixed-effects regression could so biased that the coefficient is negative when all the effects are positive. This finding potentially invalidates nearly all previous analyses concerning the effect of gun laws on deaths due to mass public shootings. Fortunately, CD and others have developed a new methodology which generates unbiased estimates of the difference-in-differences. To our knowledge, this is the first study to use this new methodology to investigate the efficacy of state laws with respect to reducing deaths due to mass public shootings.

The first analysis of mass public shootings was by Lott and Landis (1999) arguing that citizens carrying concealed weapons could deter such attacks because potential shooters would not know who might shoot back. In that study the authors studied multiple victim public shootings defined as two or more people killed, except for the perpetrator, in a public place, although they also considered alternative definitions with more people killed. They excluded incidents, "… 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" (Lott & Landis, 1999, p. 7). A Lexis/Nexis search found 931 cases over the period 1977-1995. Using a Tobit regression, they found that Right-To-Carry (RTC) laws significantly reduced the number of mass public shootings, the number killed, and the number injured. They also found that waiting periods, firearm enhancement laws, and background checks had no significant effect on the number of mass public shootings or the harm they cause.

Duwe, Kovandzic, and Moody (2002), using a combination of FBI Supplemental Homicide Reports (SHR) and Lexis/Nexis searches, identified 116 mass public shootings between 1976 and 1999, defined as four or more people killed, apart from the shooter, but including some that occurred during the commission of a crime. Using the negative binomial model on this more restrictive definition, they found that RTC laws had negative but insignificant effects on the number of incidents, the number killed, and the number wounded.

Gius (2015) using Mother Jones and SHR data, estimated the effects of federal and state assault weapons bans for the years 1982-2011, finding evidence that such bans significantly reduced fatalities in mass shootings. Gius (2018) found that state assault weapon bans were associated with lower fatalities in school shootings.

Using data compiled by the Congressional Research Service, Fox and Fridel (2016) found that mass public shooting incidents were not significantly reduced during the Federal assault weapon ban from 1994-2004. However, Klarevas, Conner, and Hemenway (2018) found that bans on large capacity magazines (LCM, more than 10 rounds), including the 1994 Federal assault weapons ban which included an LCM ban, were associated with a significant reduction in the number of incidents and the number of fatalities in mass shootings where six or more people are killed. DiMaggio, Avraham, Berry, Bukur, Feldman, and Klein (2019), using linear and Poisson regression on national data, also found that the Federal assault weapons ban significantly reduced the number of mass public shootings. The authors used data from three sources, but according to Webster, McCourt, Crifasi, Booty, and Stuart (2020, p. 173),

> Inexplicably, the researchers only included cases in their analyses that appeared in all three sources and thereby excluded many incidents of fatal mass shootings. This limited their data to only 51 public mass shootings that presumably were the most widely publicized. The study did not examine variation by state and thus did not consider state gun laws nor did it control for other covariates other than [a] linear trend.

Koper (2020) finds that mass public shootings in which the shooter used large capacity magazines result in more fatalities than those incidents where shooters did not use LCM's. He estimates that LCM bans, if effective, could reduce the number of mass shooting fatalities by 11-15 percent. Reeping, Cerdá, Kalesan, Wiebe, Galea, and Branas (2019) using SHR data find that states with more firearms and more permissive firearm laws have more mass shootings. However, SHR data misses many mass-shooting events, including Newtown, CT and Aurora, CO (Webster et al., 2020, pp. 172-173). On the other hand, Lin, Fei, Barzman, and Hossain (2018) using Mother Jones data, find no association between gun ownership or permissive gun laws and the number of fatal mass public shootings.

Siegel, Goder-Reiser, Duwe, Rocque, Fox, and Fridel (2020) constructed a database of 143 incidents of mass public shootings of four or more fatalities from 1976-2018 using a variety of sources including SHR and media reports. The authors have made their data available online and we use it in this analysis. They analyze the effect of several policies: large capacity magazine bans, assault weapons bans, permit-to-purchase laws, red flag laws, universal background checks, may-issue laws (states without right to carry or permit-less carry laws), relinquishment laws (confiscating guns from individuals that become prohibited from owning firearms), and violent misdemeanor laws (prohibiting individuals from owning firearms who commit a variety of non-felony crimes). They find two of these policies have significant effects. Permit-to-purchase laws are found to significantly reduce the number of mass public shootings but have no significant effect on the number of fatalities associated with those shootings. On the other hand, state large capacity magazine bans are found to significantly reduce fatalities but have no significant effect on the number of incidents. A serious drawback to this analysis is that, although it is a panel data of state-years, the authors do not use state fixed-effects to correct for unobserved heterogeneity. As a result, the estimates are biased due to the correlation between the number and severity of mass shootings and unobserved time-invariant factors such as climate, culture, history, political attitudes, etc.

Webster et al. (2020) analyze a data set created by supplementing SHR data with data from the Stanford Mass Shootings in America and the Gun Violence Policy data sets. The threshold is four or more victims, not counting the shooter. They exclude gang-related and crime-related mass shootings but include domestic violence mass shootings as a separate category. They find that permit-to-purchase laws significantly reduce both the number of incidents and the number of fatalities associated with mass shootings. In the robustness section, they also find that LCM bans significantly reduce incidents and fatalities. Because of reporting difficulties with the SHR, they do not include observations from Florida, Kansas, Kentucky, Nebraska, and Montana. These omissions are problematic because both the

Stanford and Gun Violence Policy datasets include both SHR and media search data, obviating the need to omit any states.

Thus, there is some evidence that permit-to-purchase laws, large capacity magazine bans, and assault weapons bans are effective in reducing deaths from mass public shootings. There is little support for any other gun control laws in this literature. Nevertheless, a cursory search of the internet on “mass shootings” and related phrases finds many other suggestions for reducing the death toll associated with mass shootings. Many sites suggest that universal background checks might be successful. Since many mass shooters, especially school shooters are quite young, some sites suggested that safe storage laws and bans on juveniles possessing firearms might be effective. Since many mass shooters are willing or committed to die in the act, policies designed to reduce suicide in general could reduce the number of people who adopt mass shooting as their suicidal act (Note 2). On the other hand, conservative, gun-rights, and libertarian sites suggest that right-to-carry laws and permit-less carry (also known as constitutional carry) laws would encourage ordinary citizens to carry concealed handguns, allowing them to intervene before police arrive. There is also some evidence that shooters tend to choose places that forbid firearms on the premises, presumably to reduce the probability of potential victims returning fire. If so, it is possible that reducing the number of these “gun-free zones” could save lives (Note 3).

2. Method

2.1 Statistical Methodology

We do an event study of the effect of the implementation of several policies on the number of deaths due to mass public shootings. The effects are estimated using the deChaisemartin and D’Haultfoeuille (2020, 2022) methodology implemented by their Stata program *did_multiplegt*, which is robust to time and state heterogeneity. The program produces a difference-in-differences estimator, DID_l which compares the outcome of treated states to untreated and yet-to-be treated states for each of the *l* periods of the event horizon (CD 2022, p. 3). The statistical significance of each effect is determined using bootstrapped standard errors. The program also generates an overall “average total effect” which is the average of the DID_l. This average effect is an unbiased estimate of the net benefit of the policy being evaluated (CD 2022, pp. 15-19).

Control variables are necessary in a crime policy analysis because after a state implements a policy it could also make significant changes in police staffing, court sentencing, prison incarceration and executions. In addition, the outcome could be affected by economic and demographic changes. All these factors could affect crime in the treatment period. The *did_multiplegt* program calculates the treatment effect using residuals from a preliminary fixed-effects regression of the outcome on the control variables and state and year fixed effects, under the null hypothesis that the coefficient on the policy dummy variable is zero. Thus, the results of the difference-in-differences analysis can be sensitive to the choice of control variables.

Including many potentially relevant control variables reduces the likelihood that the results are biased by omitted variables. However, it also increases the possibility of including irrelevant variables, which will not bias the coefficients but will increase their variance, possibly making relevant variables appear to be insignificant. It is possible to detect irrelevant variables through a general-to-specific (GETS) model specification search (Hoover & Perez, 1999; Hendry, 1995). The first step is to designate the policy variables as variables of interest. The remaining variables are control variables included only to avoid possible omitted variable bias affecting the coefficient estimates of the variables of interest. The GETS methodology has been successfully used in many applications, e.g., Owen and Weatherston (2004), Muelbauer and Nunziata (2004), Rao and Singh (2006), and Reade (2007).

Multistage automated GETS procedures are available for time series applications (Doornik, 2008; Hendry & Doornik, 2014), but these procedures are not available for panel data models. We use an approximation based on the well-known theorem that, if the true t-ratio for a given variable is less than one in absolute value, dropping this potentially irrelevant variable from the original (general) model will reduce the mean squared error (variance plus squared bias) of all the remaining coefficients in the resulting (specific) model (Rao, 1971; Wallace, 1964). If a Wald test on all the variables with t-ratios less than one, called a parsimonious encompassing test, is not significant the reduction from the general to the specific model is justified. The resulting coefficient estimates of the specific model will be more efficient with smaller mean squared errors than those of the general model but could have a small amount of bias.

Since the number of people killed in mass public shooting incidents is a count variable, we use the fixed-effects negative binomial regression model. We first estimate the general model including all the control variables. We then perform a Wald test on those control variables with t-ratios less than one in absolute value. If this test does not reject, the reduction is justified and we estimate the more efficient specific model, formed by dropping the potentially irrelevant control variables in the Wald test. If there are variables in the resulting specific model with t-ratios less than one in absolute value, we add them to the Wald test. If it does not reject, the further reduction is justified. We continue until there are no control variables in the specific model with t-ratios less than one in absolute value or the Wald test rejects. We report the results of both the general and specific models. If the signs on the policy variable of interest are the same for both the general and specific models, then the specific model is not biased to the extent that the coefficient has a different sign. In that case, the more efficient specific model should be preferred for drawing conclusions.

Since this is an exploratory analysis, it would be appropriate to use the 10 percent significance level. Estimating two regressions for each policy means we are testing the null hypothesis of no effect for each policy using the same data twice. Applying the Bonferroni correction implies that each coefficient must have a p-value less than 0.05 to be significantly different from zero (Hendry, 1995, pp. 490-491). Thus, we use the five percent significance level throughout.

2.2 Outcome

The outcome variable is the number of fatalities due to mass public shootings, excluding the shooter. In this analysis, we rely primarily on the dataset compiled by Siegel et al. (2020), which in turn is based on Duwe (2020). They define a mass public shooting as, "… an incident in which four or more victims are fatally shot in a public location within a 24-hr period in the absence of other criminal activity, such as robberies, drug deals, and gang conflict" (Siegel et al., 2020, p. 351). The sample is 1976-2018, although we have supplemented it with observations for 2019 taken from Everytown (Note 4), Violence Project, Mother Jones (Note 5), and the Crime Prevention Research Center (Note 6). The Violence Project has the database with the longest sample, from 1966 to the present (Note 7). They use the Congressional Research Service definition: "… incidents occurring in relatively public places, involving four or more deaths—not including the shooter(s)—and gunmen who select victims somewhat indiscriminately" (Note 8). We use the Violence Project dataset as a robustness check.

Klarevas (2016, pp. 72-73) has compiled a list of mass shootings that he refers to as "gun massacres". These are shootings with at least six people killed, excluding the shooter. Incidents are not limited to public shootings, and some are related to gangs or other criminal activity. Klarevas has also determined which of the gun massacres occurred in gun-free zones. We use the Klarevas and the Mother Jones datasets to test whether the number of people killed in gun massacres are different in gun-free zones. The results are reported in Section 7 below.

2.3 Policy Variables

There are four types of public policies that are potentially relevant to mass public shootings. The first consists of policies that make it difficult for potential shooters to gain access to firearms. Universal background checks are designed to close the "gun show loophole" such as sales completed in the parking lots outside gun shows and other private sales avoiding the background check that would be required if buying from a dealer. Permit-to-purchase laws require anyone who wants to buy a handgun to apply in person for a permit. The relevant authority usually has wide discretion in determining whether to issue the permit. Juvenile gun bans prohibit people under the age of twenty-one to possess firearms. Safe storage laws prevent unauthorized persons accessing privately owned firearms. Red flag laws allow individuals to go to authorities, usually the police, and report that someone they know, such as a family member, neighbor, or co-worker possesses firearms and could be a threat to himself or others. As a result of this complaint, the police can confiscate the subject's firearms temporarily until the subject can appear in court, at which time a judge will determine whether the firearms will be returned.

The second group of policies are designed to make it more difficult to kill many people in a short period of time. Bans on Large Capacity Magazines (LCM) are designed to force shooters to stop and reload more often, limiting the number of people killed. Assault weapons bans force shooters to use presumably less efficient weapons. The third group consists of laws that allow ordinary citizens to carry concealed weapons in public, thereby increasing the probability that a shooter will encounter armed resistance from potential victims and bystanders before the police arrive. Right-to-carry laws (also known as shall-issue laws) require that the authority charged with issuing concealed carry permits (e.g., police, judge, or magistrate) must issue the permit unless the applicant is disqualified according to a set of published requirements. We assume this policy increases the number people carrying concealed weapons compared to so-called “may-issue” states where the issuing authority has more discretion. Constitutional carry states allow citizens to carry concealed weapons with no permit, presumably increasing the number of people carrying concealed weapons even more than in right-to-carry states.

The fourth group consists of traditional crime-control policies. These policies (prison incarceration, police presence, and execution) are designed to deter most potential criminals. While shooters who are willing to die in the commission of their attack are unlikely to be deterred by the threat of prison or execution, expanded police presence could reduce response times, limiting the number killed. We also consider the 1994 Federal assault weapons ban, which also included a large capacity magazine ban, in this category. While it is true that individuals who are determined to commit suicide is unlikely to be deterred by any of these policies, there could be some potential mass shooters who are deterred by one or more of them.

The firearm policy variables, listed in Table 1, are dummy variables constructed from the dates of passage of state laws, taken from the RAND State Firearm Law Database (Note 9). Year zero is the year of passage.

Table 1. Policy Dummy Variables

Variables	Mean
State assault weapons ban	0.06
Universal background checks	0.08
Constitutional carry	0.02
Under-21 gun-ban	0.26
Large capacity magazine ban	0.06
Permit to purchase	0.04
Red flag law	0.12
Right to carry	0.35
Safe storage law	0.18

2.3 Control Variables

The control variables are listed in Table 2. In addition to a dummy variable for the years 1994-2004 when the federal assault weapons ban was in force, we include the traditional policy variables (prison population per capita, the number of sworn police officers per capita and executions per capita) (Note 10). We also include real per capita personal income, the unemployment rate, total employment per capita, the poverty rate, welfare payments per capita, alcohol consumption per capita, and population density that are standard in crime equations. We include military employment because military bases concentrate large numbers of young men while also sending many out of the country. The construction industry also employs many young men. As a measure of firearm ownership, we employ the widely used proxy of firearm suicides as a proportion of total suicides. Also, since mass shooters are willing to die in the commission of the act, and could be actively committing suicide by police action, we include the suicide rate as a control variable. We also include the percent of the population in two age groups, 15 to 39 and 40-64.

A potentially important factor is the emergence of crack cocaine in the 1980's. The resulting huge increase in the supply of cocaine caused turf battles among drug suppliers and increased the number of murders as well as other crime rates. We control for the crack cocaine epidemic with the Fryer, Heaton, Levitt, and Murphey (2013) crack index, a combination of indicators compiled by Fryer and his colleagues for the period of the crack epidemic. There are continuous values for 1981 to 2000 for each state. We set pre-1981 values at the 1981 levels, and post-2000 values at the 2000 levels.

Finally, we include a lagged dependent variable because widely publicized events such as a mass public shooting can inspire copycats, possibly creating a dynamic system in which each event creates more similar events. As noted above, we test all these control variables for significance and drop any with t-statistics less than one in absolute value, subject to the parsimonious encompassing test.

Table 2. Control Variables

Variable	Mean
Federal AW/LCM ban	0.22
Prisoners per capita	2.7
Police per capita	263
Executions per capita	0.092
Suicide rate	13.3
Robbery rate	121
Burglary rate	928
Unemployment rate	5.91
Real income pc	15.3
Employment per capita	551
Military employment pc	12
Construction employment pc	31.1
Poverty rate	12.9
Real welfare pc	216
Percent gun suicide	57
Alcohol per capita	1.96
Crack cocaine	0.93
Population density	174
Percent population 15-39	37.2
Percent population 40-64	28.3

3. Results

3.1 Selecting the Control Variables for the Difference-In-Differences Analysis

Before we can do the event studies, we must specify the control variables to be used by the difference-in-differences program. We regress the number of people killed in mass public shootings by state and year for the sample, 1976-2019 using the fixed-effects negative binomial model, under the null hypothesis that none of the policy dummies are effective. We use the general-to-specific modeling method to find the most significant determinants of mass shooting deaths and generate the model with the smallest mean squared error. However, since there could be a small amount of bias in the specific model, we also report the general model.

These two models are reported in Table 3.

Table 3. Negative Binomial Model to Generate Control Variables for DID

	General Model		Specific Model	
	Coeff	T-ratio	Coeff	T-ratio
Federal assault weapons ban	-0.105	-0.42		
Prison population pc	0.023	0.20		
Police per capita	-0.004	-1.28	-0.004	-1.56
Executions pc	-0.218	-0.64		
Suicide rate pc	-0.096	-1.88	-0.080	-1.82
Robbery rate	0.000	0.13		
Burglary rate	-0.000	-0.28		
Unemployment rate	0.163	2.35*	0.143	2.56*
Real income pc	1.359	0.33		
Employment pc	0.006	1.48	0.004	1.20
Military employment pc	-0.023	-0.94		
Construction employment pc	0.046	2.07*	0.043	2.44*
Poverty rate	0.000	0.30		
Real welfare pc	0.003	1.63	0.003	2.38*
Percent gun suicide	0.017	0.98		
Alcohol consumption pc	0.941	2.12*	0.855	2.40*
Crack cocaine	0.194	1.51	0.234	2.15*
Population density	-0.001	-1.29	-0.002	-2.11*
Percent population 15-39	-0.040	-0.40		
Percent population 40-64	-0.074	-0.70		
Yt-1	0.016	0.98		
N	2,009		2,009	

Note. * *p*<0.05; ** *p*<0.01; dependent variable is number killed in mass public shootings; fixed-effects negative binomial model; the parsimonious encompassing Wald-test on the dropped variables is not significant (p=0.87).

There is apparently no significant reduction in mass public shooting deaths associated with the federal assault weapons ban. Prison incarceration, police presence, and executions are also not significantly associated with deaths from mass public shootings. There is weak evidence that the suicide rate is negatively associated with mass shooting deaths indicating that policies designed to prevent suicides may have unintended consequences. Alcohol consumption is positively associated with mass shooting deaths as is the crack cocaine epidemic of 1985-1992. Unemployment, construction employment and welfare payments are also positively associated with mass shooting deaths.

3.2 Difference-in-Differences Event Study

The numerical results from the deChaisemartin-D'Haultfoeuille difference-in-difference analysis are presented in Table 4. The graphical results are presented in Figures 1-3. For each policy we do an event study with a 10-year event window. The analysis depends crucially on the parallel-trends assumption. It is impossible to test for parallel trends after the policy has been implemented, but a reasonable test can be implemented by specifying a placebo policy before the actual policy was adopted. If the placebo dummy or dummies are not significant, the parallel trends assumption is acceptable. We specify placebo dummy variables for the four years before the policy adoption date. All the placebo tests were insignificantly different from zero except for the constitutional carry analysis for which the specific model had one significant placebo test statistic (p=.035). Complete results are available at <http://cemood.people.wm.edu/ELP_mps.zip>.

Table 4. Average Effect of Policies on Mass Public Shooting Deaths

Policy	General Model			Specific Model		
	Effect	SE	T-ratio	Effect	SE	T-ratio
State assault weapons ban	-1.27	5.47	-0.23	0.07	1.86	0.04
Universal background checks	-0.80	1.23	-0.65	-0.23	1.25	-0.18
Constitutional carry	-0.15	0.51	-0.29	-0.19	0.41	-0.46
Under-21 gun-ban	0.63	0.80	0.79	0.69	0.61	1.13
Large capacity magazine ban	-1.37	1.83	-0.75	-0.27	1.49	-0.18
Permit to purchase	-2.03	2.51	-0.81	0.13	0.91	0.14
Red flag law	-0.40	0.72	-0.56	-0.19	0.62	-0.31
Right to carry	-0.09	0.42	-0.21	-0.26	0.40	-0.65
Safe storage law	0.43	0.68	0.63	0.20	0.61	0.33

Note. Estimated by *did_multiplegt*; bootstrapped standard errors, general and specific models refer to the choice of control variables for the DID model.

Two policies have both positive and negative average effects between the general and specific model (assault weapons bans and permit-to-purchase laws); two policies have positive average effects (under-21 gun-bans and safe storage laws); and four policies have negative average effects (universal background checks, red flag laws, right-to-carry laws, and constitutional carry laws). None of the policies have a significant average effect on the number of people killed in mass public shootings. Graphical results are presented in Figures 1-3.

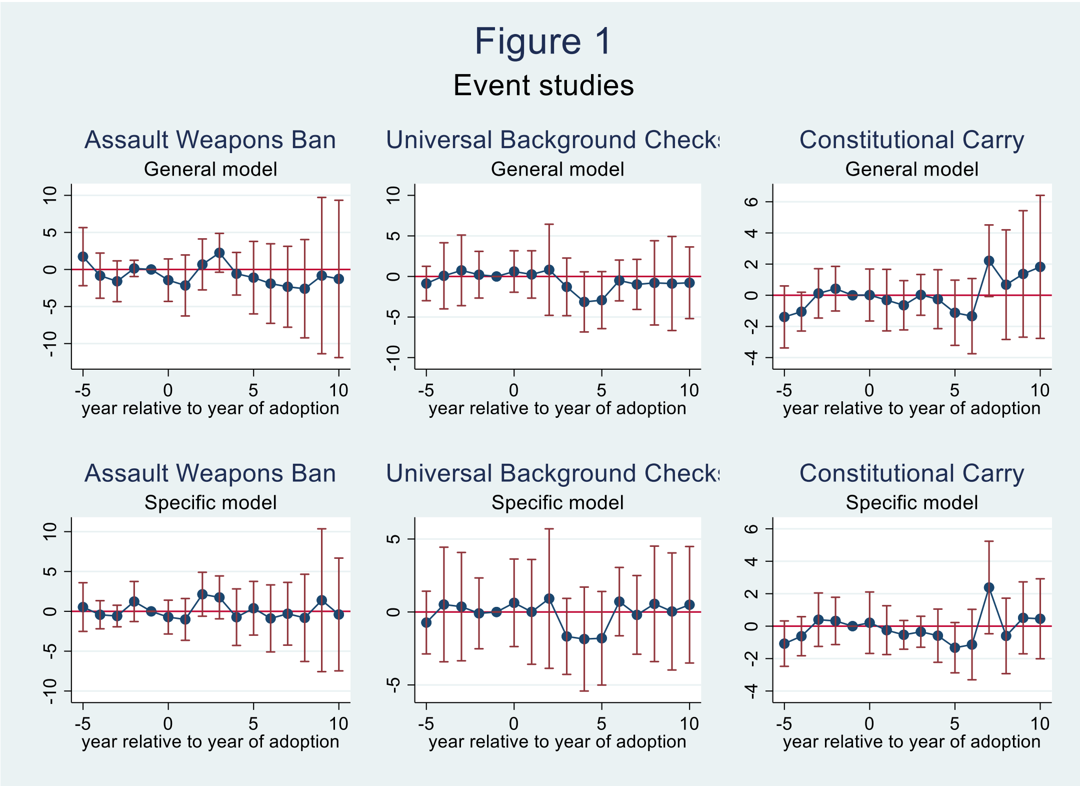

Figure 1. Event Studies

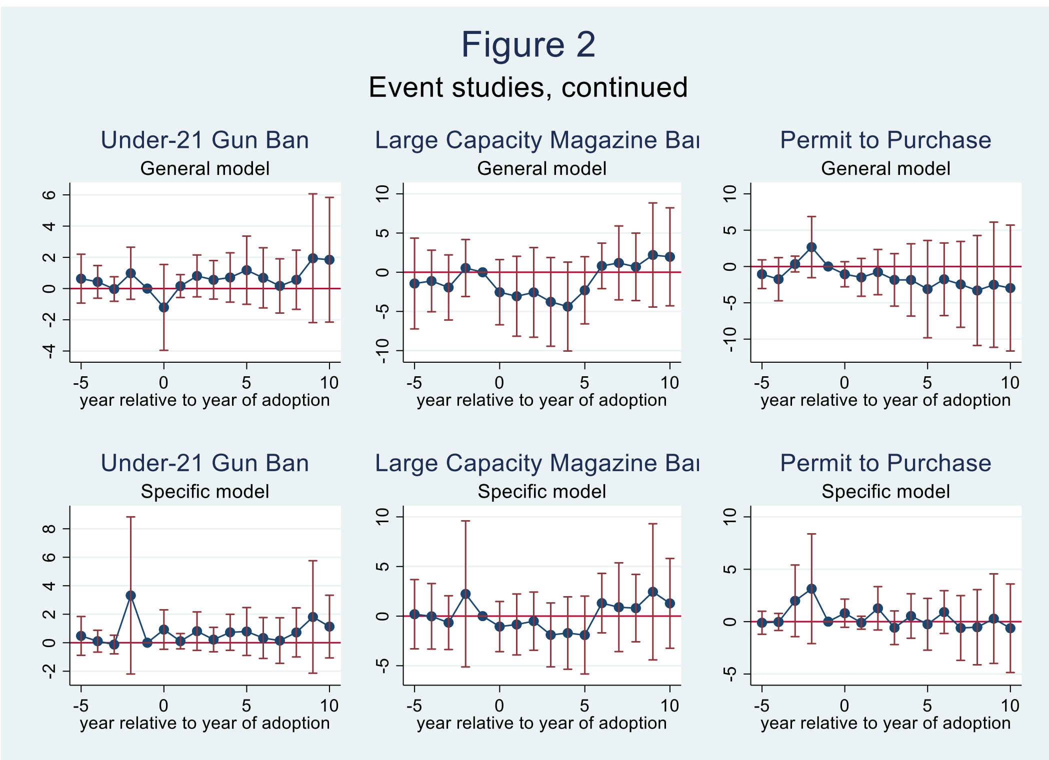

Figure 2. Event Studies, Continued

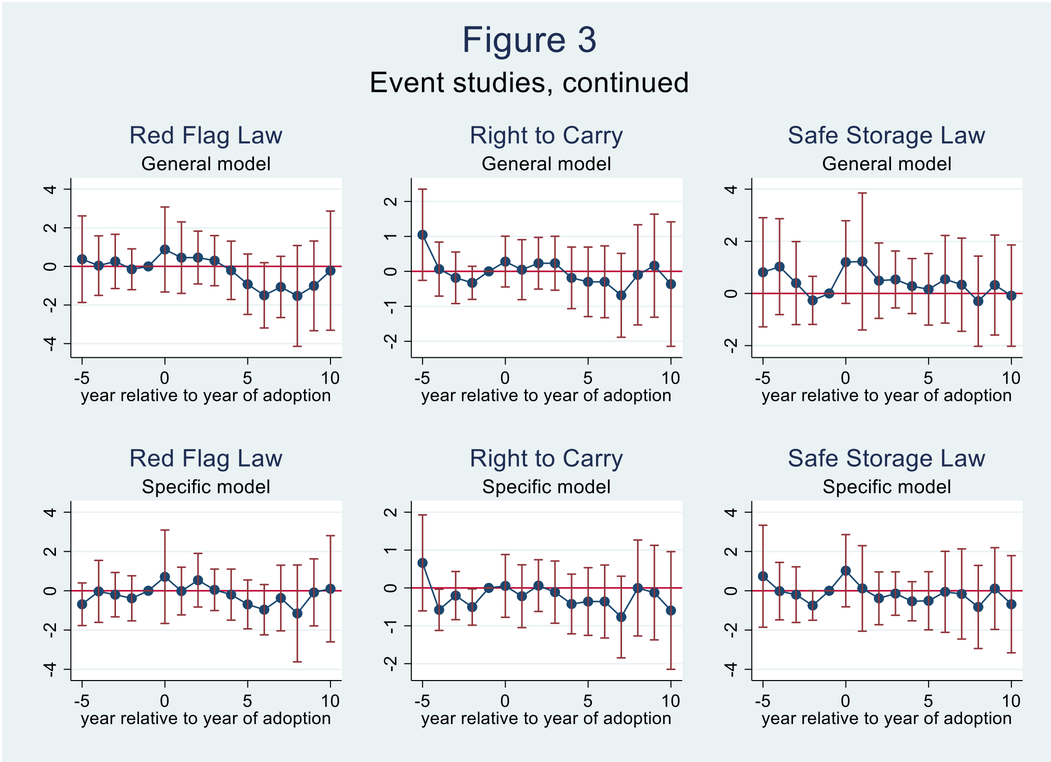

Figure 3. Event Studies, Continued

The overall lack of significance of the policy variables is what might be expected given that mass public shooters are willing to commit mass murder and are also willing to die in the attempt. Complete numerical results as well as all programs and data are available at <http://cemood.people.wm.edu/ELP_mps.zip>.

3.4 Robustness Tests

As a robustness test, we estimated the effectiveness of the various policies using Two-Way Fixed-Effects (TWFE) negative binomial models. The results are summarized in Table 5. According to CD (2020), the TWFE difference-in-differences coefficient is a weighted average of the individual average treatment effects on the treated (ATT's), where the weights could be negative. If there are many negatively weighted ATT's, the fixed-effects coefficient could be so biased that the sign is incorrect. We used the *twowayfeweights* program developed by CD to find the number of negative weights for each policy variable. For our models, there were negative weights associated with the right-to-carry laws (25 of 941) and the state assault weapons bans (4 of 392). The rest of the policy two-way fixed-effects coefficients had no negative weights. Since there were so few negative weights, we conclude that the results of the TWFE negative binomial models may be used for policy evaluation. We do not report the coefficients and t-statistics for the control variables to conserve space. None of the coefficients on the traditional policies (prison incarceration, police presence, and executions) were significantly different from zero. Complete results available at <http://cemood.people.wm.edu/ELP_mps.zip>.

Table 5. Policy Evaluation Using Two-Way Fixed-Effects Negative Binomial Models

Policy dummy	General		Specific	
	Coeff	T-ratio	Coeff	T-ratio
Right to carry	-0.522	-1.80	-0.406	-1.64
Constitutional carry	1.026	1.40	0.874	1.24
Under-21 gun-ban	-0.079	-0.27	-0.065	-0.24
Large capacity magazine ban	0.393	1.08	0.367	1.09
State assault weapon ban	0.445	1.21	0.475	1.39
Safe storage law	-0.005	-0.02	0.003	0.01
Permit to purchase	-0.041	-0.12	-0.090	-0.28
Universal background checks	0.554	1.88	0.584	2.09*
Red flag law	0.656	1.28	0.764	1.52

Note. * *p*<0.05; ** *p*<0.01; dependent variable is number killed in mass public shootings; the parsimonious encompassing Wald tests on the dropped variables are not significant; coefficients on control variables are suppressed.

The only significant coefficient is the positive coefficient on the dummy variable for universal background checks. The fixed-effects negative binomial results confirm the results from the previous section in that none of these policies significantly reduce deaths from mass public shooting attacks.

We also did an event study using the Violence Policy data set, which has a larger sample and slightly different definitions than Siegel et al. (2020). The results for the average total effects are presented in Table 6.

Table 6. Average Policy Effects Using Violence Project Data

Policy	General Model			Specific Model		
	Effect	SE	T-ratio	Effect	SE	T-ratio
State assault weapons ban	-0.71	2.16	-0.33	0.14	1.55	0.09
Universal background checks	-1.17	1.27	-0.92	0.61	1.25	0.49
Constitutional carry	-0.13	4.46	-0.03	-0.19	0.39	-0.49
Under-21 gun-ban	0.50	0.74	0.68	0.64	0.56	1.14
Large capacity magazine ban	-1.42	1.78	-0.80	-0.06	1.25	-0.05
Permit to purchase	-2.17	2.54	-0.85	0.04	0.82	0.05
Red flag law	-0.40	0.76	-0.53	-0.09	0.67	-0.13
Right to carry	-0.15	0.67	-0.22	-0.28	0.31	-0.90
Safe storage law	0.00	0.79	0.00	-0.19	0.75	-0.25

Note. * *p*<0.05; ** *p*<0.01; estimated by *did_multiplegt*; bootstrapped standard errors, general and specific models refer to the choice of control variables.

The results are close to those generated by the Siegel et al. (2020) data reported in Table 4. None of these policies appear to be effective in reducing deaths from mass public shootings.

3.5 Gun-free Zones

If mass shooters are attracted to places where firearms are not permitted, it would be expected that more people would be killed in such gun-free zones than in venues where the probability of armed resistance from civilians with concealed weapons is greater. The most expansive definition of a gun-free zone is provided by the Crime Prevention Research Center (CPRC): any public place where ordinary citizens are prohibited from carrying concealed firearms (Note 11). This definition includes places where armed police officers or armed security may be present on the theory that the shooter knows to kill the police or security officers first or wait until they leave. The definition also includes military bases because off-duty soldiers are not allowed to carry weapons while on base. Military police are armed but face the same threat as the police and private security officers guarding non-military venues. The definition also includes private businesses that have company policies prohibiting firearms on the premises. Citizens who are carrying concealed weapons and wish to work or do business there will leave their firearms in the car or otherwise disarm themselves, reducing the threat to potential shooters. Finally, any place in states that have may-issue laws are considered gun-free zones because ordinary citizens in those states are routinely denied permits to carry concealed firearms, making it extremely unlikely that the shooter will encounter armed citizens.

Mother Jones argues that shooters tend to attack places to which they have an emotional attachment, e.g., workplaces in which they were wronged, schools in which they were bullied, churches of people they hated, etc. Also, “Thirty-six of the killers we studied took their own lives at or near the crime scene, while seven others died in police shootouts they had no hope of surviving (a.k.a. ‘suicide by cop’). These were not people whose priority was identifying the safest place to attack” (Note 12). On the other hand, while mass shooters almost certainly have emotional attachments to many places, they may avoid those places where their plans could be upset by civilians with concealed weapons. Even if they plan to die, they might want to kill the largest number of people first, seeking posthumous infamy. This can be done best if no one is shooting back. Many shooters leave behind manifestos or Facebook pages where they describe their search for a venue where potential victims will not be armed (Note 13). In the Mother Jones definition, a place that allows firearms is not a gun-free zone simply because it is in a may-issue state. Places that use armed security are also not considered gun-free. We identified 60 cases where we could determine if the CPRC gun-free zone dummy could be designated a one or a zero and 44 with enough information to determine values for the Mother Jones gun-free-zone dummy.

Klarevas (2016, pp. 72-73) has done a count of mass shootings from 1966-2015 with six or more fatalities. He also identified those occurring in gun-free zones (no guns allowed, including police), gun-restricted zones (no guns allowed except for police), and all other, gun-allowing zones (Klarevas, 2016, pp. 162-164). Using Klarevas’ definition, we supplemented his data with data from 2016-2019 taken from the CPRC mass public shooting database, which includes information as to whether the incident took place in a gun-free zone. The means for the gun-free-zone dummies are shown in Table 7.

Table 7. Means for Gun-free-zone Dummies

Dummy variable indicating gun free zone	N	Mean
CPRC	60	0.88
Mother Jones	44	0.52
Klarevas gun free zone	114	0.12
Klarevas gun restricted zone	114	0.04
Klarevas guns not allowed (gun-free or gun-restricted)	114	0.16

These are not panel data, so we use the cross-section negative binomial model with heteroskedasticity robust standard errors. The control variables are the same as in the previous sections. We also present a general model with all the controls, and a specific model dropping control variables with t-statistics less than one in absolute value. The results are reported in Tables 8 and 9 where we report the coefficients on the dummy variables but suppress the results with respect to the coefficients on the control variables.

Table 8. Effect of Gun-free Zones on Mass Public Shooting Fatalities, CPRC and Mother Jones Definitions

Fatalities	General Model	Specific Model	N
CPRC	-0.201	-0.127	60
	(0.64)	(0.41)	
Mother Jones	0.418	0.541	44
	(1.68)	(2.56)*	

Note. * p<0.05; ** p<0.01; negative binomial model; robust t-statistics in parentheses; coefficients on control variables are suppressed; complete results available at <http://cemood.people.wm.edu/ELP_mps.zip>.

The CPRC gun-free zone dummy is not significant, perhaps because almost nine out of ten of the shootings took place in gun-free zones. Using the Mother Jones definition, mass public shootings that take place in gun-free zones result in more deaths than those that occur in other places, significantly more deaths using the specific model.

The results using Klarevas' gun-massacre data and the corresponding gun-free and gun-restricted zone dummies are presented in Table 9.

Table 9. Effect of Gun-free Zones on Mass Public Shooting Fatalities, Klarevas Definitions

Fatalities	General model	Specific model
Gun-free zone	0.389	0.392
	(2.46)*	(2.37)*
Gun-restricted zone	0.855	0.752
	(3.01)**	(2.54)*
Gun-not-allowed zone	0.495	0.485
	(3.49)**	(3.12)**
N	114	114

Note. * $p<0.05$; ** $p<0.01$; negative binomial model; robust t-statistics in parentheses; coefficients on control variables are suppressed; complete results are available at <http://cemood.people.wm.edu/ELP_mps.zip>.

Using the Klarevas data and definitions, both the gun-free dummy and the gun-restricted dummy are positive and significant in both models. A Wald test for the null hypothesis of equality of the coefficients was not rejected. We then combined them into the gun-not-allowed dummy, which was also positive and significant in both the general and specific models. Overall, there is evidence that mass public shootings that occur in those places where ordinary citizens are prohibited from carrying concealed weapons are associated with significantly higher fatalities compared to places that allow such weapons, especially for mass shootings in which six or more people are killed.

4. Discussion

Using the latest difference-in-differences analyses and fixed-effects negative binomial regressions, we tested nine gun-related policies and four traditional crime-control policies. Despite the findings in previous studies that permit to purchase laws and large capacity magazine bans may be effective, we find that neither these, nor the other policies investigated in this study can be shown to be effective in reducing the number of people killed in mass public shootings. This result might be expected because criminals who are willing to commit the most heinous crime and who are also willing to die in the commission of that crime, are unlikely to be deterred by a safe storage law, for example. On the other hand, there is evidence that increasing the number of places where ordinary citizens are allowed to carry concealed weapons could reduce the number of mass shooting fatalities.

Complete results, data, and programs are available at <http://cemood.people.wm.edu/ELP_mps.zip>.

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Notes

Note 1. https://www.rand.org/research/gun-policy/analysis/essays/mass-shootings.html

Note 2. https://www.apa.org/news/podcasts/speaking-of-psychology/mass-shootings

Note 3. https://crimeresearch.org/2018/06/more-misleading-information-from-bloombergs-everytown-for-gun-safety-on-guns-analysis-of-recent-mass-shootings/

Note 4. https://everytownresearch.org/mass-shootings-in-america/

Note 5. https://www.motherjones.com/politics/2012/12/mass-shootings-mother-jones-full-data/

Note 6. https://crimeresearch.org/data/

Note 7. https://www.theviolenceproject.org/mass-shooter-database/

Note 8. https://sgp.fas.org/crs/misc/R43004.pdf (p. 4)

Note 9. https://www.rand.org/pubs/tools/TLA243-2-v2.html

Note 10. As a result of including a dummy variable for the federal assault weapons ban, we do not include year dummies in this specification. We include the year dummies, and not the federal ban, in the robustness section.

Note 11. https://crimeresearch.org/2018/06/more-misleading-information-from-bloombergs-everytown-for-gun-safety-on-guns-analysis-of-recent-mass-shootings/

Note 12. https://www.motherjones.com/politics/2013/04/gun-free-zones-mass-shootings/

Note 13. https://crimeresearch.org/2023/03/vince-vaughn-explains-the-obvious-how-mass-killers-pick-out-venues-where-their-victims-are-sitting-ducks/