

# Detecting a copycat effect in school shootings using spatio-temporal panel count models

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## Abstract

School shootings are often motivated by the perpetrators' desire for media attention and notoriety. As school shootings receive intense regional and national media coverage, a high likelihood for copycat attacks can be expected. We investigate whether a copycat effect can be detected in US state-level school shooting data from 1990 to 2017. We do so by estimating spatio-temporal panel count models and control for socio-economic characteristics, as well as state and Federal gun control laws. Positive spatial and temporal dependence indicate that the risk for additional school shootings in the same and neighboring states increases after the initial attack.

## KEYWORDS

criminology, gun control, linear feedback model, state-level

## JEL CLASSIFICATION

C33; C53; K42

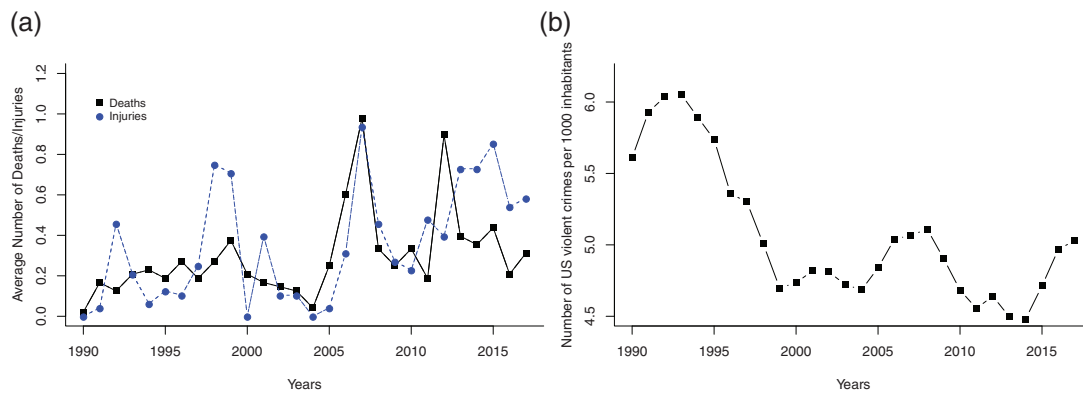
## 1 | INTRODUCTION

School shootings are tragic events with the ability to traumatize communities. The motivation for (attempted) school shootings can often be linked to the perpetrators' desire for media attention and notoriety. Intense media coverage can be observed after especially disturbing school shootings, like the Columbine school shooting in 1999, the Virginia Tech shooting in 2007, and the Sandy Hook Elementary School shooting in 2012. Although campaigns against excessive media coverage like "No notoriety" and "Don't name them" exist, it is still common that the biography of the shooter and exact details of the attack are communicated to the public. Hence, it is very likely that potential copycat shooters are exposed to initial attacks and inspired to imitate them. Follman and Andrews (2015), and Lankford and Tomek (2018) study the so-called "Columbine effect" which denotes the hypothesis that the Columbine school shooting influenced several subsequent school shootings. Some perpetrators referred directly to Columbine when they described the motivation for their actions. Lankford and Tomek (2018) report that the Columbine shooting inspired at least 21 copycat shootings and 53 thwarted plots in the United States over a 15-year period.

Strikingly, the trajectory of the number of school shooting victims over the period 1990–2017 differs markedly from the trajectory of other violent crimes in the United States over the same period. Figure 1a and b contrast the increasing average number of victims of school shootings to a general decline in violent crimes per 1,000 inhabitants.<sup>1</sup> Although

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**FIGURE 1** Time series plots: School shooting victims and violent crimes

we do not clearly see a spike in the state-level victim counts in the years immediately after the Columbine school shooting, we observe that the average number of victims is significantly larger in the second half of our sampling period. It remains an open question why the victim counts of school shootings have increased over time. Several issues have been put forth as potential explanatory factors like, for example, inadequate gun control laws (Blau et al., 2016; Gius, 2018), dysfunctional families (Gerard et al., 2016), exposure to violent video games (Ferguson, 2008), and mental illnesses (Newman & Fox, 2009).

Beyond exogenous factors, the frequency of school shooting incidents could partly be explained by copycat behavior and social contagion following the idea that vulnerable youth are susceptible to suicide ideation brought on by the influence of reports and portrayal of suicide in mass media (Lin et al., 2018; Torrecilla et al., 2019; Towers et al., 2015). In this paper, we attempt to detect and estimate copycat effects using spatio-temporal panel count models. This class of models allows us to quantify the degree of spatial and temporal dependence, that is, reveals whether an initial attack spawns subsequent attacks in neighboring states and in the same state in the following years.

The literature on violent crimes and mass shootings uses the terms copycat effects and social contagion to describe the spatio-temporal dependence of these incidents (see, e.g., Towers et al., 2015, Kissner, 2016, Lankford & Tomek, 2018, Loeffler & Flaxman, 2018, and Torrecilla et al., 2019). We adopt the definition of copycat crimes from (Helfgott, 2008, p. 377) stating that “[c]opycat crime is crime inspired by another crime that has been publicized in the news media or fictionally or artistically represented whereby the offender incorporates aspects of the original offense into a new crime” and follow Lankford and Tomek (2018) stating that “[...] the social contagion thesis suggests that perpetrators receive so much attention for their attacks that each high-profile killer ends up “infecting” the minds of other impressionable individuals.” Lankford and Tomek (2018) distinguish between contagion and copycat effects using the temporal horizon of the subsequent attacks. Contagion describes the short-term imitative effects of a crime, while the long-term imitative effects of a crime are categorized as copycat effects. We are primarily interested in estimating long-term copycat effects in school shooting data.

The rare nature of this crime makes it difficult to profile school shooters. An earlier study (United States Secret Service and United States Department of Education, 2004) analyzing the data of 41 perpetrators from 1974 to 2000 could not generate an accurate profile of the average perpetrator. However, most perpetrators had experienced important losses or personal failures and felt bullied, persecuted, or injured by others. The sample consisted only of male perpetrators and 95% of them were current students of the attacked school. Gerard et al. (2016) characterize the “average” US school shooter over a sample of 28 cases. The shooter is predominantly male, Caucasian, a US citizen, and suffered from depression. Most cases involved the suicide of the perpetrator. Langman (2018) collects a list of perpetrators and their respective influences on each other. For several school shootings, he can establish clear links to prior shootings which served as inspiration. Particularly, the Columbine school shooting has served as inspiration for many subsequent shootings. Pah et al. (2017) find evidence for the hypothesis that low perspectives of school-to-work transition increase gun violence in schools. Consequently, it is important to control for variables that accurately describe socio-economic characteristics of each state. Most perpetrators of school shootings plan their attack and take their time for preparations, while impulse attacks are rare. A study by the Federal Bureau of Investigation (FBI) on mass shootings (Federal Bureau of Investigation, 2014) reports that more than three-quarters of the perpetrators spent a week or longer planning their attacks, while two-thirds spent at least a month planning, and some spent far longer. Although school shooters are a specific type of mass shooters and results for one type cannot easily be transferred to the other, it is reasonable to assume

that school shootings are often premeditated. Estimating the temporal dependence helps us to answer whether the frequency of school shootings in one state increases if the same state reports a higher number of victims in the past year. Further, those perpetrators might be inspired by attacks in their (extended) neighborhoods and we try to answer whether the frequency of school shootings increases in one state if neighboring states report a shooting in the current or past year.

School shootings and the fear of copycat shooters have drastically negative effects on society. Besides the actual victims of a school shooting, traumatized communities often report secondary victimization in form of psychological health issues. Rossin-Slater et al. (2019) estimate that a fatal school shooting increases the use of youth antidepressants by 21.4% in the 2 years after the shooting. Moreover, Beland and Kim (2016) find that the academic performance of students who remain enrolled in the attacked school decreases. Abouk and Adams (2013) investigate the effects of school shootings on private school enrollment. They estimate that state-level enrollment in private high-schools increases by 10%–12% when a school shooting occurred in the previous year in the same state. While most severe school shooting incidents sparked an intense national debate about gun control (Luca et al., 2020; Schildkraut & Hernandez, 2014), only some incidents have led to changes in gun control laws. For example, Connecticut required background checks for all firearm purchases after the Sandy Hook Elementary School shooting. Additionally, the economic effects of mass shootings and school shootings in particular have been investigated in several studies. Deangelis et al. (2011) estimate the cost of additional school security. Gopal and Greenwood (2017) study the effects of mass shootings on stock prices of firearm manufacturers. Brodeur and Yousaf (2019) observe a significant negative effect of mass shootings on local employment, earnings, house prices, and consumer confidence, which is found to be exacerbated by extensive media coverage. Moreover, Dursun (2019) examines whether in utero exposure to mass shootings has adverse effects on infant health, and Yousaf (2019) examines the impact of mass shootings on electoral outcomes.

In our study, we follow Gius (2018) and estimate state-level panel count models for US school shooting data. Our baseline model specification is the fixed effect Poisson panel model which accounts for the integer and non-negative nature of victim counts. Our second model includes spatial terms in the form of contemporaneous and lagged neighboring victim counts. The so-called Poisson spatial panel model (P-SPM) was proposed by Glaser et al. (2020) and applied to urban crime counts in Pittsburgh, PA. Next, we apply a linear feedback model according to Blundell et al. (2002). Here, a first-order lag of the dependent variable is added to the basic Poisson panel model specification. In contrast to the previous two models which focused on either spatial or dynamic effects, our final model, the spatial panel linear feedback model (SPLFM), analyses relationships along both dimensions simultaneously. This enables us to separate both effects and provide insights which cannot be obtained in one-dimensional approaches. Such models, that have at least one spatial and temporal term, are classified as spatio-temporal models. Various approaches have been proposed in the crime literature, like the Bayesian spatio-temporal model by Li et al. (2014) or the spatial random effects model by Liesenfeld et al. (2017). However, in this paper, we follow Glaser et al. (2020) and use a multiplicative fixed effect model with spatial and dynamic lags. They assume a Poisson probability model where the spatio-temporal effects impact the intensity equation, that is, it allows us to estimate how a school shooting in one state increases or decreases the probability for a higher victim count in neighboring states and the same state in the following year. Further, we can use our results to re-evaluate the impact of gun control laws controlling for the spatial dimension of the data. More specifically, we can compare the results of the fixed effects Poisson model proposed by Gius (2018) to the results obtained after controlling for spatial effects. The available dataset is challenging from a statistical perspective with a small sample size ( $N = 48$ ,  $T = 28$ ) and many zero entries for the dependent variable. We account for the potential occurrence of excess zeros using robust inference.

One explanation for the suspected “Columbine effect” is the intense media coverage of the Columbine shooting which, in turn, might be partially explained by a changing media landscape. To control for confounding factors of this kind, we include time-fixed effects in our panel models. Thereby, we pick up any (unobservable) variables without state-level variation, for example also controlling for sentiment towards gun control, and advances in information technology which provides easier access to information about previous school shootings. Lankford and Tomek (2018) point out that imitative mass killers need to be exposed to the model killer’s behavior. Our dataset contains both intensively covered school shootings with high victim counts and some minor incidents which only received regional media coverage. While information about the former events spreads through media transmissions and the proprietors receive wide national coverage, information about the latter events is geographically restricted and proprietors gain only regional notoriety. We aim to capture this difference by modeling the spatial dependence of school shootings.

Our results can help to plan the response of authorities to school shooting incidents. For example, we can quantify whether there is a higher risk that a subsequent school shooting happens in neighboring states (spatial effect) or in the same state in the next year (temporal effect) after an initial attack. Thereby, we take a “macro” perspective and focus on which states are at a higher risk and, hence, should take preventive countermeasures, instead of a “micro”

perspective taken by behavioral scientists who attempt to prevent school shootings by identifying individual students at risk. While the main focus of this study is the quantification of copycat effects, we also contribute to the literature by providing new evidence on the effect of gun control laws on school shootings.

The remainder of the paper is organized as follows. Section 2 outlines the econometric framework applied in the empirical part of the paper. Section 3 details the dataset. Section 4 reports the results of the empirical application. Section 5 concludes.

## 2 | METHODOLOGY

We consider several model specifications to address different features of our data. First, we estimate a static Poisson fixed effects panel model and compare our results for the updated dataset to those originally reported in Gius (2018). We assume that the data are generated from the following Poisson model to capture their integer and non-negative nature. The model takes the form of

$$\begin{aligned} y_{it} | \mu_{it}, \alpha_i &\sim \text{Pois}(\mu_{it}\alpha_i), \\ \mu_{it} &= \exp(X'_{it}\beta), \end{aligned} \quad (1)$$

where  $y_{it}$  is a variable counting the number of victims injured or killed during school shootings in state  $i$  at time  $t$ ,  $X_{it} = [x_{it1}, \dots, x_{itK}]$  is a  $K \times 1$  vector of regressors,  $\beta$  is a  $K \times 1$  vector of coefficients, and  $\alpha_i = \exp(\eta_i)$  are multiplicative individual fixed effects. Time fixed effects are implemented by adding yearly time dummies to the regressor matrix.  $N$  denotes the number of states and  $T$  denotes the number of years in our sample. The coefficient estimates are obtained using maximum likelihood estimation (MLE) according to Hausman et al. (1984) and we employ cluster-robust standard errors at the state level. This model specification is our starting point before we successively add spatial and dynamic lags to capture copycat effects.

In our next model specification, we include spatial lags to account for the geographic variation in the data. In this model, spatial autocorrelation parameters are estimated to capture dependencies between different observations which rely on geographical proximity. Consequently, these parameters measure to what degree perpetrators are influenced by school shooting incidents in nearby states. For this purpose, we need to construct a spatial weights matrix  $W$  to express the geographic pattern of our data. In principle, many choices of the spatial weight matrix are available in the literature like, for example, the full inverse distance matrix, the eight nearest neighbors matrix (Lambert et al., 2010) or the queen contiguity matrix (Glaser et al., 2020). Although all choices are regularly used in empirical studies, we need to be aware that there are some conceptual differences. The full inverse distance matrix consists of the inverse Euclidean distances for the centroids of each pair of states in the sample and the eight nearest neighbors matrix is obtained by first calculating the inverse distance matrix, but then keeping only the values for the eight nearest neighbors of each observation and setting all other elements to zeros. In contrast, the entries of the queen contiguity matrix are defined in the following:  $w_{ij} = \frac{a_{ij}}{\# \text{neighbors}}$ ,  $i, j = 1, \dots, N$ , where  $a_{ij} = 1$ , if  $i$  is a neighbor of  $j$  (sharing a common border or a common vertex) and  $a_{ij} = 0$ , if  $i$  is not a neighbor of  $j$ . Diagonal elements  $w_{ii}$  are set to zero. Using this definition, the queen contiguity matrix is then row-standardized. We take the following perspective, assuming that an individual that lives in a state which is not in close proximity to the state where a school shooting happened, might only hear about this incident through the media, that is, the spill-over effect should die out after a certain distance to the initial shooting's location. As US states have very different geography, considering simple inverse Euclidean distances for each pair of states does not seem to be a reasonable measurement for our setting. The eight nearest neighbors matrix partly addresses this problem by introducing a cut-off point. However, this cut-off point is chosen arbitrarily and because this concept is inflexibly restricted to the eight nearest neighbors, border states are treated differently than centered states. Consequently, we base our main results on the queen contiguity matrix which only indicates whether two counties share a common border or vertex.

The P-SPM following Glaser et al. (2020) is given by

$$\begin{aligned} y_{it} | \mu_{it}, \alpha_i &\sim \text{Pois}(\mu_{it}\alpha_i), \\ \mu_{it} &= \rho \sum_{\substack{j=1 \\ j \neq i}}^N w_{ij} y_{jt} + \lambda \sum_{\substack{j=1 \\ j \neq i}}^N w_{ij} y_{jt-1} + \exp(X'_{it}\beta), \end{aligned} \quad (2)$$

where  $\rho$  denotes the spatial autocorrelation parameter for the cross section and  $\lambda$  measures the dependence on neighboring observations in the previous time period.<sup>2</sup> As we exclude own lags in the second term, we do not induce dynamic endogeneity and the coefficients can again be estimated using MLE. We estimate our model based on the likelihood function derived in Cameron and Trivedi (2005, p. 805). We note that the regressor matrix  $X$  might contain spatially lagged regressors in the form of  $Wx_{tk}$  for  $k = 1, \dots, K$  and the coefficients of these regressors can be estimated in the same way.

To account for temporal dependence, we add an autoregressive term to the baseline Poisson model specification. The autoregressive coefficient indicates whether perpetrators react to school shooting incidents in their home states in the previous year. The linear feedback panel model (LFPM) according to Blundell et al. (2002) includes the lag of the dependent variable additively instead of including it in the argument of the exponential function. Inclusion of the lagged dependent variable in the exponential function may lead to rapidly exploding series which does not happen in the LFPM as long as a stationarity condition is fulfilled. However, this model requires a different estimation strategy to deal with endogenous regressors. A convenient alternative is the distribution-free generalized method of moments (GMM) approach according to Hansen (1982). MLE does not yield consistent estimates for the LFPM, but GMM using quasi-differencing transformations eliminates initial values and correlated heterogeneity (Mullahy, 1997). Alternatively, Blundell et al. (2002) suggest to use a presample mean estimator, which uses presample information about the dependent variable to deal with the unobserved fixed effects. As no presample school shooting data are available, we follow the GMM approach and use the efficient two-step GMM estimator. The LFPM is given by

$$\begin{aligned} y_{it} | \mu_{it} &\sim \text{Pois}(\mu_{it}), \\ \mu_{it} &= \gamma y_{it-1} + \exp(X'_{it}\beta + \eta_i), \end{aligned} \quad (3)$$

where the autocorrelation parameter should fulfill the condition  $\gamma \geq 0$  to ensure that the conditional expectation is positive. It is further assumed that the autocorrelation coefficient satisfies the stationarity condition  $\gamma < 1$ .

Windmeijer (2008) suggests to use either the Chamberlain quasi-differencing transformation (Chamberlain, 1992) or the Wooldridge quasi-differencing transformation (Wooldridge, 1997) for GMM estimation. We assume that the lagged dependent variable is the only endogenous regressor and all regressors in  $X$  are predetermined, that is, they are uncorrelated with future and current shocks. We apply the Wooldridge quasi-differencing transformation in this paper and the quasi-differenced errors ( $q_{it}$ ) are given by

$$q_{it} = \frac{(y_{it} - \gamma y_{it-1})}{\exp(X'_{it}\beta)} - \frac{(y_{it-1} - \gamma y_{it-2})}{\exp(X'_{it-1}\beta)} = \frac{u_{it}}{\exp(X'_{it}\beta)} - \frac{u_{it-1}}{\exp(X'_{it-1}\beta)}, \quad (4)$$

where  $u_{it}$  is a mean-zero error term. In the case of a single regressor in  $X$ , the resulting  $(T-1)^2 - 1$  moment conditions hold if the respective regressor is predetermined. The conditional expectation of  $q_{it}$  is given by

$$E(q_{it} | y_{i1}, \dots, y_{it-2}, X_{i1}, \dots, X_{it-1}) = 0. \quad (5)$$

We denote with  $\theta = (\gamma, \beta)'$  the vector of parameters to be estimated. The GMM estimator  $\hat{\theta}$  can be expressed by

$$\hat{\theta} = \underset{\theta \in \Theta}{\operatorname{argmin}} \left( \frac{1}{N} \sum_{i=1}^N q_i(\theta)' Z_i \right) H_N^{-1} \left( \frac{1}{N} \sum_{i=1}^N Z_i' q_i(\theta) \right), \quad (6)$$

where  $q_i(\theta)$  is the  $T-2$  vector  $(q_{i3}, q_{i4}, \dots, q_{iT})'$ ,  $Z_i$  is the matrix of instruments, and  $H_N$  is a weight matrix of the moment conditions.

We follow Roodman (2009) and use collapsed instruments to avoid overfitting endogenous variables. Consequently, the moment conditions are summarized over  $t$  and the resulting instrument matrix for two lags of the dependent variables and the regressors takes the form of



$$Z_i = \begin{bmatrix} y_{i1} & 0 & X_{i2} & X_{i1} \\ y_{i2} & y_{i1} & X_{i3} & X_{i2} \\ y_{i3} & y_{i2} & X_{i4} & X_{i3} \\ \vdots & \vdots & \vdots & \vdots \\ y_{iT-2} & y_{iT-3} & X_{iT-1} & X_{iT-2} \end{bmatrix}. \quad (7)$$

The first step GMM estimator  $\hat{\theta}_1$  can be determined using the initial weight matrix  $H_N = 1/N \sum_{i=1}^N Z_i' Z_i$  and the second step GMM estimator using the efficient weight matrix

$$H_N = \frac{1}{N} \sum_{i=1}^N Z_i' q_i(\hat{\theta}_1) q_i(\hat{\theta}_1)' Z_i. \quad (8)$$

The asymptotic variance of the efficient two-step GMM, estimator is estimated by

$$\widehat{Var}(\hat{\theta}_2) = \frac{1}{N} \left( C(\hat{\theta}_2)' H_N^{-1}(\hat{\theta}_1) C(\hat{\theta}_2) \right)^{-1}, \quad (9)$$

where

$$C(\hat{\theta}_2) = \frac{1}{N} \sum_{i=1}^N \frac{\partial Z_i' q_i(\theta)}{\partial \theta} \bigg|_{\hat{\theta}_2}. \quad (10)$$

Note that the second step GMM estimator is efficient in the presence of heteroscedasticity of unknown form (Baum et al., 2003).

Finally, we combine the spatial and dynamic extensions to specify our spatio-temporal panel model. SPLFM is given by

$$y_{it} | \mu_{it} \sim \text{Pois}(\mu_{it}),$$

$$\mu_{it} = \gamma y_{it-1} + \rho \sum_{\substack{j=1 \\ j \neq i}}^N w_{ij} y_{jt} + \exp(X_{it}' \beta + \eta_i). \quad (11)$$

Unlike in the P-SPM specification, we do not include a lagged spatial term in our spatio-temporal model because the inclusion of such terms leads to identification problems (see, e.g., Anselin et al., 2008 and Elhorst, 2010, for a more detailed discussion). The model is estimated using the quasi-differenced GMM approach outlined for the LFPM. If we assume that the regressors in  $X$  are predetermined, we can use a similar instrument matrix as before, adding lags of the contemporaneous spatial term. For this case, two lags of the dependent variable,  $y_{t-2}$  and  $y_{t-3}$ , two lags of the contemporaneous spatial term,  $Wy_{t-2}$  and  $Wy_{t-3}$ , and two lags of the regressors,  $X_{t-1}$  and  $X_{t-2}$  are chosen. Hence, the following collapsed instrument matrix is used for our full model

$$Z_i = \begin{bmatrix} y_{i1} & 0 & [Wy_1]_i & 0 & X_{i2} & X_{i1} \\ y_{i2} & y_{i1} & [Wy_2]_i & [Wy_1]_i & X_{i3} & X_{i2} \\ y_{i3} & y_{i2} & [Wy_3]_i & [Wy_2]_i & X_{i4} & X_{i3} \\ \vdots & \vdots & \vdots & \vdots & \vdots & \vdots \\ y_{iT-2} & y_{iT-3} & [Wy_{T-2}]_i & [Wy_{T-3}]_i & X_{iT-1} & X_{iT-2} \end{bmatrix}, \quad (12)$$

where  $[Wy_t]_i$  denotes the  $i$ th row of the product  $Wy_t$ .

According to Elhorst (2008), the trade-off between the temporal and spatial autocorrelation coefficient requires that our model satisfies joint stationary conditions. The following stationary conditions, taken from Elhorst (2012) and Glaeser et al. (2020), have to be satisfied to obtain a stable system:

$$\rho w_{\max} - 1 < \gamma < 1 - \rho w_{\max} \quad \text{if } \rho \geq 0$$

$$\rho w_{\min} - 1 < \gamma < 1 - \rho w_{\min} \quad \text{if } \rho < 0,$$

where  $w_{\min}$  and  $w_{\max}$  denote the smallest and largest characteristic root of the spatial weight matrix, respectively.<sup>3</sup>

To further investigate spillover effects and compare direct effects to the findings for our baseline model specification without spatio-temporal terms, we derive the reduced form of the SPLFM and the Jacobian  $\partial E(y_i)/\partial X_{ik}$ . The derivations needed to compute those are given in Appendix B in the Supporting Information. LeSage and Pace (2009) use the mean of the diagonal of  $\partial E(y_i)/\partial X_{ik}$  as the direct effect and the average row sum, excluding the elements on the diagonals, as indirect or spillover effects. Following Yesilyurt and Elhorst (2017), we compute short-term direct and indirect effects by setting  $\gamma = 0$ . Statistical inference for marginal effects is based on the parametric bootstrap outlined in Appendix C in the Supporting Information.

### 3 | DATA

We extend the dataset on US school shootings during the period 1990–2014 originally collected by Gius (2018) to include the years 2015 to 2017. The dataset contains counts of people injured and people killed in school shootings for each state, socio-economic variables, and information on state and Federal gun control laws. Our definition of school shootings encompasses any shootings which happened at an educational institution. We model the total number of victims (injured or killed), because a single school shooting incident usually leads to fatal and nonfatal injuries and, from a statistical perspective, this specification minimizes the occurrence of zero counts. In addition, the total victim count is often communicated in the media following school shooting incidents and a higher overall victim count usually evokes increased media attention.

The socio-economic control variables include *population density*, *per household median income*, *proportion of population with college degree*, *unemployment rate*, *proportion of population aged 5–18*, *per capita alcohol consumption*, and *ratio of firearm suicides to total suicides*. As the income per capita variable was discontinued by the US Census Bureau in 2014, we instead use median income (in 10,000 USD) per household for the full sample period. The firearm suicide variable is a popular proxy for gun ownership prevalence because it is highly correlated with survey-based estimates (Azrael et al., 2004; Lang, 2013). We extend the set of variables with an additional variable to capture the effect of nonschool-related mass shooting events in a state. For this purpose, we construct an indicator variable taking the value one if a mass shooting happened in a given state-year. The list of events is based on a database of mass public shootings assembled by Siegel et al. (2020).<sup>4</sup> To further investigate whether nonschool-related mass public shootings are imitated by school shooters and whether these types of copycat behaviors act as confounding factors in our spatio-temporal models, we also include a spatial lag of the *mass shootings* variable.

Descriptive statistics on all variables used in this study are reported in Table 1. We find that the within-variation of per capita *alcohol consumption* and the *state background checks* variable is much smaller than the between-variation. Since our fixed effects models eliminate between-variation, it becomes difficult to estimate the coefficients of these variables.

We include gun law variables indicating whether a state conducts background checks for private party sales, whether it bans assault weapons, and whether it has restrictive concealed carry weapons (CCW) laws. As the Federal assault weapons ban was in effect during the period 1994–2004, we follow Gius (2018) and set the assault weapons ban dummy variable to 1 for all states during the Federal assault weapons ban period. The dummy variable for CCW laws is set to 1 for all states that either prohibit private citizens to carry concealed weapons or are “may issue” states, that is, they can deny requests for concealed carry permits from qualified applicants. We also include a dummy variable indicating whether states impose a private sales background checks law. In contrast to Gius (2018), we do not consider a Federal background check variable in our model specification. The variable is statistically insignificant in the original model which is not surprising, considering that the law is in effect from 1994 until the end of the sampling period and

TABLE 1 Descriptive statistics

Variable	Mean	Min	Max	Within	Between
Number of deaths and injuries	0.65	0	58	8.643	0.760
Assault weapons ban	0.46	0	1	0.226	0.031
Federal background checks	0.86	0	1	0.127	0.00
State background checks	0.30	0	1	0.034	0.182
“Shall” CCW	0.62	0	1	0.106	0.135
“May” CCW	0.22	0	1	0.047	0.131
“Allow” CCW	0.04	0	1	0.017	0.022
Population	59.75	0.72	393.99	77.30	4,170.54
Total area	61.63	1.04	261.80	0.00	2,191.41
Per household median income	4.37	0.36	8.21	0.93	0.38
Proportion of population with college degree	25.42	2.20	43.40	17.55	16.91
Unemployment rate	5.59	2.21	14.90	2.56	0.93
Proportion of population aged 5–18	20.08	14.00	29.80	4.89	1.55
Per capita alcohol consumption	2.33	1.20	4.80	0.03	0.23
Total suicides	0.71	0.06	4.30	0.02	0.47
Firearm suicides	0.39	0.00	2.20	0.01	0.13

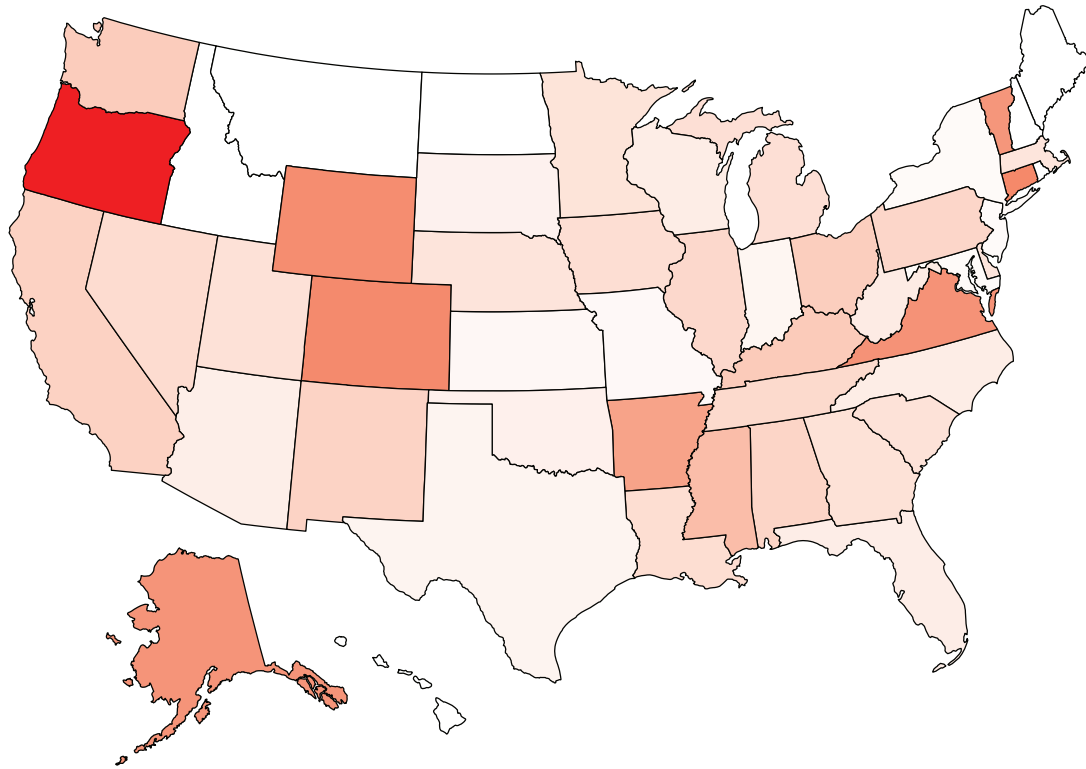
*Note:* The variables are pooled over all states. Population is given in 100,000 inhabitants, median income per household in 10,000 USD, total suicides and firearm suicides are measured per 1,000 inhabitants. Proportion of population with college degree, unemployment rate, and proportion of population aged 5–18 are given in percent. Per capita alcohol consumption is reported in gallons of ethanol. The column labeled “Within” (“Between”) contains the within-variation (between-variation) of the respective variables.

Abbreviation: CCW, concealed carry weapons.

therefore very little within-variation is available to estimate its coefficient in static models. Likewise, we find that the variable does not improve the explanatory power of our baseline Poisson model. Furthermore, estimating the LFPM and the SPLFM with the quasi-differencing GMM estimator eliminates the first 3 years from our sample so that we do not have enough variation to estimate the coefficient. Consequently, this variable has been eliminated from all models in this paper. Considering that gun control laws are often changed after debates over incidents of gun violence, we have to account for the potential endogeneity of those variables in our instrument matrix. We need to assume that our gun law variables are predetermined, that is, gun control laws are fixed for each year and cannot be changed retrospectively. Although changes in legislation are usually passed and announced a certain period of time before they are enacted, there is a concern that prolific shooting events may trigger a swift change in the legislation. To ensure that our gun law variables are independent of current and future shocks, we use one period lagged gun law variables.<sup>5</sup>

Alaska and Hawaii were dropped from our dataset because they have no neighboring states which is problematic for our spatial model specifications with global spatial autocorrelation coefficients. Particularly in our main specification using a queen contiguity spatial weighting matrix, they have no common borders or vertices with other states which means that Alaska and Hawaii can neither infect others nor be infected by others. We have excluded those states for all other models to make our results comparable across different specifications. This leaves us with a dataset over the dimensions  $N = 48$  and  $T = 28$ , a total of 1,344 observations. Unfortunately, it is not possible to extend the cross-sectional dimension of our dataset. If we considered a higher geographic resolution, say using county-level data, we would have a more substantial problem with entities reporting only zeros over the sampling period in our Poisson count models. Six states are automatically dropped from the Poisson FE model because no injuries or deaths occurred from 1990 to 2017 (Idaho, Maine, Montana, New Hampshire, North Dakota, Rhode Island).<sup>6</sup> California has the highest number of years with at least one school shooting (20), followed by Texas (11) and Ohio (10). A heatmap indicating the total number of victims (injured or killed) per million inhabitants over the sampling period is depicted in Figure 2. It is remarkable that Oregon has by far the highest victim counts per state population being a “shall” state and requiring background checks for private sales, but not regulating assault weapons.<sup>7</sup> Figure 3 depicts the spatio-temporal variation





**FIGURE 2** Heatmap indicating the total number of victims (injured or killed) per million inhabitants for all US states from 1990 to 2017

in gun control variables. In general, we observe that the number of states with restrictive gun laws has increased over the sampling period. Also, it appears that states with restrictive laws are grouped in regional clusters. For example, the states with assault weapon bans in 2017 (Figure 3b) are largely clustered on the East Coast (with the addition of California and Hawaii) and the states allowing for CCW in 1990 are predominately located in the Midwest.

## 4 | EMPIRICAL RESULTS

#### 4.1 | Explanatory variables

Our baseline model specification is a Poisson fixed effects model. As we have a fixed number of states in this setting, we cannot assume that our sample is a random draw of observations. Therefore, we account for unobserved heterogeneity with individual fixed effects thereby eliminating the between-variation of our variables. Further, we include state-specific variables like population density and median income which might be correlated with individual state fixed effects. Hence, we cannot assume independence between the unobserved fixed effect and the regressors (Wooldridge, 2002, p. 247). In contrast to Gius (2018), we do not use population weights for the Poisson FE model, but the results with and without population weights are quite similar for our baseline model specification.<sup>8</sup> As we encountered some problems with numerical instability in the dynamic and spatial models, we have decided to work without population weights throughout the paper. Otherwise the results would not be comparable across specifications. The results for all panel count models are reported in Table 2.

Our results differ from the results originally reported in Gius (2018) in some aspects. For example, the coefficient of state background checks is significantly negative instead of being insignificantly positive in the original Poisson FE model. The only change in state background checks laws over the period 2015 to 2017 has occurred for Montana and Nevada, both adopting a stricter regulation in 2016. Nevertheless, adding these periods seems to be responsible for those changes, because re-estimating our model (still excluding Alaska and Hawaii, as well as the Federal background check variable) for the period 1990–2014 yields the initial result. We also find a different sign of the CCW variable, unemployment rate, and per capita alcohol consumption. Coefficients of our gun law variables have the same sign over all

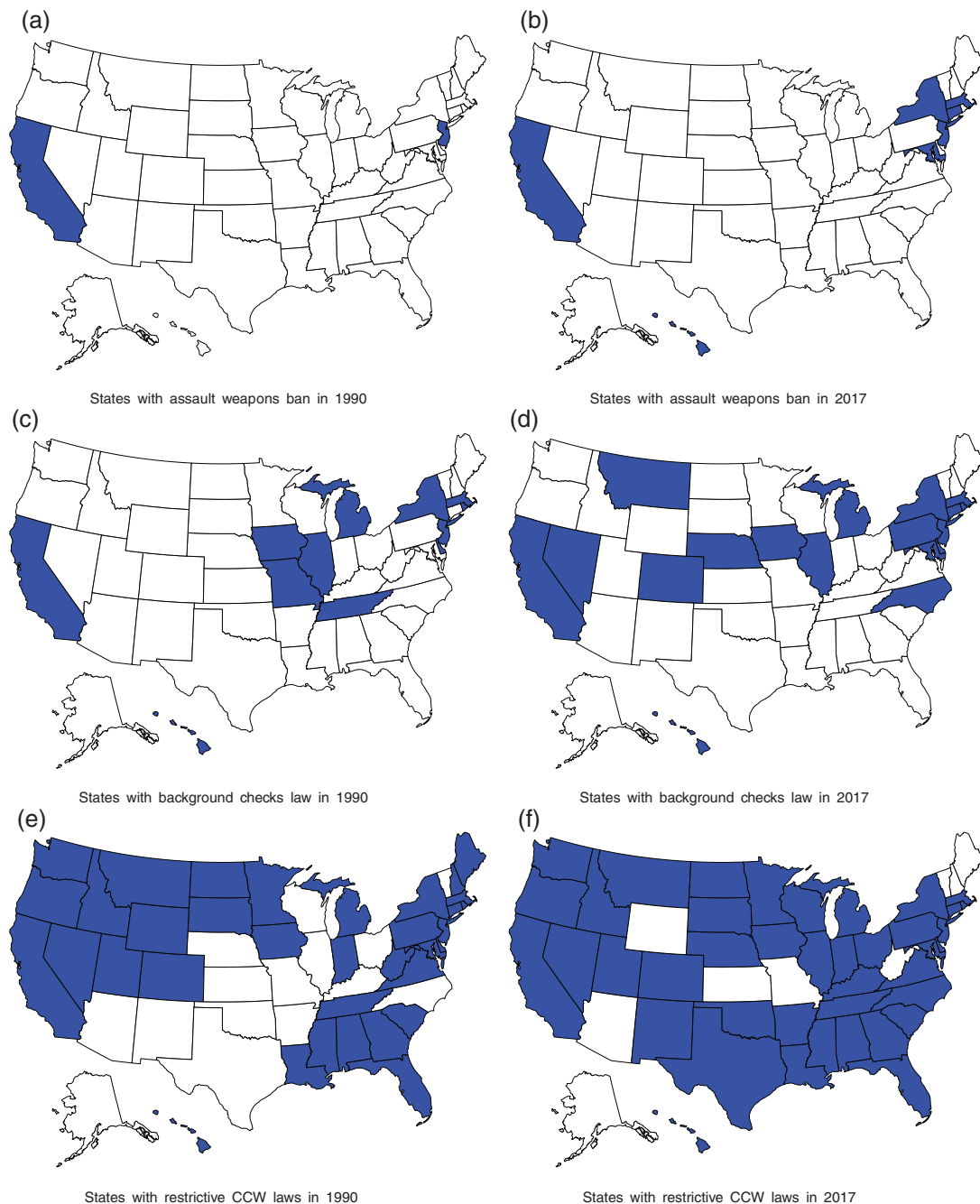


FIGURE 3 Spatio-temporal variation of state gun laws

specifications. The coefficient of the assault weapons ban variable becomes insignificant for model specifications involving temporal lags. However, if we estimate marginal effects for the SPLFM (see Table 3), we find a significant direct effect. Spillover effects are also significant in the short-run and long-run. The coefficient of state background checks (concealed carry laws) on victim counts is significantly negative (positive) over all specifications. The significant CCW coefficient suggests that states with strict concealed carry laws on average have higher victim counts. This is a surprising result considering that Gius (2018) reports a negative albeit insignificant effect for this variable and that it is generally believed that stricter concealed carry laws should prohibit perpetrators from bringing guns to school. However, our results are in line with the findings of Gius (2014) on state-level murder rates. CCW laws might be a response to a high level of gun-related acts of violence. Hence, states which tend to have a higher intensity of shootings are more likely to have (stricter) CCW laws. Although we use state-fixed effects and account for

TABLE 2 Results for spatio-temporal panel count models

	1 P-FEPM	2 P-SPM	3 LFPM	4 SPLFM
$Wy_t$				0.368*** (0.012)
$Wy_{t-1}$		0.255*** (0.092)		
$y_{t-1}$			0.426*** (0.053)	0.402*** (0.010)
Assault weapons ban	-1.008*** (0.224)	-0.978*** (0.303)	-1.189 (1.031)	-0.780 (1.015)
State background checks	-0.300*** (0.046)	-0.253*** (0.065)	-0.643* (0.400)	-0.306*** (0.048)
Concealed carry laws	0.830*** (0.079)	0.824*** (0.082)	1.963*** (0.238)	1.197*** (0.034)
Population density	1.461*** (0.017)	1.620*** (0.018)	2.166*** (0.287)	2.007*** (0.076)
Per household median income	0.697*** (0.095)	0.517*** (0.105)	2.620*** (0.088)	2.808*** (0.024)
Proportion of population with college degree	4.639*** (1.197)	8.352*** (1.344)	4.620 (4.472)	4.279*** (0.227)
Unemployment rate	-1.467 (3.211)	-3.977 (3.765)	-5.326 (13.432)	-5.037*** (1.560)
Proportion of population aged 5–18	25.426*** (1.679)	24.359*** (1.890)	25.051*** (4.176)	29.974*** (1.129)
Per capita alcohol consumption	-0.616*** (0.304)	-0.583*** (0.045)	1.294*** (0.439)	1.481*** (0.070)
Ratio of firearm suicides to total suicides	2.646*** (0.295)	2.525*** (0.400)	2.707 (2.234)	2.490*** (0.329)
Mass shooting	1.974*** (0.422)	1.960*** (0.619)	2.153*** (0.530)	1.604*** (0.051)
Mass shooting (spatial lag)		0.341 (0.557)	0.324 (1.880)	0.566*** (0.215)

Note: Results for the Poisson fixed effects panel model (P-FEPM), Poisson spatial Panel model (P-SPM), linear feedback panel model (LFPM), and spatial linear feedback panel model (SPLFM). Standard errors are given in parentheses. We use cluster-robust standard errors for Models 1 and 2, and two-step generalized method of moment standard errors for Models 3 and 4.  $W$  is a queen contiguity spatial weighting matrix. Coefficient estimates for time-fixed effects (included in all specifications) are not reported. \*, \*\*, and \*\*\* denote 10%, 5%, and 1% statistical significance, respectively.

the potential endogeneity of CCW laws in terms of school shootings, this variable might still be affected by the reverse causality that CCW laws become stricter over time in states with a higher tendency for gun violence. We report changes in CCW laws after 2014 for West Virginia and New Hampshire which both became constitutional carry states in 2016 and 2017, respectively. Also, Illinois became a “shall” state after previously banning the public carrying of firearms completely (see also Figure 3e,f for the evolution of gun control laws from 1990 to 2017). The coefficient of per capita alcohol consumption switches from negative to positive if we account for spatio-temporal terms. However, if we compute marginal effects, both direct and indirect effects become insignificant. The effect of a higher per household median income on victim counts is larger when we account for temporal dependence. The unemployment rate does not have a significant effect in our baseline specification which is a surprising difference

TABLE 3 Direct/indirect effects in the spatial panel linear feedback model (SPLFM)

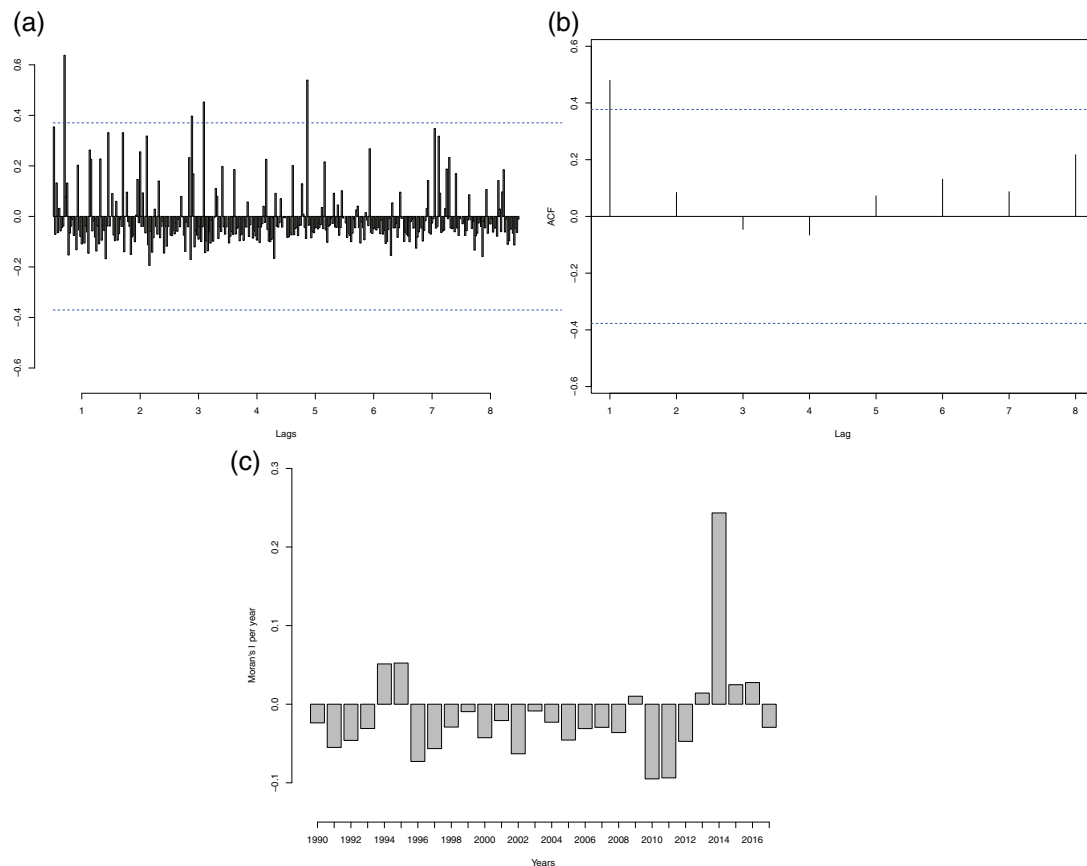
	Coefficients	Short-term effects		Long-term effects	
		Direct	Indirect	Direct	Indirect
$Wy_t$	0.368*** (0.011)				
$y_t - 1$	0.402*** (0.021)				
Assault weapons ban	-0.780*** (0.083)	-0.524*** (0.139)	-0.272*** (0.087)	-0.956** (0.260)	-1.215** (0.474)
State Background Checks	-0.306 (0.355)	-0.206 (0.545)	-0.107 (0.283)	-0.376 (1.003)	-0.477 (1.374)
Concealed Carry Laws	1.197*** (0.273)	0.804* (0.425)	0.417* (0.219)	1.467* (0.767)	1.865* (1.102)
Population density	2.007*** (0.586)	1.348 (0.903)	0.700 (0.471)	2.461 (1.617)	3.128 (2.255)
Per household median income	2.808*** (0.697)	1.886* (1.141)	0.979 (0.671)	3.442* (2.065)	4.375 (3.660)
Proportion of population with college degree	4.279*** (0.682)	2.874*** (1.079)	1.492** (0.607)	5.246*** (1.992)	6.668** (3.176)
Unemployment rate	-5.037*** (0.481)	-3.383*** (0.831)	-1.757*** (0.511)	-6.175*** (1.545)	-7.849** (3.284)
Proportion of population aged 5–18	29.974*** (0.244)	20.132*** (2.329)	10.453*** (2.042)	36.745*** (4.747)	46.705*** (14.740)
Per capita alcohol consumption	1.481 (1.670)	0.995 (2.556)	0.517 (1.312)	1.816 (4.622)	2.308 (6.115)
Ratio of firearm suicides to total suicides	2.490*** (0.701)	1.673 (1.081)	0.868 (0.567)	3.053 (1.960)	3.880 (2.852)
Mass shooting	1.604*** (0.397)	1.114* (0.626)	1.101* (0.631)	2.098* (1.162)	3.944* (2.310)
Mass shooting (spatial lag)	0.566** (0.229)				

Note: Results for SPLFM. Standard errors are given in parentheses. We use bootstrap standard errors for coefficients, direct and indirect effects.  $W$  is a queen contiguity spatial weighting matrix. Coefficient estimates for time-fixed effects (included in all specifications) are not reported. \*, \*\*, and \*\*\* denote 10%, 5%, and 1% statistical significance, respectively.

from the results reported in Gius (2018). Following Pah et al. (2017), we would expect to find a positive coefficient for this variable because a higher unemployment rate implies worse perspectives of school-to-work transition. This finding might be explained by the fact that 3 years were added to the dataset in which states with generally low unemployment rates had a relatively high victim count. The effect of the occurrence of a nonschool-related mass public shooting on the intensity of school shootings is positive and significant in our baseline specification. After the inclusion of spatio-temporal terms, we still find a significantly positive effect for the same state but also report additional spillover effects from neighboring states.

## 4.2 | Spatio-temporal effects

We now discuss our estimated spatio-temporal effects and their implications for potential copycat effects. The dimension of our dataset ( $N = 48$ ,  $T = 28$ ) allows us to estimate the temporal dependence with higher precision than the



**FIGURE 4** These figures depicts some aspects of the spatio-temporal dependence structure. Panel (a) depicts the state-specific autocorrelation function and panel (b) depicts the autocorrelation function averaged over all states. Panel (c) depicts the Moran's I statistic for each year

spatial dependence. The availability of 28 time periods is considered moderate to large in panel settings whereas the fixed number of 48 states, unfortunately, is quite small (Glaser et al., 2020). Figure 4a and b depict the individual autocorrelation functions of each state and the average autocorrelation function of the victim counts variable, respectively. We find that the first lag of the average autocorrelation function is significantly positive which gives a first hint that including a temporal effect in our panel count models could improve the explanatory power of the model.

The autoregressive coefficient of the linear feedback model has an interesting and straightforward interpretation similar to autoregressive models for continuous data (Blundell et al., 2002). A positive coefficient ( $\gamma = 0.426$  for LFPM and  $\gamma = 0.402$  for SPLFM) suggests that the intensity of school shootings increases for the next year and that the depreciation rate of this increase over the following years is  $1 - \gamma$ . Assuming exponential decay of a first-order autoregressive model, it holds that 90% of the temporal effect of a school shooting incident has decayed after approximately 4 years. It follows that the increased alertness of authorities directly after these incidents seems to be justified to deter copycat shooters. In the medium and long-term, we suspect that observing school shootings in the same state does not matter as much for the perpetrators' motivation as the notoriety of a specific attack and the subsequent media coverage (see, e.g., the well-studied “Columbine effect”). However, our current dataset does not allow us to investigate this further.

As school shootings are rare events, our dataset has clusters of zero victim counts. To investigate whether the repeated occurrence of years with zero school shootings drives our results, particularly with respect to the relatively high temporal dependence, we tried to generate data that mimic our present dataset albeit under the hypothesis that no temporal dependence of school shootings exists. To do so, we ran simulation experiments for which we generated a dependent variable ( $N = 48$ ,  $T = 28$ ) containing only zero entries. Then, we randomly selected 4 years, the average number of years with at least one school shooting per state, and drew from a Poisson distribution with intensity parameter of four, the average number of victims per school shooting. Finally, we evaluated the performance of the quasi-differenced GMM estimator. It appears that the autocorrelation parameter ( $\gamma = 0$  in this setting) is accurately estimated



which shows that our results for the empirical dataset are not driven by excess zeros. Nevertheless, we need to account for this feature of the data by using our second step GMM estimates to obtain valid standard errors.

Towers et al. (2015), Kissner (2016), and Lankford and Tomek (2018) also study the temporal dependence of crime but do so at a higher frequency. Towers et al. (2015) use a self-excitation contagion model and find that school shootings are contagious for an average of 13 days. However, their model does not use the panel structure of their state-level data to eliminate unobserved heterogeneity. Kissner (2016) analyzes active shootings using a series hazard model and reports an increased hazard for 2 weeks after the initial shooting. In contrast, Lankford and Tomek (2018) do not find evidence for short-term contagion. We test for contemporaneous spatial dependence in Figure 4c where the Moran's  $I$  statistic (Moran, 1950) is reported for each year. Moran's  $I$  for row-standardized weight matrices is given by

$$I_t = \frac{\sum_{i=1}^N \sum_{j=1}^N w_{ij} (y_{it} - \bar{y}_t) (y_{jt} - \bar{y}_t)}{\sum_{i=1}^N (y_{it} - \bar{y}_t)^2}, \quad t = 1, \dots, T. \quad (13)$$

The measure was developed for a cross-section of data so that we have to compute it repeatedly for each year, indicated by the subscript  $t$ . Unfortunately, there is no distributional theory available for Moran's  $I$  calculated from count data, but we can employ a bootstrap procedure to obtain inferential statements (Jin & Lee, 2015; Lin et al., 2011; Ren et al., 2014). To do so, we draw elements of our variable of interest, place it randomly on our map, and recompute Moran's  $I$ . Using 400 bootstrap draws, we can compare the original Moran's  $I$  statistic to the bootstrap distribution and determine the  $p$ -value. The majority of yearly victim counts do not appear to have a statistically significant spatial dependence. However, it has to be noted that the lack of significance might be attributed to the large number of zeros in school shooting data. Further, the spatial dependence is only measured for a specific year and does not control for the influence of other variables.

In our spatial panel models, we measure spatial autoregressive effects while simultaneously controlling for confounding factors and unobserved heterogeneity. Additionally, we observe that the lagged spatial term seems to have more explanatory power than the contemporaneous spatial term. We report our results for the P-SPM only with a lagged spatial term. Excluding the contemporaneous spatial term improves our results (reaching a different local optimum with a higher log likelihood value). The log likelihood value of the P-SPM is larger than the log likelihood value of the Poisson FE model ( $-2,648.49 > -2,690.14$ ) and further testing shows that controlling for the spatial lag variable significantly improves the explanatory power of our model. The significant spatial terms in the P-SPM and the SPLFM tell us that copycat shooters are influenced by school shootings in neighboring states.<sup>9</sup> Consequently, authorities of neighboring states should consider the possibility of copycat shooters if incidents happened in the current or past year. Although the value of the spatial coefficients is smaller than the autoregressive coefficient, we should not conclude that regional spillovers are less likely to occur than temporal clusters of school shootings in the same state because spatio-temporal autocorrelation lacks a common metric.<sup>10</sup> Towers et al. (2015) find little evidence for spatial clustering in their mass shootings and school shootings data.

To study the sensitivity of our results to certain influences omitted from our main specification, we conduct several robustness checks. First, we use additional variables on state-level violent crime, for example, the murder rate per 100,000 inhabitants. These variables do not seem to have a significant impact, further emphasizing that school shootings are distinct from other violent crimes. Second, we include the divorce rate per 1,000 inhabitants and variables on the percentage of minorities in the state. The percentage of Blacks and Hispanics has a negative (although insignificant) impact on the school shooting intensity. In contrast, the divorce rate has a positive and significant effect. This result provides further empirical support for the characterization in Gerard et al. (2016) that school shooters are predominantly Caucasian and often come from broken homes.<sup>11</sup> However, the inclusion of those variables does not affect the coefficient estimates of other variables in our main specification.

## 5 | CONCLUSION

As noted in the introduction, school shootings are tragic events with the ability to traumatize communities. One of the more common motivations for school shootings is the perpetrators' desire for media attention. According to Lankford and Tomek (2018), the Columbine shooting inspired at least 21 copycat shootings and 53 thwarted plots in the United States over a 15-year period. Hence, school shootings may be partly explained by copycat behavior.

In this paper, we attempted to detect and estimate copycat effects using spatio-temporal panel count models. This class of models allowed us to determine whether an initial school shooting spawned subsequent attacks in neighboring states and the same state in the following years. Estimating the temporal dependence helped us to answer whether the frequency of school shootings in one state increases if the same state reports a higher number of victims in the past year.

In our study, we followed Gius (2018) and estimated state-level panel count models for US school shooting data. Our baseline model specification is the fixed effect Poisson panel model which accounts for the integer and non-negative nature of victim counts. Our second model included spatial terms in the form of contemporaneous and lagged neighboring victim counts. Third, we applied a linear feedback model adding a first-order lag of the dependent variable to the basic model specification. In contrast to the previous two models which focused on either spatial or dynamic effects, our final model analyzed relationships along both dimensions simultaneously.

Using state-level data for the period 1990–2017, our results indicate that there are significant spatio-temporal effects in state-level school shooting data. Consequently, there is a higher risk that a school shooting happens in neighboring states (spatial effect) and in the same state in the next year (temporal effect) after an initial attack. This information may help authorities plan a response after school shooting incidents. In addition, we found that the victim counts cluster temporally after an initial shooting which suggests that the media should alter their coverage of school shootings and mass killings in general (Lankford & Tomek, 2018). Ideally, the media should find a way to cover these events while minimizing the risk of provoking additional shootings.

Another question that arises in the context of a changing media landscape is whether the advent of social media accelerates the copycat effect. Although we observe a higher number of school shootings after 2005, it is difficult to capture this effect in our model. It would be helpful to estimate our model for several sample splits, but this is infeasible considering the small sample sizes. Following the hypothesis that social media helps to disseminate information about school shootings and thereby helps to inspire copycat attacks, the spatial effect which is more related to regional news coverage should have a decreasing impact over time.

Possible extensions of our panel count models involve the use of different probability models which account for overdispersion and zero-inflated data. Spatio-temporal panel count models based on a negative binomial or generalized Poisson distribution might be better suited for our setting where many zero counts and a few very large counts are reported. To the best of our knowledge, spatio-temporal panel models with multiplicative fixed effects using these distributional assumptions are currently not available.

Our study, however, suffers from several shortcomings. First is the use of state-level data to determine the impact of copycat behavior in school shootings. States are rather large, heterogeneous areas that have diverse community settings, spanning the range from very urban to very rural, and these areas have varying gun ownership rates. Hence, firearms may be more easily obtainable in some areas of a state than in others. Therefore, it may be easier to perpetrate a school shooting in certain areas of a state than in others. Regardless of these shortcomings, the available data indicate that the use of county-level data is not viable primarily because school shootings are so rare, and most counties in the United States may go for decades with no school shootings. Hence, even though states are very heterogeneous in nature, it may not be possible to use smaller geographic divisions given the infrequency of school shootings. Another shortcoming of using state-level data is the aggregation of victim counts over a year which makes it impossible to distinguish between state-years characterized by several events with low victim counts and single events with a high victim count typically evoking more intense media coverage. Consequently, future research could be focused on modeling copycat behavior in event-specific data.

Although not of primary concern of the present study, the impact of gun control laws on school shootings is an important area of research that should be further explored in future research. Results of the present study indicate that states with restrictive CCW laws have more school shooting victims while the use of state-level background checks resulted in fewer school shooting victims. The assault weapons ban produced mixed results. These results should be viewed with caution because many school shooting perpetrators are students themselves, and, in many cases, are not legally allowed to own or possess certain types of firearms. Hence, the gun control measures examined in the present study may not apply to them.

School shootings and the fear of copycat shooters have drastically negative effects on society. Traumatized communities often report secondary victimization in form of psychological health issues. The academic performance of students who remain enrolled in the attacked school declines and additional security at those schools is costly. Given these negative outcomes related to school shootings, it is imperative that we find ways in which to reduce the number of school shooting incidents. The results of the present study may serve as a useful guide in attempting to negate the effects of the extensive media coverage of school shootings and hence reduce the likelihood of copycat school shooters.

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## ENDNOTES

- <sup>1</sup> According to the FBI's Uniform Crime Reporting Program, violent crime is composed of four offenses: murder and non-negligent manslaughter, forcible rape, robbery, and aggravated assault.
- <sup>2</sup> The P-SPM can be classified as a simultaneous autoregressive model. A recent overview of this class of models can be found in Czado et al. (2014).
- <sup>3</sup> The largest characteristic root of row-standardized spatial weight matrices is unity by definition.
- <sup>4</sup> They define 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." We delete the events classified as school shootings from this list.
- <sup>5</sup> We thank an anonymous reviewer for this suggestion.
- <sup>6</sup> Alaska reports three injured and three killed during school shootings, while Hawaii has no victims over the sampling period.
- <sup>7</sup> Not surprisingly, California being the most populous state has the highest total number of victims over the sampling period. A heatmap indicating the total number of victims is depicted in Appendix S1.
- <sup>8</sup> The results for a Poisson fixed effects model with population weights are reported in the Appendix S1. The largest differences are found for the coefficient of the population density variable which is much larger in the model without population weights.
- <sup>9</sup> We report our results only for the queen contiguity spatial weight matrix, but the results for other specifications of the spatial weight matrix can be obtained from the authors upon request. We find that our estimates of the lagged spatial effect are sensitive to the choice of the spatial weight matrix. However, estimates of all other coefficients are robust to this choice.
- <sup>10</sup> Anselin et al. (2008) discuss the lack of a common metric as a central difficulty in space–time modeling. While spatial autocorrelation is measured in the geographical units, in our case “neighbors” in space by means of a spatial weight matrix, the temporal autocorrelation is measured for “neighbors” in time by means of the customary time lags. Hence, it is not straightforward to compare the speed of both dimensions of the dynamic space–time process.
- <sup>11</sup> The results of our robustness checks are not reported in the paper, but can be obtained from the authors upon request.

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## SUPPORTING INFORMATION

Additional supporting information may be found online in the Supporting Information section at the end of this article.

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