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ABSTRACT

In the Wrong Place at the Wrong Time: The Impact of Mass Shooting Exposure on Mental Health*

We study the effect of mass shooting exposure on individuals' mental health by using the Panel Study of Income Dynamics. Our identification strategy relies on the quasi-randomness of mass shootings in a staggered difference-in-differences design. We compare changes in mental health outcomes of individuals living in affected cities with changes of matched individuals living in non-proximal and not affected cities. We find that mass shootings have a substantial adverse impact on mental health, which persists for up to six years. This impact is not statistically significant for Black individuals, whereas it is slightly more pronounced among women and older cohorts.

JEL Classification: C23, I18, K14

Keywords: mass shooting, mental health, difference-in-differences, dynamic effect

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1 Introduction

Firearm violence is a serious public issue in the United States. According to the Center for Disease Control and Prevention (CDC), in the period from 2001 to 2020, there were more than one million firearm-related injuries, with almost two thirds of them causing a fatality.[1] There are many types of firearm violence, such as familicides, gang-related killings, suicides and unintentional shootings. In recent years, a particular type of firearm violence has increasingly attracted attention in public debate: public mass shootings. Albeit small in fraction,[2] public mass shootings have gained the spotlight due to their ferocity and unpredictability. Differently from felony shootings, hate crimes or familicides, public mass shootings are typically characterized by an (almost) completely absence of interpersonal connection between perpetrators and victims (Bagalman et al., 2013), making mass shootings hard to predict, ferocious, and complicate to manage (Schildkraut and Elsass, 2016; Wilson, 2017). As a result, fear of public mass shootings has become substantial in the United States, where four out of ten people worry about becoming a victim of a mass shooting (Gallup, 2017).

Exposure to traumatic events may generate a series of adverse emotional, cognitive, behavioral and health outcomes, ranging from mild temporary stress to permanent psychopathology (Norris et al., 2002; Bharadwaj et al., 2021), especially in the case of mass violence (Norris et al., 2002). In this paper, we address the following research questions using individual level data and mass shootings which occurred in the USA between 1999 and 2019: (i) Does mass shooting exposure affect individual mental health? (ii) If yes, is the effect persistent? (iii) Is the effect heterogeneous across different groups of people? The definition of exposure is not free of controversy (Wilson, 2017). The post-trauma outcomes following mass shootings do not involve only people who directly witnessed the incident in person or who had a relative or close friend exposed, but could also extend to a wider range of individuals. For example, previous research suggests that broadcast media coverage or the amount of television viewed following an incident of mass violence has an adverse effect on mental health (Pfefferbaum et al., 2000; Fallahi and Lesik, 2009). In our study, we classify individuals as exposed to a mass shooting if they were residing in a city at the time the incident occurred.

[1] These figures are from the Web-based Injury Statistics Query and Reporting System of CDC. For more details, visit https://www.cdc.gov/injury/wisqars/index.html (last accessed February 27, 2024).

[2] According to Luca et al. (2020), mass shootings account for less than 1% of all firearm-related deaths in the USA.

While a large body of literature has studied the determinants of criminal behavior and subsequent policies of optimal deterrence (Becker, 1968; Dills et al., 2008; Nagin, 2013), less is known about the potential costs suffered by victims (Bindler et al., 2020). Because crime entails sizable societal costs (Chalfin, 2015), understanding victims' suffering is of utmost importance from a policy perspective. However, there is a general paucity of empirical research that estimates the causal effect of criminal victimization (Bindler et al., 2020). This is not an easy objective because of the endogeneity of the probability of suffering from criminal victimization. The causality may go from having bad health or precarious labor market conditions to the likelihood of getting involved in criminal activities and, thus, to the probability of becoming a victim of violence (reverse causality). Furthermore, unobserved individual characteristics may jointly determine both the likelihood of victimization and the outcome variables (omitted variables). For instance, unhealthy habits, such as drinking, may put individuals in situations where the risk of victimization is higher than normal. At the same time, they may affect health and labor market performance.

The main contribution of our study is to fill this gap by estimating the impact of mass shootings on the health of those who were exposed to this kind of firearm violence. Because mass shootings are highly unpredictable events (Luca et al., 2020; García et al., 2022; Muñoz-Morales and Singh, 2023; Soni and Tekin, 2023), this approach helps mitigate endogeneity concerns. Moreover, our study contributes to the literature on the consequences of exposure to large-scale violent events on individual economic outcomes. Past empirical research has focused on incidents such as armed conflicts and terrorist attacks, providing evidence of negative effects on education, health, and labor market outcomes (see e.g. Frey et al., 2007; Metcalfe et al., 2011; Shemyakina and Plagnol, 2013; Islam et al., 2016; Bryson and MacKerron, 2018; Clark et al., 2020). Instead, much less is known about the effects of mass shootings, although they represent a form of large-scale violence that has become more and more prominent in recent years (Wilson, 2017; Smart and Schell, 2021).

In addition, our paper is one of the few on the effects of mass shooting exposure that exploits individual-level data. Using individual-level data as the unit of analysis, instead of aggregated data at the county or state level, has significant advantages. In our framework, it allows us to focus on the effect of mass shooting exposure on variations in outcome variables at the individual level, netting out from the estimated effects spurious correlations due to individual fixed effects additively entering the conditional mean of the outcome of interest. Furthermore, the use of individual-level data allows for more accurate estimates, which is especially important when considering heterogeneous effects, both across different groups of individuals and over time. To the best of our knowledge, there are only three other articles on this subject that exploit individual-level data: Dursun (2019), Bharadwaj et al. (2021) and Sharkey and Shen (2021). Our study presents relevant differences and innovations with respect to them. Compared to Bharadwaj et al. (2021), who focused on the 2011 Utøya massacre in Norway, we use longitudinal data in a staggered design for mass shootings in the USA and assess the effects of those incidents in a context where they became relatively endemic. Unlike Sharkey and Shen (2021), who examined the effect of mass shootings on daily emotions, we investigate the health effects in more detail by focusing on measures of general health and, from a methodological point of view, making our estimates robust to treatment heterogeneity. Finally, with respect to Dursun (2019), who only dealt with intergenerational effects on pregnant women, our findings have a greater external validity because we investigated the effects on a more general population.

To perform our analysis, we used the Panel Study of Income Dynamics (PSID) over the period 1999–2019 (McGonagle et al., 2012). The PSID is a rich longitudinal database, representative of the US population (Fitzgerald et al., 1998; McGonagle et al., 2012). Started in 1968, it is the longest running household survey in the world, covering roughly seven generations of families and individuals.[3] The PSID collects a vast range of demographic and socioeconomic information at individual and household level. To study the impact of mass shooting exposure on mental health, we used the Kessler psychological distress scale (K6), which is a measure of mental health for non-specific psychological distress (Kessler et al., 2002, 2003), and self-assessed health (SAH), which is a standard measure of the general health status (Au and Johnston, 2014; Doiron et al., 2015; Lundberg and Manderbacka, 1996).

We gathered data on mass shootings from the Violence Project Mass Shooter Database (VPMSD), which is one of the most complete datasets on public mass shootings currently available (Peterson and Densley, 2019). It has collected detailed information on incidents, victims and perpetrators of high-profile public mass shootings which have taken place in the USA since 1966.

Our strategy of identifying the causal effect relied on the quasi-random nature of mass

[3]For more details, visit https://psidonline.isr.umich.edu/GettingStarted.aspx (last accessed February 27, 2024).

shooting incidents (Luca et al., 2020; García et al., 2022; Muñoz-Morales and Singh, 2023; Soni and Tekin, 2023). We defined the exposure to mass shootings at the city level after grouping together ZIP codes that pertained to the same municipality. Because mass shootings occurred at different times, we used a difference-in-differences (DiD) approach with a staggered design (de Chaisemartin and d'Haultfoeuille, 2020), comparing changes in outcome variables of people living in affected cities with changes in the outcome variables of individuals living in other non-proximal cities in the USA. Because differences in local characteristics may create non-parallel outcome dynamics between treated and controls (Abadie, 2005), we pre-processed the data using matching methods to increase the balance properties of the sample.

We found that mass shooting exposure exerts a sizable negative impact on mental health. On average, this penalty survives significantly for up to 6 years after the incident. The heterogeneity analysis reveals that it is not statistically significant for Black individuals, while women and older age cohorts suffer a somewhat more pronounced decline in mental health.

This article is organized as follows. Section 2 presents the existing economic literature on mass shootings. Section 3 focuses on data, sample and identification strategy. Section 4 reports the estimation results and comments on the main findings. Section 5 concludes.

2 Mass shooting literature

The impact on victims is an aspect of the economics of crime literature that has not received as much attention as the analysis of costs and consequences of offenders interacting with the justice system (Miceli, 2021).[4] Understanding the proportion of potential harm suffered by victims is of utmost importance in order to draw a complete picture of the consequences of criminal activities (Bindler et al., 2020).[5] In this section, we focus on the empirical literature which has sought to shed light on the effect of shootings on their victims.

As a consequence of a crime, there may be direct or indirect victims. The former category comprises people who suffered the harm themselves. The latter comprises all the other cases in which individuals are not directly involved but have ties or connections

[4]See Dills et al. (2008) and Nagin (2013) for exhaustive reviews of the literature.

[5]Theories on the cost of criminal victimization have mainly been drawn from the sociological literature. See Meier and Miethe (1993) for a review of theories of criminal victimization.

with the incident (Bindler et al., 2020). The literature on shooting events has tended to focus predominantly on the latter, placing especial emphasis on the repercussions at the community level (Yousaf, 2020). Brodeur and Yousaf (2022) showed that counties that suffer mass shootings experience a substantial worsening of local labor market conditions. According to the authors, employment and earnings per capita decrease on average by a margin of 1.3% and 2.4% after an incident. Gunadi (2021) showed that property crimes increase by 4% in affected areas. Muñoz-Morales and Singh (2023) reported that shooting incidents may even erode the value of household wealth. Using data on school shootings, the authors found that the price of housing in affected districts decreased by 2.5% compared to the price of houses located in neighboring districts. In addition, García et al. (2022) and García and Li (2023) showed that mass shootings generate negative spillovers in the local credit market in the areas where they occurred.

Mass shooting incidents may also have negative consequences on health by generating panic and fear. Rossin-Slater et al. (2020) found that the consumption of antidepressants substantially increases after a shooting event. Soni and Tekin (2023) showed that people living in areas that experienced a mass shooting are less likely to report an excellent community and emotional well-being and more likely to engage in unhealthy behaviors after an incident. Similarly, Banerjee et al. (2020) found that mass shootings have detrimental effects on the health of children: in utero exposure is associated with a reduction in the average gestational length and in the likelihood for newborns of meeting the normal weight threshold at birth.

Levine and McKnight (2021) provided suggestive evidence that shooting incidents in schools affect students' performances and behaviors. Using data from Connecticut, they found that students' performance on standardized tests deteriorated after an incident. The chronic absenteeism rate also worsened, nearly doubling after a shooting. In addition, the authors found an increase in mortality rates as a consequence of an increase in suicides. Beland and Kim (2016) reported similar negative findings on students' performances and school enrollment. Abouk and Adams (2013) showed that students tend to move from public to private education. Finally, shooting incidents have also been found to exacerbate political tensions in affected areas by fostering political polarization among voters and politicians (Yousaf, 2021; Garcia-Montoya et al., 2022).

Evidence of negative effects on educational, health and labor market outcomes has also been reported by studies that employed microdata. Using data from Texas, Cabral et al. (2021) showed that exposure to gun violence at schooling-age impairs the human capital accumulation process. The authors found that exposed students were less likely to attend school, more likely to experience grade retention and less likely to graduate from high-school. Furthermore, the authors showed that exposed students were less likely to be employed by the age of 26 and, if they were employed, to earn less than non-exposed peers. Deb and Gangaram (2024) confirmed these findings for the USA and showed that witnessing a shooting incident in school age can also affect health. In particular, the authors provided evidence that exposed individuals are more likely to report a worse overall state of health, more likely to consume alcohol and tobacco, and less likely to do physical activity. In Finland, Poutvaara and Ropponen (2018) found that the educational penalty was worse for men than for women. Sharkey and Shen (2021) found that shooting incidents also affect emotional well-being. Bharadwaj et al. (2021) detected similar large penalties on survivors of the 2011 Utøya massacre in Norway, with negative effects that spread to family members of exposed individuals. Dursun (2019) found an intergenerational health effect similar to the one shown by Banerjee et al. (2020) at aggregate level. Finally, Sezer (2022) demonstrated that the intergenerational penalty for exposure to shootings does not revert over time, but persists even in the adult age of the offspring of affected individuals.

3 Method

3.1 Data

We created our sample by combining different data sources. Firstly, we gathered information on the mass shooting incidents from the VPMSD. It is an open-access public repository of high-profile public mass shootings that have occurred since 1966 in the USA. The VPMSD adopts the Congressional Research Service (CRS) definition of mass shooting to identify incidents. The CRS has defined mass shootings as multiple homicide incidents in which at least four people are murdered by firearms, within a single event, in a public and populated location. The perpetrators are not included in the count of the victims and none of the murders shall be attributable to criminal activities or personal disputes (Krouse and Richardson, 2015). Finally, incidents should not last more than 24 hours (Duwe, 2014). The VPMSD provides detailed information on each incident, such as the number of victims, location, weapon used, etc. Further, it also collects information about the perpetrators, such as personal history of mental illness, traumas and family history.[6] The VPMSD is one of the most complete repositories on public mass shootings currently available (Peterson and Densley, 2019).

Secondly, we gathered outcome variables and control variables at individual level from the Panel Study of Income Dynamics (PSID), a household panel survey representative of the US population (Fitzgerald et al., 1998; McGonagle et al., 2012). Started in 1968, the PSID collects information on more than 80,000 individuals per each wave (Beaule et al., 2019).[7] The design of the PSID is such that once a family has been selected, the head of the household provides information about himself/herself and all the members of the household. Household members are followed for life and may remain in the sample even if they separate from the original household. When a PSID member gets married, starts a cohabitation, or becomes a parent, the partner and the offspring also join the panel. This longitudinal structure allows us to track individuals over time, regardless of complex family dynamics. Sample members exit the panel only if they die. In all other cases, the PSID adopts a series of actions to avoid losing individuals.[8] If individuals do not respond for two consecutive waves, the PSID tries to recontact them in all subsequent waves until an answer is received, minimizing attrition bias as much as possible.[9] The PSID collects information on a vast array of topics, such as demographics, education, employment, health, etc. Its restricted version also includes the geospatial information of the individuals and covers units up to census blocks.

Lastly, we collected information about local characteristics at the county level from several US agencies. From the US Census Bureau, we gathered information on real GDP, personal income, population size and demographics, poverty and median income. We also collected information on unemployment rates, average wages and the number of private establishments from the Bureau of Labor Statistics (BLS). We obtained information on the number of violent and property crimes from the FBI Uniform Crime Reporting (UCR)

[6]The VPMSD uses both primary and secondary sources to gather information. Primary sources may be perpetrators' diaries or manifestos, audio and video recordings, personal correspondence with perpetrators, etc. Secondary sources may be court transcripts, police reports, media news, etc. Each source is independently verified by two researchers, according to a double-blind protocol. Finally, a third party supervises the output before acceptance. For more details, visit https://www.theviolenceproject.org/methodology (last accessed February 29, 2024).

[7]The survey was carried out annually between 1968 and 1997 and every odd year since then.

[8]For instance, if a sample member moves to an institution, such as a prison, or the military, the PSID records this information as 'institutional status'.

[9]Panel attrition is not a major problem in the PSID (Fitzgerald et al., 1998). The average response rate for the period 1968-2019 was 95.5% (Beaule et al., 2019).

program. Finally, we collected information about voting behavior from the GitHub repository of FiveThirtyEight,[10] an American media outlet of politics, economics and sport. We used all these variables to investigate whether mass shootings were more likely to occur in areas with particular characteristics, as diagnostic tests about the robustness of our identification strategy. We also used them to control for potential local time-varying heterogeneity which might confound the treatment effect, because their change might jointly affect both the probability of a mass shooting incident occurring and human capital outcomes like mental health.

3.2 Sample selection

Our objective was to estimate the effect of exposure to a mass shooting incident on the mental health of individuals. To achieve this goal, we exploited the geospatial information present in the PSID and the VPMSD to match individuals with mass shooting locations on the basis of ZIP codes. Subsequently, we defined the treatment at the city level by grouping together ZIP codes pertaining to the same municipality.[11]

We started with a sample of 84,121 individuals for a total of 2,028,361 observations. We focused on incidents that occurred in the contiguous US during the period 1999-2019 and retained individuals accordingly. By restricting the sample to incidents that occurred in the last 20 years, we zoomed in on a period when mass shootings became more salient in both frequency and severity. Figure A.1 shows the trends of the number of incidents and related deaths since 1970. They exhibit an upward trend both before and after 1999. However, after 1999, trends became steeper, with incidents and deaths reaching their peaks in 2017 and 2018.[12]

We kept those individuals that identified themselves as household heads or spouses at the moment of the interview. By restricting on this sub-sample, we reduced the risk of reporting bias due to individuals misreporting information about someone else. We narrowed down the analysis to respondents aged between 25 and 65 years, thereby excluding individuals that may have been in education or may have been close to reaching the age

[10] See https://github.com/fivethirtyeight (last accessed February 29, 2024),

[11] In our sample, each ZIP code was assigned to a post office operating in a specific area. The post offices reported the names of the locations they were serving. We identified cities by grouping all post offices that shared the same location name.

[12] For the period 1970–1999, the values of the slopes of the fitting lines are 0.205 for the number of incidents and 0.838 for the number of deaths. For the period 1999–2019, these values are 0.254 and 2.579 respectively. The t-test for the equality of the slopes yielded p-values smaller than 0.001 in both cases.

threshold for the statutory pension.

To identify the treated and control groups, we restricted the sample to individuals living in affected cities (i.e. places that experienced at least one mass shooting during the observational period) and in places located more than 50 miles away from the epicenter of an incident. We excluded people living in nearby cities located within 50 miles because they may have been partially treated because of potential spillover effects of the mass shooting incidents.[13]

Because the PSID collects information every two years in uneven years, we required individuals not to have moved elsewhere during our observation time window. By focusing only on this group, we overcame the potential bad imputation of treatment when an incident occurred during an even year.

Finally, we also dropped individuals observed only once because they would not have contributed to the estimation of the causal effect, given that it is based on the change of health status over time.

We ended up with 8,063 individuals for a total of 45,784 observations. Table 1 summarizes the sample size reduction implied by our selection criteria.

Table 1: Sample size reduction across selection criteria

	Individuals	Individual-year observations	Dropped observations
Initial gross merged sample	84,121	2,028,361	-
After keeping individuals interviewed in 1999-2019 living in contiguous US	43,798	373,479	1,654,882
After identifying individuals with 'household head' or 'spouse' status	24,005	146,078	227,401
After restricting to individuals aged 25 to 65	21,420	120,121	25,957
After removing individuals living in partially treated areas	17,268	91,052	29,069
After restricting to individuals that did not move in 1999-2019	10,738	49,717	41,335
After removing single-observation individuals	8,490	47,436	2,281
After removing missing values in the outcomes	8,063	45,784	1,652
Final sample	8,063	45,784	

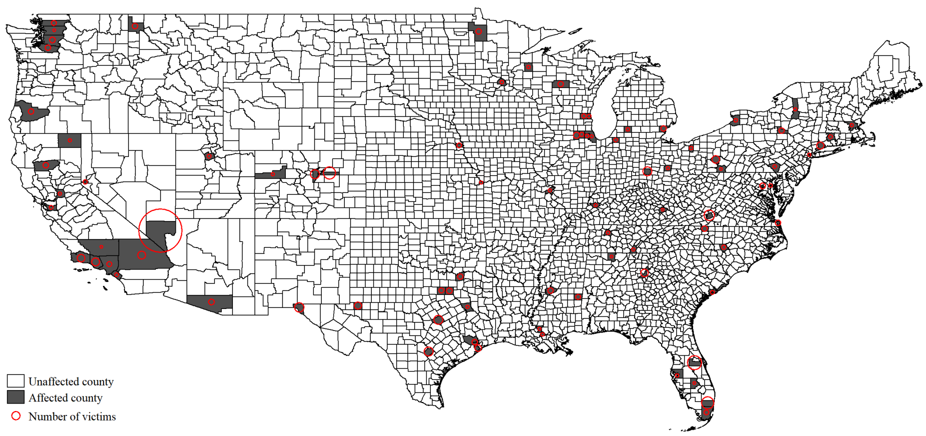
[13] Bryson and MacKerron (2018) showed that the effects of exposure to extreme events, such as terrorist attacks, may not be confined only to the places where they occurred, but may also extend to surrounding areas. However, they also showed that these effects tend to vanish for greater distances. Using distance information from the epicenter, we reduced the measurement error that arises when comparing individuals that may have been treated at different intensities and, at the same time, could have qualified the control group as not living in an actual affected place.

3.3 Identification assumption

One of the main challenges to identify the causal effect of criminal victimization is the simultaneity between criminal victimization and personal characteristics: individual health-related characteristics may positively impact the likelihood of victimization and vice versa (Bindler et al., 2020). For instance, unhealthy habits such as drinking may put individuals in situations where the risk of victimization is higher than normal. Similarly, individuals may develop unhealthy habits following a traumatic criminal episode. Mass shootings are unlikely to be affected by this endogeneity concern. Typically, these types of incident are widely regarded as random events, because neither the timing nor the location of mass shootings can be predicted (Luca et al., 2020; García et al., 2022; Muñoz-Morales and Singh, 2023; Soni and Tekin, 2023).

If it is reasonable to assume that the occurrence of mass shootings is not directly correlated to the unobserved characteristics of single individuals, it may still be possible that some local characteristics, such as poverty, income inequality or population density, are related to the incidents. If this is the case, then the people living in these particular areas may not be a random sample of the total population (Yousaf, 2021; Brodeur and Yousaf, 2022).

Figure 1: Map of US counties with at least one mass shootings in the period 1999–2019



Notes: The polygons in gray indicate the counties where at least one mass shooting occurred. The red (proportional) circles refer to the total number of deaths and injuries that occurred during various mass shooting incidents. The minimum number of victims was 4, while the maximum was 950.

Figure 1 shows the geographic distribution at county level of the mass shooting incidents that happened in the period 1999–2019. Mass shootings are relatively spread across the USA. However, the map suggests that these events may be more likely in counties that are densely populated or located in richer areas. To test this hypothesis, we estimated the following two equations:

$$z_{jt-1} = \gamma_0 + \gamma_1 \text{MS}_{jt} + \gamma_2 f(\text{Pop}_{jt}) + \zeta_j + \eta_t + \epsilon_{jt} \tag{1}$$

$$\text{MS}_{jt} = \psi_0 + \boldsymbol{\psi} \boldsymbol{Z}_{jt-1} + \psi_q f(\text{Pop}_{jt-1}) + \kappa_j + \lambda_t + \upsilon_{jt}, \tag{2}$$

where z_{jt-1} is the yearly value of a specific local characteristic (z) one year before the incident; MS_{jt} is a dummy variable for the year of the mass shooting; $f(\text{Pop}_{jt})$ ($f(\text{Pop}_{jt-1})$) is a quadratic function of the county's population the year of (before) the incident;[14] (ζ_j, κ_j) and (η_t, λ_t) are the county and year fixed effects, respectively. Finally, ϵ_{jt} and υ_{jt} are the idiosyncratic error terms. Under the assumption that mass shooting incidents are uncorrelated with the aggregate characteristics of the areas where they occur, neither γ_1 nor $(\boldsymbol{\psi}, \psi_q)$ should be statistically different from zero.

Table 2 shows the regression results with the corresponding p-values robust to heteroskedasticity, within-county correlation and multiple hypothesis testing obtained using the step-down procedure developed by Romano and Wolf (2005). We also report a randomization-based joint test for the sharp null hypothesis of all coefficients being equal to zero (Young, 2019).[15] Column (1) refers to Equation (1), whilst column (2) refers to Equation (2).

We found that there were no systematic differences between the affected and unaffected counties. The point estimates for the γ_1 coefficients were not statistically different from zero at 5% level in all but one county characteristic regression. The affected and unaffected counties were similar in economic conditions, labor market status, poverty level, and voting behaviour. Furthermore, the p-value for the null of complete irrelevance of the estimated parameters, computed using Young's (2019) randomization t-procedure, suggests that differences between the treated and untreated counties are insignificant. We obtain the same conclusion from the results in column (2):[16] local characteristics were found not to explain when and where mass shootings occurred. Taken together, these

[14] We also used linear and cubic specifications, obtaining similar findings. They are available from the authors upon request.

[15] We used the Stata commands `rwolf2` and `randcmd` developed by Clarke (2021) and Young (2020). The p-values were obtained by bootstrapping the results 1,000 times.

[16] We re-estimated Equation (2) using the conditional fixed effects logit model. The corresponding results are reported in the appendix, Table A.1. The conclusions are qualitatively equivalent to those presented in Table 2.

Table 2: Estimation of Equations (1) and (2) as exogeneity checks

	Treatment as independent variable (1)	Treatment as dependent variable (2)
Average number of private establishments (in log)	-0.014	-0.001
	(0.296)	(0.294)
Average weekly wage (in log)	0.001	0.000
	(0.971)	(0.258)
Median household income (in log)	-0.000	-0.004
	(0.993)	(0.597)
Number of property crimes (in log)	-0.172*	-0.000
	(0.063)	(0.599)
Number of violent crimes (in log)	-0.160**	-0.000
	(0.042)	(0.595)
Personal income per capita (in log)	0.007	0.003
	(0.536)	(0.595)
Real GDP per capita (in log)	-0.000	-0.000
	(0.993)	(0.599)
Republican voting share (gubernatorial)	-0.004	-0.002
	(0.906)	(0.595)
Share of population below poverty line	-0.094	-0.000
	(0.865)	(0.568)
Unemployment rate	0.089	0.000
	(0.789)	(0.572)
Population		0.000
		(0.148)
Population^2		-0.000
		(0.568)
Westfall-Young joint test p-value	0.176	0.533

Notes: * Significant at 10%, ** significant at 5% and *** significant at 1%. Romano-Wolf p-values robust to within-county correlation and the familywise error rate were obtained by bootstrapping the results 1,000 times and are reported in parenthesis. Westfall-Young p-values were obtained by bootstrapping the results 1,000 times. Column (1) shows the results for Equation (1), whilst column (2) displays the results for Equation (2). All the variables refer to the period before a mass shooting occurred. The sample covers the period 1999–2019. County fixed effects and year fixed effects are included in the model specification.

results suggest that mass shootings are an exogenous phenomenon, conditional on the observable characteristics of the areas where they occurred, county fixed effects and year fixed effects.

3.4 Estimation strategy

We began by implementing a two-way fixed effects (TWFE) model. For individual i living in city c at time t, we estimated the following specification for the health outcome variable y_{ict}:

$$y_{ict} = \beta \text{EMS}_{ict} + \boldsymbol{\theta} \boldsymbol{X}_{it} + \boldsymbol{\phi} \boldsymbol{Z}_{ct} + \alpha_i + \delta_t + \varepsilon_{ict}, \tag{3}$$

where EMS_{ict} is the binary indicator for the exposure to a mass shooting; $\boldsymbol{X}_{it}$ is a vector of family and individual controls; $\boldsymbol{Z}_{ct}$ is a vector of county characteristics common at city level; α_i and δ_t are the individual and year fixed effects; ε_{ict} is the error term.

An assumption implicit in Equation (3) is that the treatment effect β is constant across groups and time. However, the recent econometric literature has shown that estimation of the causal parameter of interest by means of TWFE may be problematic when individuals are treated at different points in time (staggered design) and the treatment effect is heterogeneous (see e.g., Borusyak and Jaravel, 2018; de Chaisemartin and d'Haultfoeuille, 2020; Callaway and Sant'Anna, 2021; Goodman-Bacon, 2021).[17]

As pointed out by de Chaisemartin and d'Haultfoeuille (2020), the regression coefficient of the TWFE model is a weighted sum of several treatment effects that compare the evolution of the mean outcome across groups between consecutive periods. When the treatment effect is constant, all the changes receive a positive weight. However, in the case of treatment heterogeneity, some changes may receive negative weights (de Chaisemartin and d'Haultfoeuille, 2020). Negative weights typically occur when there are compositional changes in the treated and control groups. For instance, observations that received the treatment earlier will be used as comparisons for observations that will receive the treatment later. This issue is known as the 'forbidden comparisons' problem and may lead to biased estimates of the true treatment effect (Borusyak and Jaravel, 2018; de Chaisemartin and d'Haultfoeuille, 2020).

In our analysis, we assumed that exposure to a mass shooting is an absorbing state type

[17] See Roth et al. (2023) for a review of the properties and pitfalls of the standard DiD estimator.

of treatment. Because mass shootings occur at different points in time, the share of individuals who receive treatment earlier may make the OLS estimator of the TWFE model inconsistent. To take this into account, we used the estimator proposed by de Chaisemartin and d'Haultfoeuille (2020) that is robust to treatment heterogeneity across groups and over time and relies on the common trend and strict exogeneity assumptions. We compared the change in the outcome of people whose treatment varied between two consecutive periods with the change in the outcome of individuals whose treatment did not vary in the same period. In practice, the method proposed by de Chaisemartin and d'Haultfoeuille (2020) estimates the following equation (Lu et al., 2023):

$$y_{ict} = \sum_{g \geq -A, g \neq -1}^{B} \beta^g \mathrm{EMS}_{ict}^g + \boldsymbol{\theta X}_{it} + \boldsymbol{\phi Z}_{ct} + \alpha_i + \delta_t + \varepsilon_{ict}. \tag{4}$$

Let g define a moment of the observational period when a particular cell received the treatment (i.e. was exposed to a mass shooting incident). Let B ($-A$) indicate the largest number of waves tested after (before) the cell received the treatment. Finally, let r indicate the specific wave when the cell received the treatment for the first time. We defined $\mathrm{EMS}_{ict}^g = 1$ when $t - r = g$, and zero otherwise for any other $-A \leq g \leq B$ with $g \neq -1$. In other words, we used the wave before a shooting incident occurred (i.e., $t - r = -1$) to calculate the treatment effect of all the following waves relative to the base. Thereafter, we computed the average treatment effect (ATT) as the average of all the estimated treatment effects.[18]

Estimation of Equation (4) presents two main advantages compared to estimation of Equation (3). First, it provides a natural way to test for pre-treatment trends (for $g < -1$). Second, it allows to study the dynamic of the treatment effect (for $g \geq 0$). We set A and B equal to 4. Because PSID collects information every two years, this specification enabled us to test the pre-treatment trends up to 8 years before the occurrence of the incident and to study the dynamic effects up to 8 years after the event.

Finally, because treated and control individuals may be subject to different trends in local characteristics, we preprocessed the sample using the entropy balancing (EB) method. The EB method reweights the sample in a such way that the joint distribution of the control units satisfies some ex-ante imposed moments conditions (Hainmueller, 2012;

[18] We used the Stata command `did_multiplegt_old` developed by de Chaisemartin and d'Haultfoeuille (2020).

Hainmueller and Xu, 2013). These conditions reflect the researcher's data knowledge and refer to the moments of the treated group. The selected weights minimize an entropy distance metric over a k-dimensional set of constraints. We imposed these constraints on the first and second moments.[19] We matched our observations over the following set of local characteristics measured at the county level: average number of private establishments (in log), average weekly wage (in log), median household income (in log), number of property crimes (in log), number of violent crimes (in log), personal income per capita (in log), real GDP per capita (in log), Republican voting share in gubernatorial elections, share of population living below the poverty line, shares of the population aged 25 to 39, 40 to 54, and 55 to 69, and the unemployment rate. We also added a set of binary indicators for the regional division in which individuals declared their residence.[20]

Tables A.2 and A.3 report the gains in balance for the mean and variance of each covariate. They show that the balance properties of the sample improved significantly. Before matching, the treated and control individuals were subject to different trends in local characteristics. Treated individuals were more likely than their control peers to live in richer areas with better labor market status. These differences were statistically significant at the 1% level. However, after matching, both treated and control units became more similar. Each difference in local characteristic became statistically insignificant and the bias was reduced to zero for almost every covariate.

3.5 Variables and descriptive statistics

3.5.1 Dependent variables

To study the effects of exposure to a mass shooting incident on individuals' health, we selected the Kessler psychological distress scale (K6) (Kessler et al., 2002, 2003) and a measure of SAH. K6 is a measure of *mental health* that captures nonspecific psychological distress (Kessler et al., 2002, 2003). It is built on six items that ask people about a set of emotions relevant to mental health (Kessler et al., 2002). These emotions are: (i) sadness, (ii) nervousness, (iii) hopelessness, (iv) worthlessness, (v) restlessness, and (vi) chronic fatigue. The respondents were asked to rate how they felt about each particular emotion

[19] We used the Stata command `ebalance` developed by Hainmueller and Xu (2013).

[20] We followed the classification system provided by the US Census Bureau. It is a two-tier hierarchical system in which federal states are grouped into nine larger contiguous divisions, which in turn are grouped into four macro-regions. The aggregation is made by grouping units that share similar local characteristics such as historical heritage, population, economy, etc.

in the 30 days preceding the interview. For each emotion, the ratings may go from 0 ('none of the time') to 4 ('all of the time'). The K6 is calculated as the sum of the scores and takes values from 0 and 24. Higher scores indicate worse mental health. The PSID introduced the items for the first time in 2001 and every two years ever since.[21]

SAH is instead a measure of *general health*. It is based on a survey item that asks individuals how they rate their health status at the time of the interview. Five answers are available: 'poor', 'fair', 'good', 'very good' and 'excellent'. We assigned value 0 to 'poor' and 4 to 'excellent'. Hence, higher scores indicate better health. The question has been part of the survey since 1984, and since 1997 it has been collected every two years.

K6 has been shown to be a valid measure of mental health and an effective screening tool for psychological disorders, such as depression (Furukawa et al., 2003; Cairney et al., 2007; Gill et al., 2007). The SAH has been shown to be highly predictive of future morbidity and mortality (Idler and Benyamini, 1997; Franzini et al., 2005). For these reasons, these measures are widely used in empirical research (see e.g. Frijters et al., 2005; Böckerman and Ilmakunnas, 2009; Drydakis, 2015; Cygan-Rehm et al., 2017; Wang et al., 2018; Bogan et al., 2022). Moreover, because both measures refer to either the moment of the interview or the 30 days preceding it, they are two indicators measuring the current health status.

Table A.4 and Figure A.2 in the appendix show summary statistics and the distribution of the outcome variables.

3.5.2 Treatment variable

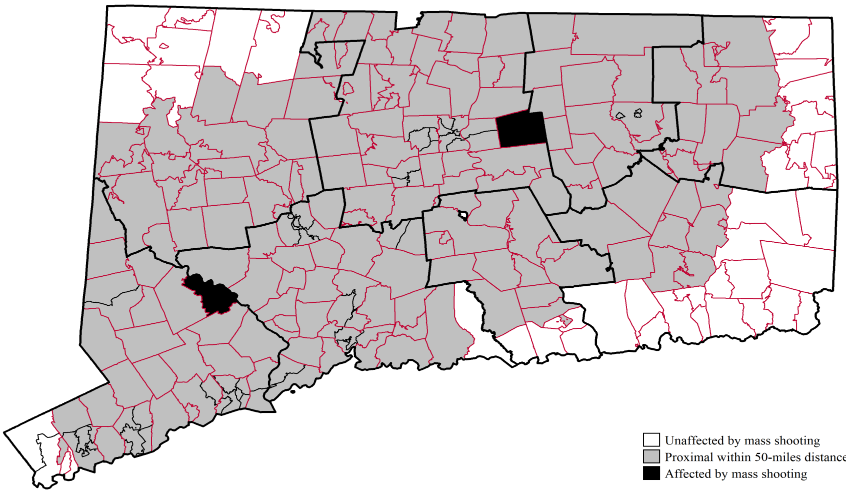
We generated the treatment indicator by exploiting information about the locations of mass shooting incidents and the individuals' place of residence. We defined the treatment at the city level. We considered as treated those individuals living in a city that experienced a mass shooting at the time when that shooting took place. Similarly, we considered as controls those individuals who were living in a city that: (i) had not yet or had never experienced a mass shooting; and (ii) was located more than 50 miles away from a city where a mass shooting occurred. We discarded individuals living in places located within 50 miles because, albeit they did not live in directly affected places, they might have suffered spillover effects due to the geographical proximity with the location of the incidents

[21] The only exception is made by the 2005 wave. For that particular year, no data are available on any of the questions.

(Bryson and MacKerron, 2018).[22]

Figure 2 visually clarifies the definition of treated and control units and the dismissal of people living in cities without mass shootings but located within 50 miles of a treated city for the state of Connecticut. The thin cranberry lines are city borders, while the thick black lines are county borders. The black polygons are cities that experienced mass shootings between 1999 and 2019. For the state of Connecticut, they are Manchester (2010) and Sandy Hook (2012). Polygons in white or gray are cities that did not experience a mass shooting during our time window. The white polygons are cities located more than 50 miles away from a treated city, while the gray ones are cities located within 50 miles from a treated city.[23] We considered as treated those individuals who lived in cities in black. We removed from the sample individuals who lived in cities in gray. Individuals who lived in white cities are our control group. Out of the 8,063 individuals in our sample, 600 people were treated.

Figure 2: Map of Connecticut with treated cities (in black), cities located within 50 miles (in gray) and cities located more than 50 miles away (in white) from a treated city, 1999–2019



Notes: Thick black lines are county borders. Thin cranberry lines are city boundaries. Polygons in black are cities affected by a mass shooting incident. Polygons in gray are cities closer than 50 miles to a mass shooting. Polygons in white are cities more than 50 miles distant from a mass shooting ('unaffected by mass shooting').

[22] We performed a sensitivity analysis by replacing the 50-mile distance with 0-mile, 30-mile and 70-mile distances. We discuss this robustness check in Section 4.4. In the appendix, Figure A.3 visually clarifies different definitions of control cities on the basis of different distances from a treated city for the states of Connecticut, Massachusetts, New York and Rhode Island.

[23] We computed distances between pairs of cities using the Vincenty's formula with the Stata command `geodist` developed by Picard (2019).

3.5.3 Control variables

Following previous studies on the effect of exposure to school and mass shootings (see e.g., Dursun, 2019; Bharadwaj et al., 2021; Sharkey and Shen, 2021; Sezer, 2022), we included an extensive set of individual and family socioeconomic characteristics. Individual characteristics were gender, age, race, whether the respondent had Hispanic/Latino origins, and the highest educational level achieved. The family characteristics were the number of siblings that the respondents had, the highest educational level achieved by each parent and the economic situation of the family when the respondent was growing up.

We also used a set of local characteristics measured at the county level to control for possible time-varying spatial heterogeneity. We included the real GDP per capita, personal income per capita and median income per capita to control for possible fluctuations of the business cycle. Furthermore, we included the share of the population living below the poverty line to control for income inequality, while demographic trends were controlled by using indicators of the shares of population aged between 25 and 39, between 40 and 54, and between 55 and 69 years. We controlled for the conditions of the labor market using the average number of private establishments, the average weekly wage paid and the unemployment rate of the counties. We also included variables of political sentiment and criminal behavior using the share of votes given to the Republican party during the last gubernatorial elections and the number of violent and property crimes that occurred in each county.

Finally, we included a set of missing indicators for each of the aforementioned variables in order to retain observations and not to lose statistical power. Panel b of Table A.4 in the appendix reports the summary statistics for the control variables.

4 Empirical findings

4.1 Main results

Table 3 presents the estimated treatment effect from Equation (3), where the impact of exposure to mass shootings is assumed to be homogeneous across groups and over time. We found that exposure to mass shootings worsens both mental health and self-assessed health. The point estimates are 0.402 and -0.078 respectively, with only the former significantly different from zero at the 5% level (p-value < 0.01). The impact on the Kessler psychological distress scale is particularly large, as it is a reduction of about 13.3% relative to the average of treated units. This finding is in line with previous empirical research, which has found that exposed individuals increase health care consumption, report lower well-being and are more likely to receive a psychological diagnosis (Bharadwaj et al., 2021; Sharkey and Shen, 2021; Deb and Gangaram, 2024).

Table 3: Two-way fixed effects estimation results

	Kessler psychological distress scale (K6)	Self-assessed health
	(1)	(2)
Mass shooting exposure	0.402***	-0.078*
	(0.128)	(0.042)
Treated sample mean	3.018	2.372
R^2	0.505	0.601
No. of clusters	1,660	1,742
No. of observations	35,871	45,784

Notes: * Significant at 10%, ** significant at 5% and *** significant at 1%. Standard errors robust to heteroskedasticity and within-city correlation are reported in parentheses. Each specification includes individual fixed effects, year fixed effects and the following individual and family characteristics: age, gender, Hispanic/Latino origins, race, educational level achieved, number of siblings, maternal educational level achieved, paternal educational level achieved, family wealth in juvenile years. Moreover, each specification includes the following local characteristics: average number of private establishments (in log), average weekly wage (in log), median household income (in log), number of property crimes (in log), number of violent crimes (in log), personal income per capita (in log), Republican voting share in the latest gubernatorial elections, real GDP per capita (in log), share of population living below the poverty line, shares of population aged between 25 and 39, 40 and 54, and 55 and 69, and unemployment rate. Each local characteristic was measured at county level. Finally, all specifications include missing indicators for the individual, family and local characteristics. The sample covers the period 1999–2019.

The TWFE results reported in Table 3 are based on the assumption that the treatment effect is constant across groups and over time. Under treatment heterogeneity, the regression coefficient of the TWFE model may be inconsistent due to the presence of negative weights. We followed de Chaisemartin and d'Haultfoeuille (2020) and estimated the weights attached to each change used to compute the β coefficients reported in Table 3. For each regression coefficient, we calculated the proportion of negative weights and the corresponding amount. In the absence of treatment heterogeneity, these quantities should be close to zero. We then computed two summary measures that are used to assess the robustness of the regression coefficient to treatment heterogeneity ($\underline{\sigma}_{fe}$ and $\underline{\underline{\sigma}}_{fe}$). The first summary measure ($\underline{\sigma}_{fe}$) refers to the minimal value of the standard deviation of the regression coefficient beyond which $\hat{\beta}$ and the ATT in all treated cells may have opposite signs. The second measure ($\underline{\underline{\sigma}}_{fe}$) refers to the minimal value of the standard deviation of the regression coefficient beyond which $\hat{\beta}$ and the ATT of each treated cell may have opposite signs. If the treatment effect is not heterogeneous, these measures should yield values larger than the standard error of the regression coefficient. Finally, we checked whether the estimated weights had a statistically significant correlation with measures of the intensity of the treatment. We selected as proxy measures the number of deaths that occurred in an incident and the time dimension. In the absence of treatment heterogeneity, these correlations should not be significantly different from zero.

Table 4 reports the results of these tests. Panel a reports the proportion of negative weights relative to the total with the corresponding amount. Panel b reports the values for the two summary measures together with the standard error of the regression coefficients ($\hat{\sigma}$). Finally, panel c reports the results for the correlations between the estimated weights and two proxy measures.

Table 4: Diagnostic tests on two-way fixed effects estimation results

	Kessler psychological distress scale (K6) (1)	Self-assessed health (2)
a) Preliminary checks		
ATT with negative weights (%)	26.262	35.430
Sum of negative weights	-0.215	-0.388
b) Diagnostic statistics		
$\hat{\sigma}$	0.128	0.042
$\underline{\sigma}_{fe}$	0.248	0.034
$\underline{\underline{\sigma}}_{fe}$	0.671	0.076
c) Testing weights randomness		
$\rho_{\text{weights, years}}$	-0.259***	-0.114***
$\rho_{\text{weights, deaths}}$	0.088**	0.061*

Notes: * Significant at 10%, ** significant at 5% and *** Significant at 1%. Model specification for each outcome variable is the same as in Table 3. We used the Stata command `twowayfeweights` developed by de Chaisemartin and d'Haultfoeuille (2020).

The proportion of estimated negative weights is large regardless of the outcome variable considered. It ranges from 26.3% to 35.4%, with a corresponding amount that is always greater than 0.2 in absolute value. Furthermore, for the SAH outcome variable, the estimated value of standard deviation of the regression coefficient ($\hat{\sigma}$) is larger than the value reported by the first summary measure ($\underline{\sigma}_{fe}$). Finally, the correlations between the estimated weights and the two proxy measures of treatment intensity are significantly different from zero.

These results provide strong evidence in favor of the presence of treatment effect heterogeneity, which may have biased the results reported in Table 3. Therefore, we computed the estimator proposed by de Chaisemartin and d'Haultfoeuille (2020), which is robust to treatment heterogeneity across groups or over time. Table 5 reports the results after the bias correction.

Table 5: Bias-adjusted estimation results

	Kessler psychological distress scale (K6) (1)	Self-assessed health (2)
Mass shooting exposure	0.638***	0.003
	(0.249)	(0.071)
Treated sample mean	3.018	2.372
No. of clusters	1,660	1,742
No. of observations	35,871	45,784

Notes: * Significant at 10%, ** significant at 5% and *** significant at 1%. Standard errors robust to heteroskedasticity and within-city correlation are reported in parentheses. They were obtained by bootstrapping the results 500 times. Model specification for each outcome variable is the same as in Table 3.

We found that exposure to a mass shooting increases psychological distress. The point estimate of the mental health indicator coefficient is 0.638, which is almost 60% larger than the estimated coefficient without bias correction. Relative to the treated average outcome, which is 3.02, it indicates that a mass shooting exposure reduces individuals' mental health by more than 20%. We replicated the analysis using standardized outcome variables and we found that a mass shooting exposure increases the psychological distress by approximately 16% of one standard deviation.[24]

An important question is whether this is a substantial effect or a trivial one. To quantify the magnitude of the effect of mass shooting exposure on psychological distress, we re-estimated Equation (3) after including individual labor income (in \$1,000) as a further control variable. By doing so, we could assess how labor income correlates with psychological distress and gain an idea of the psychological distress suffered by individuals in case of income reductions. Table A.5 in the appendix reports the estimated coefficient of labor income, which is equal to -0.004 and highly significant. This means that the increase in psychological distress induced by exposure to a mass shooting is of the same size as the one generated by a reduction of labor income by about \$160,000. If this figure were the causal effect of labor income on K6, it would be the monetary compensation needed to counteract the negative effect on mental health of exposure to a mass shooting.

With regard to SAH, the estimated coefficient is positive, pointing to a positive impact of mass shootings on general health. However, this effect is weak and not statistically

[24]Results using standardized outcome variables are not reported for the sake of brevity. They are available from the authors upon request.

different from zero. The lack of impact on SAH is not surprising, given that it is a general measure of health. As such, it is importantly related also to bodily pain, presence of illnesses and physical functioning (Kempen et al., 1998; Simon et al., 2005). These health dimensions are unlikely to react to mass shooting exposure as defined and considered in our analysis. Similarly, Au and Johnston (2014) found that SAH did not react as a consequence of an unexpected shock to income, whereas more specific measures of mental health did so significantly.

4.2 Dynamic analysis

We estimated the dynamic specification described in Equation (4) and report in Table 6 the estimate of each β^g. Investigating the dynamic of the effect is helpful for two reasons.

First, the key assumption for the causal interpretation of the estimated ATT is the common trend assumption: the average outcome of the treated and control units would have followed parallel trends in the absence of the treatment. We tested the plausibility of this assumption by looking at the evolution of the average outcome among the treated and the comparison population in the period before the mass shooting occurred. Under the common trend assumption, we would not expect to detect pre-treatment differences in trends. Panel a of Table 6 reports the treatment effect 4, 6, 8 years before the treatment occurred, with the effect 2 years before the treatment normalized to zero. Treated and controls had the same outcome evolution in both mental and self-assessed health before the treatment. The p-values for the joint significance of the placebo effects are indeed larger than 0.1. This suggests that the health outcomes of the treated and of the untreated were following a common trend before the treatment occurred, therefore supporting the validity of the parallel trend assumption.

Second, the ATT of mass shooting exposure on mental health reported in Table 5 may be heterogeneous over time since the treatment. The psychological distress generated by a trauma is not necessarily long-lasting and permanent (Breslau, 2009); instead, it may fade away after a while. Comprehending its temporal evolution could be relevant for the design of interventions to enhance resilience and mitigate more detrimental trajectories following mass traumatic incidents. Panel b of Table 6 shows that psychological distress, as measured by the Kessler psychological distress scale, increases after a mass shooting and it is persistent in the medium-term. More in detail, the effect is not significantly different from zero in the year of exposure, but it becomes sizable and significant in the

Table 6: Dynamic analysis of the estimated effect

	Kessler psychological distress scale (K6) (1)	Self-assessed health (2)
a) Pre-treatment placebo		
Placebo at t-8	0.451	-0.181**
	(0.880)	(0.083)
Placebo at t-6	-0.275	-0.076
	(0.362)	(0.060)
Placebo at t-4	0.316	-0.012
	(0.313)	(0.051)
Joint significance of placebo effects, p-value	0.499	0.170
b) Post-treatment impact		
Impact at t	0.214	0.017
	(0.298)	(0.064)
Impact at t+2	0.892***	-0.031
	(0.310)	(0.073)
Impact at t+4	0.809**	-0.001
	(0.391)	(0.083)
Impact at t+6	0.738*	0.008
	(0.388)	(0.102)
Impact at t+8	0.900	0.031
	(0.565)	(0.117)

Notes: * Significant at 10%, ** significant at 5% and *** significant at 1%. Standard errors robust to heteroskedasticity and within-city correlation are reported in parentheses. They were obtained by bootstrapping the results 500 times. Model specification for each outcome variable is the same as in Table 3. The reference period is t-2 and the effect at t-2 is therefore normalized to zero.

subsequent periods: the psychological distress generated by a mass shooting is present at least up to six years after the incident. This result is line with those of previous empirical research, which has found that exposure to extreme events may leave scars on individuals' psychological health (Bharadwaj et al., 2021), especially if they are incidents of mass violence (Norris et al., 2002).

The SAH indicator did not show a similar reaction over time since treatment. The pattern depicted by the estimated coefficients is flat, with no effect size significantly different from zero in any of the periods considered.

4.3 Heterogeneity analysis

Not all individuals who experience trauma develop persistent mental health problems. Differential risks of psychological distress have been associated with pre-exposure characteristics such as gender and disadvantaged social, intellectual and educational status, among many others (Lancaster et al., 2016). In this subsection, we investigate the heterogeneity of the effects by assessing whether different demographic factors, like gender, race and age, are related to a different mass shooting exposure gradient.

First, we split the sample by gender. Exposure to a traumatic event can generate different consequences depending on gender (or sex) for different reasons. First, coping strategies can differ between men and women. Women tend to handle stressful situations with a more ruminative style of coping (Gavranidou and Rosner, 2003), which is emotion-focused, defensive and palliative (Olff, 2003). It has been shown that reacting to a traumatic event by ruminating on misfortune leads to longer and more severe periods of depression, with a higher probability of developing post-traumatic stress disorder (Ehlers et al., 1998). A second explanation may be biological and related to sex differences in the oxytocin system, which is likely to play a sex-specific role in the stress response (Gavranidou and Rosner, 2003). Third, gender differences in trauma reaction could be correlated with different gender roles and societal constructs surrounding 'feminine' and 'masculine' conduct (Norris et al., 2001), with men less willing than women to report reactions to traumatic stress. Panel a of Table 7 shows the effect on K6 is slightly more pronounced for women: only the female ATT is significantly different from 0 and is 34% larger than the male one.[25] Similarly, Soni and Tekin (2023) found that mass shootings affect the emotional well-being index of women but not of men. This is in line with findings in the psychiatric literature which show that women have been at greater risk than men of depression, anxiety and posttraumatic stress disorder after traumatic events like assaultive violence in Detroit (Breslau, 2009), the 1989 Exxon Valdez oil spill in Alaska (Palinkas et al., 1993), the 9/11 terrorist attack in New York (Schlenger et al., 2002; DeLisi et al., 2003) or terrorist attacks during the Al-Aqsa Intifada (Solomon et al., 2005).

Second, we divided the sample using the age mid-point of the population under study (45 years). Trauma responses may change over the life span due to a combination of factors such as cognitive decline, the effects of psycho-social stress and neurological changes (Ruzich et al., 2005; Hiskey et al., 2008). Panel b of Table 7 shows that older individuals are at greater risk of developing mental conditions than younger ones.

Finally, we split the sample according to the race declared by the respondents into three categories: black, white, and the rest of the US population. The magnitude of the health effect of mass shooting exposure may correlate with race/ethnicity because people belonging to different ethno-racial groups are subject to different cultural norms determining heterogeneity in symptom disclosure, reporting style, cultural interpretations of

[25]The 95% confidence intervals for the estimated effects for men and women show substantial overlap, indicating that their difference is not statistically significant.

symptoms and distress, and coping styles (Asnaani and Hall-Clark, 2017). To date, it is unclear how and to what extent cultural variability may affect responses to traumas, with studies on US cultural groups reporting mixed results.[26] Hinton and Lewis-Fernández (2011) suggested that several methodological challenges may explain this lack of clearcut findings, encompassing variations among studies in defining traumatic events, assessing exposure to trauma, and incorporating additional predisposing or facilitating factors such as access to treatment, social support, and other resources that aid recovery and are intertwined with racial/ethnic backgrounds. Panel c of Table 7 shows that Blacks are the least affected by mass shooting exposure.[27]

Table 7: Estimation results by different individual characteristics

	Kessler psychological distress scale (K6) (1)		Self-assessed health (2)	
	No. of observations	Coefficient	No. of observations	Coefficient
a) Gender				
Male	16,591	0.506	21,107	-0.024
		(0.316)		(0.111)
Female	19,280	0.677**	24,677	0.027
		(0.323)		(0.072)
b) Age				
Under 45	18,308[(a)]	0.311	23,160[(a)]	0.035
		(0.378)		(0.096)
Over 45	16,895[(a)]	0.515*	21,906[(a)]	-0.020
		(0.277)		(0.075)
c) Race				
Black	12,507[(b)]	0.243	16,105[(b)]	0.006
		(0.524)		(0.133)
White	21,085[(b)]	1.084***	26,769[(b)]	-0.024
		(0.392)		(0.065)
Other	1,970[(b)]	5.101**	2,530[(b)]	-0.272
		(2.314)		(0.569)

Notes: * Significant at 10%, ** significant at 5% and *** significant at 1%. Standard errors robust to heteroskedasticity and within-city correlation are reported in parentheses. They were obtained by bootstrapping the results 500 times. Model specification for each outcome variable is the same as in Table 3.
[(a)] The sum of the number of observations under 45 and above 45 does not match the number of observations reported in Table 5 because some individuals became singletons and were discarded from the estimation.
[(b)] Observations by race do not sum up to what is shown in Table 5 because some individuals did not report their race and were therefore excluded from this heterogeneity analysis.

[26] See Hinton and Lewis-Fernández (2011) for a review of studies which have analyzed how different traumas generate psychological distress across racial/ethnic groups.

[27] The effect for those belonging to the residual racial group, although the largest in size, is estimated with poor precision due to a too small sample size.

4.4 Sensitivity analysis

We conducted two sets of sensitivity checks to verify the robustness of our estimates to the definition of control units and to the estimator used.

First, we checked if and to what extent our estimated effects were robust to the exclusion from our sample of those individuals living in cities located within 50 miles from a treated city and the definition of controls as those individuals living in cities more than 50 miles away from it. We considered three alternatives. First, we considered as controls those individuals living in cities which did not experience a mass shooting, independently of the distance from the mass shooting location. Second, we restricted the 50-mile radius to 30. Finally, we enlarged the 50-mile radius to 70. Table 8 reports the results. Panel a shows that if we considered as controls those individuals living in cities near the mass shooting location, the effect would be smaller, which is consistent with mass shootings generating spillover effects also in the surrounding areas which, if not considered when defining the control group, give rise to a bias towards zero of the causal effect. Panel b shows that as soon as individuals living in cities near the mass shooting city were excluded from the analysis, we obtained results which were very close to the baseline estimates. We obtained similar findings when we used as controls those individuals living in cities more than 70 miles away from the treated city, although the standard errors became less precise due to the smaller sample size.

Second, we assessed the robustness of our findings by using an alternative estimator able to deal with treatment effect heterogeneity in a staggered DiD design. We implemented the alternative estimator proposed by Callaway and Sant'Anna (2021), which is based on the identification of 'group-time' effects whereby groups are defined by the time period when units are first treated. Panel d of Table 8 shows estimates that are qualitatively in line with the benchmark ones. The negative impact of mass shooting exposure on psychological distress is even larger, although estimated with lower precision.

5 Conclusions

We studied the effect of mass shooting exposure on individuals' mental health. We identified the causal effect of mass shootings by exploiting the quasi-random nature of these incidents (Luca et al., 2020; García et al., 2022; Muñoz-Morales and Singh, 2023; Soni and Tekin, 2023). We defined the exposure to a mass shooting at the city level. Because

Table 8: Sensitivity analysis: different radius extensions to define control cities (panels a–c) and an alternative estimator (panel d)

	Kessler psychological distress scale (K6) (1)		Self-assessed health (2)	
	No. of observations	Coefficient	No. of observations	Coefficient
a) 0-mile radius				
Mass shooting exposure	46,610	0.365	58,805	0.011
		(0.235)		(0.055)
b) 30-mile radius				
Mass shooting exposure	40,671	0.535**	51,527	-0.005
		(0.223)		(0.056)
c) 70-mile radius				
Mass shooting exposure	30,877	0.551*	39,876	0.009
		(0.306)		(0.081)
d) Using Callaway and Sant'Anna's (2021) estimator[a]				
Mass shooting exposure	35,593	1.206***	45,663	0.038
		(0.449)		(0.067)

Notes: * Significant at 10%, ** significant at 5% and *** significant at 1%. Standard errors robust to heteroskedasticity and within-city correlation are reported in parentheses. In panels a, b and c, they are obtained by bootstrapping the results 500 times. Model specification for each outcome variable is the same as in Table 3.
[a] Matching was performed using the EB weighting scheme. The control group used both 'never' and 'not-yet' treated observations.

the mass shootings considered occurred in different cities during different time periods, we used a DiD approach with a staggered design. We compared the changes in the outcome variables of individuals living in affected cities with the changes in the outcome variables of matched individuals living in non-proximal cities. Non-proximal cities were identified as those cities located more than 50-miles away from the location of an incident. We gathered information on mass shootings from the VPMSD, a public repository of high-profile public mass shootings in the USA. For the period 1999–2019, we combined it with PSID data.

We found that exposure to a mass shooting deteriorated individuals' mental health: psychological distress increased by more than 20% after a mass shooting (about 16% of one standard deviation) relative to the average psychological distress of the treated. By looking at how labor income correlates with psychological distress, we quantified as \$160,000 the monetary compensation needed to counteract the negative effect on mental health of mass shooting exposure. When studying the dynamic effect of mass shootings, we found that the effect is persistent, because it may still be present 6 years after the incident. The heterogeneity analysis revealed that mass shootings do not affect the mental health of Blacks, whereas their impact is more pronounced among women and older age cohorts.

Our findings raise relevant policy considerations. First, we provided evidence of the negative effect of mass shootings on mental health of adult population in the USA. Most of the previous empirical research either focused on the effects on population of students exposed to a shooting at school during school time or studied the macroeconomic implications of these incidents in the areas where they occurred. Much less attention has been paid to the potential consequences for the general population of all types of mass shooting. Second, we found that the mental health effect lasts for several years and is more pronounced for some groups. Policy makers should be aware that mass shootings entail a relevant and lasting societal burden. They should evaluate the need for supportive therapeutic strategies for the affected communities promptly after the occurrence of an incident, in order to increase resilience, decrease adverse trajectories and prevent scars to arise.

Finally, our results are subject to some limitations which qualify them. First, we studied the costs of victimization by relying on survey data and an approximation for the actual exposure to a mass shooting. We indeed defined an individual as treated by a mass shooting if s/he was living in the city where and when the mass shooting occurred and not on the basis of the actual involvement in the crime scene. Hence, a number of persons that we considered as treated could not have felt as exposed to the mass shooting. If so, our estimates should be considered as a lower bound of the true effect. Second, we had to restrict the sample to only those individuals who did not change city. We were forced to do so because PSID runs the survey in uneven years and, therefore, we could not know the city of residence in even years and correctly assign the treatment status. By sticking to individuals who did not change residence, we avoided this problem. However, working on the sub-population of stayers limits the external validity and generalizability of our findings. Indeed, by restricting the analysis only to individuals that did not relocate during the observation period, we were able to provide only a partial estimation of the true cost of mass shooting exposure, because those who suffered most from the incident may have been more likely to relocate elsewhere. Third, even though we used an objective definition to identify mass shootings, it could still be possible that crucial incidents have been overlooked because they did not meet one of the criteria for inclusion in the Violence Project Mass Shooter Database, leading to a possible underestimation of the true treatment effect of the phenomenon.

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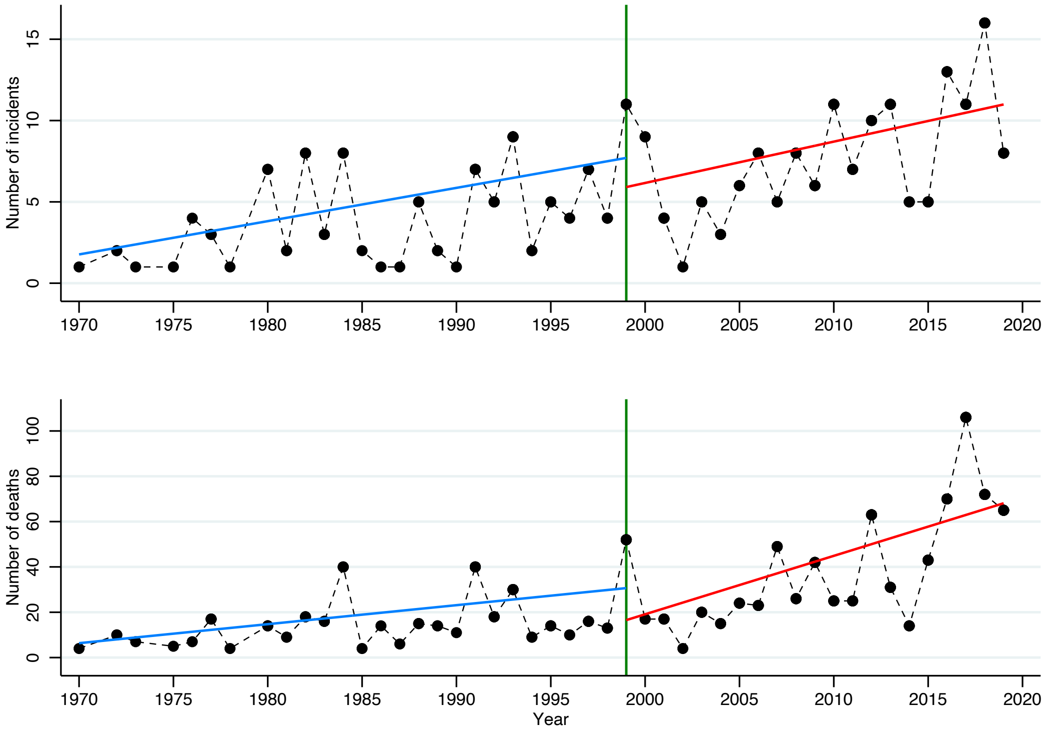
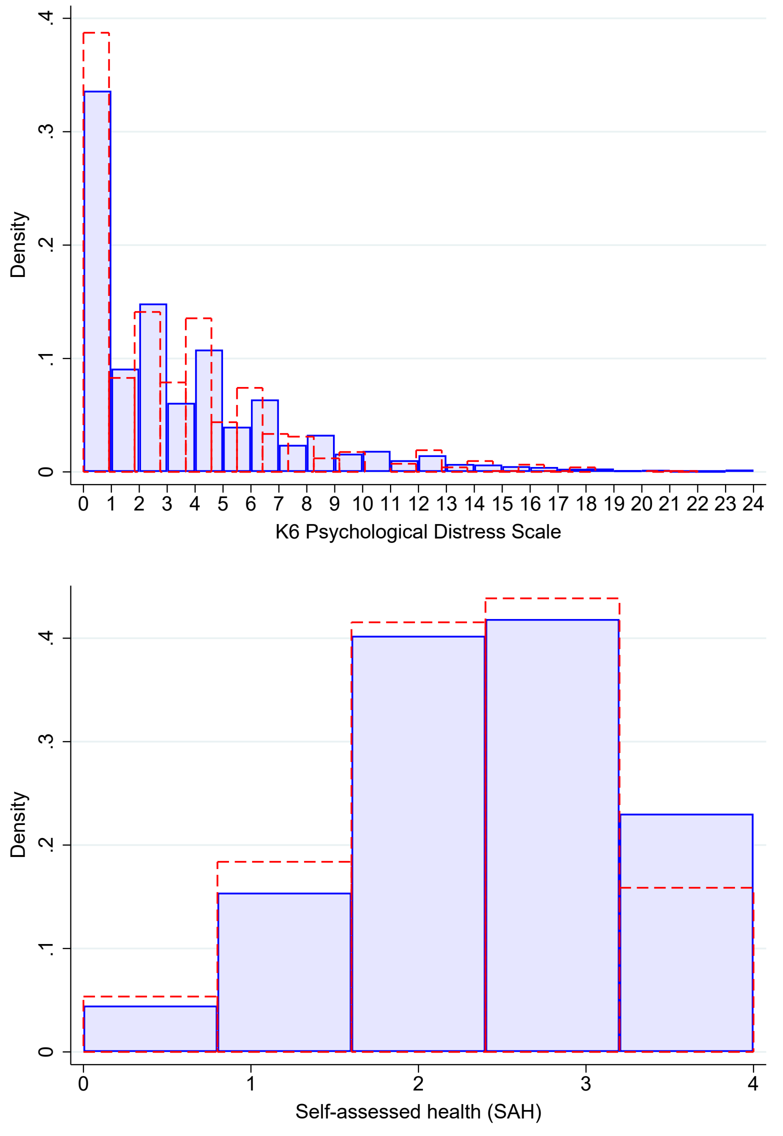
Appendix

Figure A.1: Number incidents and related deaths in the period 1970-2019



Notes: A mass shooting is identified if at least 4 people were killed during the event, perpetrators excluded. The upper panel refers to the number of incidents. The lower panel refers to the number of deaths. The vertical green line refers to the year 1999. The blue (red) lines represent the fitted trends before (after) 1999. For the period 1970–1999, the value of the slope of the fitting line for the number of incidents (deaths) is 0.205 (0.838). For the period 1999–2019, the value is 0.254 (2.579). The t-tests for the equality of the slopes returned p-values smaller than 0.001.

Figure A.2: Distribution of the outcome variables



Notes: Top panel refers to Kessler psychological distress (K6) scale. Higher scores mean worse mental health. Bottom panel refers to the self-assessed health (SAH) measure. Higher scores means better general health. Colorized solid bins in blue refer to control units. Empty dashed bins in red refer to treated units

Table A.1: Estimation of Equation (2) by conditional fixed effects logit

	Treatment as dependent variable
Average number of private establishments (in log)	-1.433
	(0.848)
Average weekly wage (in log)	4.273
	(0.848)
Median household income (in log)	-6.348
	(0.453)
Number of property crimes (in log)	-0.463
	(0.848)
Number of violent crimes (in log)	-0.259
	(0.848)
Personal income per capita (in log)	3.396
	(0.848)
Real GDP per capita (in log)	-2.176
	(0.848)
Republican voting share (gubernatorial)	-1.125
	(0.848)
Share of population below poverty line	-0.143
	(0.608)
Unemployment rate	0.185
	(0.639)
Population	0.000
	(0.848)
Population^2	-0.000
	(0.571)
Westfall-Young joint test p-value	0.703

Notes: * Significant at 10%, ** significant at 5% and *** significant at 1%. Romano-Wolf p-values robust to within-county correlation and the familywise error rate were obtained by bootstrapping the results 1,000 times and are reported in parenthesis. Westfall-Young p-values were obtained by bootstrapping the results 1,000 times. All the variables refer to the period before a mass shooting occurred. The sample covers the period 1999–2019. County fixed effects and year fixed effects are included in the model specification.

Table A.2: Pre- and post-matching bias on the mean

	Pre-Matching				Post-Matching			
	Mean Treated	Mean Controls	∣ Difference ∣	∣ Bias ∣	Mean Treated	Mean Controls	∣ Difference ∣	∣ Bias ∣
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Local characteristics								
Average number of private establishments (in log)	10.730	7.748	2.982***	27.791%	10.730	10.729	0.001	0.009%
Average weekly wage (in log)	6.850	5.913	0.937***	13.679%	6.850	6.850	0.000	0.000%
Median household income (in log)	10.732	9.647	1.085***	10.110%	10.732	10.732	0.000	0.000%
Number of property crimes (in log)	10.808	7.153	3.655***	33.817%	10.808	10.806	0.002	0.018%
Number of violent crimes (in log)	9.933	6.409	3.524***	35.478%	9.933	9.932	0.001	0.010%
Personal income per capita (in log)	10.632	9.429	1.203***	11.315%	10.632	10.632	0.000	0.000%
Real GDP per capita (in log)	3.955	3.074	0.881***	22.276%	3.955	3.955	0.000	0.000%
Republican voting share (gubernatorial)	0.493	0.442	0.051***	10.345%	0.493	0.492	0.000	0.000%
Share of population below poverty line	15.245	13.890	1.355***	8.888%	15.245	15.244	0.001	0.007%
Share of population aged 25 to 39	0.231	0.181	0.050***	21.645%	0.231	0.231	0.000	0.000%
Share of population aged 40 to 54	0.203	0.184	0.019***	9.360%	0.203	0.203	0.000	0.000%
Share of population aged 55 to 69	0.140	0.138	0.002**	1.429%	0.140	0.140	0.000	0.000%
Unemployment rate	5.886	5.640	0.245***	4.162%	5.886	5.886	0.000	0.000%
Federal division of residence (reference: New England)								
Middle Atlantic	0.020	0.109	0.089***	445.000%	0.020	0.020	0.000	0.000%
East North Central	0.232	0.168	0.064***	27.586%	0.232	0.232	0.000	0.000%
West North Central	0.120	0.096	0.024***	20.000%	0.120	0.120	0.000	0.000%
South Atlantic	0.184	0.211	0.027***	14.674%	0.184	0.184	0.000	0.000%
East South Central	0.005	0.118	0.113***	2,260.000%	0.005	0.005	0.000	0.000%
West South Central	0.268	0.103	0.165***	61.567%	0.268	0.268	0.000	0.000%
Mountain	0.066	0.046	0.020***	30.303%	0.066	0.066	0.000	0.000%
Pacific	0.103	0.121	0.018***	17.476%	0.103	0.103	0.000	0.000%

Notes: *** Significant at 1%. The various federal states are grouped in nine divisions. The reference division is 'New England'. It includes Connecticut, Maine, Massachusetts, New Hampshire, Rhode Island, and Vermont. 'Middle Atlantic' includes New Jersey, New York, and Pennsylvania. 'East North Central' includes Illinois, Indiana, Michigan, Ohio, and Wisconsin. 'West North Central' includes Iowa, Kansas, Minnesota, Missouri, Nebraska, North Dakota, and South Dakota. 'South Atlantic' includes Delaware, Florida, Georgia, Maryland, North Carolina, South Carolina, Virginia, District of Columbia and West Virginia. 'East South Central' includes Alabama, Kentucky, Mississippi, and Tennessee. 'West South Central' includes Arkansas, Louisiana, Oklahoma, and Texas. 'Mountain' includes Arizona, Colorado, Idaho, Montana, Nevada, New Mexico, Utah, and Wyoming. Finally, 'Pacific' includes California, Oregon, and Washington.

Table A.3: Pre- and post-matching bias on the variance

	Pre-Matching				Post-Matching			
	Variance Treated	Variance Controls	| Difference |	| Bias |	Variance Treated	Variance Controls	| Difference |	| Bias |
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Local characteristics								
Average number of private establishments (in log)	2.336	9.306	6.970***	298.373%	2.336	2.338	0.002	0.086%
Average weekly wage (in log)	0.588	4.010	3.422***	581.973%	0.588	0.588	0.000	0.000%
Median household income (in log)	1.386	10.510	9.124***	658.297%	1.386	1.385	0.001	0.072%
Number of property crimes (in log)	2.606	12.747	10.141***	389.140%	2.606	2.616	0.010	0.384%
Number of violent crimes (in log)	2.493	11.058	8.565***	343.562%	2.493	2.503	0.010	0.401%
Personal income per capita (in log)	1.379	10.058	8.679***	629.369%	1.379	1.378	0.001	0.072%
Real GDP per capita (in log)	1.325	2.385	1.060***	80.000%	1.325	1.325	0.000	0.000%
Republican voting share (gubernatorial)	0.010	0.029	0.019***	190.000%	0.010	0.010	0.000	0.000%
Share of population below poverty line	18.323	53.637	35.314***	192.730%	18.323	18.325	0.002	0.011%
Share of population aged 25 to 39	0.001	0.004	0.003***	300.000%	0.001	0.001	0.000	0.000%
Share of population aged 40 to 54	0.001	0.004	0.003***	300.000%	0.001	0.001	0.000	0.000%
Share of population aged 55 to 69	0.001	0.003	0.002***	200.000%	0.001	0.001	0.000	0.000%
Unemployment rate	4.971	10.478	5.507***	110.782%	4.971	4.973	0.002	0.040%
Federal division of residence (reference: New England)								
Middle Atlantic	0.020	0.097	0.077***	350.000%	0.020	0.020	0.000	0.000%
East North Central	0.178	0.140	0.038***	21.348%	0.178	0.178	0.000	0.000%
West North Central	0.106	0.087	0.019***	17.924%	0.106	0.106	0.000	0.000%
South Atlantic	0.150	0.167	0.017***	11.333%	0.150	0.150	0.000	0.000%
East South Central	0.005	0.104	0.099***	1,980.000%	0.005	0.005	0.000	0.000%
West South Central	0.196	0.093	0.103***	52.551%	0.196	0.196	0.000	0.000%
Mountain	0.061	0.044	0.017***	27.869%	0.061	0.061	0.000	0.000%
Pacific	0.092	0.106	0.014***	15.217%	0.092	0.092	0.000	0.000%

Notes: *** Significant at 1%. The various federal states are grouped in nine divisions. The reference division is 'New England'. It includes Connecticut, Maine, Massachusetts, New Hampshire, Rhode Island, and Vermont. 'Middle Atlantic' includes New Jersey, New York, and Pennsylvania. 'East North Central' includes Illinois, Indiana, Michigan, Ohio, and Wisconsin. 'West North Central' includes Iowa, Kansas, Minnesota, Missouri, Nebraska, North Dakota, and South Dakota. 'South Atlantic' includes Delaware, Florida, Georgia, Maryland, North Carolina, South Carolina, Virginia, District of Columbia and West Virginia. 'East South Central' includes Alabama, Kentucky, Mississippi, and Tennessee. 'West South Central' includes Arkansas, Louisiana, Oklahoma, and Texas. 'Mountain' includes Arizona, Colorado, Idaho, Montana, Nevada, New Mexico, Utah, and Wyoming. Finally, 'Pacific' includes California, Oregon, and Washington.

Table A.4: Summary statistics

	Treated					Controls				
Variables	n	μ	σ	Min.	Max.	n	μ	σ	Min.	Max.
a) Dependent variables										
Kessler Psychological Distress Scale (K6)	1,367	3.018	3.564	0.000	22.000	34,504	3.193	3.902	0.000	24.000
Self-assessed health	2,591	2.372	1.019	0.000	4.000	43,193	2.508	1.039	0.000	4.000
b) Control variables										
Individual characteristics										
Age	2,591	49.363	9.255	25.000	65.000	43,193	44.440	11.513	25.000	65.000
Gender (1 if female)	2,591	0.601	0.490	0.000	1.000	43,193	0.535	0.499	0.000	1.000
Hispanic origins:										
Missing	2,591	0.001	0.039	0.000	1.000	43,193	0.009	0.093	0.000	1.000
Spanish or hispanic	2,591	0.115	0.319	0.000	1.000	43,193	0.095	0.293	0.000	1.000
Race:										
Missing	2,591	0.003	0.052	0.000	1.000	43,193	0.009	0.092	0.000	1.000
Black American	2,591	0.568	0.495	0.000	1.000	43,193	0.339	0.473	0.000	1.000
White American	2,591	0.367	0.482	0.000	1.000	43,193	0.598	0.490	0.000	1.000
Other	2,591	0.062	0.241	0.000	1.000	43,193	0.055	0.228	0.000	1.000
Educational level achieved:										
Missing	2,591	0.152	0.359	0.000	1.000	43,193	0.101	0.301	0.000	1.000
Less than high school	2,591	0.384	0.486	0.000	1.000	43,193	0.420	0.493	0.000	1.000
High school diploma	2,591	0.248	0.432	0.000	1.000	43,193	0.253	0.435	0.000	1.000
More than high school	2,591	0.216	0.411	0.000	1.000	43,193	0.226	0.418	0.000	1.000
Family characteristics										
Number of siblings:										
Missing	2,591	0.130	0.337	0.000	1.000	43,193	0.171	0.377	0.000	1.000
Siblings	2,591	3.493	2.718	0.000	8.000	43,193	3.027	2.564	0.000	8.000
Educational level achieved (mother):										
Missing	2,591	0.095	0.293	0.000	1.000	43,193	0.075	0.264	0.000	1.000
Less than high school	2,591	0.736	0.441	0.000	1.000	43,193	0.723	0.448	0.000	1.000
High school diploma	2,591	0.078	0.267	0.000	1.000	43,193	0.098	0.297	0.000	1.000
More than high school	2,591	0.091	0.288	0.000	1.000	43,193	0.104	0.306	0.000	1.000
Educational level achieved (father):										
Missing	2,591	0.157	0.364	0.000	1.000	43,193	0.132	0.338	0.000	1.000
Less than high school	2,591	0.656	0.475	0.000	1.000	43,193	0.675	0.468	0.000	1.000
High school diploma	2,591	0.062	0.242	0.000	1.000	43,193	0.074	0.262	0.000	1.000
More than high school	2,591	0.124	0.330	0.000	1.000	43,193	0.119	0.324	0.000	1.000
Family economic situation while growing up:										
Missing	2,591	0.100	0.300	0.000	1.000	43,193	0.177	0.382	0.000	1.000
Poor	2,591	0.327	0.469	0.000	1.000	43,193	0.263	0.440	0.000	1.000
Average	2,591	0.349	0.477	0.000	1.000	43,193	0.360	0.480	0.000	1.000
Good	2,591	0.224	0.417	0.000	1.000	43,193	0.201	0.400	0.000	1.000
Local characteristics										
Average number of private establishments (in log)										
Missing	2,591	0.000	0.000	0.000	0.000	43,193	0.000	0.000	0.000	0.000
Value	2,591	10.890	0.968	5.989	13.124	43,193	8.637	1.692	0.000	13.124
Average weekly wage (in log)										
Missing	2,591	0.000	0.000	0.000	0.000	43,193	0.000	0.000	0.000	0.000
Value	2,591	6.957	0.196	6.368	7.516	43,193	6.577	0.317	0.000	7.922
Median household income (in log)										
Missing	2,591	0.000	0.000	0.000	0.000	43,193	0.000	0.000	0.000	0.000
Value	2,591	10.902	0.190	10.391	11.586	43,193	10.729	0.263	9.813	11.930
Number of property crimes (in log)										
Missing	2,591	0.000	0.000	0.000	0.000	43,193	0.036	0.186	0.000	1.000
Value	2,591	10.934	1.102	0.000	12.344	43,193	7.897	2.881	0.000	12.560
Number of violent crimes (in log)										
Missing	2,591	0.000	0.000	0.000	0.000	43,193	0.036	0.186	0.000	1.000
Value	2,591	10.058	1.135	0.000	11.671	43,193	7.082	2.762	0.000	11.846
Personal income per capita (in log)										
Missing	2,591	0.000	0.000	0.000	0.000	43,193	0.000	0.000	0.000	0.000
Value	2,591	10.798	0.237	10.078	11.463	43,193	10.484	0.297	9.504	12.061
Real GDP per capita (in log):										
Missing	2,591	0.000	0.000	0.000	0.000	43,193	0.090	0.286	0.000	1.000
Value	2,591	4.287	0.318	3.188	5.072	43,193	3.469	1.150	0.000	5.883
Republican voting share (gubernatorial)										

(continued on next page)

Table A.2: *Continued from previous page*

Variables	Treated n	μ	σ	Min.	Max.	Controls n	μ	σ	Min.	Max.
Missing	2,591	0.000	0.000	0.000	0.000	43,193	0.000	0.000	0.000	0.000
Value	2,591	0.500	0.088	0.111	0.776	43,193	0.493	0.091	0.111	0.792
Share of population below poverty line										
Missing	2,591	0.000	0.000	0.000	0.000	43,193	0.000	0.000	0.000	0.000
Value	2,591	15.076	3.681	5.200	36.400	43,193	15.378	5.997	2.500	50.400
Share of population aged 25 to 39										
Missing	2,591	0.000	0.000	0.000	0.000	43,193	0.090	0.286	0.000	1.000
Value	2,591	0.233	0.021	0.142	0.288	43,193	0.183	0.064	0.000	0.359
Share of population aged 40 to 54										
Missing	2,591	0.000	0.000	0.000	0.000	43,193	0.090	0.286	0.000	1.000
Value	2,591	0.204	0.014	0.137	0.263	43,193	0.185	0.062	0.000	0.300
Share of population aged 55 to 69										
Missing	2,591	0.000	0.000	0.000	0.000	43,193	0.090	0.286	0.000	1.000
Value	2,591	0.144	0.026	0.091	0.235	43,193	0.142	0.054	0.000	0.323
Unemployment rate:										
Missing	2,591	0.000	0.000	0.000	0.000	43,193	0.001	0.025	0.000	1.000
Value	2,591	5.991	2.301	2.300	13.500	43,193	6.250	2.920	0.000	29.900

Notes: *** Significant at 1%. The various federal states are grouped in nine divisions. The reference division is 'New England'. It includes Connecticut, Maine, Massachusetts, New Hampshire, Rhode Island, and Vermont. 'Middle Atlantic' includes New Jersey, New York, and Pennsylvania. 'East North Central' includes Illinois, Indiana, Michigan, Ohio, and Wisconsin. 'West North Central' includes Iowa, Kansas, Minnesota, Missouri, Nebraska, North Dakota, and South Dakota. 'South Atlantic' includes Delaware, Florida, Georgia, Maryland, North Carolina, South Carolina, Virginia, District of Columbia and West Virginia. 'East South Central' includes Alabama, Kentucky, Mississippi, and Tennessee. 'West South Central' includes Arkansas, Louisiana, Oklahoma, and Texas. 'Mountain' includes Arizona, Colorado, Idaho, Montana, Nevada, New Mexico, Utah, and Wyoming. Finally, 'Pacific' includes California, Oregon, and Washington.

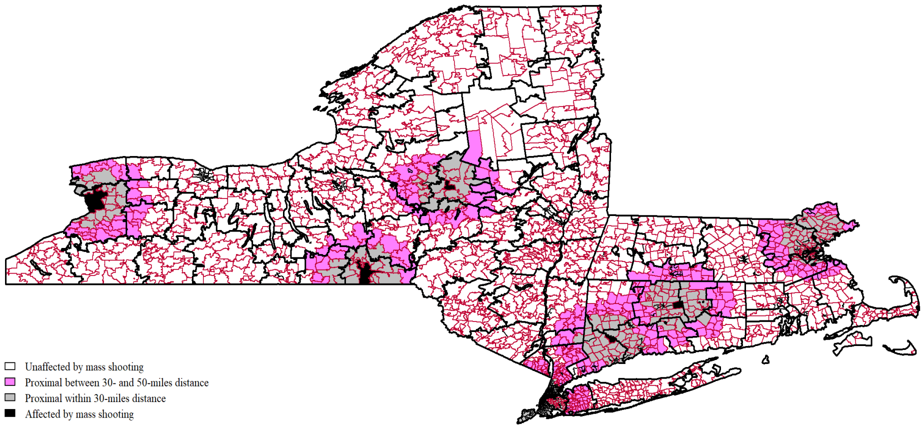
Table A.5: Correlation between individual health and total labor income (in $1,000)

	Kessler psychological distress scale (K6) (1)	Self-assessed health (2)
Total labor income	-0.004***	0.0008**
	(0.001)	(0.0003)
R^2	0.506	0.602
No. of clusters	1,660	1,742
No. of observations	35,871	45,784

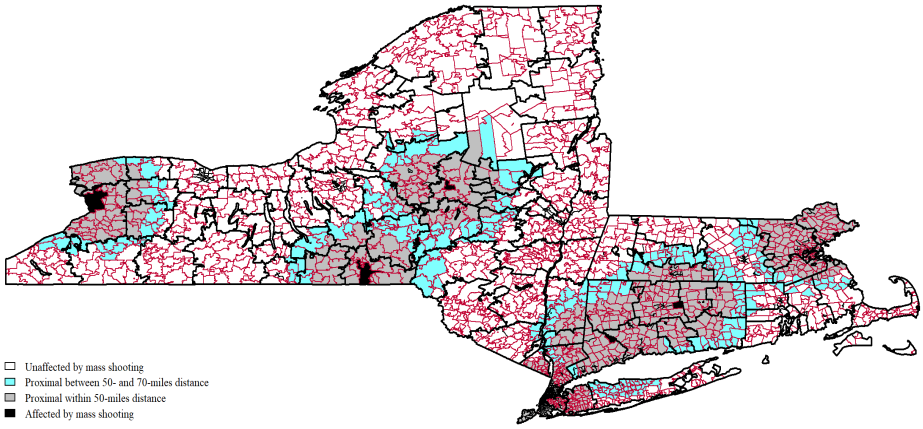
Notes: * Significant at 10%, ** significant at 5% and *** significant at 1%. Standard errors robust to heteroskedasticity and within-city correlation are reported in parentheses. Model specification for each outcome variable is the same as in Table 3.

Figure A.3: Map of the states of Connecticut, Massachusetts, New York and Rhode Island with affected and proximal cities using different radius extensions, 1999-2019

a) Maximum radius extension: 30-miles



b) Maximum radius extension: 70-miles



Notes: A mass shooting is identified if at least 4 people were killed during the event, perpetrators excluded. The thick black lines delimit county borders. The thin cranberry lines delimit city borders. Polygons in black refer to cities that experienced by a mass shooting incident. Polygons in gray refer to cities geographically close to the epicenter. Polygons in magenta (cyan) refer to excluded (included) cities located 30-(50-) to 50-(70-)miles away from the epicenter. Polygons in white refer to unaffected cities.