Main Manuscript for

Mass shootings durably increase the sale of alcohol in American communities

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Author Contributions: NB & SO designed the study, SY cleaned and prepared the raw data, NB analyzed the data and drafted the manuscript, SY & SO edited.

Competing Interest Statement: We have no competing interests to disclose.

Classification: Social Sciences/Social Sciences

Keywords: Mass shootings, gun violence, alcohol, community response

Funding: There is no funding to report for this project

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Table 1

Abstract

Mass shootings are devastating events. Communities can cope with the ensuing trauma in a number of ways, including changing their behavioral patterns. Using point-of-sale data from 35,000 individual retailers, including more than half of all American grocery and drug-store purchases, and all American mass shootings from 2006-2019, we find, in a set of two-way fixed-effects counterfactual analyses, that a mass shooting in a given community (the area covered by the ZIP-3 code) predicts a significant increase in the sales of alcohol that lasts at least two years past the shooting. The effect is especially strong for the subset of mass shootings that take place in public settings, whereas we find no evidence for an increase in alcohol sales in the aftermath of mass shootings that take place in private homes or residences. As alcohol is an accelerant for violence, especially firearm-related violence, we suggest the importance of whole-community-approaches to addressing the trauma of mass shootings.

Significance Statement

How does a community cope with a mass shooting? Even those not directly affected may feel some sense of trauma or dislocation when multiple people are shot to death nearby. Using a dataset covering the majority of alcohol sales in the United States, we find evidence that after a public mass shooting, rates of alcohol purchases in the affected communities stay elevated for at least two years. This study demonstrates the long reach that such catastrophic events can have not just for those involved, but for the wider fabric of the community.

Introduction

Shootings are traumatic experiences (see [1] for a review), not just for those directly involved but for the broader community as well (e.g. [2]). One analysis of ~16,000 American neighborhoods has found that communities that experience mass shootings are more likely to have worsened physical and mental health up to a year after the incident [3], and another analysis of census tracts across Philadelphia found that a shooting in the neighborhood (especially a non-fatal shooting) increased neighborhood-level rates of obesity and inactivity [4]. Even simply witnessing or hearing about a shooting can be enough to worsen physical health [5]. In this paper, we investigate the ways that communities behaviorally-cope with the trauma of a mass shooting, analyzing increases in the weekly sales of alcohol in affected communities up to two years after both mass shootings that took place both in public and shootings that were confined to private homes or residences.

How do mass shootings spread their traumatic effects? For a shooting to affect the well-being of the uninvolved, people need to know about it and fear that such violence indicates something about their community [1]. Many shootings go unreported on [6], and in those cases, the effects may only extend as far as those directly impacted. In their analyses of the effects of police killings, for example, [7] find that 82% of the 292 police killings in Los Angeles County, California between 2002-2010 went unreported-on in local newspapers. For the killings that were unreported, electoral turnout increased only across the neighboring blocks, while for the 18% of killings that did get coverage, the civic effects extended throughout the entire community. Research has shown that the impact of the 2007 Virginia Tech shooting was felt most strongly by students on an uninvolved campus who were paying the most attention to media coverage (while students who were paying next-to-no attention were far less likely to report symptoms of psychological distress, [8]); and that, in the aftermath of the Boston Marathon bombing, increased consumption of bombing-related media predicted individual post-traumatic stress up to six months later, which then made them especially vulnerable to stress after learning about the Pulse nightclub shooting a full three years later. [9].

An especially-potent demonstration of the effects of knowledge on transmitting trauma comes from analyses of the economic effects of mass shootings. Residents of counties that experience mass shootings are more likely to view the local economy more pessimistically, and that, combined with a worsening of mental health in these counties, predicts decreased economic activity (lowered wages and higher unemployment). Critically, researchers demonstrate that the effect is enhanced by media coverage of the shooting - when an unrelated newsworthy event happens to cooccur with the shooting, therefore diminishing total coverage of the shooting, the economic effects of the shooting are more limited [10]. Similarly, heavy media coverage is a significant predictor of whether or not gun sales increase in the aftermath of mass shootings [11,12].

In the absence of media coverage, simple proximity seems to be a powerful predictor of who will be affected by a shooting. The vicarious effects of an American shooting seem to be fairly-localized - strongest in the immediate vicinity of the shooting and fading as one gets farther away. For example, one analysis of the emotional response to a shooting found the strongest effects on those living in the same ZIP-code, weaker effects for those living in the same city, and the weakest effects for those living in the same state [13]. In research on the aftermath of school shootings, one study in California found that hospitals within five miles of the school saw increased stress-related visits while those 10-15 miles away were unaffected [14]; similarly, researchers have documented increased youth antidepressant prescribing in the aftermath of American school shootings, but mainly within five miles of the school [15]. In the analysis of one particularly-traumatic mass-shooting event, the October 2002 DC sniper killings, students who lived within five miles of an attack did worse on year-end exams than those who lived in the same region but farther away from the attacks [16]. Outside the US, researchers have found similarly place-restricted effects of local violence on school performance in São Paulo [17] and Rio de Janiero, Brazil [18]. Similar hyperlocal analyses of police-involved killings show that when police kill a Black or Hispanic victim, Black or Hispanic students living close-by (less than half a mile from the site of the shooting) have lower GPAs and have increased prevalence of PTSD symptoms [19], whereas those living farther away are generally unaffected.

The duration of the effects of a mass shooting may vary. For those only casually-connected, the effects may fade fairly-quickly - when looking across an entire city, for example, increased feelings of sadness or decreased feelings of happiness in the aftermath of mass shootings seems to dissipate after a week or so ([13]; though see [20] who find evidence for effects on community well-being three months later). However, when looking at the effects on those more tightly-connected, such as students enrolled at school when a shooting occurred, the traumatic effects can last a lifetime, with researchers documenting lower educational attainment and early-career earnings among students directly exposed to a shooting ([21]; see [17] for similar findings from Brazil); worse self-reported health and well-being, as well as increases in risky levels of alcohol consumption as adults [22]; and, among those students who survived the 1999 Columbine shootings, increased likeliness of death before age 30, especially death by suicide [23]. Research into the aftereffects of the 2011 attack in Utøya, Norway found similarly that students who survived, as well as their parents, are still feeling the physical and mental health effects a decade later [24]. For those in the community, researchers have documented increases in stress-related emergency-room by people living in the vicinity of school shootings persisting for at least one additional year [14]; increases in youth antidepressant prescriptions by those living in the vicinity of a school shooting that persist for at least two years [15]; and decreased economic activity in a county lasting at least 3-5 years [10].

In the present analysis, we look at a behavioral response to the community stress engendered by a mass shooting, specifically investigating increases in the local sales of alcohol. Behavioral responses have the benefit of allowing researchers to investigate responses beyond those people that can be surveyed, and have a trace that can be recaptured from relevant archives well after the behavior has been performed. In their broad coverage and historical depth, comprehensive behavioral data are an ideal repository for studying the widespread effects of events on people's everyday lives. Alcohol consumption is a well-studied coping mechanism for stressful situations (see [25, 26] for reviews) and those who have survived disasters, including the survivors of mass and school shootings are especially susceptible to alcohol use disorders [22, 27]. At the community level, changes in alcohol sales and deaths by alcoholism have been linked to levels of anxiety and stress [28-30], as well as levels of perceived disorder, lower rates of social capital and community support, and lack of personal safety (see [31] for a review). A population traumatized by a mass shooting, would, then, be expected to increase their levels of alcohol consumption in order to manage their sense of ill-being.

We look at the effects of a particular class of shootings - mass shootings, or intentional homicides with 4 or more victims within a 24-hour period. These events are disproportionately covered by the media and therefore loom disproportionately-large in the American imagination (see [6, 32, 33]; and see [34] for analyses of social media discourse). In our analyses, we further differentiate between public and non-public shootings, as shootings that take place in public settings receive far more media coverage than the more prevalent mass killings that take place in private homes and residences [35], and, perhaps consequently, are central to the American conception of what a mass shooting entails (e.g., [36]). Researchers have argued that public shootings create the sense that "that could have been me," whereas private killings do not create that same sense of vulnerability [37-38]. In 2019, for example, 79% of Americans reported experiencing stress as a result of the possibility of a mass shooting - however, they tended to worry about being in public spaces and contemplated changing the places they go (i.e., minimizing the potential of a public mass shooting) not increasing their suspicion of family or loved ones [39]. By distinguishing between public and private mass shootings, therefore, we can capture both the impact of multiple firearm-related deaths in a community and the impact of realization that one's community is not safe from public mass violence.

Our mass-shooting data comes from the USA TODAY/AP/Northeastern University mass killing database [40], covering the years 2006-2019. Using the FBI's definition of a 'mass killing' as four or more intentional homicides (excluding the offender) within a 24-hour period, the database cross-references FBI data with news reports, court documents, and law enforcement records to compile a validated dataset of American mass killings. Of the 326 mass shootings in the data (with an average of 5.48 people killed in each shooting), we identified 64.7% (211 in total) as taking place in private: these were killings coded by the compilers as taking place in a residence or other shelter. We identified 35.0% of the mass shootings (114 total) as taking place in public: these were killings coded by the compilers as taking place either in a commercial/retail/entertainment venue (40 killings), in open space (17 killings), in a vehicle (11 killings), at a school or college (10 killings), within a government/transit location (8 killings), in a house of worship (6 killings), or at a medical facility (1 killing). If a shooting was coded as taking place in more than one location, we also coded it as a public shooting (22 killings). One shooting was given no location, so it was dropped from the dataset.

We match this shooting data with large-scale data on alcohol purchases from the Nielsen Retail Scanner database covering the same time-period. This data consists of point-of-sale purchase information from over 90 major American retail chains (i.e., grocery stores, mass-merchandizers, drugstores, etc.). With data from over 35,000 individual retailers, the dataset contains more than half of all American grocery and drug-store purchases. Using UPC data, we select out all alcohol sales, and additionally subdivide purchases into separate aggregates of beer sales, wine sales, and liquor sales. Nielsen estimates that their data captures 73.5% of all off-premise beer sales in the United States, as well as 66.8% of all off-premise wine sales, and 45.6% of all off-premise liquor sales [41].

We aggregated the data, geographically, at the ZIP-3 level, the smallest geographic area that Nielsen provides. A ZIP-3 comprises all ZIP codes that share the same first three digits and are therefore served by the same US Postal Service sectional center facility - a contiguous area larger than a neighborhood but smaller than a city. In the US, there are 923 ZIP-3 geographic areas, with an average area of 8,330.68 km^2 (*SD* = 14,065.12), containing an average of 346,509.50 people (*SD* = 372,965.20). We aggregated the sales of alcohol within a ZIP-3 by week, creating a measure of total sales within a ZIP-3 (in dollars) for each week of the dataset. In our data, a ZIP-3 spends an average of $325,176/week on alcohol (*SD* = 571,870.40).

To analyze the impact of mass shootings on local alcohol sales, we take a modified difference-in-difference approach, examining whether alcohol sales increase in ZIP-3s that have recently experienced a mass shooting, as compared both to the sales of alcohol in the ZIP-3 before the shooting and contemporaneous ZIP-3s that did not experience a mass shooting. Researchers using this methodology have become increasingly-aware of potential issues with using misspecified difference-in-difference approaches, and there have been a slew of new, more robust, statistical estimation strategies for determining the real-world effects of treatments that vary both by time and by place (see [42]). In interpreting treatment effects from such models, it is vital to establish the parallel-trends assumption: whether treated areas differ from non-treated areas before the treatment is instigated - if the parallel-trends assumption is violated and the treated and untreated areas are diverging before the treatment, it becomes far harder to assert that the treatment itself is the causal factor in the observed change (e.g. [43]). Models may also provide misleading estimates where treatment effects are heterogeneous - classic designs assume that effects are constant across treated units at each time, and when that assumption is violated, the average treatment effect from a two-way fixed effect model may be unreliable [44].

To test for, and address these two issues, we use a two-way fixed-effects counterfactual approach [45], using both ZIP-3 and week fixed effects. In these models, data for the treated units are imputed, given models trained on the rest of the dataset (i.e., the non-treated units), and then the imputed values are compared against the true values to compute an average treatment effect. This approach allows for treatments to switch ‘on’ and ‘off,’ and in addition, these estimators allow for extensions that take into account the possibility of heterogeneous treatment effects and unobserved time-varying confounders, by using either a factor-augmented interactive fixed effects model [46], or a matrix-completion estimator [47].

In testing for the validity of the parallel trends assumption, this approach provides two diagnostic tools. One approach uses equivalence tests to establish whether the average treatment effect for each pretreated unit/time is smaller than a pre-specified value: if the pretreatment effect is shown to be within this bound (i.e., equivalent to zero) for the entire pretreatment range, a researcher can reasonably conclude that the trends for the treated and untreated units are indeed parallel. Additionally, one can run a placebo test, where a window of data from the treated units before the treatment is held out, and then the model-imputed values for the hidden period (again, using a model trained on untreated units) are compared against the actual values. If the model-imputed values for the treated units are a good match, then there is unlikely to be a notable change in trend-form of treated units in the periods running up to the treatment, and therefore the parallel-trends assumption likely holds.

A similar counterfactual approach can be conducted at the end of a treatment period to test whether effects plausibly carryover - if held-out posttreatment values are well-predicted by untreated units, there is unlikely to be an effect lasting longer than the treatment itself; whereas if the untreated units do not well-predict the post-treatment units, there is evidence for significant carryover after the treatment has expired. To test for carryover effects in our data, we first estimate the effect for just the first year after the offset of the treatment, then for just the second year after offset, then just for the third year after offset, then just for the fourth year after offset. Significant carryover effects in consecutive periods would suggest a continuation of the mass-shooting effect lasting for a substantial additional duration. We limit our test of carryovers to four years due to the window of data with which we have to work - with just a ten-year window, we are limited in our ability to model time effects for longer durations.

This approach to difference-in-difference models has been shown to be appropriate for situations such as the present analysis where there is variation in treatment timing across units (i.e. shootings happen in different ZIP-3s at different times) and where there can potentially be multiple treatments in the same unit (i.e. ZIP-3s that can experience multiple mass-shootings over our ten-year period) - situations which traditional difference-in-difference approaches may potentially be biased [48-49]. Recent work, for example, has used this approach to model the impact of mass shootings on local electoral outcomes [42], the impact of mass shootings on the passage of firearms laws [50], and the impact of school shootings on NRA donations [51].

Results

Following prior research on the mental health effects of mass shootings [8,13], we examine the effects of a mass shooting in a community up to two years after the event. We begin by investigating the effects of all mass shootings on all weekly alcohol sales in a 'treated' ZIP-3.[1] According to a matrix-completion specification with lambda = 0.0133, a mass-shooting in a ZIP-3 increases the weekly sales of alcohol for the next two years by an average of $22,224.72 [183.26, 44,266.18], *SE* = 11,245.85, $p = .048$. This represents a 3.5% increase in sales as compared to the same units when untreated. Our model passes identifying assumptions, as we find strong evidence for parallel trends, equivalence $p < .001$; and we find no evidence for a placebo effect (investigating the week of the mass-shooting and the four weeks beforehand), $p = .49$. Analyses of carryover effects suggest the effect is present in the first year after the offset ($p = .005$), in the second year after the offset ($p = .007$), and in the third year after the offset ($p = .025$), meaning the increase in alcohol sales in the aftermath of a mass shooting is still detectable five full years after the incident itself. See Figure 1.

Our two-year effects appear to be driven largely by shootings that took place in public. Looking at the 121 shootings that occurred in public spaces (again, using a matrix-completion estimator with lambda = 0.0133), we find that a public mass-shooting in a ZIP-3 increases the weekly sales of alcohol by an average of $42,132.65 [2,288.72, 81,976.58], *SE* = 20,328.91, $p = .038$. This represents a 5.5% increase in sales as compared with the same units when untreated. We find strong evidence for parallel trends, equivalence $p < .001$; and no evidence for a placebo

[1] Our results are unchanged if we replace raw sales with sales per capita.

effect, $p = .26$. Carryover tests suggest that the effect may last for at least an additional year, carryover $p = .007$.

By contrast, when looking at the 211 shootings that took place in private settings or homes (using a matrix-completion estimator with lambda = 0.00562), we find no evidence for a significant change in weekly alcohol sales over the next two years, $p = .69$.

We broke down sales of alcohol separately into sales of beer, wine, and liquor, and we find that weekly sales of beer significantly increase in the aftermath of a mass shooting (average increase of $9,607.66 [1507.56, 17,707.77], *SE* = 4,132.78, $p = .020$). Effects are stronger when looking just at the aftermath of public mass shootings, where we find significant increases in the sales of beer (average weekly increase of $18,082.45 [2,300.40, 33,864.50], *SE* = 8,052.21, $p = .025$) and wine (average weekly increase of $22,311.36 [3,334.61, 41,288.12], *SE* = 9682.20, $p = .021$).

We find no evidence for changes in beer, wine, or liquor, specifically, in the aftermath of private mass shootings (all *p's* > .39). See Table 1 for parameter estimates for all models, including tests for parallel trends, placebo effects, and year-by-year carryover effects.

Discussion

Using a dataset tracking over half of all the supermarket and drugstore purchases made by Americans from 2006-2019, we find that a mass shooting in the community (i.e. shared ZIP-3) increases alcohol sales for the subsequent two years by an average of over $20,000/week. This increase is especially-concentrated in areas that experienced a public mass-shooting, where the average weekly increase in alcohol sales jumps by over $40,000/week over the next two years. This is a meaningful increase, representing between 3.5%-5.5% increase in weekly sales in these communities. We find evidence that the increases in weekly alcohol sales may continue for an additional three years after our two-year window, meaning that there are detectable increases in alcohol purchasing a full five years after a mass shooting event. By contrast, for mass-shootings that take place in private settings, we find no evidence for any change in alcohol sales. We suggest that the vicarious trauma caused by a mass shooting is therefore carried, at least in part, by the event's public nature, as such a public event may confirm that one's community is no longer a protective, safe place to live. When a mass killing occurs in private, it may not implicate the community in the same way. Not all mass death carries the same traumatic power.

With fewer than 350 shootings (and just over 100 public killings) we note that we are somewhat limited in the statistical power for any of our tests. We are in no way asking for greater observed power here (we, of course, would like the number of mass killings to be far lower), but the relatively small number of events does lead to imprecision in our parameter estimates, especially when estimating effects across smaller time-windows. We are therefore not fully-confident in using this data to determine how quickly a mass-shooting is translated into increased sales. We are additionally limited in disentangling the many different types of shootings - while we can broadly differentiate private shootings from public shootings, our sample size does not allow us to meaningfully-distinguish between the many different forms of public violence, nor does it allow us to understand meaningful variance in the locations attacked. These are distinctions that likely matter - an analysis of school shootings, for example, shows that not all school shootings have the same effect on students, and that treating indiscriminate shootings, suicides, personal attacks, and crime-related shootings as equivalent masks important heterogeneity in the ensuing traumatic effects [52]. Moreover, violence may have differential effects in different sorts of neighborhoods (e.g., [3]), and future work with more powerful designs will be vital in further understanding consequential differences between public attacks.

If anything, these findings likely underestimate the true effect. While our dataset contains a tremendously-large volume of retail transactions, we are still unable to model a significant fraction of alcohol sales. We are unable to glimpse alcohol sales at bodegas, independent liquor stores and wineshops, as well as other places where people consume alcohol, such as bars and restaurants. Nevertheless, we have no reason to believe that our observed proportion of sales is unrepresentative, or that people would shift their overall consumption patterns (shifting from supermarkets to bars, say) in the aftermath of a shooting. Indeed, we suspect that our sample helps to explain why we find stronger evidence for the increased sales of beer and wine than for increased sales of liquor in areas touched by a mass-shooting. Sales of liquor, and to a lesser extent, wine, are restricted to particular retailers in many states (see [53]) and therefore we are able to capture less of the total sales volume in our dataset: we can capture roughly 3/4 of all beer sold in stores, 2/3 of wine sales, but less than 1/2 of liquor sales. As we have less data on liquor and wine sales to work with, it becomes harder to detect changes.

We are also likely underestimating the true effect thanks to limitations in our ability to geographically-place our retailers. Due to data-use restrictions, we are only able to identify the ZIP-3 area of any of our retailers, meaning that while we know that a retailer is in the general vicinity of a mass shooting, we are unable to know anything more granular. Prior work suggests that the impact of mass shootings is felt most strongly by those directly nearby, and our data likely mixes in retailers that are close to the site of the mass shooting with retailers several miles away. This additional geographically-indeterminate noise may mask some of the underlying signal, and an analyst who was able to get ZIP-level data or closer may find even stronger effects[2].

Finally, while we know roughly *where* a transaction took place, we know nothing about *who* the purchaser was. Theory suggests that the impact of a mass shooting should be strongest on people who are closest to the event, either physically or in terms of their group identification, especially if a shooter targets a particular group. Research has shown, for example, that when police shoot an unarmed Black civilian, it worsens the mental health of Black residents of the state in which the shooting occurred, but there is no evidence for worsening mental health among White residents of the state [54]; and the mental health of gay men (but not heterosexual men), was worsened in the aftermath of the 2016 Pulse nightclub shooting [55]. Other examinations of the effects of shootings on community well-being similarly point to differential effects based on demographics [7,19] and political ideology [13]. This additional layer of noise suggests that the effects of mass shootings on alcohol purchasing may be even stronger if an analyst was able to take into account features of the purchaser such as whether they shared a racial or gender identity with the victims, their media diet, and their overall proximity to the shooting, not just the location of the store.

Increases in alcohol purchases are not just a sign of stress; they can also be a cause of stress. Increased community-level access to alcohol is associated with increased rates of community violence and stress (see [56-57] for reviews). Consumption of alcohol is durably associated with both firearm homicide [58] and suicide [59] as well increasing risk of victimization by firearm violence (see [60] for a review). By increasing the purchase of alcohol, a mass shooting can extend its traumatic impact far beyond the initial time horizon, potentially driving negative spirals of violence.

Mass shootings affect more than just the victims - they can alter the ways that the community at large comes to define itself and the ways that individuals think about how they are or are not protected by the institutions of the state (see, e.g., [61]). We show that simply having a mass shooting in one's community meaningfully-alters the ways that people go about their everyday lives, far into the future. In addressing the aftereffects of a mass shooting, therefore, it is incumbent on public health practitioners to address not just those directly-impacted (e.g. [62]), but work to mend the broader fabric that a shooting tears asunder. Preventing harm is often more effective than repairing community damage however, and reducing mass shootings through policies such as minimum-age requirements for purchasing a firearm, prohibitions on firearm ownership by domestic abusers, increased waiting periods for firearms purchase, and bans on

[2] In line with this focus on geographic specificity, previous researchers examining alcohol sales at the county level find no evidence for the effects of school shootings, specifically, on county-level purchases over the next three months [28].

high-capacity magazines (see [63] for a review) has the potential to disproportionately improve long-term community health.

Materials and Methods

USA TODAY/AP/Northeastern University Mass Killing Database

The database tracks all U.S. homicides since 2006 involving four or more people killed (not including the offender) over a short period of time (24 hours) regardless of weapon, location, victim-offender relationship or motive. Data are initially identified using the FBI's Supplemental Homicide Reports, and are then augmented with media accounts, court documents, academic journal articles, books, and local law enforcement records obtained through FOIA requests, as well as cross-compared against all other known mass-killing databases. See https://data.world/associatedpress/mass-killings-public for the full set and all documentation. For this project, we selected all mass shootings in the database, covering the years 2006-2019.

Nielsen Retail Scanner Database

The Retail Scanner Database consists of weekly pricing, volume, and store merchandising conditions generated by participating retail store point-of-sale systems at approximately 30,000-50,000 participating grocery, drug, mass merchandiser, and other stores representing approximately 90 retail chains across the United States. UPC codes are classified into product categories by Nielsen researchers. We use all sales categorized as purchases of beer, wine, or liquor, aggregated at the week and ZIP-3 level. See https://www.chicagobooth.edu/research/kilts/research-data/nielseniq for more information, including how to access the database.

Statistical Analyses

Our analyses use a two-way fixed-effects counterfactual trends (FECT) variant of a difference-in-difference estimator [45], examining whether alcohol sales increase in ZIP-3s that have recently experienced a mass shooting, as compared both to the sales of alcohol in the ZIP-3 before the shooting and contemporaneous ZIP-3s that did not experience a mass shooting. Using fixed effects for both geography (ZIP-3) and time (week), this approach starts by hiding the data from treated units during their treatment phase (i.e., data from the 104 weeks in a ZIP-3 after a mass shooting has occurred), and then models a response surface for all untreated unit-periods. Based on this response surface, counterfactual (imputed) values are then estimated for the treated unit-periods. Treatment effects are calculated by comparing actual values in the treated unit-periods against the model-generated counterfactuals, which are then averaged to generate average treatment effects.

All analyses were conducted using the *fect* package in R [45], with a model specified as *sales ~ indicator, index=(ZIP3, Week)*, where *indicator* takes a 1 if treated and a 0 if untreated, using both geographic and time-based fixed effects. Starting in the week after the shooting, we took the next 104 periods for the ZIP-3 area in which the shooting occurred (i.e. 104 weeks, or two years) as our treated area/times, leaving all other area/times as untreated. Standard errors were calculated with 1000 bootstrapped runs. The *FECT* approach uses one of three different possible algorithms for creating the response surface from which counterfactuals are derived: a straightforward fixed effects estimator; an interactive fixed-effects estimator, which uses factor-augmented models (a "hard impute") to handle unobserved time-varying confounders; and a matrix-completion estimator, which takes a matrix-imputation approach (a "soft impute") to handle unobserved time-varying confounders. To identify the correct estimator for their data, researchers can use a cross-validation procedure that compares the results of each estimator, attempting to maximize out-of-sample prediction performance by minimizing mean-squared prediction error. In our data, matrix-completion models provided the best fit for all models.

This approach provides two different tools to test whether the parallel-trends assumption is justified - a critical step in determining whether the proposed difference-in-difference is, in fact, causal, or instead whether it is epiphenomenal. If the parallel-trends assumption is upheld, it indicates that there are no notable differences in untreated and to-be-treated units in the pre-treatment period: that there's nothing happening before treatment that might instead be causing differences in the post-treatment period. If the parallel-trends assumption is violated, by contrast, there is evidence that, even before the treatment, the treated and to-be-treated units are already starting to diverge, suggesting that the treatment may itself not be causing the differences in the post-treatment units.

The first diagnostic test for the parallel-trends assumption is an equivalence test: investigating whether the imputed counterfactual treatment effect in the pretreatment period is statistically-equivalent to zero (i.e., whether imputed data for all to-be-treated units, for all periods before the treatment, differs from actual values). For equivalence tests of the parallel trends assumption, we use the package-default bounds of +/- .036 x the standard deviation of the residualized untreated outcome, meaning that any average treatment effect in the pretreated period between untreated and to-be-treated units smaller than this boundary is considered equivalent to no effect, thereby providing evidence that the parallel-trends assumption is not violated.

The second diagnostic test for the parallel-trends assumption looks at the immediate pre-treatment period to determine if, right before the treatment, there are any deviations between the treated and to-be-treated units. For these placebo tests, we investigate a period consisting of the week of the mass-shooting and the four weeks beforehand: if an average treatment effect is detected in this immediate pre-treatment window between untreated and to-be-treated units, then there is evidence that the parallel-trends assumption is violated.

In all our models, we find evidence in favor of equivalence (i.e., that our overall pre-treatment effects are statistically equivalent to zero) and we do not find evidence that our placebo period has treatment effects greater than zero, both suggesting that the parallel-trends assumption holds.

To identify the duration of our effects after the treatment period, we conduct a series of carryover tests, testing whether the imputed counterfactual effect is still present in periods after the offset of our treatment window. To gauge the magnitude of the carryover, we investigate the presence of an effect in four separate models: the 52 weeks after the offset of the treatment period, weeks 53-104 after the offset of the treatment period (i.e., the second year after offset), weeks 105-156 (i.e., the third year after offset), and weeks 157-208 (i.e., the fourth year after offset).

For annotated analysis and data-aggregation scripts, see https://osf.io/p83wb/?view_only=8b9685ca1beb4ef3a1e817fecd5050ab

<u>Ethics Approval</u>

As these analyses were conducted on pre-aggregated deidentified data, we did not seek oversight from an institutional review board.

<u>Data Availability</u>

The Mass Killing Database is freely accessible at https://data.world/associatedpress/mass-killings-public. The Nielsen Retail Scanner Database is proprietary - researchers should contact Nielsen for access at https://www.chicagobooth.edu/research/kilts/research-data/nielseniq

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Figures and Tables

Table 1. Parameter estimates for all models.

	Average Treatment Effect			Tests for Parallel Trends		Carryover Effects			
Alcohol	ATT [95% CI]	*SE*	*p*	Equivalence Test	Placebo Test	Year 1	Year 2	Year 3	Year 4
All Shootings									
All	$22,224.72 [183.25, 44,266.18]	11,245.85	0.048*	<.001***	0.489	0.005**	0.007**	0.025*	0.133
Beer	$9,607.67 [1,507.56, 17,707.77]	4,132.78	0.02*	<.001***	0.131	0.004**	0.008**	0.142	0.334
Wine	$6,972.15 [-837.10, 14,781.39]	3,984.38	0.08	<.001***	0.419	0.006**	0.002**	0.009**	0.067
Liquor	$5,266.67 [-1,702.78, 12,236.12]	3,555.90	0.139	<.001***	0.508	0.135	0.321	0.164	0.298
Public Shootings									
All	$42,132.65 [2,288.72, 81,976.58]	20,328.91	0.038*	<.001***	0.261	0.007**	0.051	0.161	0.111
Beer	$18,082.45 [2,300.40, 33,864.50]	8,052.21	0.025*	<.001***	0.104	0.019*	0.154	0.902	0.784
Wine	$22,311.36 [3,334.61, 41,288.12]	9,682.20	0.021*	<.001***	0.248	0.008**	0.01**	0.015*	0.014*
Liquor	$11,488.59 [-2,967.03, 25,944.21]	7,375.45	0.119	<.001***	0.841	0.022*	0.209	0.316	0.226
Private Shootings									
All	$6,815.49 [-26,803.06, 40,434.03]	17,152.63	0.691	<.001***	0.646	0.16	0.431	0.332	0.456
Beer	$4,224.56 [-5,450.84, 13,899.96]	4,936.52	0.392	<.001***	0.833	0.074	0.141	0.206	0.393
Wine	$2,299.48 [-6,095.89, 10,694.84]	4,283.43	0.591	<.001***	0.884	0.174	0.29	0.244	0.301

Liquor	$1,818.48 [-4,787.20, 8,424.15]	3,370.30	0.59	<.001***	0.282	0.772	0.606	0.915	0.949

Note: Equivalence Test investigates whether the pre-shooting trend is equivalent to zero (within the bounds of +/- .036 x the standard deviation of the residualized untreated outcome). Placebo Test investigates whether there is a significant treatment effect in the run-up to the treatment: the week of the shooting plus the preceding four weeks; Carryover Tests investigate whether there is a significant treatment effect in the listed year after the two-year window expires. ATT = Average Treatment Effect; 95% CI = 95% confidence interval; *SE* = standard error; ***$p < .001$, **$p < .01$, *$p < .05$.

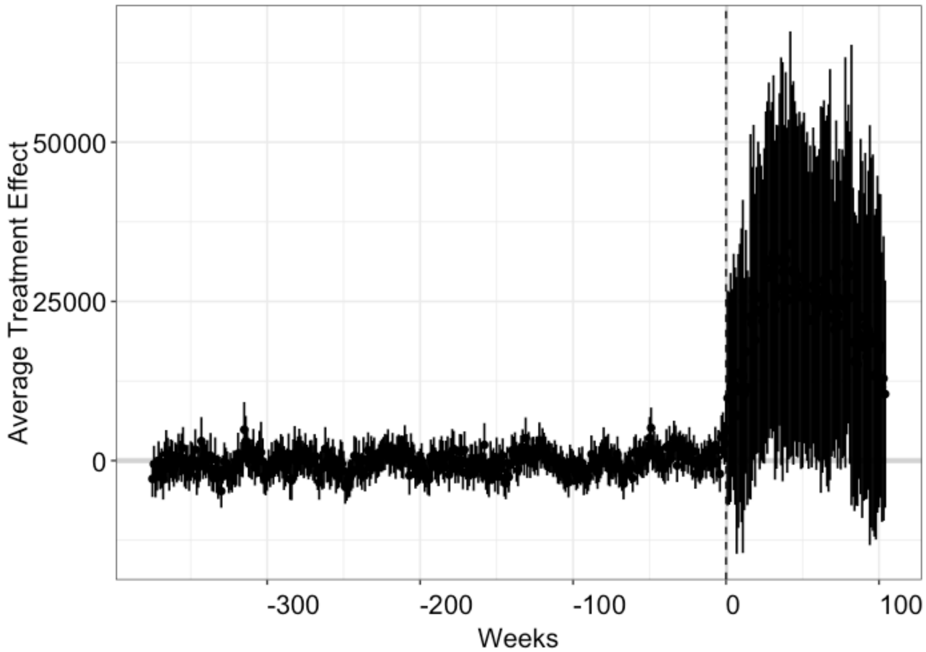

Figure 1. Average treatment effect for the effect of a mass shooting on sales of alcohol. Dots indicate point estimates for each week and lines indicate 95% confidence intervals. The dashed line indicates the week of the mass shooting.