Friends and Firearms: Social Ties and Gun Demand After Distant Mass Shootings

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Abstract

Gun ownership is central to debates over household security and public safety, yet research on its determinants remains limited. This paper examines how social networks shape the decision to own a firearm. Using gun-store visits as a proxy for gun demand and Facebook social connectedness to measure network ties, we show that local gun demand significantly increases after a mass shooting in socially connected but geographically distant communities. Specifically, gun-store visits in counties that are more socially connected to shooting locations rise by 1.0–1.9% after distant shootings, with the effect that peaks around week five before decaying. Mechanism analysis indicates that the effect operates through an information-salience channel: socially connected areas exhibit larger increases in attention to shootings and react more strongly in gun-store visits when the focal areas have no prior shooting events. Finally, heterogeneity tests reveal a stronger effect in states with looser gun-related control policies and in counties with more women, older residents, families with children, and Democrats. These findings highlight how gun ownership decisions are shaped through social networks in response to distant traumatic events.

JEL classification: D83, I12, K42, Z13
Keywords: Gun ownership, Firearm demand, Social networks, Distant mass shootings, Information salience

"A well regulated Militia, being necessary to the security of a free State, the right of the people to keep and bear Arms, shall not be infringed."

— U.S. Constitution, Second Amendment

1 Introduction

As enshrined in the Second Amendment of the U.S. Constitution, the right to keep and bear arms is a fundamental right secured by the constitution. Yet gun ownership is not merely a constitutional symbol, it is also central to debates over household security and public safety. Higher gun prevalence has been linked to higher homicide rates and other social costs (Duggan, 2001; Cook and Ludwig, 2006; Levine and McKnight, 2017; Chalak et al., 2022), underscoring the importance of understanding the determinants of firearm acquisition. While previous research has examined demographic, cultural, and policy determinants of gun ownership (Koenig and Schindler, 2023; Armona and Rosenberg, 2024; Rosenberg, 2024*a*; Moshary et al., 2025; Alsan, Schwartzstein and Stantcheva, 2025), the role of social ties and the extent to which friends shape decisions to purchase firearms remain largely unexplored.

This paper addresses that gap by quantifying the impact of social networks on gun demand. To identify causal effects, we exploit distant mass shootings as an exogenous shock to the perceived risk that can be transmitted through social networks. Our analysis focuses on two key questions: (i) Is household gun demand influenced when socially connected friends experience a mass shooting? (ii) If so, through which mechanisms does this response occur?

To answer these questions, we face two key empirical challenges: data availability and causal identification. We begin with data limitations, which arise for two main reasons. First, measuring social networks is inherently difficult due to the lack of large-scale, representative data on social connectedness across individuals or regions. We address this by leveraging the Social Connectedness Index (SCI), a widely used measure based on anonymized friendship links among U.S. Facebook users (Bailey et al., 2018*a*). Second, granular and high-frequency data on gun purchases are scarce. Prior research typically relies on background check data, which, while informative, are only available at the state-month level and obscure important spatial and temporal variation.[1] To overcome this limitation, we leverage cellphone mobility data from Advan Research Inc. and construct the weekly county-level counts of foot traffic to gun retailers as a proxy for gun purchasing activities.[2]

Beyond data constraints, another key empirical challenge is causal identification. The intensity of friendship links tends to decline with geographic distance, so households in counties that are highly socially connected to a shooting location may also be geographically close and thus subject to similar local shocks. In that case, observed changes in gun demand could reflect shared geographic factors rather than social interactions. To mitigate this concern, we focus on variation in social connectedness among counties that are geographically distant (that is, 500 km, 750 km, or 1000 km) from the shooting location and include focal-county-by-event fixed effects to directly control for geographic distance to the incident, as well as state-by-week fixed effects so that we compare counties within the same state and same week that differ only in their degree of social connectedness to the mass-shooting location. This approach allows us to more credibly isolate the impact of social networks on household gun demand.

In this paper, we leverage mass shooting events from USA Today's database over the period 2021–2024. And we stack individual event studies with respect to every mass shooting event and estimate a difference-in-differences design that compares counties with above- versus below-median SCI ties to the shooting county within the same state, which teases out the fixed differences, such as the inherent local risk, as well as the common time-varying confounding factors, such as the media coverage, for all counties. Our findings show that households in counties more socially connected to the shooting location experience a statistically and economically significant increase in gun store visits in the aftermath of mass

[1] NICS background checks data are available at https://github.com/BuzzFeedNews/nics-firearm-background-checks.

[2] We validate this measure later by showing a strong positive correlation between aggregated gun-store visits and NICS background checks at the state-month level.

shootings, despite being geographically distant from the incident. Counties above the within-state median in SCI to the shooting county exhibit an increase of about 1–1.9 percent in visits to gun retailers in the weeks following the event. Event-study estimates show no pre-treatment differences, which validates parallel trend assumption. The effect peaks around five weeks post-shooting before gradually returning to baseline, consistent with a Bayesian learning model that allows for forgetting information about past events.

We perform several additional analyses to test the robustness of the main findings and ensure the results are not driven by other confounding factors. First, there is a concern that counties that are more socially connected to the shooting locations contemporaneously experience a shooting event while the less-connected counties do not. We conduct a balance check to test whether there is any difference in mass shooting frequency between the more- and less-connected counties, before and after the distant shooting shock. Second, to further rule out omitted variables that may drive our results, we conduct falsification tests by randomly assigning the treatment status to counties. Third, we implement propensity score matching to construct a matched DID analysis, enhancing comparability between treated and control counties. Fourth, we demonstrate whether the results are robust to alternative specifications of dependent and independent variables. Our results are insensitive to these extra exercises.

Next, we investigate the mechanisms through which social ties to geographically distant friends exposed to mass shootings shape households' decisions to acquire firearms. We posit that this effect operates through an information salience channel. When friends in the incident county share experiences, socially connected households update beliefs about the frequency of such events, as well as their own victimization risk and the value of self-protection, thereby increasing demand for firearms, as reflected in more visits to firearm retailers. Supporting this interpretation, we find Google searches for gun-shooting and firearm-related keywords rise dramatically in the counties that are more socially connected to the shooting location, indicating the attention to shooting increases when their far-away friends experience mass shooting events. Besides, consistent with the argument that a socially connected far-away shooting event draws the attention and increases the salience of personal victimization risk to them, we find larger effects for households in counties with no prior shooting events.

We also implement several heterogeneity tests to find for which kinds of neighborhoods the distant shooting have a more pronounced effect on the local visits to gun stores. We first document that stronger effects exist in states with looser gun-related control policies. Our heterogeneity tests further reveal that counties with more women, more minorities, and more households with children have a stronger response when their geographically distant friends are exposed to shooting events, which confirms our hypothesis that the information salience drive the increases in gun demand because those groups are more sensitive and feel less safe when they are aware of gun shootings in their social networks.

This paper makes several contributions. First, we provide new evidence on the determinants of gun ownership, a topic of central importance in public policy and economics. While prior work has examined demographic, political, and economic drivers of firearm demand (Koenig and Schindler, 2023; Armona and Rosenberg, 2024; Moshary et al., 2025; Alsan, Schwartzstein and Stantcheva, 2025), we extend this strand of literature and show that social networks, through the transmission of information and perceived risk, play a critical role in shaping household gun purchase decisions.

Second, we contribute to the literature on social connections and behavioral responses. Prior research has shown that social interactions influence a wide range of behaviors, including crime and migration dynamics (Stuart and Taylor, 2021*a*,*b*), and that online network structures can shape collective movements (Müller and Schwarz, 2021; Flückiger and Ludwig, 2025). A growing body of work documents how social networks transmit information and beliefs across regions, affecting economic and risk-related decisions. For example, friends' experiences with housing price growth influence home-buying decisions (Bailey et al., 2018*b*) and consumption patterns (Makridis, 2019); exposure to natural disasters increases demand for flood insurance (Ratnadiwakara, 2021; Hu, 2022) and alters deposit behavior (Flynn and Wang, 2025); COVID-19 experiences shape mobility (Bailey et al., 2024) and preventive behaviors (Hou, Liang and Chen, 2024); policy shocks such as state EITC implementation affect local take-up (Wilson, 2022); and crises like the Flint water contamination increase bottled water demand (Khanal, 2021). Our findings contribute to this literature by showing that information about mass shootings propagates through social networks and, in turn, significantly influences gun purchase decisions among households that do not experience the shootings directly.

Third, we contribute to the literature on the effects of mass shootings. Existing studies primarily document local responses to mass shootings, including changes in local economy (Brodeur and Yousaf, 2025; Gayán-Navarro and Sanso-Navarro, 2025), property values (Munoz-Morales and Singh, 2023; Gill and John, 2025), consumption (Shaik et al., 2024; Chiong, Kim and Kim, 2025; Cuellar and Jung, 2025), human capital (Bharadwaj et al., 2021; Cabral et al., 2025; Deb and Gangaram, 2024), crime (Gunadi, 2021), municipal bond yields (Chordia, Jeung and Pati, 2022) and gun policy (Luca, Malhotra and Poliquin, 2020; Reny et al., 2023). In contrast, we document a non-local effect of mass shootings and examine how households in socially connected yet geographically distant communities respond, leveraging a measured network of friendship ties.

Lastly, we also address a key data limitation in research on gun ownership. Existing studies lack precise measures of individual gun acquisition and typically rely on background checks from the National Instant Criminal Background Check System (NICS) as a proxy (Koenig and Schindler, 2023; Kim and Chen, 2025; Bollman et al., 2025). However, NICS data are available only at the state–month level and offer limited spatial and temporal granularity. We introduce a novel, high-frequency proxy, that is, weekly foot traffic to gun stores, constructed from anonymized cellphone mobility data with exact geographic coordinates. We validate this measure by documenting a strong correlation between gun-store visits and NICS background-check volumes. We advocate the use of this new measure in future research on household gun-demand behavior.

Our results also have important policy implications. If gun demand is amplified through social networks following mass shootings, then stricter regulations such as waiting periods or enhanced background checks may help dampen these surges. We find patterns consistent with prior evidence on the effectiveness of gun regulations in reducing firearm prevalence and violence (Kim and Chen, 2025; Bollman et al., 2025; Donohue et al., 2025; Arnold and Priestley, 2025; Rosenberg, 2024*b*; Edwards et al., 2018; Luca, Malhotra and Poliquin, 2017; Colmer and Doleac, 2023). By uncovering a social propagation channel for firearm demand, our findings contribute to ongoing debates on the design and effectiveness of regulatory interventions aimed at reducing gun-related externalities.

The remainder of this paper is organized as follows. Section 2 provides institutional background. Section 3 details the data sources and research design. Section 4 presents the main results, robustness checks, mechanisms and heterogeneity tests. Section 5 concludes.

2 Background

2.1 Gun ownership and gun violence in the U.S.

The United States exhibits an unparalleled level of civilian firearm ownership. Recent surveys indicate that in 2023, roughly one-third of U.S. adults personally own firearms, and the civilian stock is about 393 million guns, exceeding the nation's population, according to Pew Research Center and Business Insider (Panel a of Figure A1 illustrates state-level gun ownership rate).[3] Gun purchasing activity spiked during the COVID-19 period. The Guardian estimates 7.5 million first-time buyers entered the market from 2019 to early 2021, with annual sales rising from 13.8 million in 2019 to 16.6 million in 2020 and bringing millions of additional individuals, including roughly 5 million children, into gun-owning households.[4]

[3] https://www.pewresearch.org/short-reads/2024/07/24/key-facts-about-americans-and-guns/; https://www.businessinsider.com/us-20-million-ar-15-style-rifles-in-circulation-2022-5

[4] https://www.theguardian.com/us-news/2021/dec/20/us-gun-purchases-2020-2021-study

The widespread prevalence of firearms has significant implications for public safety and economic outcomes. Measures of gun violence worsened over the same period according to Pew Research Center.[5] The total number of gun-related deaths hit 48,830 in 2021 (up from roughly 39,000 annual gun deaths in 2019), reaching the highest on record. This record remained around 48,000 in 2022 before slightly declining to 46,728 in 2023 (See Panel b of Figure A1 for state-level distribution). The spike in gun-related deaths during 2020–2021 was especially pronounced for homicides: gun murder incidents rose from roughly 16,000 in 2019 to over 20,000 in 2021, then fell below the 2021 peak by 2023 but remained elevated relative to pre-2020 levels. Gun violence has imposed a profound economic burden. The estimated annual cost of gun violence reaches $557 billion, equivalent to 2.6% of GDP.[6]

[Insert Figure A1 here]

2.2 Measurement of gun demand

The institutional environment in the U.S. creates a clear measurement challenge for gun demand. U.S. law prohibits a centralized registry of firearm ownership, and detailed transaction-level sales records are not publicly accessible. Consequently, researchers cannot directly observe gun demand at the state or national level. To approximate demand, the literature typically relies on the National Instant Criminal Background Check System (NICS), established by the Brady Handgun Violence Prevention Act of 1993 (FBI, 2016). Retailers are required to query NICS before completing most legal firearm transfers, and the FBI releases monthly counts of these checks by state. Because a background check is a necessary step for many retail purchases, NICS volumes have become a standard proxy for firearm demand and ownership flows. Nonetheless, NICS has well-known limitations: the data are relatively coarse in time and geography (state-by-month), a single check can cover multiple firearms, and some checks correspond to permits or other administrative processes

[5]For a detailed discussion, see Pew Research Center (2021): What we know about the increase in U.S. murders in 2020.

[6]Everytown Research: Economic Cost of Gun Violence

rather than purchases.

We address these limitations by introducing a new, high-frequency and spatially granular measure of gun demand based on anonymized smartphone location data. Specifically, we construct weekly foot-traffic to firearm retailers using device-location pings matched to exact store coordinates. This measure is intuitive, easy to implement, and captures fine-grained variation. We do not claim to observe purchases directly but instead, we use store visits as a revealed-preference indicator of acquisition intent. Validation exercises show that our visit measure is highly correlated with NICS background-check volumes, supporting its use as a complementary proxy for household gun demand that offers substantially greater temporal and geographic granularity than existing approaches.

2.3 Definition of mass shootings

There is no a single, universally accepted definition of a "mass shooting." Different organizations employ distinct thresholds, reflecting varying emphases on lethality, public setting, and social meaning. The Gun Violence Archive (GVA) defines a mass shooting as an incident in which four or more individuals are injured or killed excluding the perpetrator. This definition casts a wide net, capturing a large number of incidents, including those with multiple injuries but no fatalities. By contrast, the USA Today database requires at least four fatalities within a 24-hour period, producing a narrower but more severe sample.

In this paper, we adopt the definition from USA Today. This choice aligns closely with the convention Krouse and Richardson (2015) and with the FBI's long-standing "mass murder" construct, that is, four or more victims killed during one incident, typically at a single location (Morton and Hilts, 2008).

3 Data and Research Design

3.1 Data

3.1.1 Mass Shootings

We obtain a comprehensive list of mass shooting events from USA Today's database for the period 2021–2024. We begin the sample in 2021 to focus on the post-COVID period and mitigate confounding effects of the pandemic. The database provides rich incident-level information, including exact location, victim counts (injured or killed), shooter and victim demographics, weapons used, and geographic coordinates that allow precise mapping to counties.

Our inclusion criteria of mass shooting events in our study are as follows: (i) we retain incidents coded as "Shooting"; (ii) we exclude events labeled as "Gang conflict", "Drug trade", "Profit", and "Family issue" as these categories are less likely to propagate through broader social networks; and (iii) within each county, if multiple events occur within a six-month window, we keep only the earliest event in that window to avoid overlapping exposure.[7]

After applying our sample restrictions, we identify a total of 95 mass-shooting events. Figure 1 presents the timeline and geographic distribution of these events, highlighting both the temporal clustering of incidents and the broad coverage across U.S. counties. Furthermore, Panel (a) and (b) of Figure 2 illustrate the spatial and state-level distribution of mass shootings in our sample: Panel (a) shows the geographic spread of mass shootings and it highlights clusters in densely populated and urbanized areas; Panel (b) presents the aggregate number of incidents by state and it indicate substantial variation across states.

[Insert Figure 1 here]

[7] For robustness, we test alternative definitions by extending the window to twelve months or by keeping only the first event ever recorded in each county. See Appendix Table A2 and Table A3.

3.1.2 Gun Stores and Visits

We use point-of-interest (POI) data from the SafeGraph Global Places dataset to identify gun stores across the United States. The SafeGraph Global Places dataset offers detailed attributes for each POI, including place name, street address, lat/long coordinates, NAICS code and category tags. Each POI is associated with a unique identifier, PLACEKEY, which is the persistent ID tied to this POI. For the purposes of this study, establishments whose category tags include "Guns and Ammo" are classified as gun stores, yielding 7,528 stores in our sample. These gun stores are illustrated in Panel (c) and (d) of Figure 2, both geographically and by state totals. The figures highlight the widespread accessibility of gun stores nationwide, with notable variation in density across regions.

Moreover, we collect foot-traffic data from Advan Research, which contains aggregated counts of weekly visits to POIs from a panel of mobile devices. The PLACEKEY for each POI is also provided in the Advan Research dataset. We then match weekly foot-traffic data from Advan Research to gun stores identified in SafeGraph Global Places based on PLACEKEY, resulting in 1,479,728 gun store–week visit records from 2021 to 2024. Finally, we map the stores' coordinates to county FIPS codes and aggregate visits across all stores within each county and week to construct a county-week measure of gun-store activity, as our proxy for local gun demand.

To validate that our aggregated foot-traffic counts provide a reasonable proxy for gun demand, we benchmark them against the standard measure used in the literature, state-level NICS background checks. Figure A2 plots the binned scatterplots of the natural logarithm of monthly gun store visits against the natural logarithm of monthly background checks at the state level. The two series are strongly positively related, with a significant coefficient of 0.725. This strong association indicates that variation in our visit measure closely mirrors variation in background checks, supporting the use of aggregated gun-store foot traffic as a valid proxy for household gun demand.

3.1.3 Social Connectedness Index

Social networks are measured by the Facebook Social Connectedness Index. Leveraging Facebook's active users' data in the United States, the index measures how frequently Facebook friendship links occur between each pair of locations and provides the first comprehensive, nationally representative measure of friendship networks (Bailey et al., 2018*a*).[8] The SCI is widely used in academic research on social networks (Khanal, 2021; Hu, 2022; Wilson, 2022; Bailey et al., 2024).

In our empirical design, we utilize the within-state comparison and classify the counties' SCI values with the shooting county above the within-state median as the *treated* group, while those below the median serve as the *control* group for each shooting event.

3.1.4 Additional Dataset

We draw county-level demographic characteristics from the American Community Survey (ACS) 5-year estimates, including total population, income, unemployment rate, educational attainment, age structure, and racial composition, etc. We incorporate a measure of local partisan index that is derived from ICPSR County Presidential Election Returns. By combining demographic indicators and partisan measures with our main dataset, we can examine heterogeneous treatment effects across different neighborhoods. We also incorporate weekly DMA-level Google Search Index data for gun-related keywords.

[Insert Figure 2 here]

3.2 Research Design

We leverage distant mass shootings as an exogenous shock to the perceived risk that can be transmitted through social networks.[9] We illustrate our identification strategy with a

[8] The SCI data are publicly available at: `https://data.humdata.org/dataset/social-connectedness-index`. We utilize the US counties to US counties Facebook Social Connectedness Index.

[9]

concrete example. Figure 3 depicts the March 22, 2021 mass shooting in Boulder County, Colorado (FIPS 08013), where a gunman opened fire at a King Soopers supermarket, killing ten people. We leverage Facebook's Social Connectedness Index (SCI) to distinguish counties that are more versus less socially tied to Boulder County. To ensure that the estimated effect of distant mass shootings reflects social-network exposure rather than common geographic shocks, we exclude counties located within 500 km of the incident.[10]

Our research design highlights the key source of identifying variation: conditional on geography and event-specific shocks, socially connected counties are more exposed to distant shootings through interpersonal networks rather than shared local shocks. Comparing their behavioral responses to those of less-connected counties within the same state allows us to estimate the causal effect of social exposure, rather than spatial spillovers or nationwide trends.

[Insert Figure 3 here]

We stack individual event studies with respect to every shooting event and estimate the causal effect of social exposure to a distant mass–shooting event on gun–store visits using a difference-in-differences (DiD) design. For every individual event in our study, we explore the effect twelve weeks before and twelve weeks after the relevant shooting date. The twelve-week event window is based on the dynamic impact of friend-linked mass shootings on local gun-store visits, as shown in Figure 4 and Figure 5 The effect peaks at five weeks post-shooting before attenuating, and then steadily declines to the baseline in twelve weeks. As such, the estimation on the twelve-week event window provides a conservative test of our hypothesis.

We adopt Poisson pseudo-maximum likelihood (PPML) as our baseline estimator as the dependent variables are skewed and could take the value of zero. Cohn, Liu and Wardlaw (2022) and Chen and Roth (2024) show that taking the natural logarithm of one plus the dependent variable when the dependent variable contains the zero value could lead to severe

[10] In robustness checks, we vary this exclusion threshold and find that the choice of distance does not materially affect our estimates.

bias and both recommend using the Poisson fixed effect model to mitigate the bias.

For each event e, our baseline PPML specification is

$$\mathbb{E}[Y_{iet}] = \exp\Big\{\alpha + \beta\big(\text{Treat}_{ie} \times \text{Post}_{et}\big) + \delta_{i\times e} + \lambda_{t\times e}\Big\}, \tag{1}$$

where $E[\ ]$ is the expectation operator, Y_{iet} denotes the number of gun-store visits in county i and week t with respect to an event e. Treat_{ie} equals one if the county i's SCI with the shooting county associated with a shooting event e is above the within-state median and zero otherwise. Post_{et} is an indicator that takes one if the week t is after the shooting week of an event e. County-by-event fixed effects $\delta_{i\times e}$ absorb all time-invariant county heterogeneity within each stack or each event e, and week-by-event fixed effects $\lambda_{t\times e}$ control for the common event-specific shocks to all counties in that stack. Standard errors are clustered at the county-by-event level ($i \times e$), allowing arbitrary serial correlation within each stacked panel. Under PPML, $\hat{\beta}$ admits a multiplicative interpretation: $100 \times (\exp\{\hat{\beta}\} - 1)\%$ is the change in gun-store visits for treated relative to control counties before versus after the distant mass shootings.

To assess pre-trends and dynamics, we estimate the following PPML event–study with respect to each event e:

$$\mathbb{E}[Y_{iet}] = \exp\Big\{\alpha + \sum_{\ell=-12}^{12}{}^{\ell\neq -1}\, \beta_\ell\, \mathbf{1}\{k = \ell\} \times \text{Treat}_{ie} + \delta_{i\times e} + \lambda_{t\times e}\Big\}, \tag{2}$$

where $k \equiv t - t_e$ (t_e is the week of an event e). We include leads/lags from -12 to $+12$ weeks, and omit $k = -1$ as the reference period. The interaction coefficients, β_ℓ, capture the effect of socially connected mass shootings at different points in time relative to each event e. This event-study specification not only allows us to assess the validity of the parallel trends assumption but also provides insight into the dynamic trajectory of the impact over time.

3.3 Summary Statistics

Table 1 summarizes the stacked county–event–week panel with about 3.3 million observations. Panel A reports gun-store characteristics. Weekly gun-store visits average 237 per county–week, and the number of unique visitors averages 208. The mean Social Connectedness Index (SCI) between the county and the shooting county is 1,894, and the average distance between them is about 1,716 km (with a minimum of 500 km given our sample restriction). On average, 58 percent of county–event pairs are classified as high-SCI and thus fall into the treatment group. Panel B reports county-level demographic characteristics: the average county has a median household income of about $59,394, a Democratic vote share of 40 percent, 57 percent of residents with a college degree, and 18 percent of residents aged 65 or above. The population is roughly evenly split by gender (50 percent male), and 40 percent of families have children under age 18.

[**Insert Table 1 here**]

4 Results

4.1 Baseline Results

Table 2 reports the baseline estimates of how friend-linked distant mass shootings affect local gun-store visits. In Column (1) and (2), we exclude all counties within 500 km of the incident county to mitigate geographic spillovers. The specification in Column (1) includes county–event fixed effects and week–event fixed effects, absorbing time-invariant cross-county differences within an event and event-specific weekly trends. Column (2) additionally includes state × week fixed effects to net out unobserved time-varying state-level factors.

Across Columns (1)–(2), the coefficient on Treat × Post is positive and statistically significant: treated (more socially connected) counties increase gun-store visits by about

1–1.2% following a shooting. Columns (3)–(6) tighten the spatial exclusion to 750 km and 1,000 km to ensure that our results are not driven by discrete selection of the threshold. The estimates remain stable under these alternative exclusion thresholds, and, if anything, slightly larger (1.22–1.85%), with statistical significance unchanged, reinforcing the conclusion that the observed effect is not attributable to direct geographic proximity.

Compared to existing literature, the magnitude of our estimated treatment effect is economically meaningful. For example, Hu (2022) reports a 1% increase in local flood insurance purchases when the geographically distant friends are exposed to flooding events. Our estimates fall within the expected range of local demand effects triggered by remote events.

[Insert Table 2 here]

4.2 Validation of Parallel Trend Assumption

The causal interpretation of the above estimate relies on one key identification assumption, that is, the parallel trend assumption, which is central to the credibility of applying DID analysis. Specifically, the parallel trend assumption requires that the trends in the average gun-store visits in treated versus control counties would be parallel from before to after the distant mass shooting events in the absence of the event.

To validate the parallel trend assumption, we estimate Equation 2 and plot the estimated coefficient β_ℓ with the corresponding confidence intervals in Figure 4. Figure 4 reveals that the differences in gun-store visits in treated versus control counties are not statistically significant for any weeks prior to the event, which suggests that the parallel trend assumption is likely to be satisfied. This observation is essential since it provides justification and credibility for our DID approach.

[Insert Figure 4 here]

The positive effect of socially connected mass shootings on local gun-store visits emerges shortly after the event, becomes statistically significant in the second week, and peaks around the fifth week. After this peak, the effect declines steadily and becomes statistically insignificant by approximately the tenth week. This pattern indicates that the spillover effect of socially connected mass shootings through social networks is short-lived. The observed dynamics are most consistent with a Bayesian learning framework, in which individuals update their beliefs in response to the event but gradually discount its salience over time.

As an additional robustness check, Figure 5 reports estimates after excluding counties located within 100 km, 200 km, 750 km, and 1,000 km of the incident location. The dynamic pattern remains qualitatively similar across these specifications, indicating that our choice of exclusion threshold does not materially affect the results. Even when excluding counties at larger distances from the shooting location, there remain sufficient within-state county pairs to estimate how social connectedness influences gun-purchasing behavior, as reflected in gun-store visits.

[**Insert Figure 5 here**]

4.3 Robustness Checks

To ensure the robustness of our main results, we conduct a series of additional checks. These include testing whether more- and less-connected counties differ in their own exposure to mass shooting events, implementing randomization-based placebo tests, applying propensity score matching (PSM), and examining the sensitivity of our findings to alternative definitions of both dependent and independent variables. Across all these analyses, the results consistently confirm that the social-network spillover effect of distant mass shootings on local gun-store visits is robust to multiple specifications and methodological approaches.

4.3.1 Balance of Mass Shooting Events in Treated and Control Counties

A potential concern is that counties more socially connected to the shooting location might contemporaneously experience their own mass shooting events, while less-connected counties do not. If treated counties were disproportionately exposed to additional shootings, our estimates could reflect differences in local events rather than the effect of distant ones. It is therefore important to verify that treated and control counties do not differ systematically in their own exposure to mass shootings.

To address this concern, we replace the outcome variable in Equation 1 with the number of mass shooting events occurring in treated and control counties, aggregated to the county-month level. We then examine whether treated counties experience more shootings after the distant event. The regression results are reported in Table 3, and the event-study estimates are plotted in Figure 6. Both Table 3 and Figure 6 show that the distribution of shootings is similar across the two groups before and after the distant shock. These findings indicate that treated counties do not experience more mass shooting events following the incident, suggesting that our estimated effects are not driven by systematic differences in local exposure.

[**Insert Table 3 and Figure 6 here**]

4.3.2 Placebo Test

To rule out the possibility that the observed effect is due to random chance, we implement a randomization-based placebo test. We conduct a permutation test that reassigns treatment within each event 1,000 times, re-estimating Equation 1 with the same set of fixed effects.

Figure 7 plots the distribution of placebo coefficients for Treat × Post together with the baseline estimate (dashed line). The placebo distribution is tightly centered around zero, whereas the baseline estimate lies far in its tail. These results indicate that our estimated post-event increase in gun-store visits among high-SCI counties is unlikely to be driven by spurious assignment of the treatment effect.

[**Insert Figure 7 here**]

4.3.3 Alternative Independent Variables

Table 4 reports robustness checks using alternative definitions of social connectedness. Panel A employs a continuous measure by interacting log(SCI) with the post-event indicator. Across all exclusion radii (500 km, 750 km, and 1,000 km), the coefficients are positive and highly significant: a one-unit increase in log(SCI), which corresponds approximately to a doubling of connectedness, is associated with an increase in gun-store visits of about 0.8% to 1.5%. Panels B and C adopt stricter binary treatments, comparing counties in the top 60 quantiles to those in the bottom 40 quantiles, and the top 75 quantiles to the bottom 25 quantiles, respectively. The estimated effects become larger as the contrast strengthens, ranging from 1.5% to 3.7%, consistent with a monotonic dose–response pattern. Figure 9 complements these results with event-study estimates for the baseline split and for the 60–40 and 75–25 contrasts. The figure shows no evidence of differential pre-trends, while post-event responses diverge sharply about two weeks after the shooting, especially for the 75–25 comparison, where the effect reaches roughly 2%. Combined with the robustness check in Figure 5, which shows that excluding counties within different geographic radii does not alter the results, these findings reinforce that the observed pattern is driven by social connectedness rather than geographic proximity.

[Insert Table 4 and Figure 9 here]

We further examine whether the magnitude of the response varies with the strength of social connectedness to the shooting county. Specifically, we stratify counties into six quantile-based bins according to their SCI intensity and estimate separate treatment effects for each group. Figure 8 shows that the magnitude of the treatment effect increases monotonically with the intensity of social connectedness to the shooting county. Counties in the lowest SCI bin (reference group) exhibit no discernible response, while those in higher bins show progressively larger effects. In particular, the top two SCI bins experience the most pronounced increases, with estimated effects exceeding 2%. This gradient underscores that the observed behavioral response is strongly mediated by social ties rather than mere geographic proximity, reinforcing the interpretation that social networks amplify the diffusion of shock-related behaviors.

[Insert Figure 8 here]

4.3.4 Alternative Dependent Variables

The baseline outcome captures the total number of visits to gun stores, which may reflect either a larger customer base or repeated trips by the same individuals. To disentangle these mechanisms, we re-estimate the model using the number of unique visitors as the dependent variable. Table 5 shows that the effect remains positive and statistically significant, with magnitudes comparable to those for total visits. This pattern suggests that the observed increase is driven primarily by more individuals visiting gun stores rather than by a small group making multiple trips.

[Insert Table 5 here]

4.3.5 Propensity Score Matching

To further ensure the similarity between treated and control counties, we implement a propensity score matching (PSM) approach. In the first stage, we estimate the probability of being classified as treated (above-median SCI within the state) using a logistic regression on pre-treatment county-level demographic characteristics, including median household income, population size, percentage male, educational attainment, age composition, and racial composition (% White, % Black). Based on the estimated propensity scores, we perform nearest-neighbor matching with a 1:2 ratio without replacement, imposing a caliper of 0.10 on the logit propensity score and exact matching on state.[11]

Table 6 presents the results of re-estimating the baseline model with the matched sample. The estimates remain positive and statistically significant, with magnitudes comparable to

[11] Figure A3 illustrates the effectiveness of the matching procedure. The standardized mean differences (SMD) across covariates are substantially reduced after matching, with nearly all covariates falling below the conventional threshold of $|\text{SMD}| = 0.10$, indicating good balance between treated and control groups.

the main estimates. Taken together, these findings suggest that our results are not driven by systematic differences in observable characteristics between high- and low-SCI counties.

[Insert Table 6 here]

4.3.6 Sample Restrictions

Additionally, we conduct several other robustness checks by restricting the sample, including restricting the sample to events occurring at least 12 weeks apart, retaining only the first event for each county, and excluding the COVID-19 period. These results, reported in Appendix Tables A2, A3, and A4, remain consistent with our baseline estimates, confirming the robustness of our findings.

4.4 Mechanism analysis: Information-salience effect

We argue that when far-away friends share gun-shooting experiences through social networks, individuals' attention is drawn to these otherwise infrequent events. This heightened attention can lead households to learn from the public information set about their own risk exposure and update their beliefs. Such an attention-triggered information-salience effect serves as a key mechanism through which socially connected communities respond to mass shootings. We provide two evidences to support such argument as follows.

Panel A of Table 7 examines whether socially connected areas exhibit greater attention to shooting events. We use Google search activity for the keywords "shooting," "firearm" as proxies for attention. Since search data are only available at the Designated Market Area (DMA) level, we reconstruct a DMA-to-DMA Social Connectedness Index by aggregating county-level connections.[12] The results show significant increases in searches for "shooting" in Column (1) and similar positive responses for firearm-related terms in Columns (2). These findings indicate that information about distant shootings diffuses more intensely through social networks, heightening the salience of the event for socially connected locations.

[12] Specifically, we map each county to its DMA using spatial joins and then sum the county-to-county SCI values for all pairs belonging to two DMAs.

Importantly, this occurs without geographic proximity, suggesting that socially linked communities become more aware of and focused on the incident, consistent with an attention-driven salience effect.

Panel B provides further evidence by examining the heterogeneity based on local prior local shooting experience. Our hypothesis predicts that households in counties without recent local shootings should exhibit stronger responses to distant events, as these incidents are more salient in those counties. The results confirm this prediction: the estimated effect of distant shootings is substantially larger in Column 2 for counties without prior shootings, while the response is muted and statistically insignificant in Column 1 for counties with recent local exposure. This pattern suggests that a distant tragedy produces a greater behavioral reaction in places where such events are unusual, reinforcing the interpretation that salience, rather than geographic proximity, is the key channel linking social exposure to increased gun demand.

[**Insert Table 7 here**]

4.5 Heterogeneous Effect

We next investigate for which types of local institutional environments and demographic characteristics the socially connected mass shootings have stronger effects on local gun–store visits.

We begin by exploiting cross–state variation in mandatory waiting periods, which create a "cooling off" period among buyers. Following Luca, Malhotra and Poliquin (2017), Edwards et al. (2018), Koenig and Schindler (2023) and Arnold and Priestley (2025), we classify states according to whether they impose a waiting period between the initiation of a purchase and the transfer of the firearm. Column (1) of Table 8 reports interaction-based estimates that allow the post-shooting response in gun-store visits to differ between states with and without mandatory waiting-period laws. The increase in gun–store visits following socially connected shootings is concentrated in states without waiting periods, whereas the effect is substantially attenuated and becomes no longer statistically significant in states that require a waiting period. When the shootings become salient through social networks, cooling–off policies dampen the translation of heightened attention into immediate purchasing behavior. This attenuation is consistent with prior evidence that waiting periods curb impulsive gun acquisitions and reduce gun–related harms by forcing households to wait before completing a purchase (Luca, Malhotra and Poliquin, 2017; Edwards et al., 2018; Koenig and Schindler, 2023; Arnold and Priestley, 2025).

Columns (2) examine heterogeneity by universal background check (UBC) requirements. We find a similar pattern: the effect of socially connected shootings on gun-store visits is large and statistically significant in states without UBC laws, but insignificant in states that require universal background checks. This suggests that screening requirements, like waiting periods, impose frictions that limit the immediate behavioral response to socially transmitted information about mass shootings. These findings underscore the importance and effectiveness of gun policies in curbing short-term surges in firearm demand. By introducing institutional frictions, such as waiting periods and universal background checks, policymakers can mitigate impulsive purchases triggered by heightened salience of distant violence transmitted through social networks. These measures therefore play a critical role in reducing reactive gun acquisitions and, ultimately, in limiting the broader societal spillovers of gun violence.

[Insert Table 8 here]

We then examine whether the effect varies across counties with different demographic profiles. Table 9 splits the sample based on median values of key characteristics: the share of families with children, the share of older residents, the male share, and the Democratic vote share.[13] If the information-salience channel is operative, distant mass shootings should be particularly salient for populations that perceive themselves or their families as vulnerable. The results are consistent with this prediction. The estimated *Treat* × *Post* coefficient is

[13]Graphic results are provided in Figure 10.

positive and statistically significant only in counties with above-median shares of families with children, above-median shares of older residents, lower male shares (i.e., relatively more women), and higher Democratic vote shares. Event-study evidence in Figure A5 confirms that these differences are driven by stronger post-event surges rather than pre-trend imbalances.

Taken together, these heterogeneous effects are naturally interpreted through the lens of information salience. Distant mass shootings are likely to be more emotionally vivid and personally relevant for women, parents, older individuals, and residents of Democratic counties, who on average may be more sensitive to perceptions of public safety and victimization risk. When such individuals learn through social networks that socially connected communities have experienced a mass shooting, the event becomes particularly salient for these groups, leading to a larger increase in precautionary gun demand.

[**Insert Table 9 and Figure 10 here**]

5 Conclusion and Future Research

This paper examines the social-network effect on gun ownership, using distant mass shootings as an exogenous shock. Leveraging incident-level data on 95 mass shootings between 2021 and 2024, anonymized smartphone mobility data on weekly visits to firearm retailers, and the Facebook-based Social Connectedness Index (SCI), we show that interpersonal ties to affected communities meaningfully shape local gun-purchasing behavior in response to distant traumatic events. Using a difference-in-differences framework that compares counties within the same state with varying SCI ties to the shooting county, we find that counties with above-median social connectedness to the incident location experience a statistically and economically significant increase in gun-store visits of approximately 1–1.9 percent in the weeks following a mass shooting, despite being geographically remote.

We argue that the observed effect operates through an information-salience channel as we show that (1) following a mass shooting, Google searches for the terms "shooting" and "firearm" increase more in areas that are socially connected to the affected county and (2) the behavioral response is concentrated in counties that have not recently experienced their own mass shooting. Taken together, these findings support the interpretation that socially connected mass shootings increase local gun demand primarily by making the risk of victimization more salient and personally relevant for households in exposed areas.

Our heterogeneity analyses underscore the importance of both institutional and demographic context. We find that the effect of socially connected mass shootings on gun-store visits is concentrated in states without mandatory waiting periods or universal background check requirements, while it is substantially attenuated and often statistically insignificant in states that impose such cooling-off and screening measures. We also document larger effects in counties with more families with children, a higher share of older residents, relatively more women, and a higher Democratic vote share.

Overall, we show that information transmitted through social networks can trigger behavioral changes among individuals who are never directly exposed to the underlying shock. In this sense, social connectedness amplifies the impact of mass shootings well beyond the directly affected communities.

Our results also have policy implications. More stringent gun regulations, such as waiting-period laws and universal background checks, appear to mitigate surges in gun demand that follow network-mediated events. These measures may therefore play an important role in reducing impulsive firearm purchases and limiting the broader societal spillovers of gun violence.

One limitation of our study is that the data are relatively recent and high-frequency, capturing weekly patterns over a short time horizon. While this design allows us to identify immediate behavioral responses, it limits our ability to evaluate longer-term consequences of socially connected mass shootings. Future research could build on our analysis by examining whether these network-induced increases in gun ownership persist over time and exploring their downstream effects, such as changes in crime rates or shifts in attitudes toward gun regulation.

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Figures and Tables

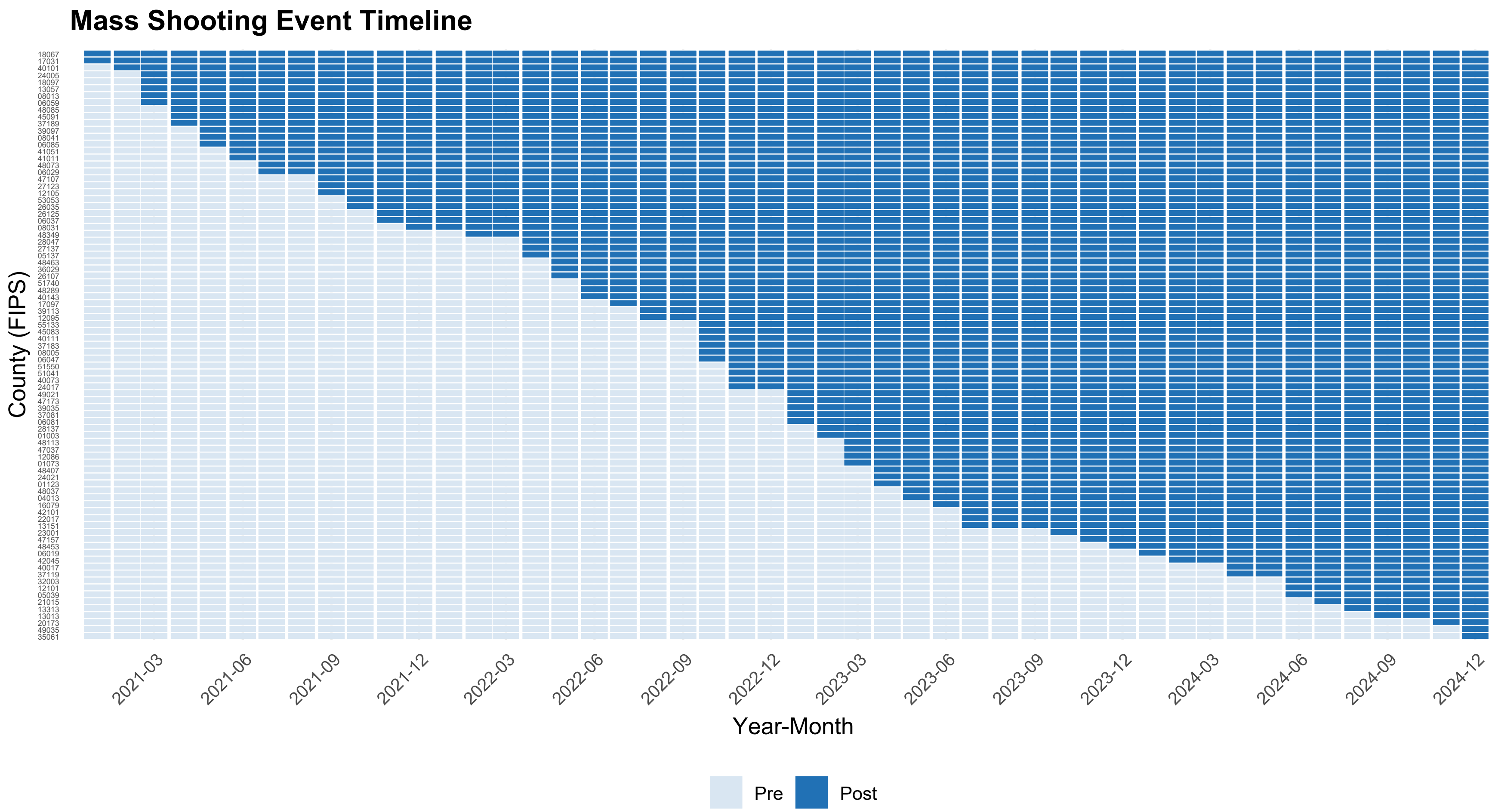
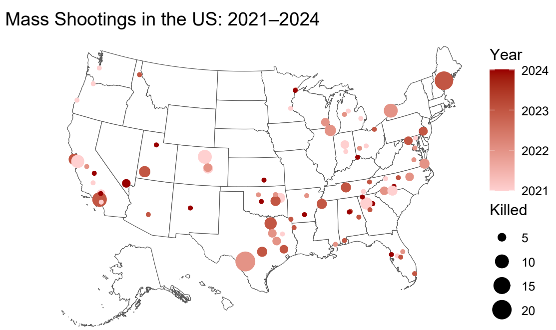
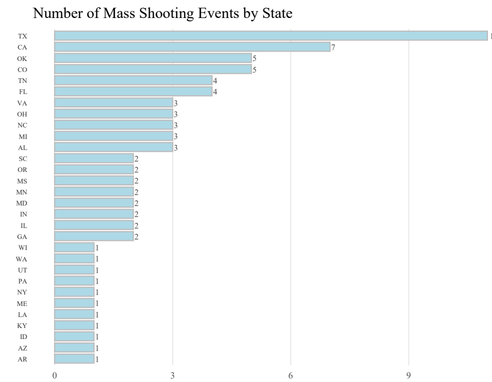


Figure 1: Timeline of Mass Shooting Events

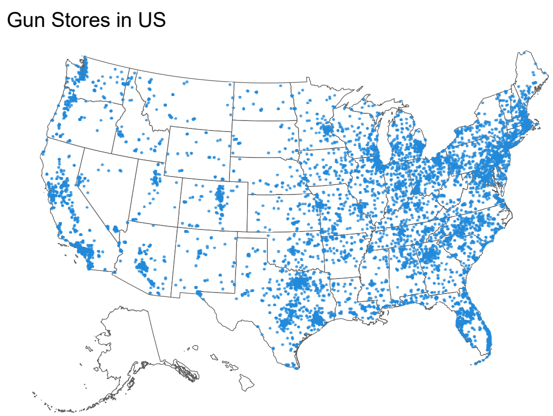
Notes: This figure illustrates the timeline of mass shooting events from 2021 to 2024. The horizontal axis represents the event date, while the vertical axis shows the county FIPS codes where shootings occurred. Light blue segments indicate months preceding the event, and dark blue segments represent the period following the event.



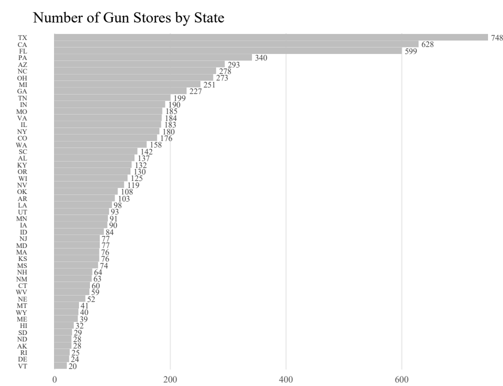
(a) Mass shootings in US (2021-2024)



(b) Number of Mass shootings by State



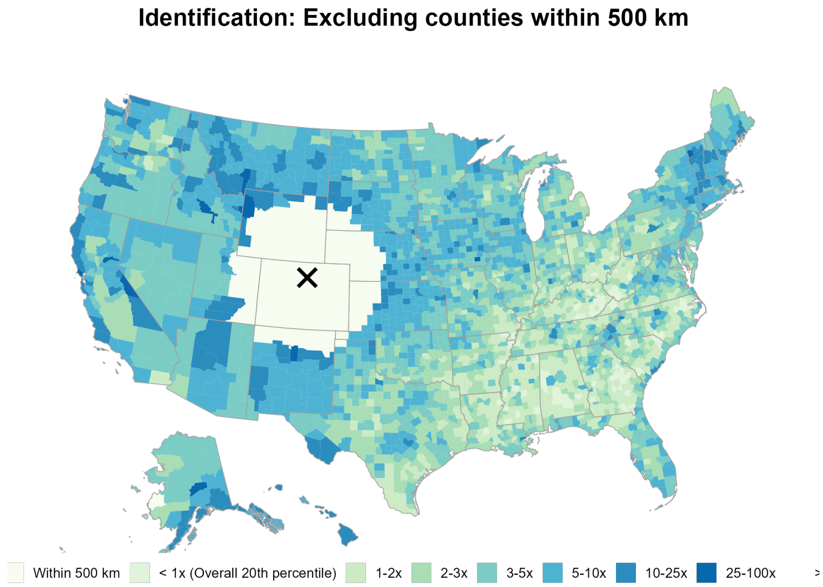
(c) Gun Stores in US (2021-2024)



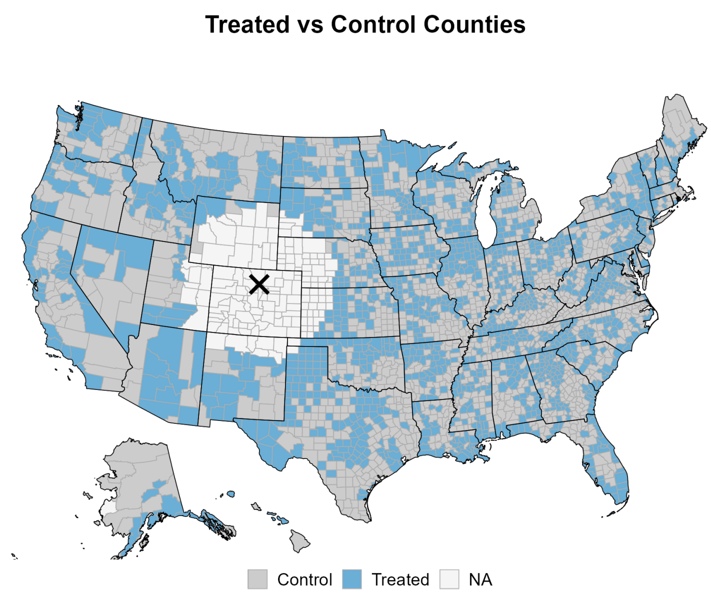
(d) Number of Gun Stores by State

Figure 2: Geographic distribution of mass shootings and gun stores in the U.S., 2021–2024

Notes: This figure shows the geographic distribution of mass shootings and gun stores in the United States between 2021 and 2024. Panel (a) maps the geographic coordinates of mass shooting events from 2021 to 2024, with marker size indicating event severity measured by fatalities. Panel (b) shows the distribution of mass shootings across states. Panel (c) maps the geographic coordinates of gun stores, and Panel (d) reports the number of gun stores by state.



(a) Social connectedness to shooting county



(b) Treated and control counties

Figure 3: Illustration of the identification strategy

Notes: This figure illustrates our identification strategy using the mass shooting in Boulder, Colorado on March 22, 2021, where a gunman killed ten people at a King Soopers supermarket. The cross ($\times$) marks the coordinates of the shooting event. Panel (a) shows county-level social connectedness to the shooting county. Counties within 500 km are excluded to rule out geographically local spillover effects, which is our baseline identification strategy. We also test alternative exclusion distances of 750 km and 1,000 km in robustness checks. Panel (b) displays treated and control counties, classified by whether their state-level SCI is above or below the median. Blue counties are treated; gray counties are controls.

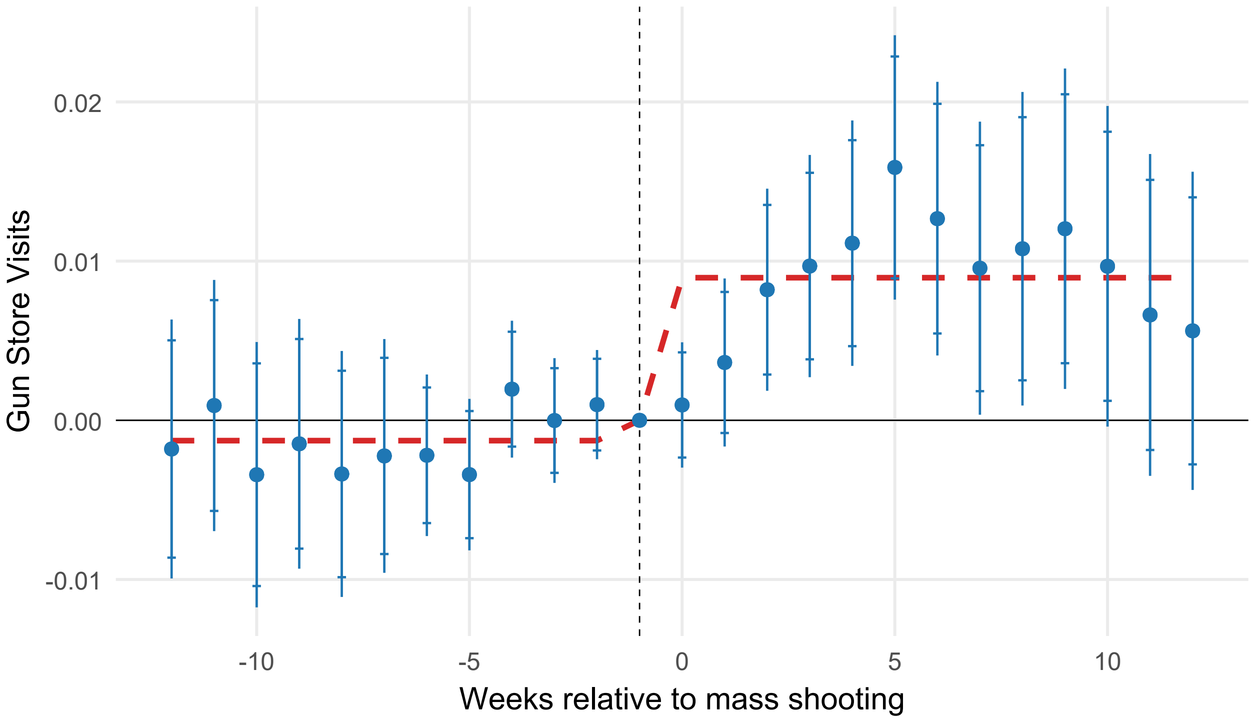


Figure 4: Parallel Trend Test

Notes: This figure shows the results of parallel trend using event study analysis. The dependent variable is the weekly number of gun store visits at the county level. The week before the shooting serves as the reference group. The dashed red line indicates the average effect before and after the event, and 90% and 95% confidence intervals are shown. All regressions are estimated using a PPML estimator. Specifications include county-by-event, month-by-event, and state-by-month fixed effects. Standard errors are clustered at the county-by-event level.

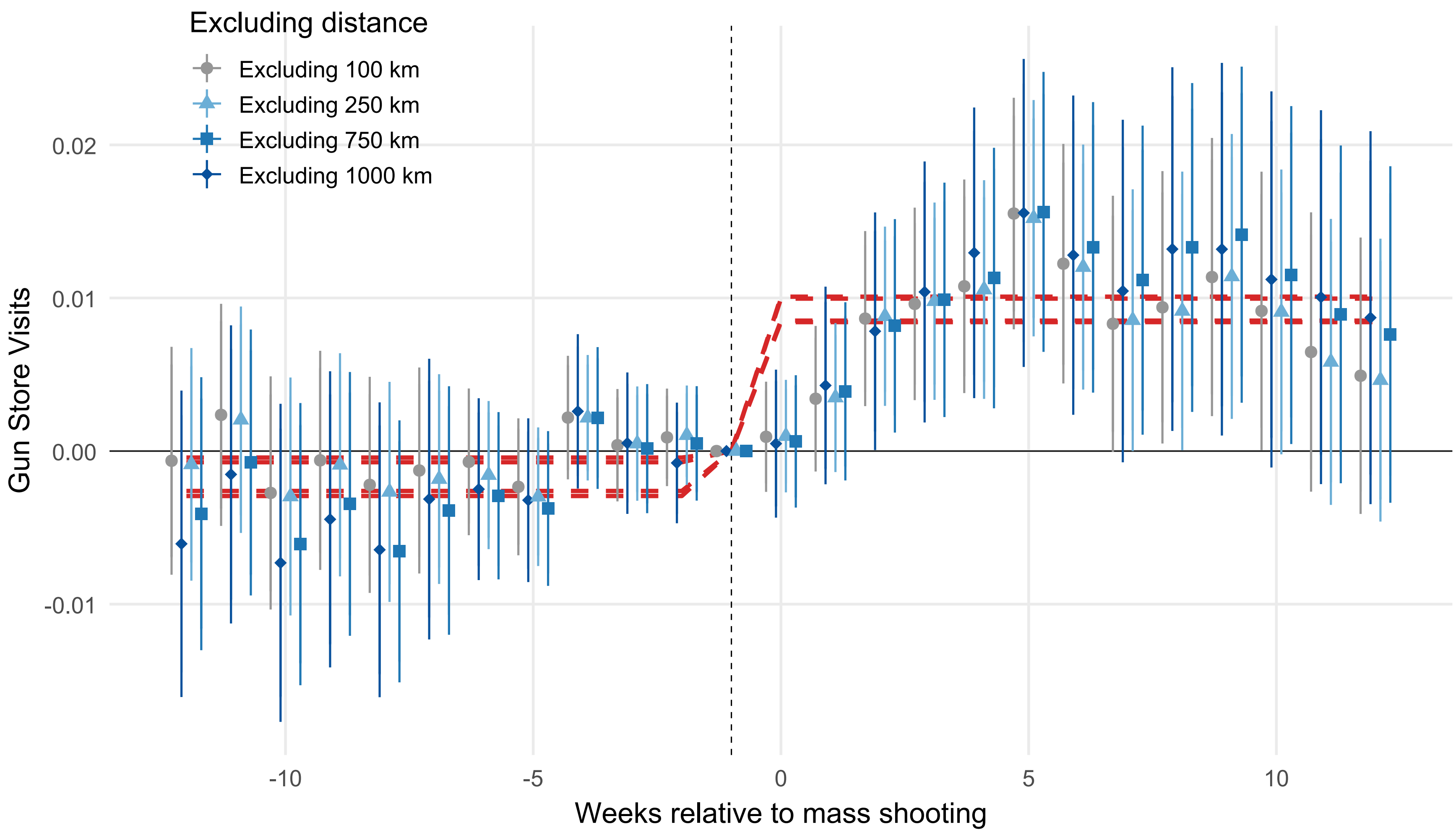


Figure 5: Event Study: Alternative Excluding Distances

Notes: This figure shows the event study results of changing alternative excluding distances to shooting county. Each line corresponds to a different exclusion radius (100 km, 250 km, 750 km, 1000 km). All regressions are estimated using a PPML estimator. The dashed red line indicates the average effect before and after the event. The 95% confidence intervals are presented. Specifications include county-by-event, month-by-event, and state-by-month fixed effects. The Standard errors are clustered at the county-by-event level.

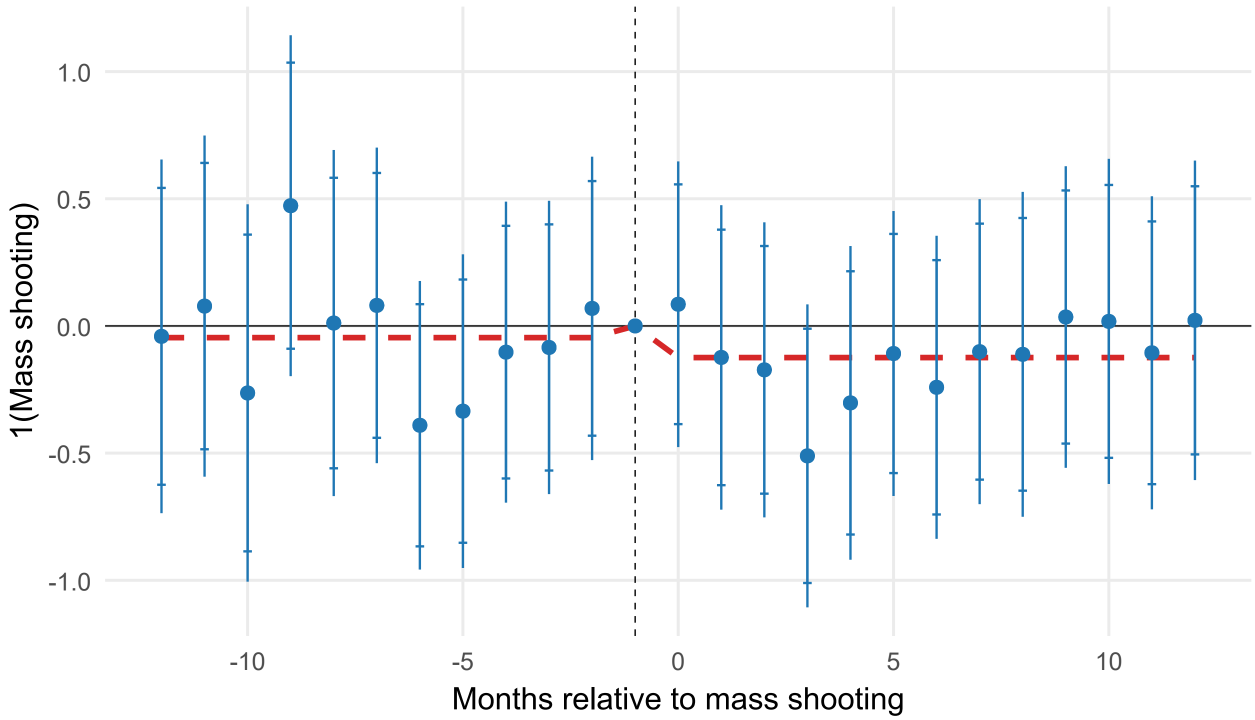


Figure 6: Balance of Shooting Events Between Treated and Control Counties

Notes: This figure examines whether shooting events are balanced between treated and control counties after the focal shooting, addressing concerns that mass shooting events might propagate through social networks. All regressions are estimated using a PPML estimator and exclude counties within 500 km of the shooting county. The dependent variable is a binary indicator equal to 1 if a mass shooting occurred in a given county-month. The dashed red line indicates the average effect before and after the event. The 90% and 95% confidence intervals are presented. Specifications include county-by-event, month-by-event, and state-by-month fixed effects. Standard errors are clustered at the county-by-event level.

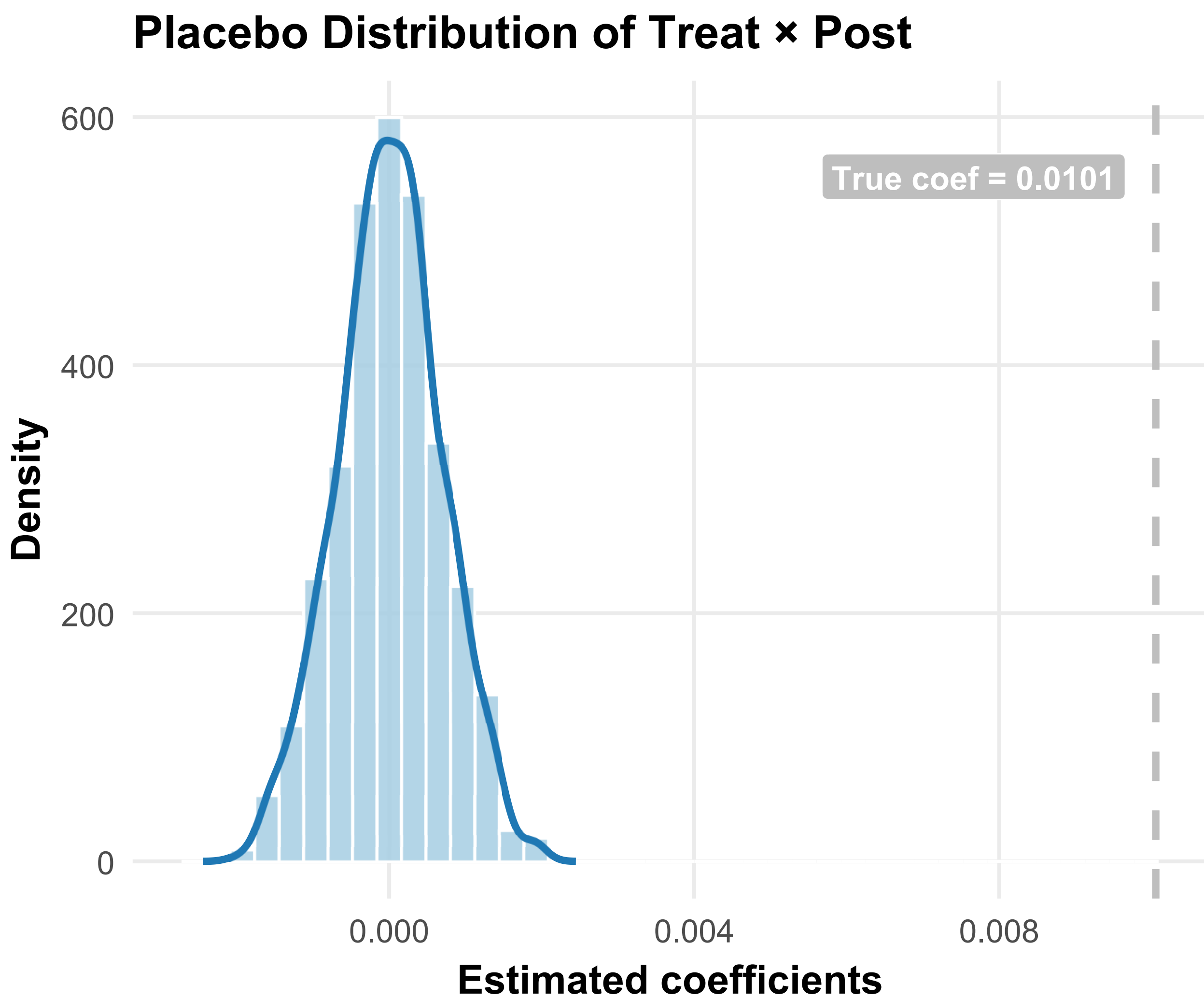


Figure 7: Placebo test

Notes: This figure presents the results of a placebo test. The blue bars show the distribution of 1,000 estimated coefficients for the interaction term (Treat × Post) obtained from randomly assigned treatment groups, while the grey vertical dashed line indicates the baseline estimate from the actual specification. All regressions are estimated using a PPML estimator and exclude counties within 500 km of the shooting county. Specifications include county-by-event, week-by-event, and state-by-week fixed effects. Standard errors are clustered at the county-by-event level.

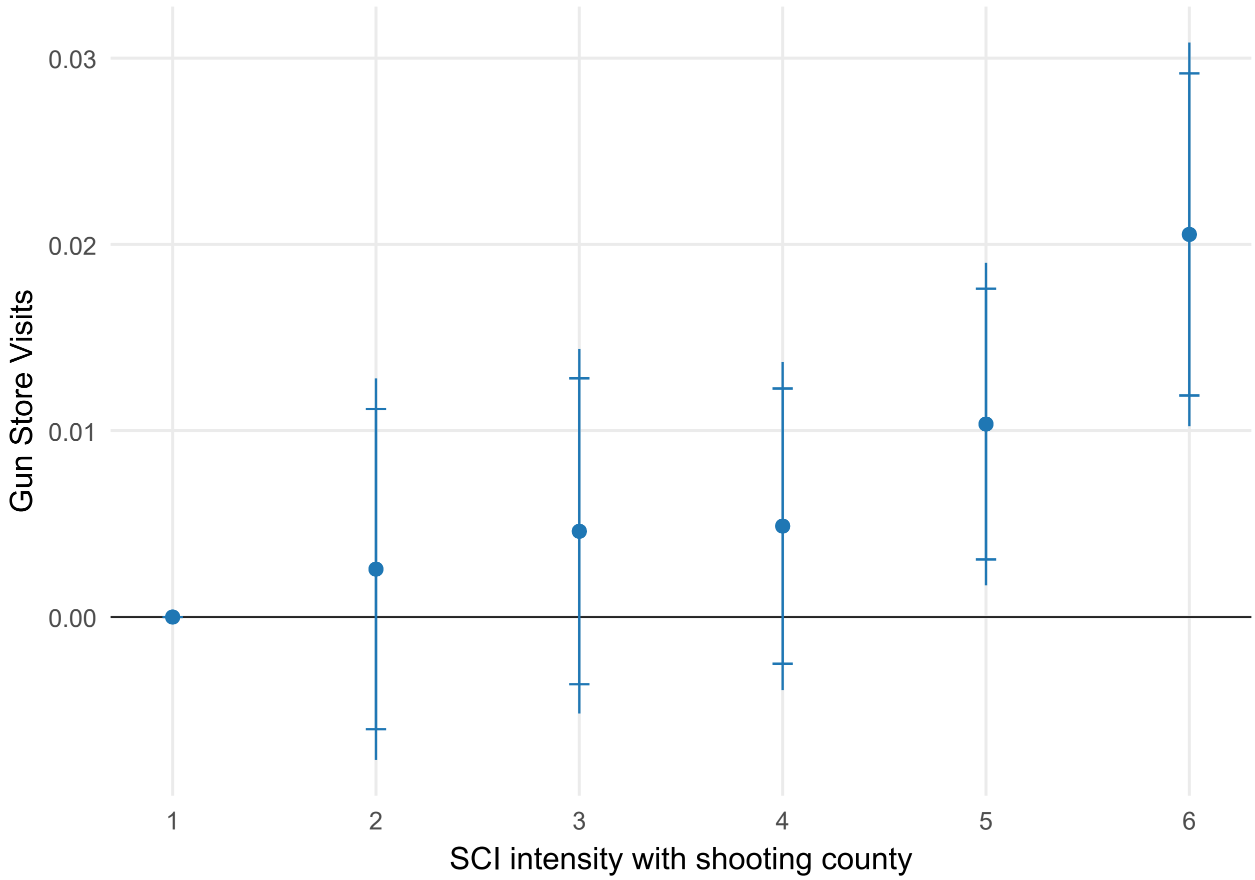


Figure 8: Treatment Effect by Social Connectedness Intensity

Notes: This figure shows how the treatment effect varies with the intensity of social connectedness (SCI) to the shooting county. SCI intensity is divided into six equal-sized bins (by quantiles), with the lowest bin serving as the reference category. The 90% and 95% confidence intervals are presented. All regressions are estimated using a PPML estimator and exclude counties within 500 km of the shooting county. All regressions include county-by-event, week-by-event, and state-by-week fixed effects. Standard errors are clustered at the county-by-event level.

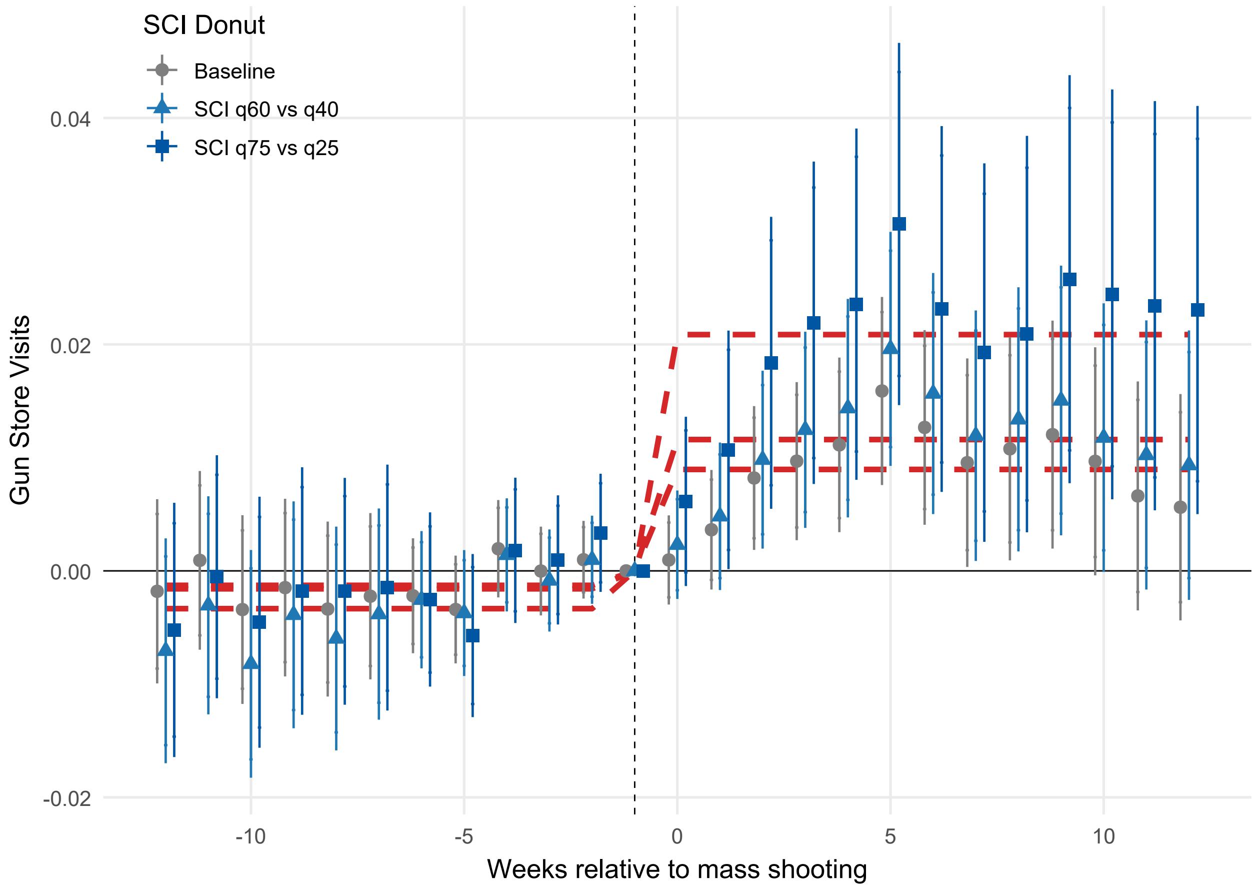
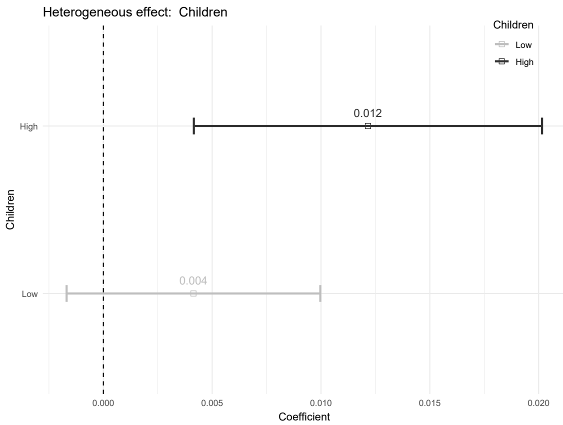
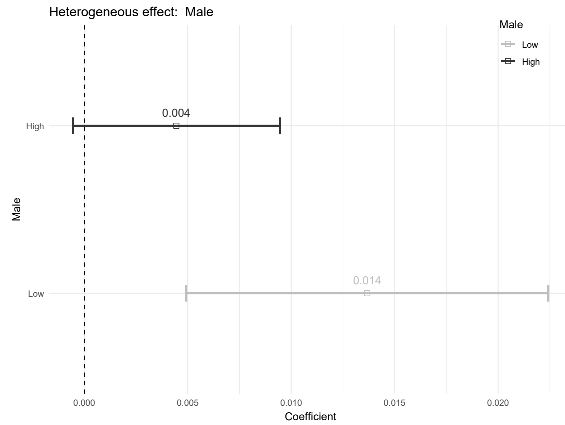


Figure 9: SCI donut measurement

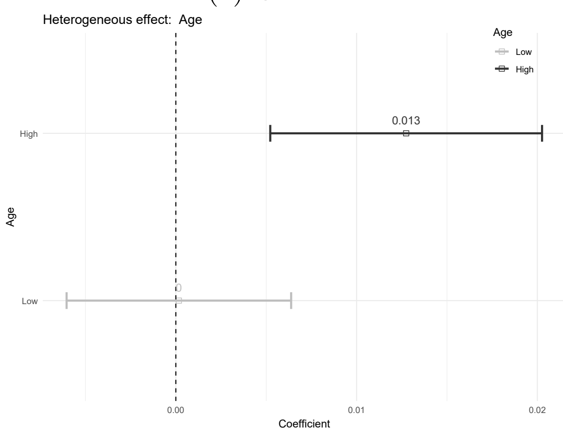
Notes: This figure presents a robustness check of the donut SCI measurement. The gray line represents the baseline specification, while the light blue line compares SCI donut measures at the 60th vs 40th percentile and the dark blue line compares the 75th vs 25th percentile. The 95% confidence intervals are presented. All regressions are estimated using a PPML estimator and exclude counties within 500 km of the shooting county. All regressions include county-by-event, week-by-event, and state-by-week fixed effects. Standard errors are clustered at the county-by-event level.



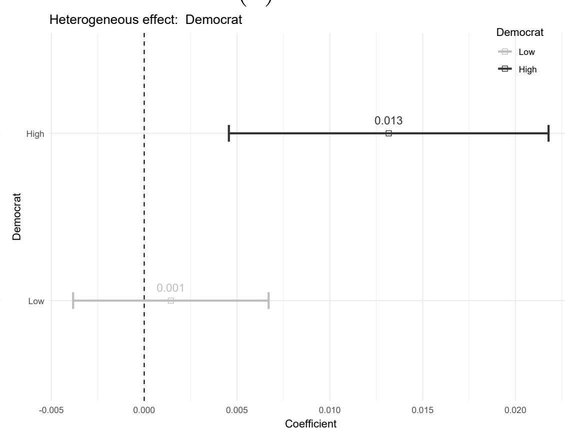
(a) Children



(b) Male



(c) Age



(d) Democrat

Figure 10: Heterogeneous effects

Notes: This figure shows the heterogeneous effects. For each characteristic, the sample is split into two groups based on the median, and separate regressions are estimated for each subsample. The grey line represents the lower group, and the black line represents the higher group. The 95% confidence intervals are presented. All regressions are estimated using a PPML estimator and exclude counties within 500 km of the shooting county. All regressions include county-by-event, week-by-event, and state-by-week fixed effects. The Standard errors are clustered at the county-by-event level.

Table 1: Summary statistics

Statistic	Mean	St. Dev.	Min	Max	N
Panel A: Gun store characteristics					
Gun store visits	237.31	589.53	0	16,941	3,233,830
Gun store visitors	208.18	514.99	0	13,828	3,233,830
Social Connectedness Index (SCI)	1,894.48	2,034.41	1	215,743	3,233,830
Distance to shooting county (km)	1,716.29	948.49	500.01	8,512.97	3,233,830
High-SCI treatment indicator	0.58	0.49	0	1	3,233,830
Panel B: County level demographic characteristics					
Median household income (USD)	59,393.97	15,539.62	26,074	147,111	3,233,830
Partisan index: Democratic (%)	0.40	0.16	0.08	0.92	3,222,290
Share of residents with college degree (%)	0.57	0.10	0.24	0.87	3,233,830
Share of male residents (%)	0.50	0.02	0.45	0.67	3,233,830
Share of residents aged 65 (%)	0.18	0.04	0.07	0.58	3,233,830
Families with children under 18 (%)	0.40	0.06	0.08	0.61	3,233,830

Notes: This table shows summary statistics of our key variables. Gun store visits and visitors are weekly device-based foot-traffic counts to gun-store POIs. The Social Connectedness Index (SCI) measures the intensity of friendship links between the focal county and the incident (shooting) county; the High–SCI treatment indicator equals 1 for counties above the within-state median SCI. "SCI-weighted exposure intensity" is the normalized measure of a county's social exposure to the incident in a given week. "Distance to shooting county" is the great-circle distance; the baseline sample excludes counties within 500 km of the incident county. County-level demographic characteristics are sourced from the American Community Survey (ACS) 2020 five-year estimates. Median household income reflects the annual median income in U.S. dollars. Partisan index is derived from ICPSR County Presidential Election Returns, capturing the relative strength of Democratic vote shares in the 2020 presidential election at the county level. Educational attainment is captured by the share of adults aged 25 and older holding a college degree or higher. Gender composition is represented by the proportion of male residents, while age structure is summarized by the share of individuals over 65 years old. Family with children under 18 is indicated by the share of family households that include at least one child under 18 years of age.

Table 2: Baseline results

	Visits					
	500 km		750 km		1000 km	
	(1)	(2)	(3)	(4)	(5)	(6)
Treat × Post	0.0121**	0.0100***	0.0154***	0.0122***	0.0185***	0.0126***
	(0.0049)	(0.0037)	(0.0054)	(0.0040)	(0.0061)	(0.0044)
County-Event FE	Yes	Yes	Yes	Yes	Yes	Yes
Week-Event FE	Yes	Yes	Yes	Yes	Yes	Yes
State-Week FE		Yes		Yes		Yes
Observations	3,233,830	3,232,891	2,849,090	2,848,199	2,400,447	2,399,660
Pseudo R^2	0.9752	0.9779	0.9751	0.9779	0.9753	0.9779

Notes: This table shows the baseline results indicating that social connectedness to a shooting county increases local gun demand, as proxied by gun store visits. Counties within 500 km of the shooting county are excluded from the treated group (Columns 1–2), and the analysis is extended to 750 km and 1000 km thresholds (Columns 3–6). All regressions are estimated using a Poisson Pseudo Maximum Likelihood (PPML) estimator. The dependent variable is the weekly number of gun store visits at the county level. The coefficient on Treat × Post captures the change in visits in treated counties relative to control counties after the focal shooting event. Specifications in Columns 1, 3, and 5 include county-by-event and week-by-event fixed effects, while Columns 2, 4, and 6 additionally include state-by-week fixed effects to allow within-state comparisons. Standard errors are clustered at the county-by-event level in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: Events balance between treated and control counties

	1(Mass shooting)					
	500 km		750 km		1000 km	
	(1)	(2)	(3)	(4)	(5)	(6)
Treat × Post	0.0523	-0.0776	0.0584	-0.0667	0.0706	-0.0681
	(0.0630)	(0.1011)	(0.0661)	(0.1081)	(0.0702)	(0.1155)
County-Event FE	Yes	Yes	Yes	Yes	Yes	Yes
Month-Event FE	Yes	Yes	Yes	Yes	Yes	Yes
State-Month FE		Yes		Yes		Yes
Observations	112,270	20,099	99,330	18,178	83,767	16,038
Pseudo R^2	0.0504	0.1151	0.0527	0.1162	0.0522	0.1191

Notes: This table examines whether mass shooting events propagate through social networks by examining post-event differences in the likelihood of mass shootings between treated and control counties. Counties within 500 km of the shooting county are excluded from the treated group (Columns 1–2), and the analysis is extended to 750 km and 1000 km thresholds (Columns 3–6). All regressions are estimated using a PPML estimator. The dependent variable is a binary indicator equal to 1 if a mass shooting occurred in a given county-month. Specifications in Columns 1, 3, and 5 include county-by-event and month-by-event fixed effects, while Columns 2, 4, and 6 additionally include state-by-month fixed effects to allow within-state comparisons. Standard errors are clustered at the county-by-event level in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Alternative social connectedness measurement

	Visits					
	500 km		750 km		1000 km	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Continuous SCI measurement						
Log(SCI) × Post	0.0126***	0.0083***	0.0153***	0.0120***	0.0149***	0.0112***
	(0.0030)	(0.0029)	(0.0036)	(0.0036)	(0.0039)	(0.0041)
Observations	3,233,830	3,232,891	2,849,090	2,848,199	2,400,447	2,399,660
Pseudo R^2	0.9752	0.9779	0.9752	0.9779	0.9753	0.9779
Panel B: Top 60 quantiles as treatment						
Treat_{60} × Post	0.0179***	0.0145***	0.0220***	0.0176***	0.0261***	0.0185***
	(0.0061)	(0.0046)	(0.0068)	(0.0050)	(0.0078)	(0.0055)
Observations	2,591,516	2,590,709	2,284,566	2,283,791	1,925,746	1,925,059
Pseudo R^2	0.9757	0.9785	0.9756	0.9784	0.9756	0.9784
Panel C: Top 75 quantiles as treatment						
Treat_{75} × Post	0.0286***	0.0219***	0.0316***	0.0249***	0.0374***	0.0264***
	(0.0095)	(0.0068)	(0.0106)	(0.0075)	(0.0123)	(0.0082)
Observations	1,619,206	1,618,644	1,428,094	1,427,564	1,203,132	1,202,658
Pseudo R^2	0.9763	0.9795	0.9761	0.9794	0.9762	0.9795
County-Event FE	Yes	Yes	Yes	Yes	Yes	Yes
Week-Event FE	Yes	Yes	Yes	Yes	Yes	Yes
State-Week FE		Yes		Yes		Yes

Notes: This table presents robustness checks using alternative measures of social connectedness (SCI). Panel A uses a continuous measure of SCI, where treatment intensity is captured by the log of SCI interacted with the post-event indicator (Log(SCI) × Post). Panel B defines treated counties as those in the top 60% of SCI relative to the shooting county, compared to counties in the bottom 40% (Treat_{60} × Post). Panel C defines treated counties as those in the top 75% of SCI relative to the shooting county, compared to counties in the bottom 25% (Treat_{75} × Post). (Treat_{75} × Post). Counties within 500 km of the shooting county are excluded from the treated group (Columns 1–2), and the analysis is extended to 750 km and 1000 km thresholds (Columns 3–6). All regressions are estimated using a PPML estimator. The dependent variable is the weekly number of gun store visits at the county level. Specifications in Columns 1, 3, and 5 include county-by-event and week-by-event fixed effects, while Columns 2, 4, and 6 additionally include state-by-week fixed effects to allow within-state comparisons. Standard errors are clustered at the county-by-event level in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Alternative dependent variable

	Visitor					
	500 km		750 km		1000 km	
	(1)	(2)	(3)	(4)	(5)	(6)
Treat × Post	0.0113**	0.0095***	0.0148***	0.0117***	0.0178***	0.0120***
	(0.0048)	(0.0037)	(0.0053)	(0.0040)	(0.0060)	(0.0044)
County-Event FE	Yes	Yes	Yes	Yes	Yes	Yes
Week-Event FE	Yes	Yes	Yes	Yes	Yes	Yes
State-Week FE		Yes		Yes		Yes
Observations	3,233,830	3,232,891	2,849,090	2,848,199	2,400,447	2,399,660
Pseudo R^2	0.9742	0.9769	0.9741	0.9769	0.9742	0.9769

Notes: This table presents robustness checks using an alternative dependent variable: the number of visitors to gun stores. Counties within 500 km of the shooting county are excluded from the treated group (Columns 1–2), and the analysis is extended to 750 km and 1000 km thresholds (Columns 3–6). All regressions are estimated using a PPML estimator. The dependent variable is the weekly number of gun store visits at the county level. Specifications in Columns 1, 3, and 5 include county-by-event and week-by-event fixed effects, while Columns 2, 4, and 6 additionally include state-by-week fixed effects to allow within-state comparisons. Standard errors are clustered at the county by event level in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: Propensity score matching

	Visits					
	500 km		750 km		1000 km	
	(1)	(2)	(3)	(4)	(5)	(6)
Treat × Post	0.0140**	0.0121***	0.0178***	0.0139***	0.0215***	0.0147***
	(0.0056)	(0.0041)	(0.0062)	(0.0044)	(0.0070)	(0.0048)
County-Event FE	Yes	Yes	Yes	Yes	Yes	Yes
Week-Event FE	Yes	Yes	Yes	Yes	Yes	Yes
State-Week FE		Yes		Yes		Yes
Observations	2,316,603	2,315,929	2,037,240	2,036,606	1,713,462	1,712,900
Pseudo R^2	0.9592	0.9631	0.9599	0.9638	0.9598	0.9639

Notes: This table presents robustness checks using propensity score matching (PSM). Counties within 500 km of the shooting county are excluded from the treated group (Columns 1–2), and the analysis is extended to 750 km and 1000 km thresholds (Columns 3–6). All regressions are estimated using a PPML estimator. The dependent variable is the weekly number of gun store visits at the county level. Specifications in Columns 1, 3, and 5 include county-by-event and week-by-event fixed effects, while Columns 2, 4, and 6 additionally include state-by-week fixed effects to allow within-state comparisons. Standard errors are clustered at the county by event level in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 7: Mechanism Analysis: Information Salience

Panel A: Google Search Index

	Shooting (1)	Firearm (2)
Treat × Post	0.0074**	0.0132*
	(0.0030)	(0.0072)
Week-Event FE	Yes	Yes
DMA-Event FE	Yes	Yes
Observations	525,805	357,643
Pseudo R^2	0.4617	0.4431

Panel B: Prior Mass Shooting Events

	Has Prior Events (1)	No-Prior Events (2)
Treat × Post	0.0072	0.0146**
	(0.0045)	(0.0063)
County-Event FE	Yes	Yes
Week-Event FE	Yes	Yes
State-Week FE	Yes	Yes
Observations	1,648,737	1,584,152
Pseudo R^2	0.9780	0.9778

Notes: This table presents results of mechanism analysis. All regressions are estimated using a PPML estimator and exclude counties within 500 km of the shooting county. Panel A uses Google Trends data to capture changes in online search activity for firearm-related terms ("Shooting" and "Firearm"). All regressions include week-by-event and DMA-by-event fixed effects. Standard errors are clustered at the DMA-by-event level. Panel B explores heterogeneity based on prior exposure to mass shooting events, comparing counties with and without previous incidents. All regressions include county-by-event, week-by-event, and state-by-week fixed effects. Standard errors are clustered at the county-by-event level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 8: Heterogeneous Effects: By Gun Policy

	Visits	
	(1)	(2)
Treat × Post × Waiting period	0.0079	
	(0.0057)	
Treat × Post × No-waiting period	0.0101***	
	(0.0037)	
Treat × Post × UBC		-0.0026
		(0.0040)
Treat × Post × No-UBC		0.0164***
		(0.0043)
County-Event FE	Yes	Yes
Week-Event FE	Yes	Yes
State-Week FE	Yes	Yes
Observations	3,232,891	3,232,891
Pseudo R^2	0.9779	0.9779

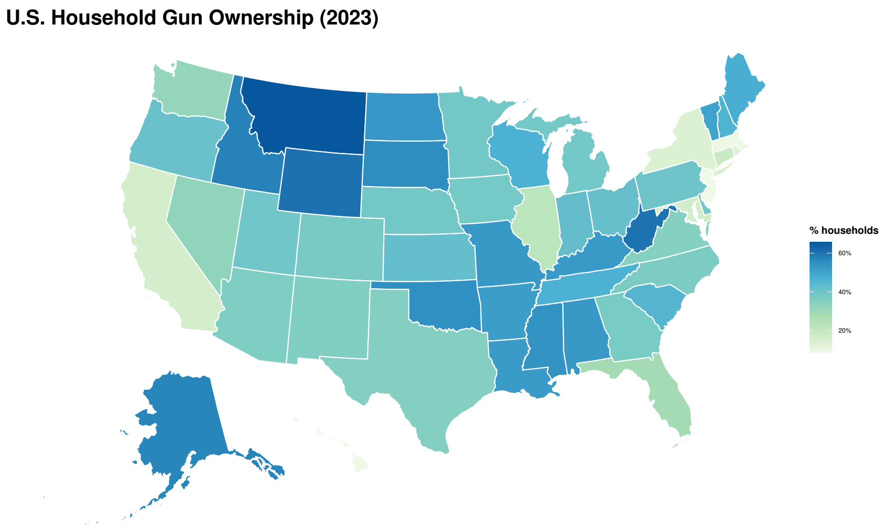
Notes: This table shows the heterogeneous effect by gun policy, focusing on waiting periods and universal background checks (UBC). All regressions are estimated using a PPML estimator and exclude counties within 500 km of the shooting county. All regressions include county-by-event, week-by-event, and state-by-week fixed effects. Standard errors are clustered at the county-by-event level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 9: Heterogeneous Effects: By Demographic

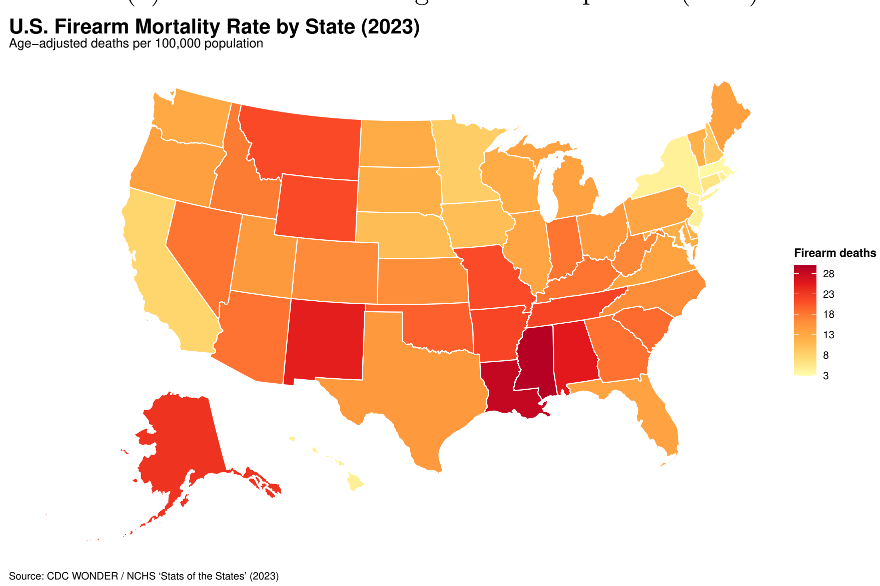
	Visits			
	Children		Age	
	Low	High	Low	High
	(1)	(2)	(3)	(4)
Treat × Post	0.0041	0.0122**	0.0002	0.0127***
	(0.0035)	(0.0049)	(0.0038)	(0.0046)
Observations	1,574,117	1,658,774	1,524,914	1,707,483
Pseudo R^2	0.9690	0.9806	0.9425	0.9777
	Male		Democrat	
	Low	High	Low	High
	(1)	(2)	(3)	(4)
Treat × Post	0.0137**	0.0044	0.0014	0.0132**
	(0.0053)	(0.0030)	(0.0032)	(0.0052)
Observations	1,623,996	1,607,142	1,575,103	1,646,248
Pseudo R^2	0.9794	0.9762	0.9670	0.9802
County-Event FE	Yes	Yes	Yes	Yes
Week-Event FE	Yes	Yes	Yes	Yes
State-Week FE	Yes	Yes	Yes	Yes

Notes: This table presents heterogeneous effects by county-level demographics. For each characteristic (share of households with children, median age, male population share, and Democratic vote share), counties are divided into "Low" and "High" groups based on median value. All regressions are estimated using a PPML estimator and exclude counties within 500 km of the shooting county. All regressions include county-by-event, week-by-event, and state-by-week fixed effects. Standard errors are clustered at the county-by-event level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Online Appendix



(a) State-level civilian gun ownership rates (2023)



(b) State-level annual gun-related deaths (2023)

Figure A1: Gun Ownership and Gun-Related Mortality Across U.S. States

Notes: Panel (a) shows estimated state-level civilian gun ownership rates in 2023, where darker shades indicate higher ownership levels. Panel (b) displays annual gun-related deaths by state in 2023, with darker shades representing higher mortality rates.

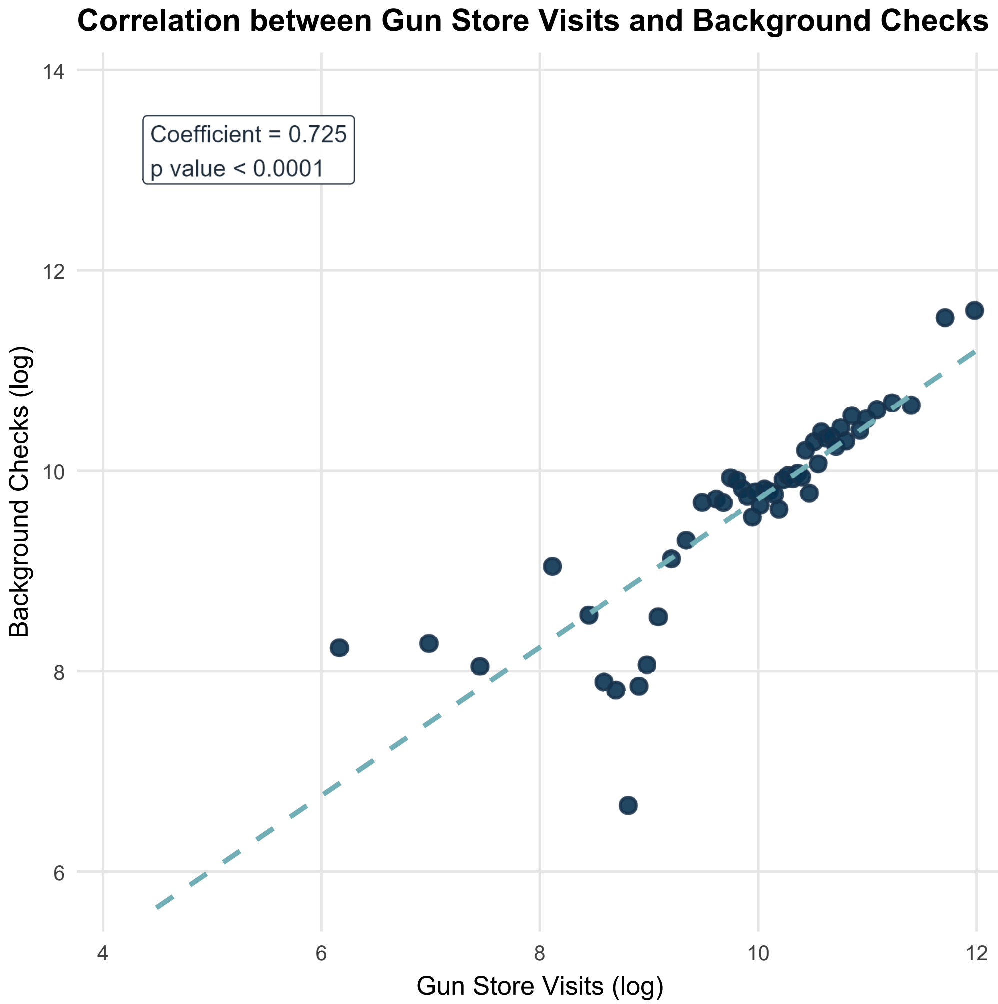


Figure A2: The correlation between gun store visits and state-level background checks

Notes: This figure plots the correlation between monthly gun store visits and total background checks at the state level, both on a log scale. Black dots represent binned means of visits, where the data are divided into 60 equal-width bins based on the log of gun store visits. The dashed line shows the ordinary least squares (OLS) fit. The estimated coefficient is 0.725 and statistically significant.

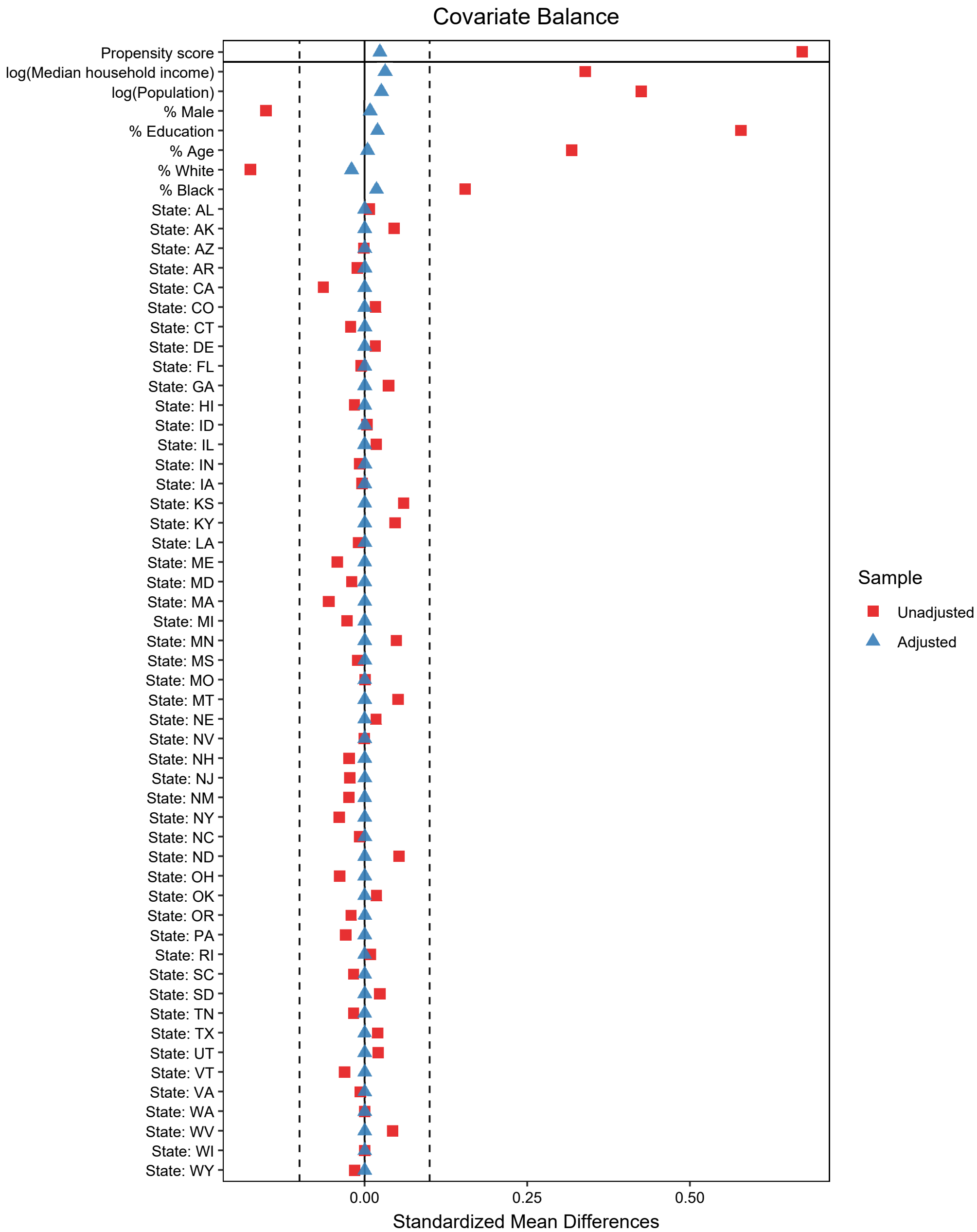


Figure A3: Covariate Balance

Notes: This figure illustrates covariate balance from propensity score matching (PSM). Points represent standardized mean differences (SMDs) for each covariate before matching (red squares) and after matching (blue triangles). Points display standardized mean differences (SMDs) for each covariate before matching (red squares) and after matching (blue triangles). The vertical reference line at |SMD| = 0.10 marks the conventional balance threshold; covariates to the left are considered well balanced. Matching is performed via nearest-neighbor (1:2) without replacement on the logit propensity score with a standardized caliper of 0.10, exact matching on states, and estimand = ATT. Binary covariates are standardized.

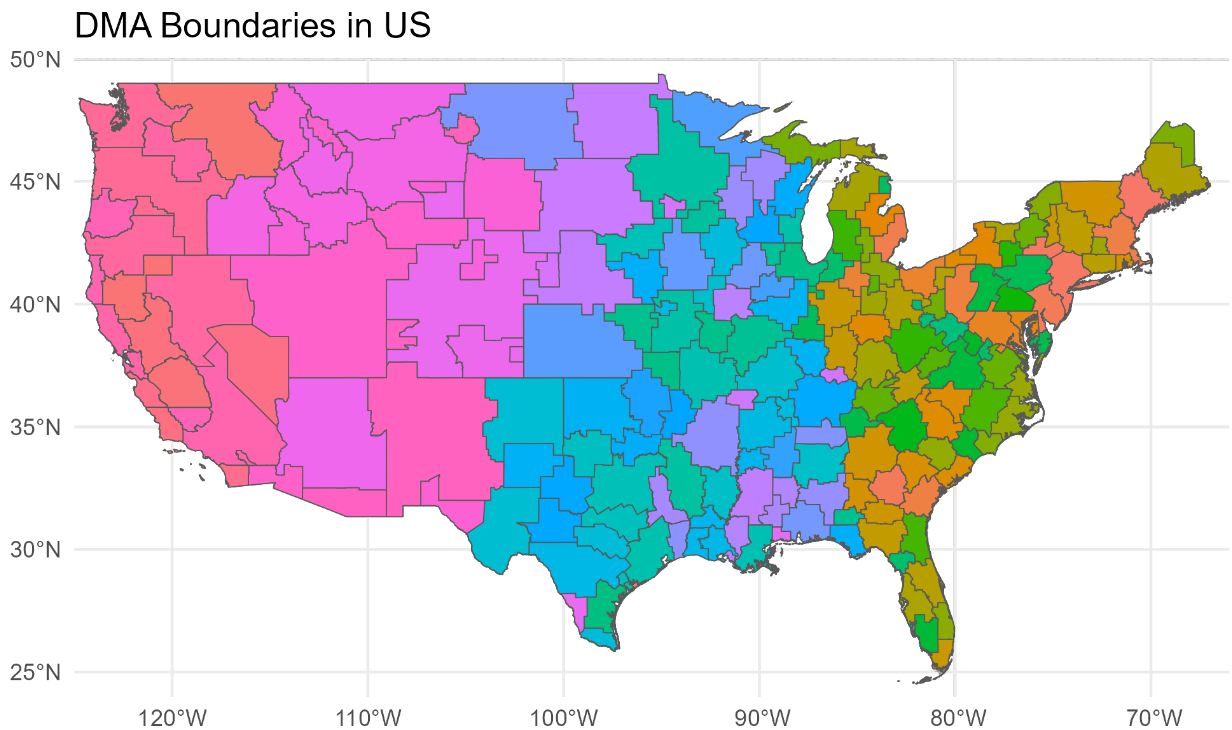
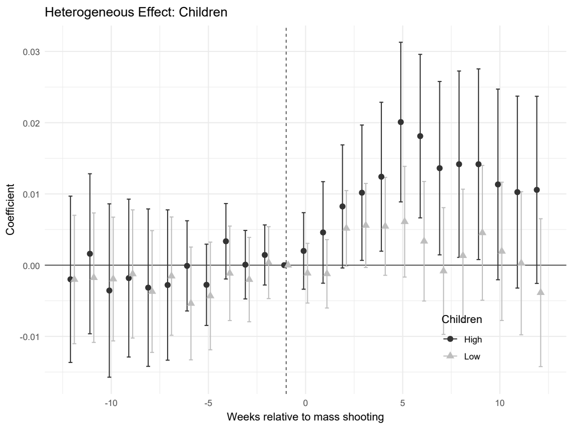
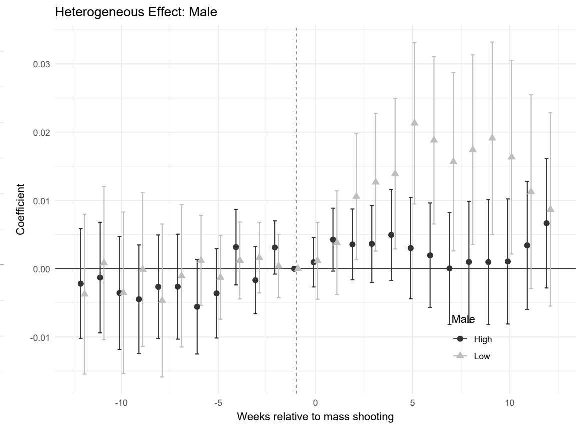


Figure A4: Designated Market Areas (DMA) in the United States

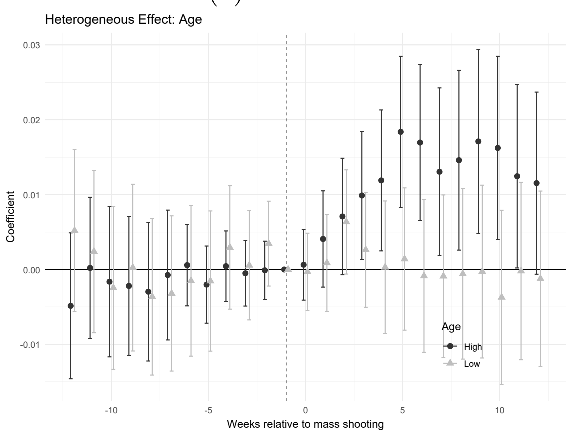
Notes: This figure shows DMA boundaries across the United States. The Google Search Index used in the mechanism analysis is available only at this level of aggregation. We construct a DMA-to-DMA social connectedness index by aggregating county-to-county SCI values.



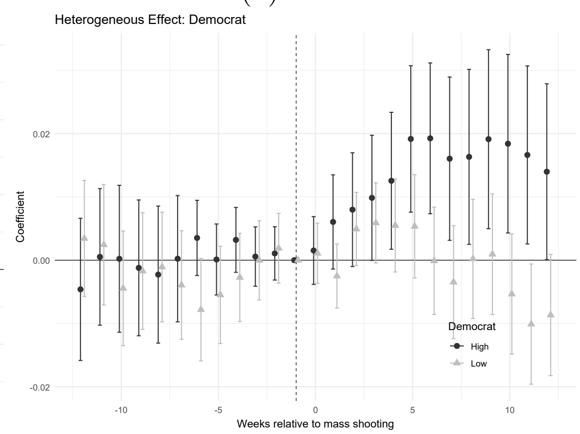
(a) Children



(b) Male



(c) Age



(d) Democrat

Figure A5: Event study: heterogeneous effect

Notes: This figure shows the heterogeneous dynamic effects. For each characteristic, the sample is split into two groups based on the median, and separate regressions are estimated for each subsample. The grey line represents the lower group, and the black line represents the higher group. The 95% confidence intervals are presented. All regressions are estimated using a PPML estimator and exclude counties within 500 km of the shooting county. All regressions include county-by-event, week-by-event, and state-by-week fixed effects. The standard errors are clustered at the county-by-event level.

Table A1: Local Impact of Mass Shooting on Gun Store Visits

	Visits (1)	Visitors (2)
Post	0.1129*	0.1126*
	(0.0681)	(0.0651)
County FE	Yes	Yes
Observations	2,780	2,780
Pseudo R^2	0.9604	0.9604

Notes: This table reports the local impact of mass shootings on gun store visits in the shooting county. All regressions are estimated using a PPML estimator. The dependent variable is the weekly number of gun store visits at the county level in Column (1), and the weekly number of unique gun store visitors in Column (2). All specifications include county fixed effects. Standard errors are clustered at the county level in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A2: Robustness Check: Restricting Event Intervals to $\geq$ 12 Weeks

	Visits					
	500 km		750 km		1000 km	
	(1)	(2)	(3)	(4)	(5)	(6)
Treat × Post	0.0129**	0.0107***	0.0163***	0.0128***	0.0192***	0.0130***
	(0.0050)	(0.0038)	(0.0056)	(0.0041)	(0.0063)	(0.0045)
County-Event FE	Yes	Yes	Yes	Yes	Yes	Yes
Week-Event FE	Yes	Yes	Yes	Yes	Yes	Yes
State-Week FE		Yes		Yes		Yes
Observations	3,130,915	3,129,976	2,760,341	2,759,450	2,329,087	2,328,300
Pseudo R^2	0.9752	0.9779	0.9752	0.9779	0.9753	0.9780

Notes: This table reports robustness checks that restrict the sample to events occurring at least 12 weeks apart within the same county, compared to the baseline specification where events are excluded only if they occur within 6 weeks. Counties within 500 km of the shooting county are excluded from the treated group (Columns 1–2), and the analysis is extended to 750 km and 1000 km thresholds (Columns 3–6). All regressions are estimated using a PPML estimator. The dependent variable is the weekly number of gun store visits at the county level. Specifications in Columns 1, 3, and 5 include county-by-event and week-by-event fixed effects, while Columns 2, 4, and 6 additionally include state-by-week fixed effects to allow within-state comparisons. Standard errors are clustered at the county by event level in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A3: Robustness Check: Keeping Only the First Event per County

	Visits					
	500 km		750 km		1000 km	
	(1)	(2)	(3)	(4)	(5)	(6)
Treat × Post	0.0112**	0.0090**	0.0147***	0.0110***	0.0167***	0.0106**
	(0.0051)	(0.0039)	(0.0057)	(0.0042)	(0.0064)	(0.0046)
County-Event FE	Yes	Yes	Yes	Yes	Yes	Yes
Week-Event FE	Yes	Yes	Yes	Yes	Yes	Yes
State-Week FE		Yes		Yes		Yes
Observations	2,889,924	2,888,985	2,535,497	2,534,606	2,129,067	2,128,280
Pseudo R^2	0.9758	0.9784	0.9759	0.9785	0.9761	0.9787

Notes: This table shows the robustness result of keeping the first event for each county. Counties within 500 km of the shooting county are excluded from the treated group (Columns 1–2), and the analysis is extended to 750 km and 1000 km thresholds (Columns 3–6). All regressions are estimated using a PPML estimator. The dependent variable is the weekly number of gun store visits at the county level. Specifications in Columns 1, 3, and 5 include county-by-event and week-by-event fixed effects, while Columns 2, 4, and 6 additionally include state-by-week fixed effects to allow within-state comparisons. Standard errors are clustered at the county by event level in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A4: Robustness Check: Excluding COVID-19 Period

	Visits					
	500 km		750 km		1000 km	
	(1)	(2)	(3)	(4)	(5)	(6)
Treat × Post	0.0158**	0.0142***	0.0203***	0.0168***	0.0247***	0.0176***
	(0.0062)	(0.0047)	(0.0069)	(0.0051)	(0.0079)	(0.0057)
County-Event FE	Yes	Yes	Yes	Yes	Yes	Yes
Week-Event FE	Yes	Yes	Yes	Yes	Yes	Yes
State-Week FE		Yes		Yes		Yes
Observations	2,360,727	2,359,788	2,068,887	2,067,996	1,729,940	1,729,153
Pseudo R^2	0.9732	0.9762	0.9730	0.9760	0.9731	0.9761

Notes: This table reports robustness checks that exclude observations potentially affected by the COVID-19 pandemic in 2021. We exclude the samples in 2021. Counties within 500 km of the shooting county are excluded from the treated group (Columns 1–2), and the analysis is extended to 750 km and 1000 km thresholds (Columns 3–6). All regressions are estimated using a PPML estimator. The dependent variable is the weekly number of gun store visits at the county level. Specifications in Columns 1, 3, and 5 include county-by-event and week-by-event fixed effects, while Columns 2, 4, and 6 additionally include state-by-week fixed effects to allow within-state comparisons. Standard errors are clustered at the county by event level in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.