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Financial Firepower: School Shootings and the Strategic Contributions of Pro-Gun PACs
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ABSTRACT

Fatal school shootings often spark support for stricter gun laws, threatening the gun lobby's influence and agenda. To prevent political fallout, do pro-gun Political Action Committees increase contributions after fatal school shootings? Leveraging a novel dataset of pro-gun PAC contributions and school shooting incidents, we implement a difference-in-differences design with staggered treatment adoption to estimate the causal effect of school shootings on contributions to House candidates. We find that pro-gun PACs increase contributions by 30.2% to candidates in districts with fatal school shootings, but show no significant response to non-fatal school shootings or other mass shootings. The temporal pattern reveals strategic behavior: contribution spikes emerge in the wake of fatal school shootings and in proximity to elections, with effects dramatically amplified as Election Day approaches; within two months of Election Day, contributions increase by 1,730%. These effects are concentrated in competitive districts (margins of 5%). Our findings provide robust evidence that pro-gun PACs deploy targeted financial contributions in response to school shootings, with the magnitude and timing suggesting a strategic counter-mobilization effort to maintain influence in affected districts when gun policy becomes locally salient and elections are near. Our findings underscore a gap in democratic accountability: while public opinion should drive policy change, organized interests with financial power can insulate political candidates from public pressure and obstruct its translation into legislative reform.

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I. Introduction

Few issues in American politics generate as much sustained controversy as gun violence, and none evoke deeper national anguish than the shooting of children in schools (Levine and McKnight 2020; Lowe and Galea 2017; Riehm et al. 2021; Rossin-Slater et al. 2020; Sharkey and Shen 2021). Despite the persistence of school shootings, meaningful firearm regulation remains elusive (Cook and Donohue 2017; Donohue et al. 2017; Donohue et al. 2022; Donohue 2023a; Donohue 2023b; Donohue et al. 2024a; Donohue et al. 2024b; Jay 2022; Kalesan et al. 2016; Leff and Leff 1981). Between 2000 and 2024, more than 500 school shootings resulted in at least one fatality, triggering widespread media coverage, heightened public attention, and intensified calls for reform (Parker et al. 2022; Rapa et al. 2024; Riedman 2025; Siders 2021). Yet rather than leading to significant policy change, these tragedies rarely result in more than empty promises. Why does a policy domain marked by overwhelming public concern so often end in stalemate?

The puzzle over the failure to achieve strong federal legislation designed to reduce the threat of gun violence is even more striking when one sees that many gun safety measures have very strong support in the populace. For example, over 90 percent of Americans favor the adoption of universal background checks,[1] and 79 percent favor increasing the minimum age for buying guns to 21 years old (Gallup 2022; Pew Research Center 2023). Moreover, according to Pew (2023) surveys, "[s]izable majorities also support banning high-capacity ammunition magazines that hold more than 10 rounds (66%) and banning assault-style weapons (64%)." How can measures that are widely used in other countries to reduce gun violence and that, in some cases, enjoy overwhelming support from the American public still fail to result in federal legislation?

We argue that the answer lies in the gun lobby's strategic financial mobilization directed

[1] In June 2022, 92 percent of Americans favored "requiring background checks for all gun sales" (Gallup 2022).

at congressional candidates.[2] When a fatal school shooting occurs, pro-gun Political Action Committees (PACs) face a credible threat: heightened media scrutiny and a spike in pro-regulation sentiment. We show that these shocks prompt pro-gun PACs to increase contributions—both donations and independent expenditures—to candidates representing the impacted congressional district.[3] In turn, this reinforces candidates' incentives to maintain the status quo. In sum, fatal school shootings induce a strategic counter-mobilization by pro-gun PACs to blunt the policy impact of any grassroots demands for gun safety regulation. Understanding this targeted financial response is crucial in shedding light on why legislators frequently resist gun safety measures and candidates resist adopting gun safety positions.

We provide the first systematic test of that proposition. Leveraging novel panel data on all House races from 2000 to 2024, we merge monthly PAC contribution data with event-dated records of school shootings. Our difference-in-differences (DiD) design identifies whether, and under what conditions, pro-gun PAC money flows into districts after a school shooting, and the extent to which the effect intensifies as Election Day nears.

This study makes three overarching contributions. Empirically, the study introduces a comprehensive and original panel dataset that couples incident-level gun violence with monthly PAC activity, enabling a more granular analysis than previous work. We establish that the gun lobby is acutely sensitive to and only responds financially to the types of gun violence that threatens its goals: school shootings are only a threat if they result in a fatality, the threat increases the closer to a Congressional election the shooting occurs, and the gun lobby ramps up its financial contributions only in House races that are contestable. Substantively, it

[2]Throughout the paper we refer to "candidates." Other than when explicitly made distinct, "candidates" refers to those individuals running for a House seat as either an incumbent or a challenger.

[3]Donation(s) refers to the legally capped direct donations of $5,000 per candidate per election cycle from any one given donor. Independent expenditure refers to contributions made to support a candidate indirectly but not made directly to a candidate and without any coordination with the candidate or the candidate's campaign team. Independent expenditures are uncapped. Independent expenditures are commonly referred to as "dark money" but are not the full universe of dark money. While much of independent expenditure can be found in FEC disclosures, another portion, made through 501(c) groups, is far more difficult to trace. Throughout the paper we use the term "contributions" because we aggregate donations and independent expenditures.

illuminates a mechanism—targeted political contributions—that helps explain the persistent disjuncture between public outrage and legislative inertia on gun policy.

Our findings carry important implications for policy. If pro-gun PACs reliably increase spending in the wake of school shootings, reforms aimed solely at shifting public opinion or pressuring legislators may not be enough on their own; the role of campaign finance must be addressed. More broadly, our results suggest that issue-specific tragedies can entrench, rather than erode, well-organized and well-funded policy coalitions, highlighting the outsize role of money in American politics in insulating officeholders and candidates against public pressure.

The paper proceeds as follows: section II situates our argument within existing work on mass shootings, public opinion change, and interest group strategy. Section III then details the data, identification strategy, and methods of our empirical approach. We then turn to results and robustness checks in section IV. Section V concludes with a discussion of the implications of our research for gun policy debates, the study of organized interests, and American democracy.

II. Background and Conceptual Framework

Mass shootings in the United States often spark intense public discourse on gun policy, yet their impact on political behavior remains contested. Public reaction to mass shootings is immediate and intense, typically producing short-term spikes in support for gun-control measures (Hassell et al. 2020; Newman and Hartman 2019; Semenza et al. 2023; Parker et al. 2017), yet these shifts generally fade as partisan attitudes reassert themselves (Sharkey and Shen 2021).

In the case of school shootings, the targeting of young children often provokes moral panic over gun safety and gun regulation among parents of school-age children. Certain segments of the public, the media, and politicians use school shootings to support a gun safety agenda, although such initiatives have not been successful in changing hearts and minds on gun regulations (Burns and Crawford 1999). The resulting shift in public opinion is often transient, as partisan identity quickly reasserts itself, limiting the long-term policy impact of these events (Sharkey and Shen 2021). Media coverage plays a crucial role in shaping public discourse, often stoking public fear and pushing for policy change, yet failing to sustain long-term shifts in activism or preferences (Fox et al. 2021; Porfiri et al. 2019).

Despite strong public concern, legislative action remains rare. Legislators, particularly those representing strong pro-gun constituencies or receiving significant financial support from gun rights organizations, are resistant to enacting stricter firearm regulations (Garcia-Montoya et al. 2022). Political responses often manifest symbolically through public statements, memorials, or the introduction of legislation unlikely to pass (Hassell et al. 2020). This gap between public opinion and legislative inaction highlights the importance of organized interests in structuring political incentives (Laschever and Meyer 2021). Moreover, these attitude shifts rarely translate into policy change, creating what some scholars identify as a representation gap in gun policy (Reny et al. 2023).

Public opinion polling consistently finds broad support among Americans for stricter gun laws. Polling shows that 57% of Americans support stricter laws covering the sale of firearms (Jones 2023), 64% of U.S. adults believe gun laws should be stricter in the future than they are today (Schaeffer 2024), and 72% support laws requiring gun owners to comply with safe storage rules (Crifasi et al. 2018; Johns Hopkins Center for Gun Violence Solutions). Yet, despite these widespread policy preferences, mass shootings rarely result in successful legislative reform. This pattern is evident in findings by Luca et al. (2020) who show that in the aftermath of high-profile mass shootings, there is a 15% increase in the introduction of gun safety legislation, but these regulatory measures are rarely enacted. In fact, in Republican controlled state legislatures, mass shootings are associated with an *increase* in laws loosening gun restrictions (Luca et al. 2020).

Studies of electoral accountability following mass shooting incidents provide mixed evidence of the effects on voter behavior. Beyond public attitudes, recent research by Hassell et al.

(2020) shows that school shootings, even when emotionally charged and highly visible, fail to produce measurable changes in voter turnout, registration, or incumbent accountability in subsequent elections. Hassell et al.'s (2020) findings suggest that electoral mechanisms are insufficiently incentivizing to create policy change regarding gun violence. This absence of electoral consequences highlights a gap in democratic accountability, creating space for interest groups, rather than voters, to exert influence on candidates and incumbents.

Interest group theory suggests that organizations do not merely respond to policy threats but actively mobilize resources to influence legislative outcomes (Heersink et al. 2021; Laschever and Meyer 2021). Pro-gun organizations, including the National Rifle Association (NRA), Gun Owners of America (GOA), and the National Shooting Sports Foundation (NSSF), have consistently used financial contributions as a way to sustain legislative inaction against gun safety measures (Spitzer 2020). Empirical research shows these organizations increase their activity in the form of lobbying when gun rights appear politically vulnerable, either due to legislative proposals or heightened public scrutiny following mass shootings (Cook and Donohue 2017; Luca et al. 2020). Existing scholarship confirms that pro-gun PACs direct funds toward candidates who support the Second Amendment and gun rights broadly, but there is no systematic analysis of whether these contributions increase in direct response to fatal school shootings (Foreman 2018; Grossman 2020; Kahane 1999; Lacombe 2019; Lacombe 2021; Langbein 1993; Laschever and Meyer 2021; Musa 2016; Richards 2017).

Research on campaign finance has established that financial contributions from interest groups are a primary means of exerting policy influence (Bruce and Wilcox 1998; Goss 2006; Panagopoulos and Bergan 2007; Persily et al. 2018). The pro-gun lobby significantly outspends gun safety advocates, and candidates receiving substantial financial backing from gun rights organizations consistently oppose restrictive firearm legislation (Garcia-Montoya et al. 2022). Pro-gun PACs have a substantial resource advantage and possess the capacity to engage selectively where they perceive gun rights to be under threat (Laschever and Meyer 2021). Our study builds on these findings by testing whether school shootings function as exogenous shocks that trigger measurable increases in PAC contributions.

Despite the extensive research on gun policy attitudes, there has been no systematic study of whether pro-gun PACs respond to school shootings by increasing financial contributions to aligned political candidates. By analyzing campaign finance data, school shooting data, and the timing of PAC contributions, this research assesses whether gun rights organizations respond strategically to mass shootings by mobilizing additional financial support for policymakers who oppose firearm restrictions. Understanding this dynamic is especially critical in evaluating how political financing, and particularly the role of money, influences gun policy debates and legislative inertia in the wake of mass casualty events.

We show that pro-gun PACs strategically increase contributions to House districts in the wake of fatal school shootings, with effects amplified by proximity to the next election. This mobilization operates through three mechanisms: first interest groups adopt a defensive posture when their core policy goals come under threat (Heersink et al. 2021; Laschever and Meyer 2021). School shootings with at least one fatality pose such a threat to pro-gun policy positions by opening the door to firearm regulation. Following from this logic, this threat-response mechanism predicts that pro-gun PACs will expand their financial support precisely when public sentiment shifts towards stricter gun safety regulation.

Second, campaign finance operates as policy insurance (Bruce and Wilcox 1998; Goss 2006). Pro-gun PACs use contributions not only to reward past behavior but also as forward-looking investments with the goal of preserving policy alignments in future periods of increased scrutiny. This insurance is valuable in congressional districts directly impacted by fatal school shootings, where representatives face local pressure to support and enact stricter gun safety regulation.

Third, interest groups strategically allocate resources to competitive districts where even modest financial support can have an outsized impact for electoral outcomes (Laschever and Meyer 2021). To that end, pro-gun PACs are expected to channel additional funds to districts after fatal school shootings, especially as elections approach.

Based on our framework, we test the following three hypotheses. First, pro-gun PACs will significantly increase financial contributions to districts where school shootings occur compared to districts without such incidents, but only when the school shooting results in a fatality. Second, the effect of fatal school shootings on pro-gun PAC contributions will increase in proximity to elections, with increases growing as the November election approaches. Third, the effect of fatal school shootings on pro-gun PAC contributions will be stronger in competitive districts (electoral margins $\leq$5%) than in safe districts.

Our study offers the first systematic analysis of pro-gun PACs' political spending in the aftermath of fatal school shootings. Prior work examines mass shootings' effects on public attitudes (Newman and Hartman 2019; Rogowski and Tucker 2019) or individual contributions to the NRA (Roemer 2023), but institutional aspects have been overlooked. By analyzing PAC contribution patterns before and after mass shootings, this research assesses whether gun rights organizations increase financial support to protect allied candidates from potential electoral consequences. This study extends the literature on interest group strategy, campaign finance, and policy responsiveness by providing new evidence on how financial mobilization operates in the aftermath of fatal school shootings. Through a careful analysis of PAC contributions to House candidates, this research clarifies the role of pro-gun interest groups in one way of shaping the firearm agenda and maintaining the status quo in U.S. gun policy.

III. Empirical Approach

A. Data

This study leverages a novel dataset on pro-gun and pro-Second Amendment Political Action Committee (PAC) contributions to candidates at the federal level. The NRA, its subsidiaries and off-shoots, and an assortment of additional PACs form the American pro-gun and pro-second amendment lobby. While some of these contributors are well known–such as the NRA affiliated PACs–many of the gun pro-gun PACs are opaque and do not lend themselves easily to systematic study. They use their funds, collected from wealthy benefactors and rank-and-file supporters alike, to support likeminded candidates, oppose candidates who speak out against gun rights, and push a pro-gun agenda in society.

Our study is focused on contributions *from* pro-gun PACs *to* House candidates. Our dataset contains all 41,769 pro-gun PAC contributions to every candidate for a federal elected office (House, Senate, and Presidency) from 2000 to 2024, with precise details regarding the contributor and recipient as well as the transaction.[4] Our data is sourced from OpenSecrets (2025) and contains both PAC donations limited to $5,000 per candidate per election cycle as well as independent expenditures by super PACs–which forms part of the universe of dark money. Contributions were matched to congressional districts and all records were aggregated to the district-month level.[5]

For incident data, we use the Riedman K-12 School Shooting Database (2025). The data is then filtered to our years of analysis (2000–2024) and filtered for incidents with at least one fatality.[6] Additionally, our data is filtered to include those with a reliability score of 2 or higher (out of 5) indicating that the incident is reported in at least one news story published by a network, cable, or online mainstream media source with a named author. With these filters, we have 503 incidents.[7]

School shooting data is then matched to congressional districts using UCLA congressional shapefiles for the 106th to the 114th Congress (January 3, 1999–January 3, 2017) (Lewis et al. 2017). For the 115th through the 118th Congress (January 4, 2017–December 31, 2024), school shooting incidents were matched using the R tigris package drawing on US

[4]For an exhaustive list of the Pro-Gun PACs featured in our data, please see Appendix E.

[5]Contribution data are matched to congressional districts in `R` by using fuzzy matching, string distance, and a name-normalization procedure leveraging FEC bulk records. For more details on this, please see Appendix D.

[6]We filter to these years, 2000–2024, because we have complete data for both our primary data sources—PAC contributions and congressional districts—as well as complete data for the appended covariates.

[7]In our data, there are a total of 662 school shooting fatalities from 2000 to 2024.

Census data. The incident data was aggregated to the district-month level, then balanced for months and all possible congressional districts, from 2000 to 2024. Districts that cease to exist or newly created districts due to redistricting are excluded to maintain consistency. This amounts to 166,584 district-month observations.[8]

Our final merged panel includes variables on congressional district, incident count, binary treatment variable, fatality count, total count of PAC contributions, total dollar amount of PAC contributions. We append covariates for demographics, education, unemployment, and household income at each district-month. Additionally, we append detailed House election results, including margin of victory and partisan vote shares, to control for district political dynamics. Each district is appended with the House results from the most recent election and the next election. These appended data are proxies for district ideology and competitiveness.

Our core analysis will focus on what moves the gun lobby to action: school shootings with at least one fatality reported. A district-month is classified as treated starting in the month a school shooting with at least one fatality occurs, with the treatment effect assumed to persist for 24 months. Our analysis restricts the data to competitive districts, defined as those districts with a $\leq$5% margin in the most recent House election. We also restrict the dataset to PAC contributions totaling at least $10,000 per month, thus focusing our analysis on large contributions and capturing the high-dollar sums that are more typical of the legally uncapped realm of independent expenditures by super PACs.

B. Identification Strategy

This study employs a difference-in-differences (DiD) framework with staggered treatment adoption. Our approach presupposes school shooting incidents as unpredictable and quasi-random events to estimate their causal effect on pro-gun PAC contributions. We perform two-way fixed-effect (TWFE) analyses as a baseline specification to control for time-invariant

[8]For additional details on the construction of the panel data and the variables, please see Appendix D.

factors for each district and factors common across districts. Our research design assumes that the impact of school shooting attenuates over time, lasting 24 months.[9]

Since the TWFE model assumes homogeneous treatment impact, recent scholarship has developed DiD models that account for heterogeneity of treatment effects and thereby relax the homogeneity assumption (Borusyak et al. 2024; Callaway and Sant'Anna 2021; de Chaisemartin and D'Haultfœuille 2020; Goodman-Bacon 2021; Imai and Kim 2019; Sun and Abraham 2021). Accordingly, we also apply methods developed by de Chaisemartin and D'Haultfœuille (2020; 2024), Liu et al. (2024), and Imai et al. (2023), all of which allow once-treated units to revert to the control group.[10] [11] Through these frameworks, we estimate an average treatment effect on the treated (ATT) based on different assumptions to assess how pro-gun PAC contributions respond in the aftermath of fatal school shootings.

Our identification strategy relies on the key assumption that, conditional on observed covariates and in the absence of treatment, districts that experience school shootings would have followed parallel trends in PAC contributions with districts that did not experience such events. Since this assumption cannot be directly tested, we provide several pieces of evidence to support its plausibility. First, we show extensive pre-trends in our event-study plots and confirm their parallel pre-treatment trends between treatment and control groups. Second, we implement placebo tests to check whether there is any pre-treatment trend on treated units. Third, we take into account unit-specific time trends to examine the parallel trends assumption further. Fourth, we apply the interactive fixed effects model to address unobserved time-varying confounders, thus achieving better parallel pre-trends fit (Liu et al. 2024). Fifth, we examine the characteristics of districts that potentially influence both the occurrence of school shooting and pro-gun PAC contributions to confirm a balanced distribution between

[9]For treatment effects by different treatment periods, see Table 17 in Appendix B.

[10]While other methods, such as Callaway and Sant'Anna (2021) and Sun and Abraham (2021) also take into account heterogeneous treatment effects, they assume "absorbing" treatment and do not allow for treatment reversal.

[11]The de Chaisemartin and D'Haultfoeuille (2024) method is hereafter known as "DCDH." The method put forth by Liu et al. (2024) is hereafter referred to as "imputation method." The method put forth by Imai et al. (2023) is hereafter referred to as "panel match."

treatment and control groups and to control for them in our DiD analyses. Lastly, as part of our robustness checks, we implement propensity score matching to further address potential imbalance in covariates between treatment and control groups.

C. Methods

We first use a TWFE model for our baseline analysis:

$$Y_{it} = \alpha_i + \delta_t + \beta\,\text{TreatmentWindow}_{it} + \mathbf{X}_{it}\,\gamma + \varepsilon_{it}, \tag{1}$$

where Y_{it} is the log of total PAC contributions or the count of contributions for district i in month t; α_i and δ_t are district and month fixed effects; $\text{TreatmentWindow}_{it}$ is a binary treatment indicator showing prior exposure to fatal school shootings within the past 24 months; and $\mathbf{X}_{it}$ is a vector of district-level controls.[12] [13] We estimate standard errors clustered at the district level.

Next, we implement the event study model to test for parallel pre-trends and capture dynamic treatment effects. The event study specification is:

$$\text{Average}_{it} = \alpha_i + \delta_t + \sum_k \beta_k\, D_{k(it)} + \mathbf{X}_{it}\,\gamma + \varepsilon_{it}, \tag{2}$$

where Average_{it} represents moving average for the past 3, 4, or 6 months of log dollar amount or number count of PAC contributions in district i in month t;[14] [15] α_i and δ_t are district and month fixed effects; and $\mathbf{X}_{it}$ is a covariate vector. The key component is $\sum_k \beta_k D_{k(it)}$, where $D_{k(it)}$ are indicator variables for each month k relative to school shooting incidents. These

[12] For a list of these controls, see Appendix D.

[13] We implement a robustness check by using different periods of treatment range (12-60 months), to find that our baseline results are robust. For its results, see Table 17 in Appendix B.

[14] To avoid pre-treatment periods that reflect any treatment effect, we don't use moving averages covering future periods. This approach estimates treatment effects more conservatively, while there is a lag until our estimates fully reflect monthly treatment effects.

[15] For the dollar amount outcome, we use the average of logged dollar amount over the six month period. For the count outcome, we use the average of counts over the six month period.

indicators allow us to trace out the dynamic response pattern of pro-gun PAC contributions before and after school shooting incidents.

IV. Results

Our primary analysis examines how fatal school shooting incidents affect two key outcomes: contribution amounts and number of contributions from pro-gun PACs to U.S. House candidates in competitive districts (margin $\leq$5%). Table 1 displays descriptive statistics, confirming that background characteristics are well balanced between treated (N=268) and control observations (N=7,510), supporting the assumption of conditional exchangeability. The similar distribution of demographic characteristics, education and economic indicators, and political factors across treatment and control groups strengthens causal inference in our DiD design.[16] [17]

A. Major Findings

1. Gun Lobby Targets Competitive House Elections After Fatal School Shootings

Our baseline TWFE estimates (Table 3) reveal that pro-gun PAC contributions to U.S. House candidates increase by 30.2% (p=0.008) in competitive districts following a fatal school shooting (one or more fatalities). This effect is robust to alternative specifications, including different contribution thresholds (the original $10,000 in the baseline as well as $5,000) and whether measured as dollar amounts or contribution counts.[18]

[16] For the details of the distribution of each characteristic between treatment and control groups, see violin plots (Figures 14-16) in Appendix A.

[17] We implement propensity score matching to demonstrate a more balanced distribution between treatment and control groups. See Figure 20 and Table 14 in Appendix B.

[18] Our baseline analyses focus on large dollar contributions ($\geq$$10,000) to capture the impact of uncapped independent expenditures and concentrated funding by PACs and super PACs. The results covering all PAC contributions, including small dollar contributions, show non-significant results. We determine that large-sized contributions are indicative of a concerted effort by PACs to direct funding in support of a given candidate in a time-sensitive manner after a fatal school shooting, while small dollar contributions are more predictable, cyclical, and routine.

The result remains robust when compared with non-fatal school shootings. When filtered by number of fatalities in a given incident, the effect on PAC contributions clearly emerges in the presence of school shootings with exactly one fatality (Table 2). Incidents with one fatality generate a 40.2% increase in contributions (logged effect: 0.338, p=0.001), while incidents with no fatalities show a non-significant effect (logged effect: 0.022, p=0.797).[19] This pattern demonstrates that single-fatality incidents create the threshold conditions for mobilizing PAC resources and generating sufficient visibility to motivate action.

We also examine the heterogeneity of impacts across different levels of district competitiveness (Table 4). The treatment effect is strongest (0.264, p=0.008) in competitive districts with voting margins between -5% and +5%. This effect becomes statistically insignificant in less competitive districts, regardless of whether they lean Republican (10% to 5%, p=0.834; 20% to 10%, p=0.418) or Democratic (-10% to -5%, p=0.809; -20% to -10%, p=0.076). Such patterns underscore that PACs concentrate resources where marginal political returns are greatest.

2. The Gun Lobby is Most Active When School Shootings Occur Close to Elections

The temporal proximity of school shootings to elections emerges as the most powerful determinant of pro-gun PAC contribution patterns. Contribution amounts follow a clear temporal gradient, with effect sizes increasing as incidents occur closer to Election Day (Figure 2). This figure shows that when limiting treatment to school shootings close to the next House election, those incidents generate much larger increases in contributions until the elections. Results vary based on how close incidents occur to House elections. Shootings within two months of an election trigger a 1,730% surge in contributions (logged effect: 2.907, p=0.000), with diminishing but substantial effects at three months (934% increase; 2.336, p=0.000), and four months (598% increase; 1.943, p=0.000) thresholds. Beyond eight months,

[19] As opposed to incidents with exactly one fatality, incidents with two or more fatalities yield a non-significant estimate (logged effect: -0.083, p=0.725). This is probably the result of the limited number of such incidents; approximately 80% of all fatal school shooting cases in competitive districts generate exactly one fatality.

point estimates are significant but smaller, indicating that timing isn't just a detail–it's central to understanding how PACs respond.

We assess the robustness of our findings by different specifications. First, we observe a similar temporal pattern on the count of PAC contributions (Figure 3): the number of contributions increases substantially in response to school shootings that occur closer to the next election, with the largest effects concentrated in the final two to four months. These results are also robust when examining the additional impact of school shootings close to elections, compared with those more distant from elections (Table 5). As the last robustness check, we add an additional covariate showing the number of months to an election, and find our results remain robust.[20]

Taken together, these findings reveal a strategically timed, district-targeted deployment of pro-gun PAC money. Pro-gun PACs channel substantially more resources to vulnerable House districts in the aftermath of fatal school shootings, and the magnitude of this response escalates sharply as Election Day approaches.

Table 1: Descriptive profile of U.S. House districts with and without fatal school shootings, 2000–2024.

	History of Incidents (0 = Control, 1 = Treatment)	
Variable	0 N = 7,510	1 N = 268
Ratio of Bachelor's degree holders (%)	29.7 (10.0)	29.8 (9.3)
White Population (%)	76.7 (15.1)	65.5 (17.0)
Black Population (%)	8.3 (8.9)	9.8 (5.4)
Asian Population (%)	4.4 (5.1)	6.7 (9.7)
Other Races' Population (%)	4.4 (5.3)	7.9 (6.1)
Unemployment rate (%)	6.1 (2.3)	5.9 (2.5)
Logged median annual income	11.0 (0.3)	11.1 (0.2)
Estimated Household Firearm Possession (%)	32.1 (11.2)	32.0 (10.6)
Margin for Rep. candidates at the latest election (%)	–0.4 (2.8)	–0.9 (3.1)
Republican incumbent (1=Yes, 0=No)		
0	4,129 (55%)	161 (60%)
1	3,381 (45%)	107 (40%)
US President (1=Rep., 0=Dem.)		
0	4,420 (59%)	218 (81%)
1	3,090 (41%)	50 (19%)

*Values for continuous variables are presented as mean (standard deviation). Treatment includes from the month of incidents to 23 months after.

[20]For the details of the last robustness check, see figures 21-22 in Appendix B.

Table 2: The effect of school shootings on gun lobby contributions in competitive districts by number of fatalities, 2000–2024.

	0 or ≥ 1	0 or ≥ 1 (Number outcome)	0, 1, or ≥ 2	0, 1, or ≥ 2 (Number outcome)
Incidents with no killed	0.022 (0.084)	0.000 (0.008)	0.025 (0.084)	0.001 (0.008)
	$p = 0.797$	$p = 0.958$	$p = 0.767$	$p = 0.923$
Incidents with at least 1 killed	0.261 (0.102)	0.026 (0.010)		
	$p = 0.011$	$p = 0.011$		
Incidents with exactly 1 killed			0.338 (0.103)	0.034 (0.010)
			$p = 0.001$	$p = 0.004$
Incidents with 2 or over killed			-0.083 (0.237)	-0.008 (0.023)
			$p = 0.725$	$p = 0.719$
Num. Obs.	7,778	7,778	7,778	7,778
Fatality Dummy	0 or ≥ 1	0 or ≥ 1	0, 1, or ≥ 2	0, 1, or ≥ 2
Outcome	Dollar	Number	Dollar	Number
Variables Excluded (multicollinearity)	None	None	None	None

*Baseline covariates include ratios of bachelor holders, of black, and of white; unemployment rate, logged median income, incumbency in the previous House election, and estimated household firearm possession rate, unless removed due to multicollinearity.

Table 3: The effect of school shootings on gun lobby contributions in competitive districts with alternative contribution thresholds and outcome metrics, 2000–2024.

	All Data	5k	Basic filtering (10k)	Number Outcome; All Data	Number Outcome; 5k	Number Outcome; 10k
Treatment	-0.003 (0.021)	0.326 (0.138)	0.264 (0.099)	-0.002 (0.003)	0.033 (0.014)	0.026 (0.010)
	$p = 0.889$	$p = 0.019$	$p = 0.008$	$p = 0.529$	$p = 0.022$	$p = 0.008$
Num.Obs.	128,686	7,778	7,778	128,686	7,778	7,778
Outcome	Dollar	Dollar	Dollar	Number	Number	Number
Minimum Contribution Threshold	$1	$5,000	$10,000	$1	$5,000	$10,000
Variables Excluded (multicollinearity)	None	None	None	None	None	None

*Baseline covariates include ratios of bachelor holders, of black, and of white; unemployment rate, logged median income, incumbency in the previous House election, and estimated household firearm possession rate, unless removed due to multicollinearity.

Table 4: The effect of school shootings on gun lobby contributions across House district partisan margins, 2000–2024. Gun lobby contributions rise by a highly statistically significant 26.4 log points (or 30 percent) in competitive districts decided by five or fewer points.

	$-20 \sim -10$	$-10 \sim -5$	$-5 \sim 5$ (Basic Filtering)	$5 \sim 10$	$10 \sim 20$
Treatment	0.084 (0.047)	−0.026 (0.107)	0.264 (0.099)	−0.029 (0.139)	0.078 (0.096)
	$p = 0.076$	$p = 0.809$	$p = 0.008$	$p = 0.834$	$p = 0.418$
Num. Obs.	8 162	3 652	7 778	4 120	11 658
Voting Margin at the previous HoR election	$-20 \sim -10$	$-10 \sim -5$	$-5 \sim 5$	$5 \sim 10$	$10 \sim 20$
Variables Excluded (multicollinearity)	rep_incumbent_before	rep_incumbent_before	None	rep_incumbent_before	rep_incumbent_before

*Baseline covariates include ratios of bachelor holders, of black, and of white; unemployment rate, logged median income, incumbency in the previous House election, and estimated household firearm possession rate, unless removed due to multicollinearity.

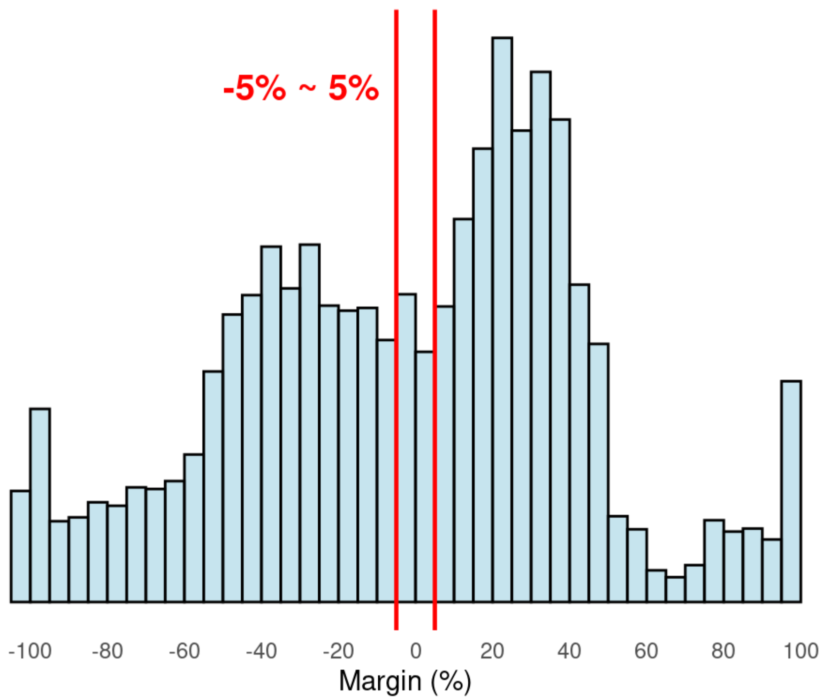

Figure 1: Distribution of congressional election margins, highlighting competitive races, 2000–2024. U.S. House general election margins, with competitive races defined as those decided by 5 percentage points or fewer (–5% to +5%, marked in red). These closely contested districts (6.11% of total) are central to our analysis of the gun lobby's strategic contributory behavior.

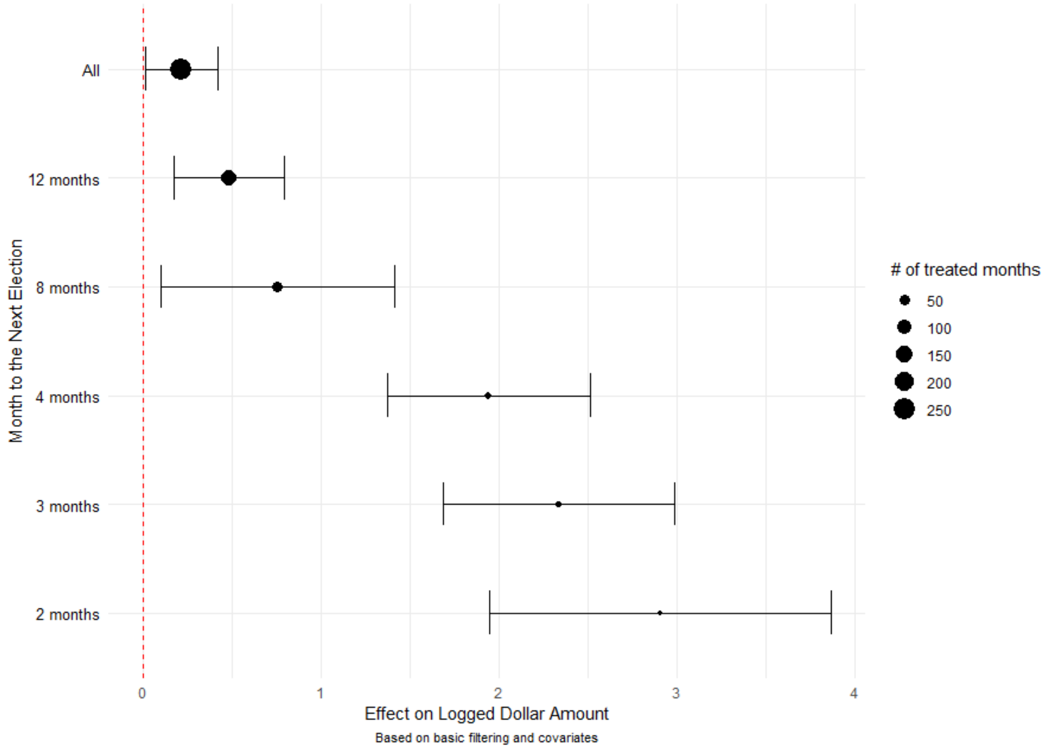

Figure 2: Baseline DiD estimates of the effect of fatal school shootings on logged dollar PAC contributions, by incident timing and treatment window, 2000–2024. Gun lobby political contributions grow dramatically if the fatal school shooting occurs close in time to House elections. The 2.907 point estimate for shootings within 2 months of the election represents an increase in dollar contributions of 1,730 percent.

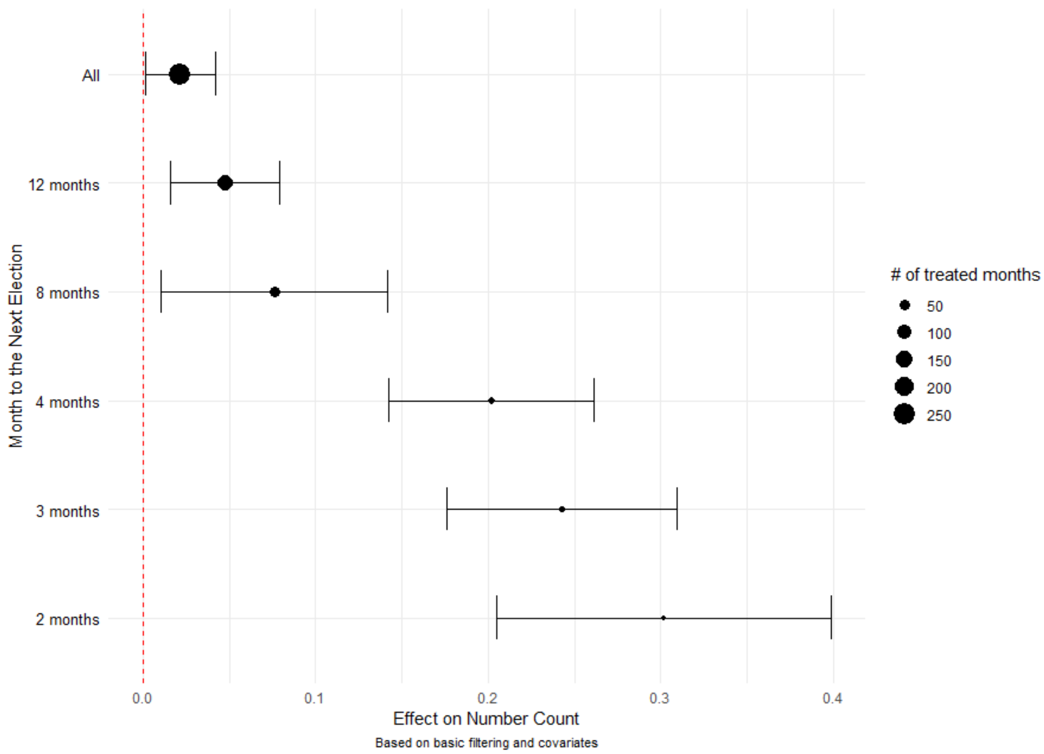

Figure 3: Baseline DiD estimates of the effect of school shootings on PAC contribution counts, by incident timing and treatment window, 2000–2024. Results use contribution counts as the outcome and reflect variation in timing thresholds and treatment duration.

Table 5: The effect of school shootings on gun lobby contributions (in log dollars) in competitive districts, showing the sensitivity of the estimates to how close incidents occur before House elections, 2000–2024.

	≤ 4	≤ 3	≤ 2	≤ 4 or > 4	≤ 3 or > 3	≤ 2 or > 2
Incidents within 4 months or closer	1.943 (0.290)			1.963 (0.291)		
	$p = 0.000$			$p = 0.000$		
Incidents within 3 months or closer		2.336 (0.333)			2.356 (0.338)	
		$p = 0.000$			$p = 0.000$	
Incidents within 2 months or closer			2.907 (0.489)			2.916 (0.486)
			$p = 0.000$			$p = 0.000$
Distant incidents (>4, >3, or >2)				0.051 (0.120)	0.068 (0.129)	0.059 (0.124)
				$p = 0.670$	$p = 0.600$	$p = 0.636$
Num.Obs.	7,778	7,778	7,778	7,778	7,778	7,778
Treatment Restrictions on Months	4	3	2	4	3	2
Variables Excluded (multicollinearity)	None	None	None	None	None	None

Baseline covariates include ratios of bachelor holders, of black, and of white; unemployment rate, logged median income, incumbency in the previous House election, estimated household firearm possession rate, unless removed due to multicollinearity.

B. Robustness Checks

Causal interpretation of our baseline TWFE model rests on the dual assumptions of parallel trends and homogeneous treatment effects across districts and months. Following from that logic, we first implement four core robustness checks on the parallel trends assumption: (I), a TWFE event study; (II), placebo tests on both dollar and number outcomes; (III), unit-specific time trends test using interaction terms, and; (IV), interactive fixed effects analysis.[21] Then, to account for heterogeneity treatment effects, we implement: (V), DCDH and imputation method for overall treatment effects, and; (VI), DCDH, imputation method, and panel match to estimate dynamic effects through event study. As an additional robustness check for spillover effect, we test for the same impact for statewide offices to highlight the local impact of school shootings. An additional battery of robustness checks appear in Appendix C.

1. TWFE Event-Study and Placebo Tests

We first examine pre-treatment trends through an event-study plot. Our event study estimates monthly effects for an 18-month period leading up to a school shooting incident. We do this by aggregating all observations' pre-trends history to either treatment or control groups. Coefficient estimates for every pre-treatment month fluctuate around zero, and none of the 95 percent confidence intervals excludes zero, confirming that there is no consistent pattern of differences in PAC contributions between treatment and control districts before shootings occurred (Figure 4). This clean pre-trend pattern supports the parallel-trends assumption and the interpretation of shootings as a random treatment. Moreover, the stable pre-trend pattern implies that there is no anticipation on behalf of the PACs in the months leading up to incidents.

We conduct an additional placebo test by artificially assigning treatment during pre-treatment placebo periods to evaluate whether the placebo effect in those periods is statistically

[21]For the results of the interactive fixed effects model, see Table 18 and Figures 33-34 in Appendix C.

distinguishable, as well as whether the placebo effect is statistically close enough to zero in an equivalence test (Liu et al. 2024). We assign the period within six months before a school shooting as placebo and no effect is produced in either outcome (Figures 5-6). The placebo test shows p=0.614 for logged dollars and p=0.596 for counts, supporting the absence of no pre-treatment bias. The equivalence tests produce p<0.001 in both outcomes, statistically confirming that coefficients over the placebo period are close enough to zero. The combination of these two placebo tests supports our parallel trends assumption in our TWFE analyses.

2. Unit-Specific Time Trends

We further test the validity of the parallel pre-trend assumption by accounting for unit-specific time trends (Hassell and Holbein 2025). We use interaction terms between unit fixed effects and time fixed effects to mitigate bias resulting from each unit's trend over a pre-treatment period.[22] However, redistricting of House districts occurs at each decennial census — solely adding interaction terms cannot accurately grasp each unit's time trend.

To solve this issue, we use each state's unique time trend, which is not affected by redistricting. First, we perform a robustness check by using different clustering criteria; our baseline setting is district-level, and we test state-level as well as no clustering and double-clustering with time (Table 6). We find that state-clustering shows almost the same results as district-clustering (p=0.008 for district clustering and p=0.009 for state-clustering). This implies that districts in each state show similar time trends. To further check the validity of our analysis using state-based time trends, we implement an additional robustness check using state fixed effects instead of district fixed effects (Table 7). We find that using state fixed effects shows robust positive impacts of school shooting on PAC contributions with either outcome; for logged dollar amount the results were 0.188 (p=0.007) and for count, 0.026 (p=0.008). These robustness checks validate our approach where we use each state's

[22] In addition to the analysis where we add interaction terms, we also implement the interactive fixed effects model to account for each unit's heterogeneous time-varying characteristics and to further validate the parallel trend assumption (Liu et al. 2024). See Table 18 and Figures 33-34 in Appendix C for its results.

unique time trend, successfully avoiding the impact of redistricting.

Next, we consider state-specific time trends by adding interaction terms between state fixed effects and month fixed effects.[23] We implement two patterns, linear and quadratic time trends (Table 8). Both patterns show similar positive results: 0.180 (p=0.005) for quadratic time trend and 0.138 (p=0.017) for linear time trend. These results show robustness when using number count instead of dollar amount (0.018 (p=0.004) for quadratic and 0.015 (p=0.013) for linear). This consistency across multiple patterns confirms that unique state time trends do not drive results, and our findings are not indicative of pre-existing patterns in contribution dynamics across different jurisdictions.

3. Heterogeneous Treatment Effects

To relax the TWFE's homogeneity assumption and account for heterogeneity treatment effects, we implement DCDH and imputation method, which allow treatment units to return to untreated status after two years to capture the attenuation of the impact. We use both methods here, since each method has different advantages; DCDH only requires the parallel trend assumption instead of strong exogeneity, and the imputation method captures heterogeneity for both unit and time whereas DCDH only accounts for time heterogeneity. While the imputation method requires a strong exogeneity assumption, our interactive fixed effects analysis supports the validity of the assumption.[24] Following from this, we conduct two additional approaches for overall impact and monthly impact.

For overall impact of school shootings on PAC contributions, we find that the DCDH model yields positive and significant effects for both dollar amounts (0.336, p=0.001) and contribution counts (0.033, p=0.001) (Table 9). Our imputation approach shows similar

[23]Our dataset includes over 400 districts; adding interaction terms dramatically increases the number of covariates. Thus, based on the assumption that each state's unique time trend is addressed by these interaction terms, we remove other control variables. (Hassell and Holbein 2025; Roemer 2023).

[24]Our interactive fixed effects model shows that adding no unobserved factors minimizes MSPE compared with the cases where we add 1 or more unobserved factors (Liu et al. 2024). For the model's results, see Table 18 and Figures 33–34 in Appendix C.

results, with coefficients of 0.281 (p=0.004) for dollar amounts and 0.028 (p=0.004) for contribution counts (Table 9).

We further examine dynamic treatment effects using an event study approach based on DCDH, imputation method, and panel match.[25] The event-study plots, regardless of the method or specification used, show stable pre-treatment trends near zero, and significant positive post-treatment impacts.[26] When looking at different moving average ranges for the DCDH model (three, four, and six months), we observe that all three time ranges show near-zero, stable pre-trends, as well as positive and significant results for post-treatment impacts (Figure 7). We also find that when using a six month moving average as an outcome for any model, all three methods (DCDH, imputation method, and panel match) show similar and robust positive results (Figure 8). These analyses relaxing the TWFE's homogeneity assumption show that school shooting has an overall positive impact on pro-gun PAC contributions even when considering heterogeneous treatment effects.

4. Impact on State-wide Elections

We analyze whether contributions to state-wide elections show similar patterns to House races, to test for state-wide spillover effects. To that end, we use data regarding federal (U.S. Senators) and state-level political offices (governors and state attorneys general), all of which are elected through state-level elections (Table 10). We found contributions to these state-wide elections saw no significant impact of school shootings (p-values 0.123-0.820), indicating no spillover effects to state-wide races.[27] This pattern highlights pro-gun PAC's targeted response focused on the district where the shooting occurred, rather than broadly increasing contributions to all state-wide offices.

[25]For the results of the panel match, see Figures 31-32 in Appendix C.

[26]For results not mentioned in the main text, see Figures 24-25 and 27-32 in Appendix C.

[27]Here, our findings are about the impact of school shootings (in a House district) on state-wide pro-gun PAC contributions. While we have already shown that adopting state-level fixed effects or clustering by state is not impactful for our results, that analysis pertained to similarities between overall state-wide time trends and district-level time trends, not to the specific effects of school shootings.

5. Conclusion of Robustness Checks

Collectively, our robustness checks establish that: (I) pre-treatment trends are parallel between treatment and control groups; (II), the post-treatment estimates are not explained by the continuing impact of pre-trends; (III), the results persist when we account for state-specific time trend; (IV), each unit's time-varying characteristics do not bias the estimates; (V), overall treatment effects are positive and significant under both DCDH and imputation methods; (VI), monthly effects are also significant under DCDH, imputation method, and panel match, and; (VII), no similar effects emerge for statewide offices, implying no state-wide spillover effects. The consistency and stability of our results across a wide range of specifications and methodologies provide unequivocal support for the robustness of our findings.

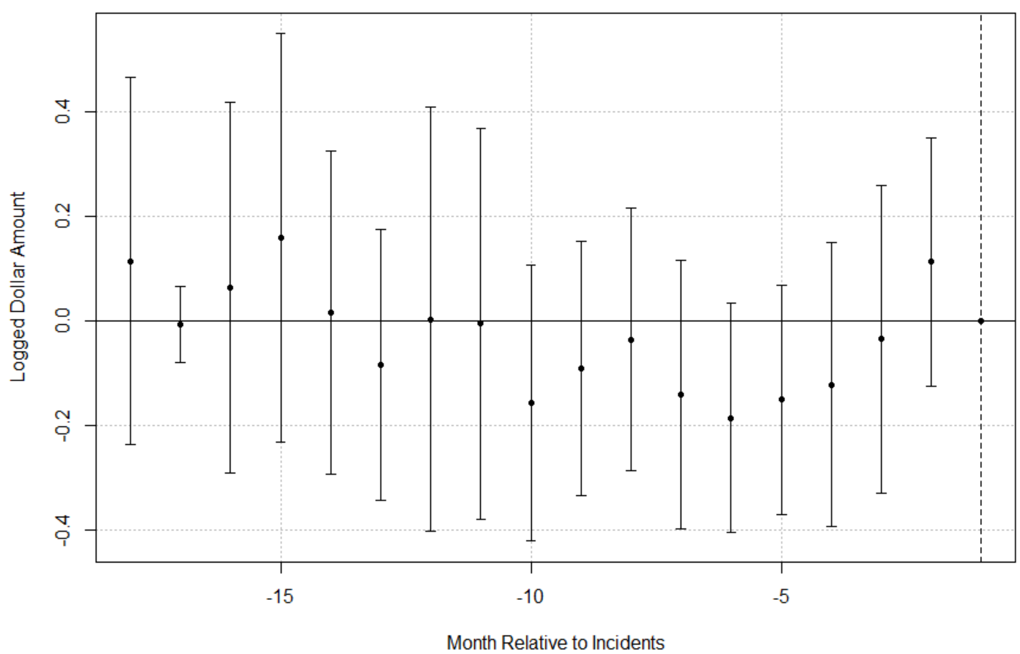

Figure 4: Event-study plot of school shootings' effect on logged dollar PAC contributions to House candidates, 2000–2024. No significant pre-treatment trends are observed, supporting the parallel trends assumption.

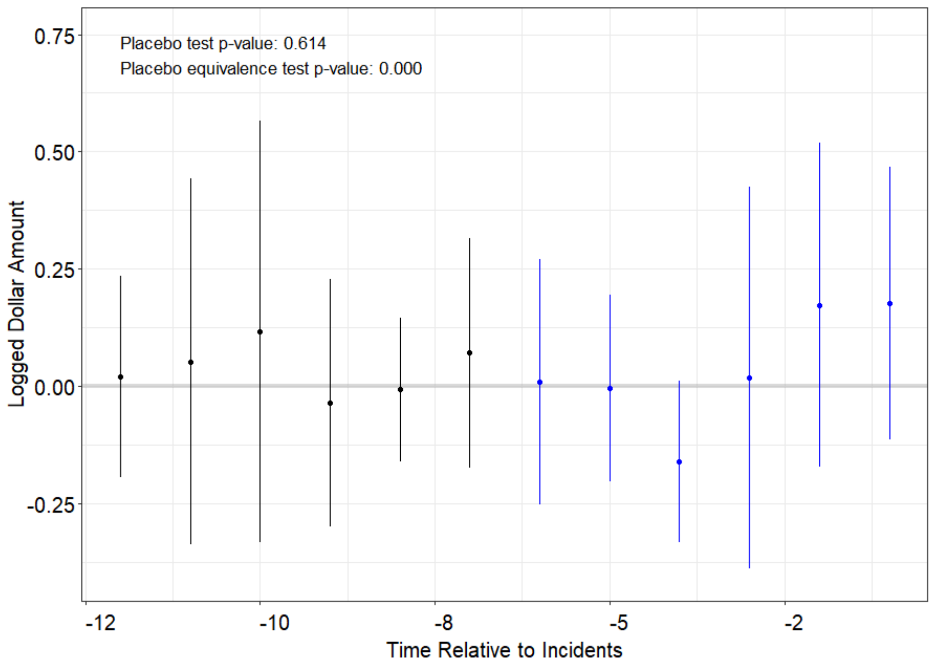

Figure 5: Placebo test showing non-significant effect on logged PAC dollars in pre-treatment period, 2000–2024. Estimates from the baseline DiD model using placebo treatments within six months before incidents (corresponding to blue vertical confidence intervals) show no statistically significant treatment effects and these effects are close enough to zero, supporting the parallel trends assumption.

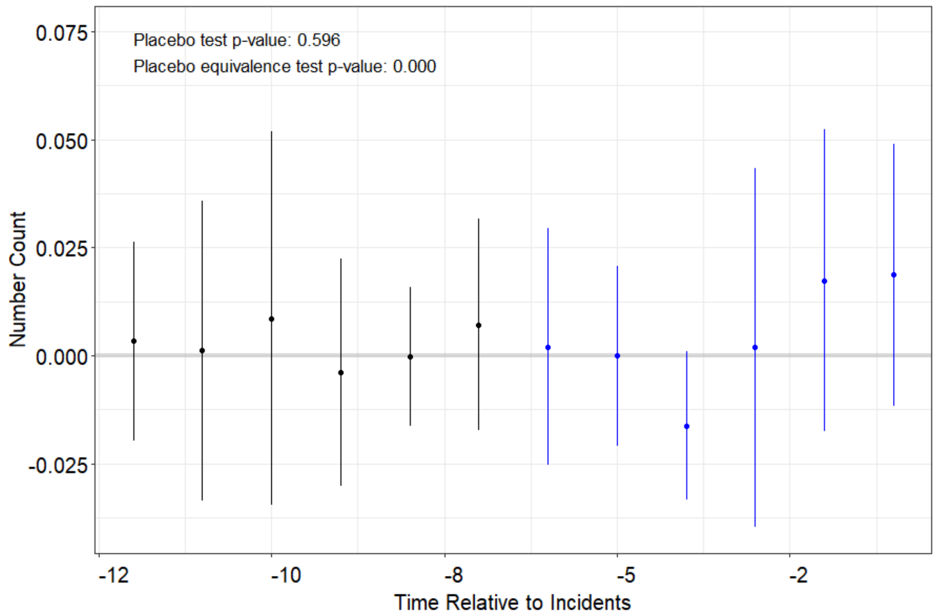

Figure 6: Placebo test showing no effect on contribution counts in pre-treatment period, 2000–2024. Estimates using contribution counts as the outcome variable also show non-significant and close to zero treatment effects within six months before incidents (corresponding to blue vertical confidence intervals), with placebo treatment tests supporting the parallel trends assumption.

Table 6: The effect of school shootings on gun lobby contributions (in log dollars) in competitive districts, 2000–2024; estimates remain robust when standard errors are clustered by district, clustered by state, double-clustered by unit and time, or left unclustered.

	No clustering	District	District+Month	State	State+Month
Treatment	0.264 (0.093)	0.264 (0.099)	0.264 (0.103)	0.264 (0.096)	0.264 (0.104)
	$p = 0.004$	$p = 0.008$	$p = 0.011$	$p = 0.009$	$p = 0.014$
Num. Obs.	7,778	7,778	7,778	7,778	7,778
Clustering	No	District	District+Month	State	State+Month
Variables Excluded (multicollinearity)	None	None	None	None	None

*Baseline covariates include ratios of bachelor holders, of black, and of white; unemployment rate, logged median income, incumbency in the previous House election, estimated household firearm possession rate, unless removed due to multicollinearity.

Table 7: The effect of school shootings on gun lobby contributions in competitive districts, 2000–2024; comparative results from district fixed-effects models with those from state fixed-effects models.

	Baseline (District FE)	State FE	Number Outcome	State FE; Number Outcome
Treatment	0.264 (0.099)	0.188 (0.069)	0.026 (0.010)	0.020 (0.007)
	$p = 0.008$	$p = 0.007$	$p = 0.008$	$p = 0.004$
Num. Obs.	7,778	7,778	7,778	7,778
Unit Fixed Effects	District	State	District	State
Outcome	Dollar	Dollar	Number	Number
Variables Excluded (multicollinearity)	None	None	None	None

*Baseline covariates include ratios of bachelor holders, of black, and of white; unemployment rate, logged median income, incumbency in the previous House election, and estimated household firearm possession rate, unless removed due to multicollinearity.

Table 8: The effect of school shootings on gun lobby contributions in competitive districts estimated using TWFE models that incorporate state-specific linear or quadratic time trends, 2000–2024.

	Quadratic	Linear	Quadratic; Number Outcome	Linear; Number Outcome
Treatment	0.180 (0.061)	0.138 (0.056)	0.018 (0.006)	0.015 (0.006)
	$p = 0.005$	$p = 0.017$	$p = 0.004$	$p = 0.013$
Num. Obs.	7,778	7,778	7,778	7,778
Time Trend per Unit	Quadratic	Quadratic	Linear	Linear
Outcome	Dollar	Number	Dollar	Number
Variables Excluded (multicollinearity)	None	None	None	None

*Includes interaction terms between states and time (or squared time), instead of baseline covariates.

Table 9: The effect of school shootings on gun lobby contributions in competitive districts, 2000–2024; presenting the overall treatment effects in the DCDH model and the imputation method assuming heterogeneity.

	DCDH; Dollar Outcome	DCDH; Number Outcome	Imputation; Dollar Outcome	Imputation; Number Outcome
Treatment	0.336 (0.152)	0.033 (0.015)	0.281 (0.098)	0.028 (0.010)
	$p = 0.001$	$p = 0.001$	$p = 0.004$	$p = 0.004$
Model	DCDH	DCDH	Imputation	Imputation
Outcome	Dollar	Number	Dollar	Number

*Baseline covariates include ratios of bachelor holders, of black, and of white; unemployment rate, logged median income, incumbency in the previous House election, and estimated household firearm possession rate, unless removed due to multicollinearity.

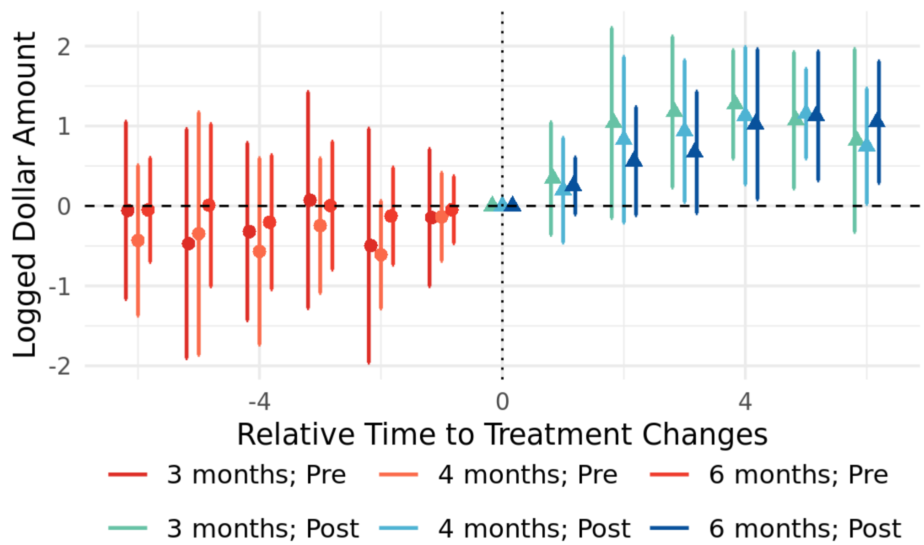

Figure 7: Estimated effect of school shootings on logged dollar PAC contributions using the DCDH event study with 3, 4, and 6 month moving-average outcomes, 2000–2024. The plot compares dynamic treatment effects based on different outcome timing windows.

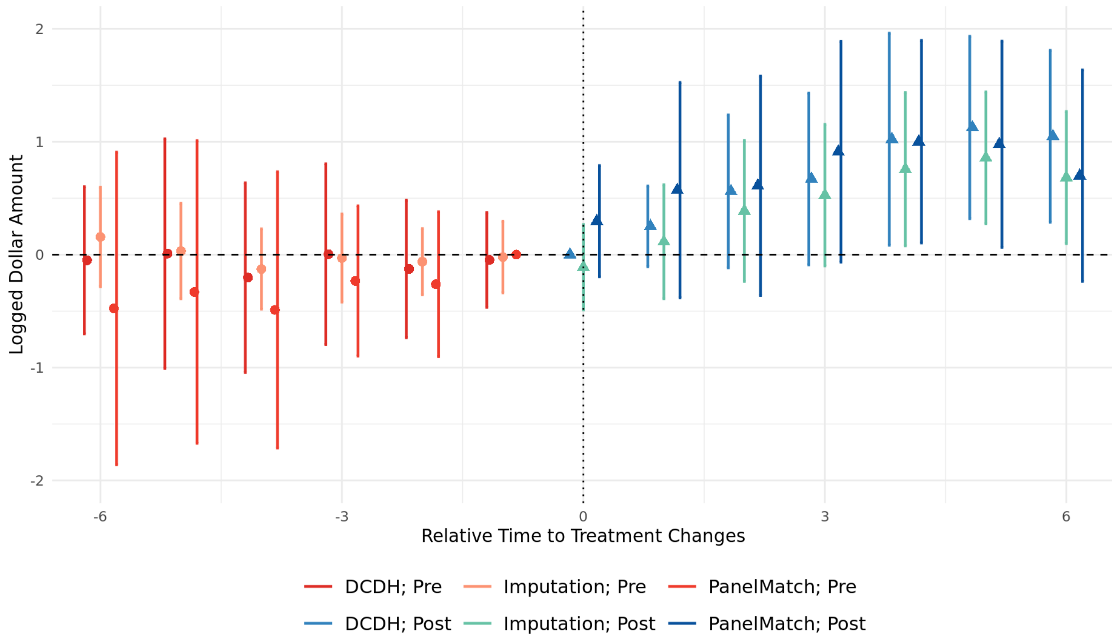

Figure 8: Estimated effect of school shootings on logged dollar PAC contributions using DCDH, imputation method, and panel match with 6 month average outcome and covariates, 2000–2024. This specification applies log transformation to multi-month average contribution values.

Table 10: The effect of school shootings on gun lobby contributions (in log dollars) in competitive districts, assessing possible spillovers to statewide offices, such as U.S. Senate, governor, and state attorney general, 2000–2024.

	Senator Baseline	Senator Number Outcome	Governor Baseline	Governor Number Outcome	Attorney General Baseline	Attorney General Number Outcome
Treatment	-0.074 (0.074)	-0.006 (0.006)	-0.026 (0.038)	-0.000 (0.001)	0.041 (0.026)	0.001 (0.001)
	$p = 0.320$	$p = 0.339$	$p = 0.504$	$p = 0.820$	$p = 0.123$	$p = 0.334$
Num. Obs.	15,000	15,000	15,000	15,000	12,900	12,900
Recipient	Senator	Senator	Governor	Governor	Attorney General	Attorney General
Outcome	Dollar	Number	Dollar	Number	Dollar	Number
Variables Excluded (multicollinearity)	None	None	None	None	None	None

*Baseline covariates include ratios of bachelor holders, of black, and of white; unemployment rate, logged median income, and estimated household firearm possession rate, unless removed due to multicollinearity.

V. Discussion

A. Identifying the Congressional Strategy of the Gun Lobby

Our findings provide the first systematic evidence that pro-gun PACs strategically contribute to U.S. House races following fatal school shootings, revealing how the gun lobby mobilizes financially in ways that directly counteract public demand for reform. We find a 30.2% mean increase in contributions to districts that experience these incidents. This pronounced effect in competitive districts–those with electoral margins of 5% or less in the most recent House race–indicates that PACs target races where their financial influence can have the greatest impact. We also find that the gun lobby is most concerned about the impact of school shootings that occur within two months of Election Day, leading to an increase of 1,730% in the total dollar amount of contributions. This intentional and time-sensitive response pattern aligns with our theoretical framework: pro-gun PACs adopt a defensive posture when their policy goals are threatened, deploy contributions as a form of policy insurance, and direct resources to competitive districts where electoral stakes are highest.

Our findings advance three central themes. First, fatal school shootings trigger a strategic and precisely timed mobilization of pro-gun PAC resources, concentrated in competitive districts and amplified when incidents take place closer to elections. Second, this pattern reflects a broader logic of "strategic insulation": the use of financial power to shield candidates from electoral backlash when public demand for reform is high. Third, these dynamics expose a fundamental breakdown in democratic responsiveness, showing how organized interests can counteract public outrage and preserve policy inertia even in the wake of profound tragedy. These insights shape the theoretical and normative implications we develop below.

Pro-gun PACs appear acutely aware of the reputational and electoral vulnerabilities created by school shootings. These events reliably spark spikes in public attention, media coverage, and mobilization, especially at the local level. The increase in both the volume and frequency of contributions suggests that PACs perceive such moments as threats to their policy agenda and respond with defensive and targeted spending. The timing and precision of these contributions, targeting competitive districts in the weeks before elections, reveals a strategy designed to protect candidates from constituent backlash.

Beyond electoral consequences, these incidents may also endanger the gun lobby's broader economic interests. Unlike urban crime or generalized fear, which can be harnessed to drive gun sales, school shootings are politically and emotionally toxic events that tend to galvanize calls for regulation rather than increased firearm ownership. The gun lobby's rapid financial response may therefore serve not only to shield allied candidates but also to preserve a political environment conducive to continued sales of high-lethality weapons. While we do not observe firearm market behavior directly, our findings are consistent with an agenda-defending logic in which PAC activity functions as both a political and commercial safeguard.

Our findings underscore a critical gap in democratic accountability: while public opinion should theoretically drive policy change in representative democracies, financial resources can obstruct this relationship. The strategic allocation of PAC money reflects what Gilens and Page (2014) describe as "economic elite domination," whereby organized interests exert outsized influence over policy outcomes. As a result, democratic institutions fail to consider popular preferences in policymaking when those preferences conflict with those of concentrated interests (Bartels 2008; Gilens 2014).

This "insulation effect," as we coin it, blunts what should be moments of heightened accountability. Candidates receiving these contributions often represent districts where public demand for reform is especially high. Instead of yielding to constituent pressure, candidates are fortified to resist it and are empowered to reframe their opposition to reform as principled rather than unresponsive (Jacobs and Shapiro 2000).

In this context, contributions are best understood not as rewards for past behavior but as strategic interventions—timed to neutralize the political fallout of focusing events. This logic is consistent with earlier work on campaign finance as a tool of electoral influence (Ansolabehere et al. 2003; Ansolabehere et al. 2004; Fouirnaies and Hall 2014) and reflects the realities of reelection incentives in contemporary American politics (Mayhew 1974).

The dynamics we observe build on Schattschneider's (1960) "mobilization of bias" framework, illustrating how financial resources can be used to suppress pressure for reform and sustain policy stasis. Tragedy, instead of triggering change, becomes a cue for counter-mobilization and an opportunity for entrenched interests to reaffirm control. Finally, this pattern contributes to negative policy feedback (Mettler and SoRelle 2014; Pierson 1993). When school shootings fail to result in legislative reform, constituents may grow demobilized and cynical, reinforcing a cycle in which future mobilization is less likely and political institutions appear increasingly unresponsive.

1. Limitations of Our Research

Several limitations of our research deserve further consideration. While we establish a strong causal relationship between fatal school shootings and increased pro-gun PAC contributions, we do not assess the downstream effects on how these financial responses influence legislative activity in Congress. Clarifying these downstream effects would illuminate precisely how and to what extent strategic PAC contributions effectively shape legislative outcomes. In future research, we explicitly address these questions, examining whether recipients of pro-gun PAC contributions after fatal school shootings subsequently alter their legislative behavior such as introducing gun rights legislation or voting against gun safety measures.

Additional limitations include the difficulty of exhaustively capturing independent expenditures and dark money.[28] While our data is thorough and exhaustive, it is possible that additional 501(c)(4) organizations that we were not able to identify could be directing money to support efforts of the gun lobby. Part of our future research will further explore these opaque organizations. Additionally, while our focus on House districts limits the scope of our

[28] The universe of uncapped independent expenditures and the underworld of dark money, expectedly opaque, does not lend itself easily to empirical study. Organized lobbies, and particularly the gun lobby, are adept at shielding financial activity from public view and academic scrutiny.

understanding of how the dynamics of contributions play out in other political contexts and through other political institutions.

Finally, although our findings do not demonstrate that PAC activity causes electoral or policy outcomes, they provide compelling evidence of a strategic and time-sensitive financial response to moments of heightened public salience. This upstream mobilization helps explain how organized interests may blunt public demand for reform.

2. Implications of Our Research

Our findings raise important questions about the potential for counter-mobilization forces to challenge the pattern of strategic insulation we observe. Future research could look at places where gun regulation has been successful and identify what conditions made it possible. In a similar vein, future research could examine places where gun regulation has been successful and identify what conditions made it possible as well as the strategic financial behavior of the gun safety lobby.

Taken together, our analysis provides comprehensive evidence of how pro-gun PAC contributions respond to fatal school shootings across congressional districts. The robust patterns we observe show that pro-gun PACs are strategic in their deployment of financial resources in response to potential or perceived threats to their policy agenda. By shedding light on this strategic financial response, our study illuminates a critical mechanism by which pro-gun PACs systematically undermine democratic will and preserve the gun policy status quo, despite the relentless carnage of gun violence in America that destroys communities and shatters the illusion of safety in schools.

This pattern of strategic financial mobilization to maintain the policy status quo despite popular preferences represents a paradox: the very mechanisms designed to ensure representative government are used to obstruct democratic responsiveness on highly salient policy issues. Understanding such a paradox is essential for advancing gun policy that may reduce violence and for strengthening democratic institutions in an era when their capacity to reflect public preferences and produce meaningful policy is increasingly in question.

Acknowledgments and Disclosures

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Use of AI Tools

We used OpenAI's ChatGPT and Anthropic's Claude for limited assistance with punctuation, grammar, dictionary and thesaurus queries, debugging LaTeX and R code, support with Chicago Manual of Style usage, and alphabetizing references. No AI tool was used to generate hypotheses, analyze data, interpret results, or draft any content. All content, including writing, panel data, data visualization, and analysis, reflects the authors' original work. The authors bear full responsibility for the final manuscript.

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Conflicts of Interest

The authors declare no conflicts of interest.

Institutional Review Board (IRB) Status

This study was reviewed and determined exempt by the Stanford University Institutional Review Board on April 10, 2025.

Data Availability

The master panel combines proprietary PAC-level contribution data licensed from OpenSecrets with the covariates used in our models. We are able to provide the panel data upon request. These data will be made available in an appropriate repository upon publication.

All analysis code, detailed documentation, and the directory structure required to reproduce every table and figure in the paper will be deposited in an appropriate repository upon publication. Until the public release, the code package is available from the authors at their discretion and upon reasonable request.

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Appendix A — Additional Descriptive Statistics

This appendix presents additional figures that provide context and support for the descriptive statistics presented in the main text, primarily in the Results section.

Temporal Patterns in Contributions

We first examine the temporal patterns in pro-gun contributions at different levels of granularity:

- **Figure 9**: annual trends in nationwide pro-gun contributions (including those under $10,000) from 2000-2024, showing both dollar amounts, and contribution counts with dotted lines indicated House election years, showing periodic and cyclic hikes in House election years.
- **Figure 10**: monthly pro-gun contribution patterns from 2000-2024, revealing granular fluctuations in both dollar amounts and contribution counts.

Relationship Between Shootings and Contributions

We explore the relationship between school shootings and pro-gun contributions:

- **Figure 11:** yearly trends from 2000-2024 comparing pro-gun contribution amounts with school shooting occurrences across the whole dataset.
- **Figure 12:** the same yearly comparison based on the dataset with basic filtering (i.e., contributions with $10,000 and over; $\leq 5\%$ voting margin at the previous election).

Treatment and Control Group Characteristics

We visualize the distribution of treatment and control observations:

- **Figure 13:** treatment status by district and month, showing treatment (teal), control (red), and filtered (gray) observations.
- **Table 11:** descriptive statistics table comparing demographic, economic, educational and political characteristics between control and treatment groups for school shooting incidents.

Covariate Balance Assessment

We assess the balance of key covariates between treatment and control groups:

- **Figure 14:** violin plots showing the distribution of racial demographic variables between treatment and control groups.
- **Figure 15:** violin plots comparing socioeconomic variables (unemployment rate and log median income) between groups.

- **Figure 16:** violin plots illustrating the balance of education, firearm possession, and a political variable (a representative candidate's voting margin against a democratic candidate in the previous election) between groups.

Contribution Patterns and Geographic Distribution

We examine the distribution of contributions and incidents:

- **Figure 17:** histogram showing the distribution of logged dollar amounts of contributions, with reference lines for the mean and key thresholds ($5,000 and $10,000).
- **Figure 18:** geographic distribution of pro-gun contributions and school shooting incidents since 2000, aggregated by state.

Alternative Data Sources

We compare findings using different datasets:

- **Figure 19:** yearly trends comparing mass shooting incidents from multiple datasets (Mother Jones, Violence Prevention Project and Gun Violence Archive).
- **Tables 12-13:** descriptive statistics tables for the VPP and MJ datasets with basic filtering, comparing control and treatment groups.

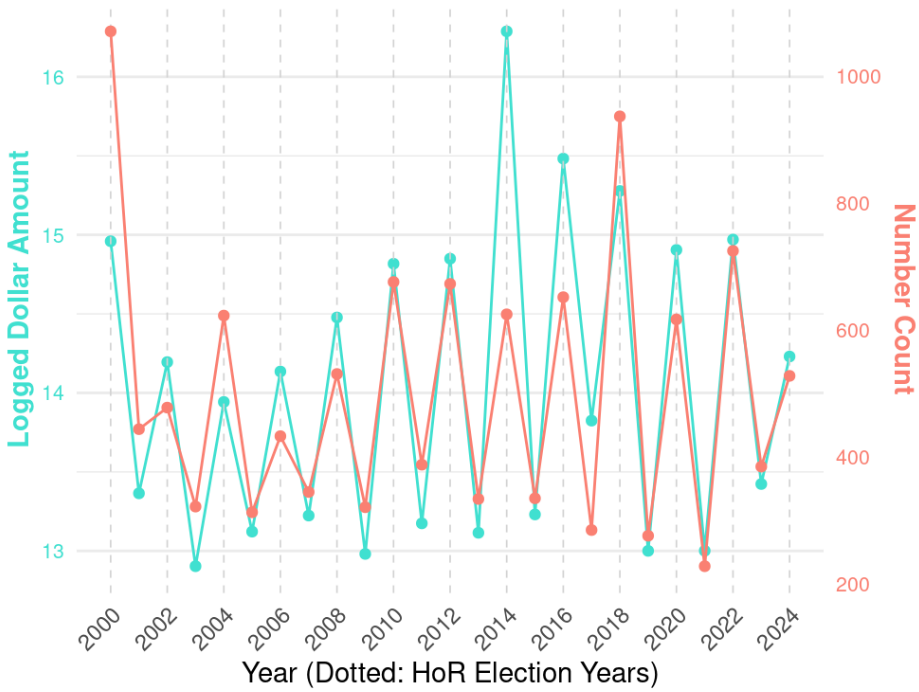

Figure 9: Nationwide Pro-Gun Contributions, 2000–2024

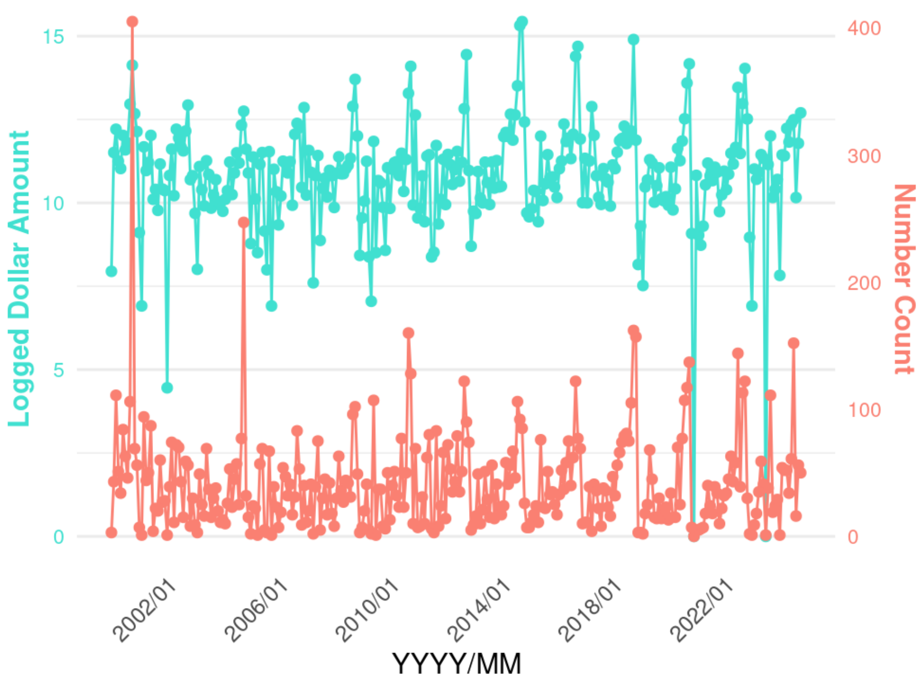

Figure 10: Nationwide Pro-Gun Monthly Contributions Over Time, Unfiltered Dataset, 2000–2024

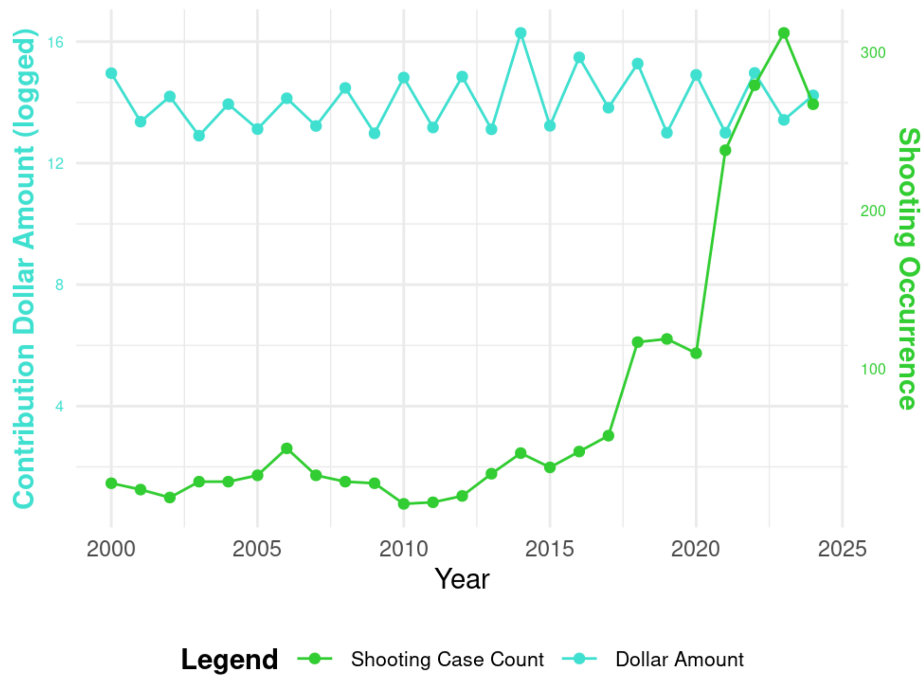

Figure 11: School Shootings and Pro-Gun Contributions, Unfiltered Dataset, 2000–2024

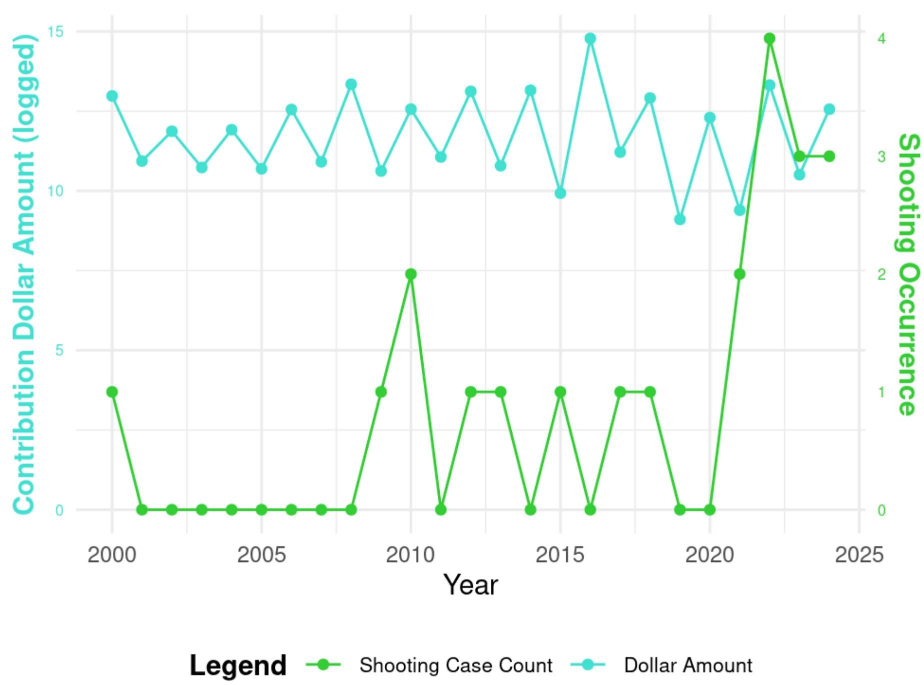

Figure 12: School Shootings and Pro-Gun Contributions, Filtered Dataset, 2000–2024

Figure 13: Treatment Status (School Shootings; Basic Filtering), 2000–2024

Table 11: Descriptive Statistics Table: School Shooting (Without Filtering), 2000–2024

Variable	History of Incidents (0 = Control, 1 = Treatment) 0 N = 99,969	1 N = 28,717
Ratio of Bachelor's degree holders (%)	27.5 (11.1)	30.1 (11.1)
White Population (%)	73.9 (17.6)	64.0 (20.6)
Black Population (%)	11.2 (13.0)	17.2 (17.4)
Asian Population (%)	5.0 (6.6)	4.8 (5.9)
Other Races' Population (%)	5.4 (6.9)	6.2 (7.7)
Unemployment rate (%)	6.6 (2.6)	6.1 (2.6)
Logged median annual income	10.9 (0.3)	11.0 (0.3)
Estimated Household Firearm Possession (%)	31.4 (12.0)	33.5 (11.1)
Margin for Rep. candidates at the latest election (%)	-0.5 (45.7)	-7.6 (45.3)
Republican incumbent (1=Yes, 0=No)		
0	46,833 (47%)	15,487 (54%)
1	53,136 (53%)	13,230 (46%)
US President (1=Rep., 0=Dem.)		
0	49,419 (49%)	17,177 (60%)
1	50,550 (51%)	11,540 (40%)

*Values for continuous variables are presented as mean (standard deviation). Treatment includes from the month of incidents to 23 months after.

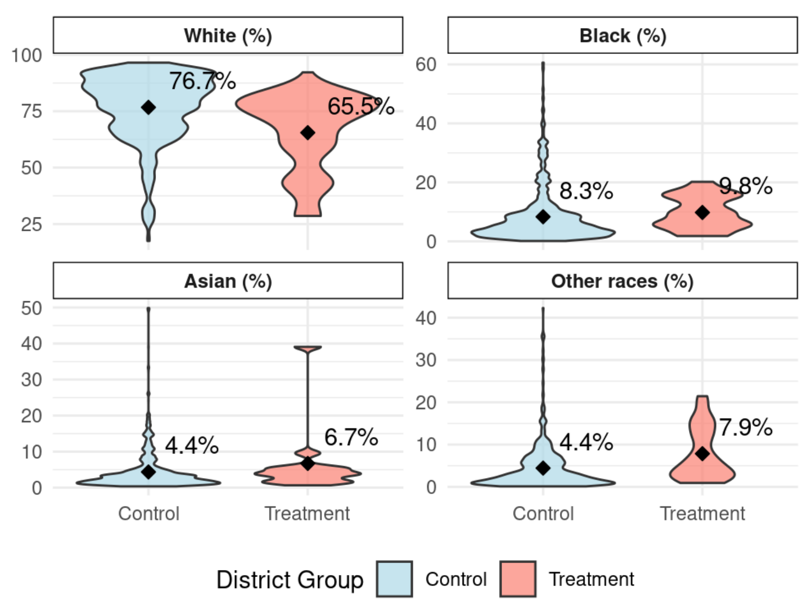

Figure 14: Covariate Balance: School Shooting (Basic Filtering) — Race (%)

Figure 15: Covariate Balance: School Shooting (Basic Filtering) — Economic Covariates, 2000–2024

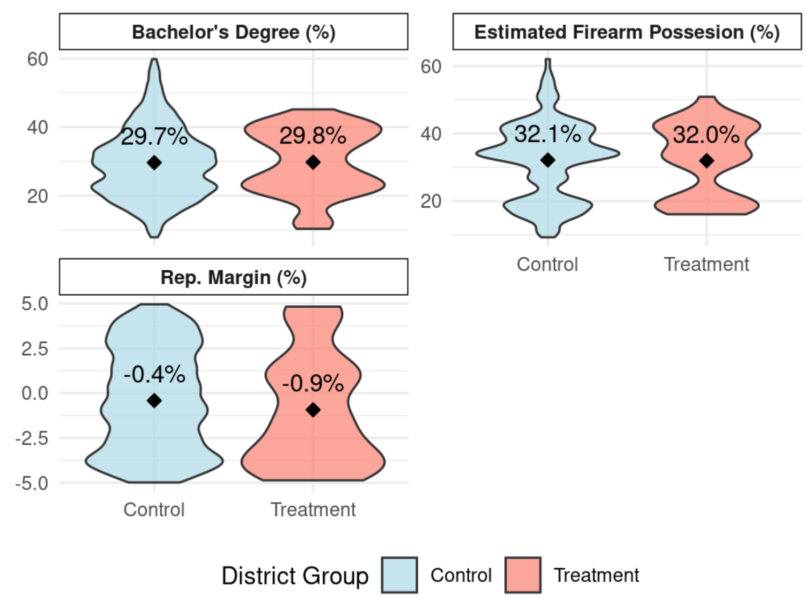

Figure 16: Covariate Balance: School Shooting (Basic Filtering) — Non-Economic Covariates, 2000–2024

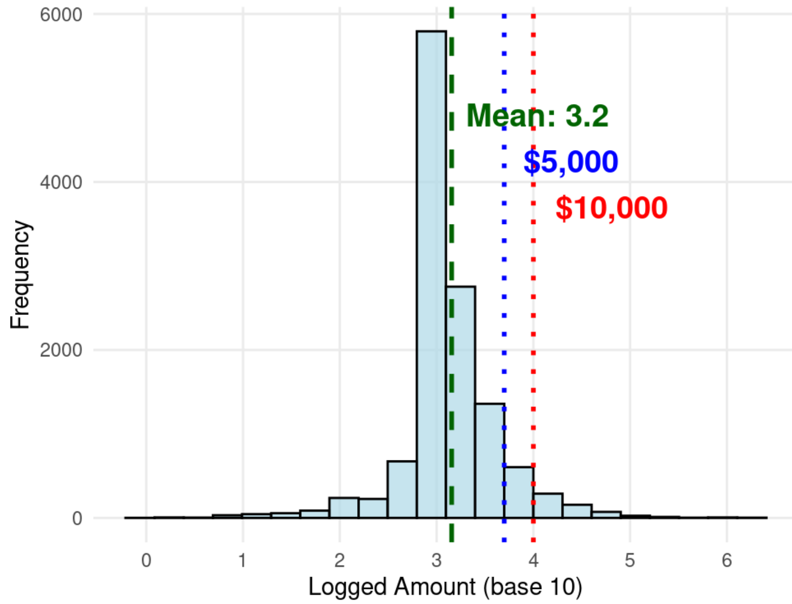

Figure 17: Distribution of Log Dollar Amount of Contributions, 2000–2024

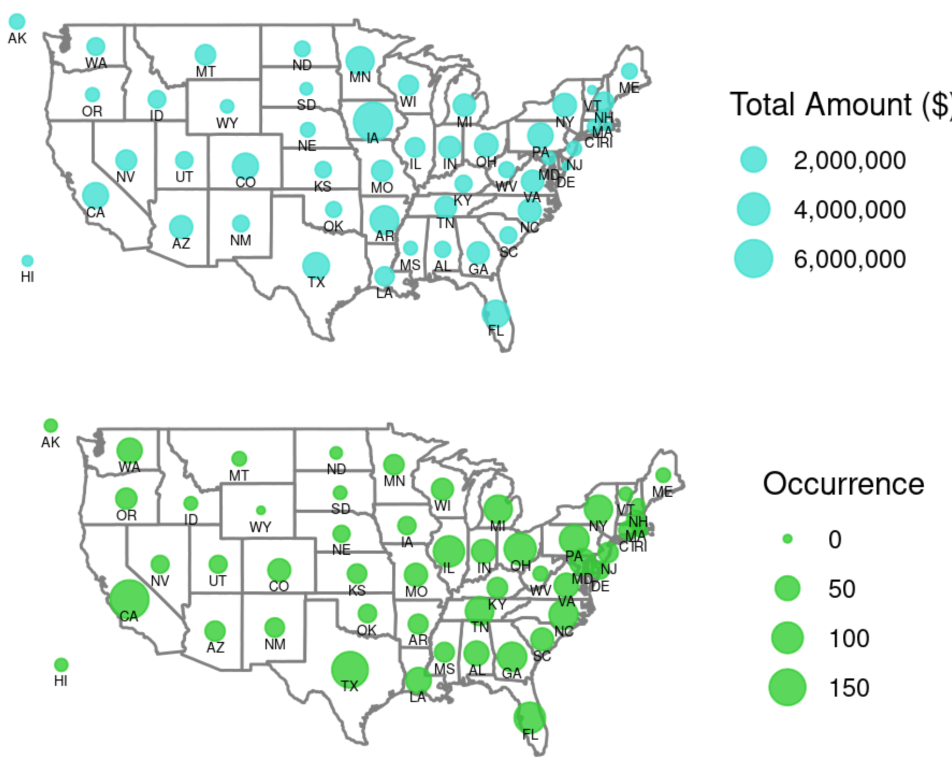

Figure 18: Aggregated Pro-gun Contributions by State, 2000–2024

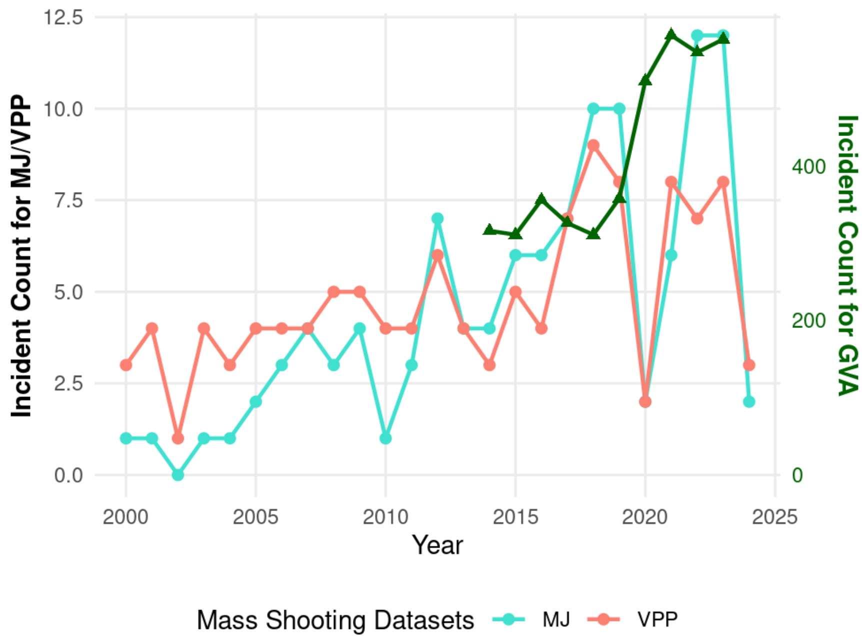

Figure 19: Mass Shooting and PAC Contributions Trends, 2000–2024

Table 12: Descriptive Statistics Table: VPP Dataset (Basic Filtering), 2000–2024

	History of Incidents (0 = Control, 1 = Treatment)	
Variable	0 N = 7,696	1 N = 132
Ratio of Bachelor's degree holders (%)	29.6 (10.0)	33.2 (7.8)
White Population (%)	76.2 (15.5)	77.1 (9.7)
Black Population (%)	8.4 (8.9)	8.3 (3.1)
Asian Population (%)	4.5 (5.4)	3.8 (1.7)
Other Races' Population (%)	4.6 (5.4)	5.6 (3.3)
Unemployment rate (%)	6.1 (2.3)	6.4 (2.0)
Logged median annual income	11.0 (0.3)	11.0 (0.1)
Estimated Household Firearm Possession (%)	32.1 (11.2)	30.5 (9.5)
Margin for Rep. candidates at the latest election (%)	-0.4 (2.9)	-2.5 (1.6)
Republican incumbent (1=Yes, 0=No)		
0	4,184 (54%)	126 (95%)
1	3,512 (46%)	6 (5%)
US President (1=Rep., 0=Dem.)		
0	4,614 (60%)	74 (56%)
1	3,082 (40%)	58 (44%)

*Values for continuous variables are presented as mean (standard deviation). Treatment includes from the month of incidents to 23 months after.

Table 13: Descriptive Statistics Table: MJ Dataset (Basic Filtering), 2000–2024

Variable	History of Incidents (0 = Control, 1 = Treatment) 0 N = 7,703	1 N = 112
Ratio of Bachelor's degree holders (%)	29.7 (10.0)	31.9 (7.0)
White Population (%)	76.2 (15.4)	77.2 (11.3)
Black Population (%)	8.4 (8.9)	7.4 (4.2)
Asian Population (%)	4.5 (5.4)	3.8 (1.9)
Other Races' Population (%)	4.6 (5.4)	5.6 (3.8)
Unemployment rate (%)	6.1 (2.3)	6.7 (2.0)
Logged median annual income	11.0 (0.3)	11.0 (0.1)
Estimated Household Firearm Possession (%)	32.2 (11.2)	28.5 (9.1)
Margin for Rep. candidates at the latest election (%)	-0.4 (2.9)	-2.2 (1.6)
Republican incumbent (1=Yes, 0=No)		
0	4,206 (55%)	106 (95%)
1	3,497 (45%)	6 (5%)
US President (1=Rep., 0=Dem.)		
0	4,601 (60%)	74 (66%)
1	3,102 (40%)	38 (34%)

*Values for continuous variables are presented as mean (standard deviation). Treatment includes from the month of incidents to 23 months after.

Appendix B — Additional Statistical Analysis Results

This appendix presents additional analyses that support the results presented in the main text, primarily in the Results section.

Covariate Balance After Matching

We first assess covariate balance between treated and matched control units:

- **Figure 20:** standardized mean differences before and after matching, indicating strong post-matching balance across key covariates. The following specifications were used for matching: control-treatment ratio: 1000, time-related matching: every four years, caliper: 0.2.

Propensity Score Matching

As a robustness check, we explore how estimated treatment effects vary across different matching strategies:

- **Table 14:** summary of results from ten matching specifications, including variations in caliper width, time-related criteria for matching (every one, two, and four years), and control-to-treatment ratios.

Election Proximity Thresholds

To assess the robustness of our estimates, we test how results vary when restricting incidents to specific windows of time preceding the next House election:

- **Figure 21:** results when controlling for months remaining until the next House election and using dollar amounts as the outcome.

- **Figure 22:** the same estimates using contribution counts as the outcome.

Political Covariate Adjustments

We examine whether adding political covariates to our basic pattern affects estimated treatment effects:

- **Table 15:** comparison of baseline results to specifications that further control for voting margin, absolute voting margin, or presidential tenure (i.e., Republican incumbent or not).

Timing Covariate Adjustments

To evaluate the role of time-related political context:

- **Table 16:** comparison of baseline pattern's estimates to models that add a covariate for the number of months to the next House election or for whether the observation occurred in an even-numbered year.

Treatment Window Durations

Finally, we examine whether results vary by the length of the post-treatment window:

- **Table 17:** effect estimates using 1-, 2-, 3-, 4-, and 5-year treatment periods (2-year period as our basic pattern).

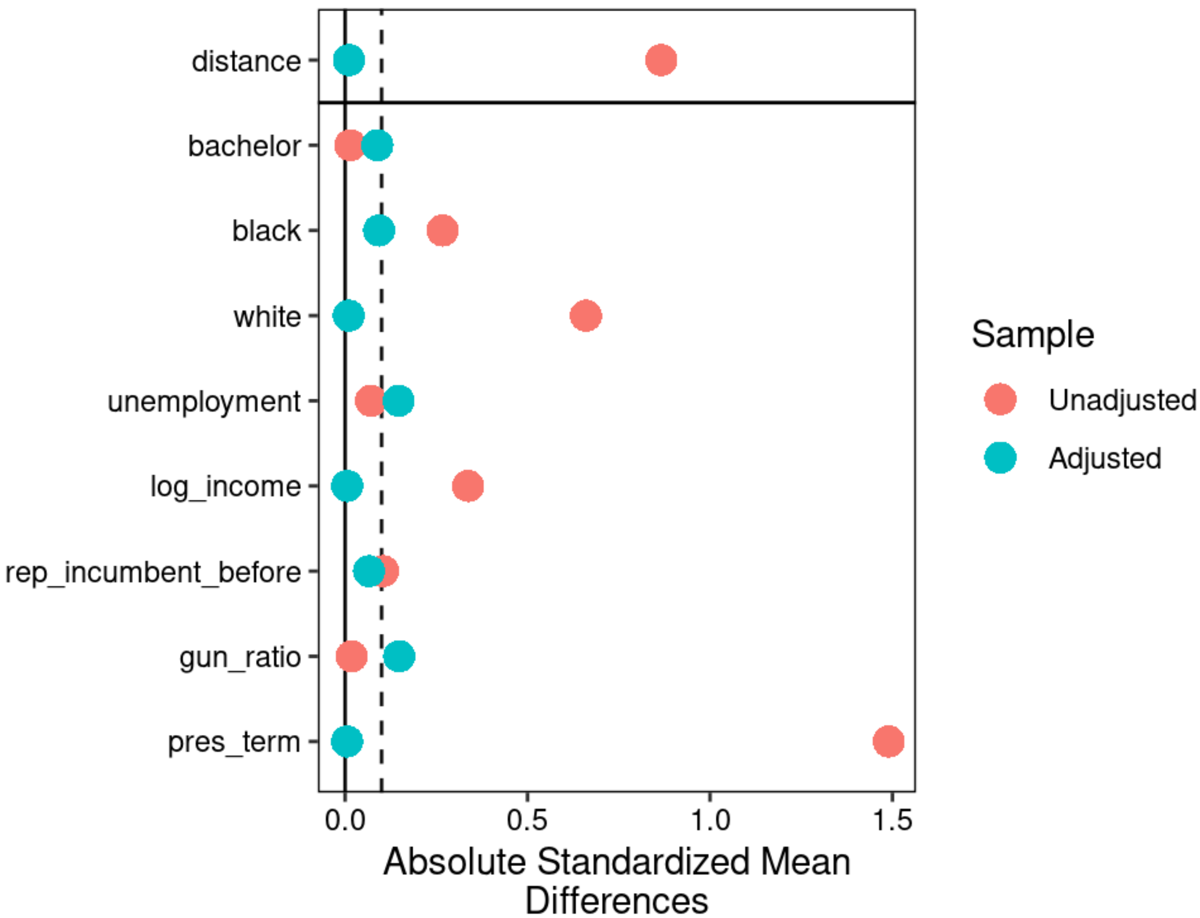

Figure 20: Love Plot for Covariate Balance Before and After Matching

Table 14: Propensity Score Matching Results, 2000–2024

	School Shooting Baseline Dataset: HoR									
	Baseline	Number Outcome	House Matching	House Matching (Number)	Year Matching	Year Matching (Number)	0.1 Caliper	0.1 Caliper (Number)	2000 Matching Ratio	2000 Matching Ratio (Number)
Treatment	0.236 (0.114) p = 0.040	0.024 (0.012) p = 0.036	0.268 (0.116) p = 0.022	0.027 (0.012) p = 0.019	0.307 (0.154) p = 0.048	0.032 (0.015) p = 0.042	0.243 (0.099) p = 0.015	0.025 (0.010) p = 0.012	0.248 (0.117) p = 0.035	0.026 (0.012) p = 0.031
Num.Obs.	6579	6579	6254	6254	6249	6249	6089	6089	7510	7510
Outcome	Dollar	Number	Dollar	Number	Dollar	Number	Dollar	Number	Dollar	Number
Time-related Matching	Every 4 years	Every 4 years	Every 2 years	Every 2 years	Every year	Every year	Every 4 years	Every 4 years	Every 4 years	Every 4 years
Caliper	0.2	0.2	0.2	0.2	0.2	0.2	0.1	0.1	0.2	0.2
Control # per one treated unit	1000	1000	1000	1000	1000	1000	1000	1000	2000	2000
Variables Excluded (multicollinearity)	None	None	None	None	None	None	None	None	None	None

*1. Baseline matching: Treated-control ratio: 1:1000. Nearest matching; Allowing multiple time use of the same unit; Using multiple units as control units; time-related variables for matching: presidential term, caliper: 0.2.
2. Baseline covariates include ratios of bachelor holders, of black, and of white; unemployment rate, logged income, incumbency in the previous election, the number of months to its next House election, and estimated household firearm possession rate, unless removed due to multi-collinearity.
3. Outcome regression: TWFE using district- and month-level fixed effects

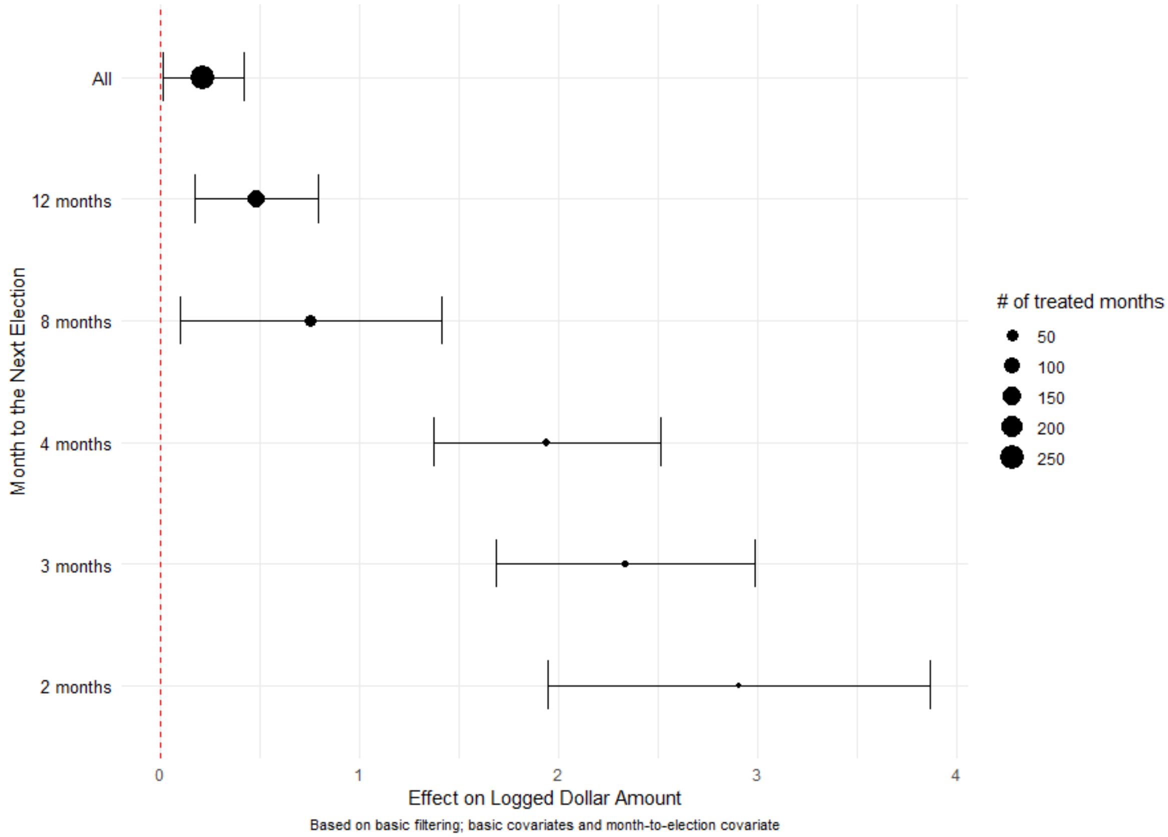

Figure 21: Baseline DID by Different Thresholds on Incident Timing and Treatment Period, 2000–2024

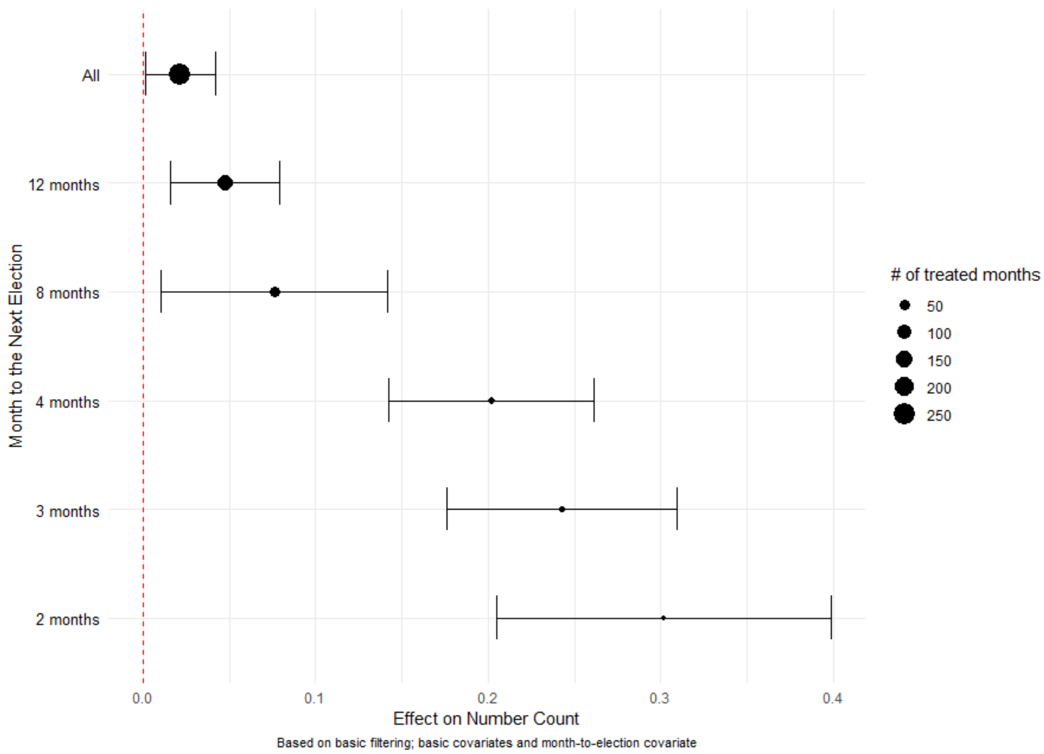

Figure 22: Baseline DID by Different Thresholds on Incident Timing and Treatment Period, 2000–2024

Table 15: Baseline DID Comparison
School Shooting Dataset: Politics-Related Covariates, 2000–2024

	Basic Pattern	Voting Margin	Abs. Voting Margin	Presidency Tenure
Treatment	0.264 (0.099)	0.265 (0.099)	0.264 (0.100)	0.264 (0.099)
	p = 0.008	p = 0.008	p = 0.009	p = 0.008
Voting Margin	–	0.001 (0.017)	–	–
		p = 0.973		
Abs. Voting Margin	–	–	0.008 (0.014)	–
			p = 0.560	
Presidency Tenure	–	–	–	1.345 (25629.688)
				p = 1.000
Num. Obs.	7778	7778	7778	7778
Outcome	Dollar	Dollar	Dollar	Dollar
Voting Margin Covariate	No	Yes	Yes (Abs.)	No
Presidency Covariate	No	No	No	Yes
Variables Excluded (multicollinearity)	None	None	None	None

*Baseline covariates include ratios of bachelor holders, of Black, and of White; unemployment rate, logged median income, incumbency in the previous House election, and estimated household firearm possession rate, unless removed due to multicollinearity.

Table 16: Baseline DID Comparison
School Shooting Dataset: Timing-Related Covariates, 2000–2024

	Basic Pattern	Months to HoR elections	Even Year
Treatment	0.264 (0.099)	0.264 (0.099)	0.264 (0.099)
	p = 0.008	p = 0.008	p = 0.008
Months to HoR Election	–	-2.636 (50238.766)	–
		p = 1.000	
Even Year	–	–	2.049 (39180.842)
			p = 1.000
Num. Obs.	7,778	7,778	7,778
Outcome	Dollar	Dollar	Dollar
Timing-related Covariate	None	Months to HoR election	Even year dummy
Variables Excluded (multicollinearity)	None	None	None

*Baseline covariates include ratios of bachelor holders, of Black, and of White; unemployment rate, logged median income, incumbency in the previous House election, and estimated household firearm possession rate, unless removed due to multicollinearity.

Table 17: Different Treated Ranges at Baseline DID
School Shooting; House of Representatives, 2000–2024

	1 Year	2 Years (Basic Pattern)	3 Years	4 Years	5 Years
Treatment	0.346 (0.101)	0.264 (0.099)	0.300 (0.098)	0.238 (0.094)	0.233 (0.092)
	p = 0.001	p = 0.008	p = 0.003	p = 0.012	p = 0.012
Num. Obs.	7,778	7,778	7,778	7,778	7,778
Treatment Range	1 year	2 years	3 years	4 years	5 years
Variables Excluded (multicollinearity)	None	None	None	None	None

*Baseline covariates include ratios of bachelor holders, of Black, and of White; unemployment rate, logged median income, incumbency in the previous House election, and estimated household firearm possession rate, and , unless removed due to multicollinearity.

Appendix C — Supplementary Robustness Checks

This appendix presents supplementary robustness checks that further validate our main results.

DCDH Event Studies

We implement alternative DCDH event study specifications to the analysis presented in the main text.

- **Figure 23:** event study using monthly (non-moving-average) logged dollar amounts with covariates.
- **Figure 24:** event study using moving average of logged dollar amounts and removing covariates.
- **Figure 25:** event study using moving average of contribution counts as the outcome with covariates.

Imputation Variants

We assess the sensitivity of results to different imputation patterns:

- **Figure 26:** results using monthly (non-moving-average) logged dollar amount with covariate adjustment.
- **Figure 27:** results using moving average of logged dollar amounts (for six months) without covariate adjustment.
- **Figure 28:** results using moving average of contribution counts (for six months) as the outcome without covariate adjustment.
- **Figure 29:** results using a three-month moving average with covariates.
- **Figure 30:** results using a four-month moving average with covariates.

Panel Match Estimates

We apply the matching-based approach of Imai et al. (2023) to achieve better balance between treatment and control groups:

- **Figure 31:** event study of treatment effects by months since a school-shooting event, estimated with `PanelMatch` using matched sets built from pre-treatment covariates and simple outcomes.
- **Figure 32:** results for pre-treatment placebo test of the panel match in figure 31.

Interactive Fixed Effects Models

We fit interactive fixed effects (IFE) models to address heterogeneous fixed effects across districts and time:

- **Table 18:** static IFE estimates for two outcome patterns.
- **Figure 33:** pre-trends check through IFE event study without covariates.
- **Figure 34:** pre-trends check through IFE event study with covariates.

Mass shooting Dataset Comparison

Finally, we assess robustness across different mass shooting datasets:

- **Table 19:** baseline difference-in-differences estimates across the Violence Prevention Project, Mother Jones, and Gun Violence Archive datasets.

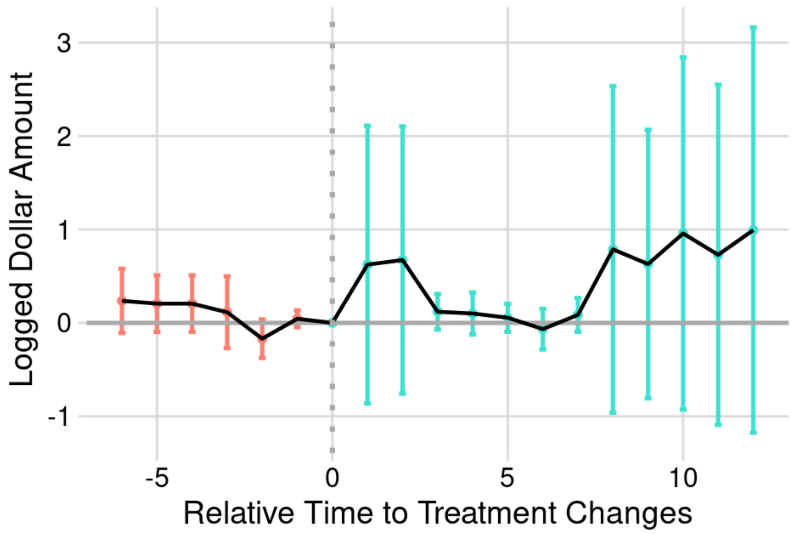

Figure 23: Estimated effect of school shootings on monthly (non-moving-average) logged dollar PAC contributions using the DCDH event study with all basic covariates, 2000–2024. Basic filtering specification is applied.

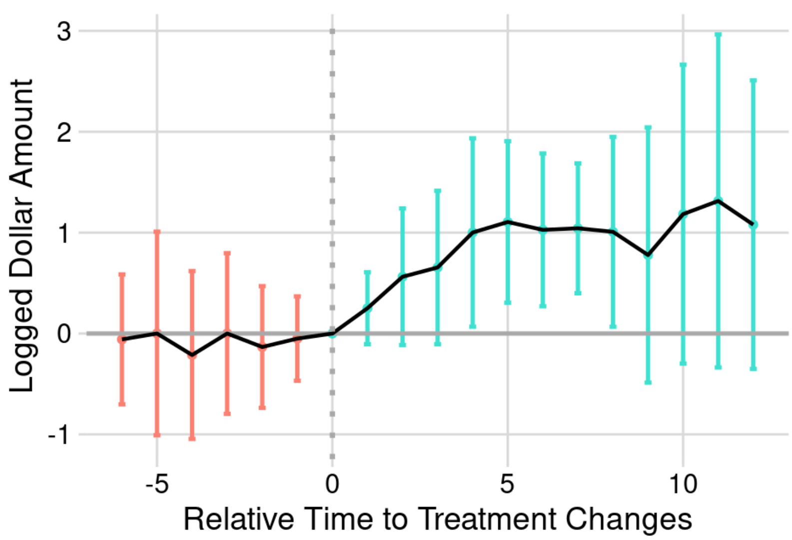

Figure 24: DCDH; Moving Average of Dollar Amount (for six months); No Covariates; 2000–2024

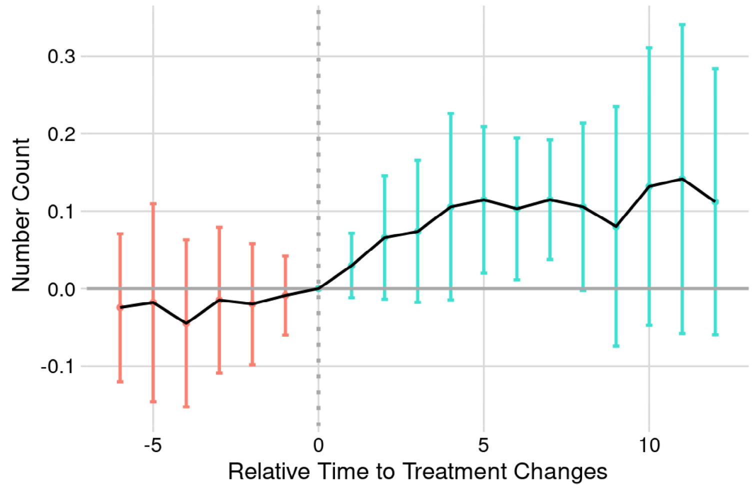

Figure 25: DCDH; Moving Average of Number Count (for six months); with Covariates; 2000–2024

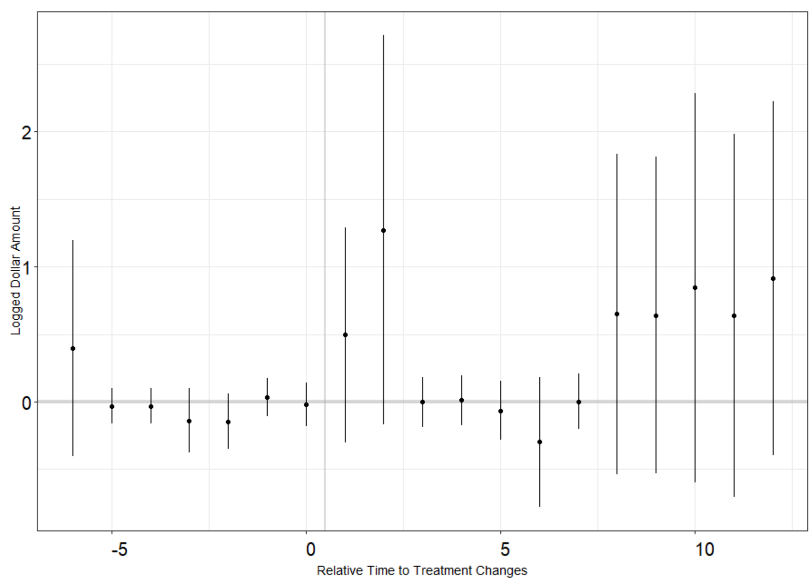

Figure 26: Estimated effect of school shootings on logged dollar PAC contributions using imputation method with basic covariates, 2000–2024. This plot presents results from the imputation-based event study model with basic filtering specifications and a simple (i.e., non-moving-average) dollar outcome, using covariate adjustments.

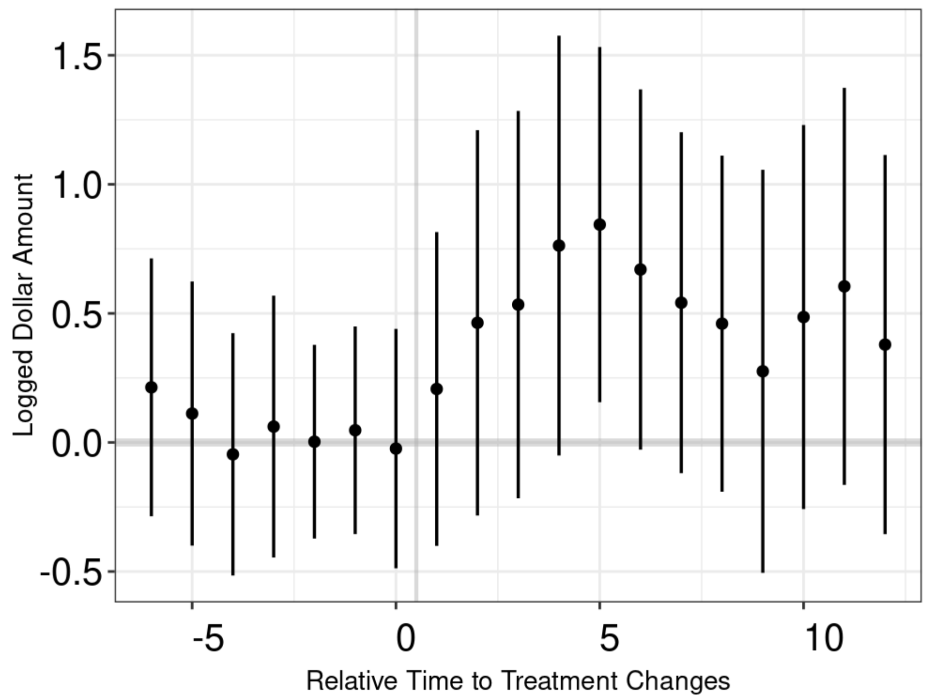

Figure 27: Imputation Method; Moving Average of Dollar Amounts (for six months); No Covariates; 2000–2024

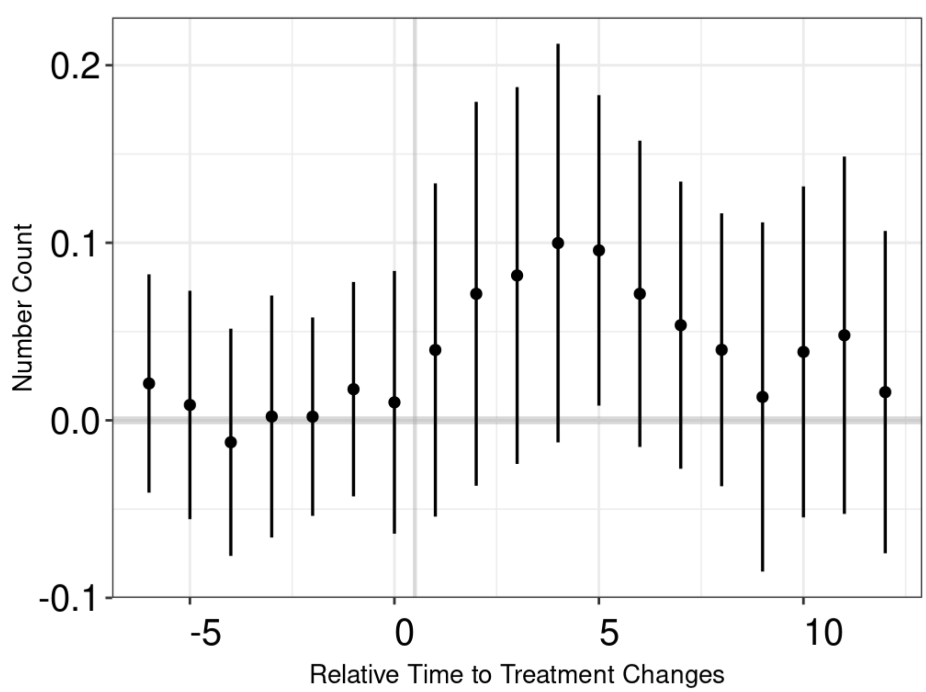

Figure 28: Imputation Method; Moving Average of Number Outcome (for six months); No Covariates; 2000–2024

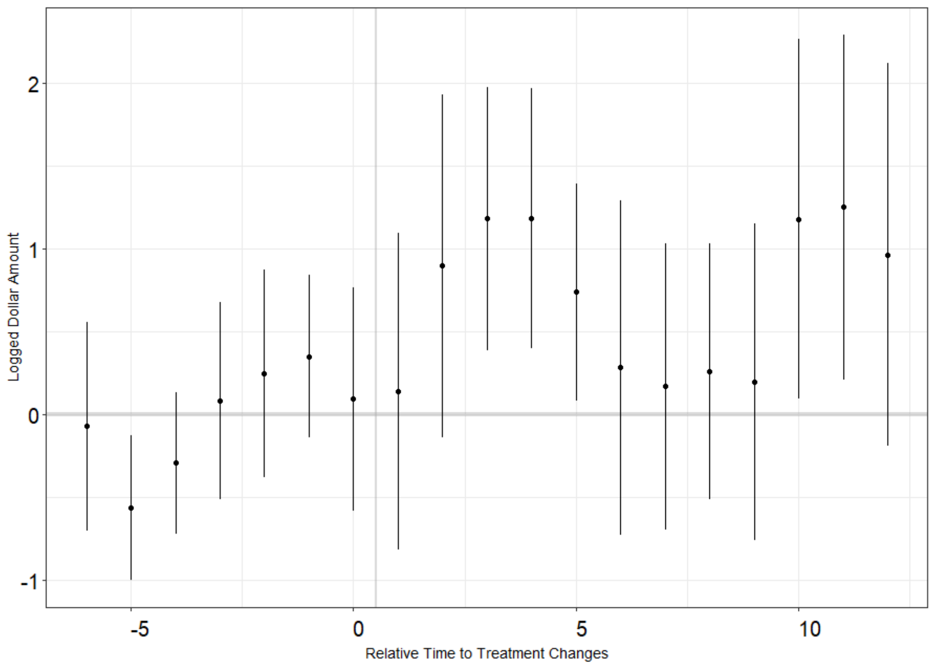

Figure 29: Imputation Method; Moving Average of Dollar Amount (for three months); With Covariates; 2000–2024

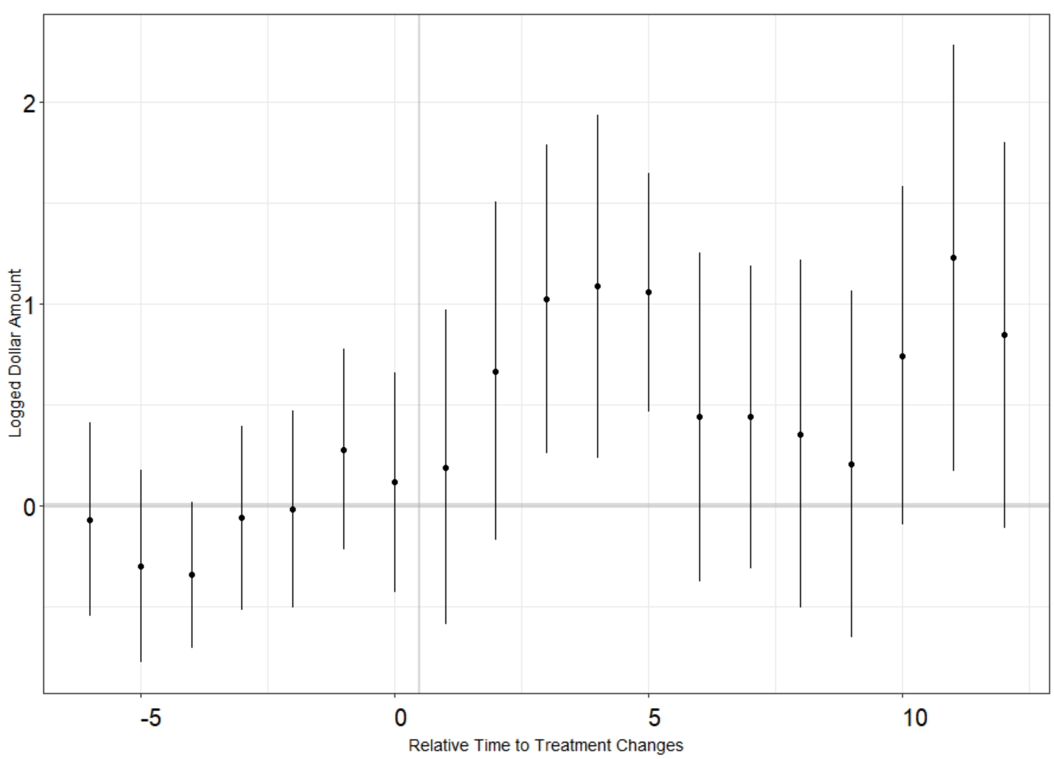

Figure 30: Imputation Method; Moving Average of Dollar Amount (for four months); With Covariates; 2000–2024

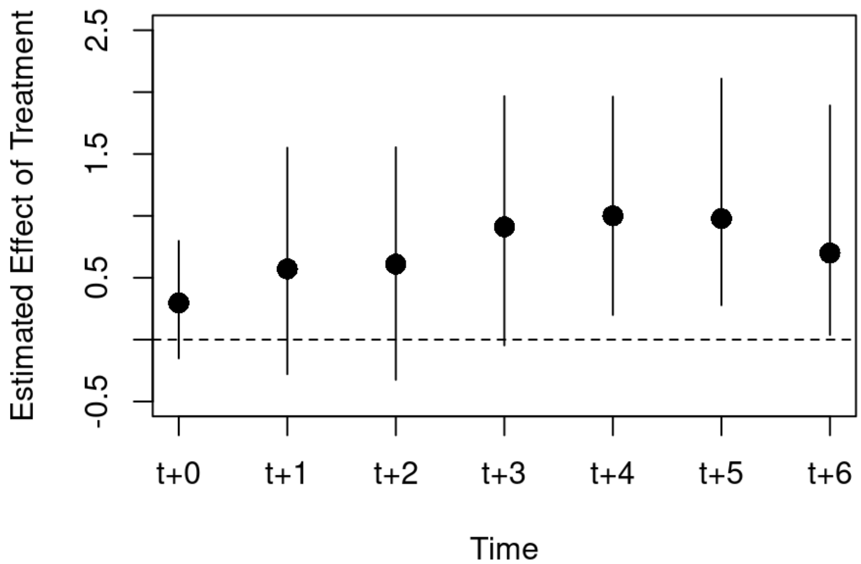

Figure 31: Event Study by Post-Treatment Month Using `PanelMatch`, 2000–2024

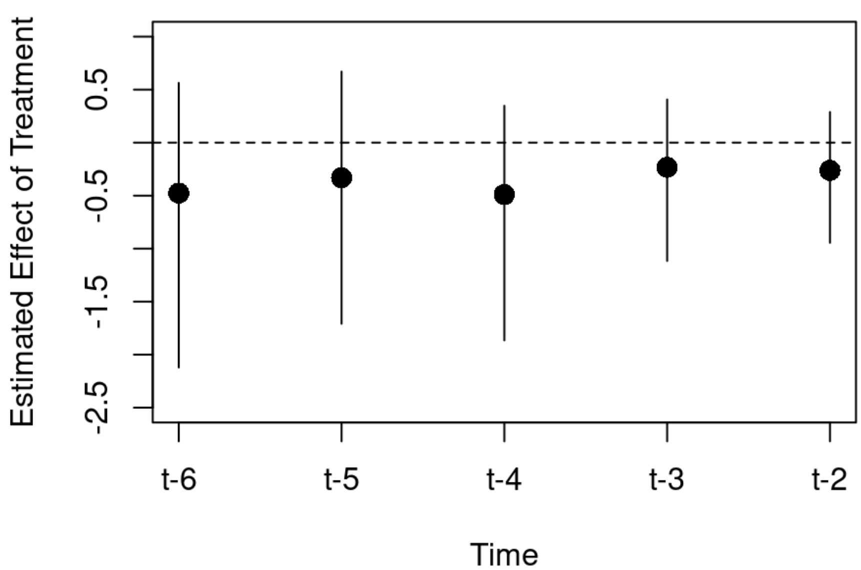

Figure 32: Results for Pre-Treatment Placebo Test of Panel Match in Figure 31

Table 18: Interactive Fixed Effects Model
School Shooting: House of Representatives, 2000–2024

	Dollar Outcome	**Number Outcome**
Treatment	0.275 (0.109) p = 0.012	0.027 (0.011) p = 0.015
Outcome	Dollar	Number
Optimal No. of Unobserved Covariates	0	0
MSPE	0.69935	0.00685

*Baseline covariates include ratios of bachelor holders, of Black, and of White; unemployment rate, logged median income, incumbency in the previous House election, and estimated household firearm possession rate, unless removed due to multicollinearity.

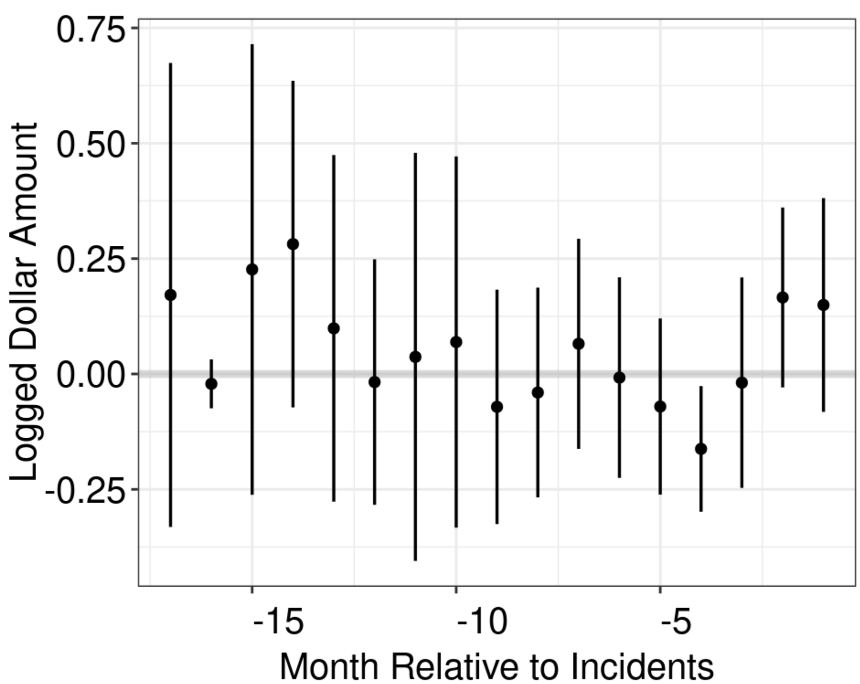

Figure 33: Interactive Fixed Effects
School Shooting; House of Representatives; No Covariates, 2000–2024

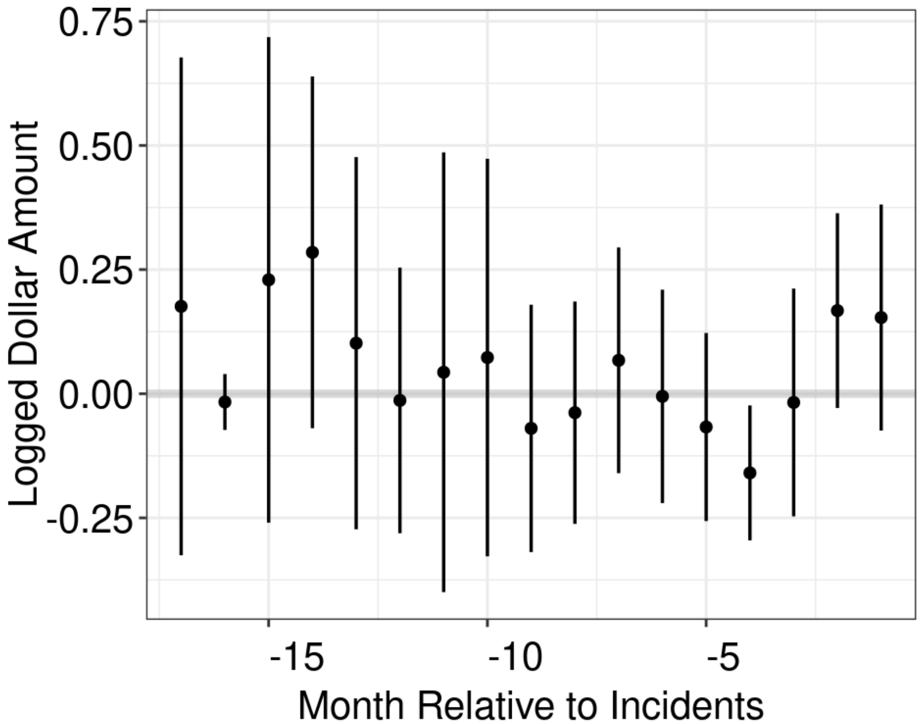

Figure 34: Interactive Fixed Effects
School Shooting; House of Representatives; With Covariates, 2000–2024

Table 19: Mass Shooting Baseline DID Comparison, 2000–2024

	VPP/MJ/GVA Datasets: HoR								
	VPP, Baseline DID	VPP, Number Outcome	VPP, State FE	MJ, Baseline DID	MJ, Number Outcome	MJ, State FE	GVA, Baseline DID	GVA, Number Outcome	GVA, State FE
Treatment	0.031 (0.150) p = 0.835	0.003 (0.015) p = 0.839	0.033 (0.230) p = 0.886	0.010 (0.177) p = 0.892	-0.003 (0.017) p = 0.873	-0.132 (0.094) p = 0.162	0.069 (0.099) p = 0.485	-0.006 (0.010) p = 0.573	-0.028 (0.060) p = 0.639
Num.Obs.	7828	7828	7828	7815	7815	7815	2930	2930	2930
Dataset	VPP	VPP	VPP	MJ	MJ	MJ	GVA	GVA	GVA
Outcome	Dollar	Number	Dollar	Dollar	Number	Dollar	Dollar	Number	Dollar
Unit FE	District	District	State	District	District	State	District	District	State
Variables Excluded (multicollinearity)	None	None	None	None	None	None	None	None	None

*Baseline covariates include ratios of bachelor holders, of black, and of white; unemployment rate, logged median income, incumbency in the previous House election, and estimated household firearm possession rate, unless removed due to multi-collinearity.

Appendix D — Panel Data Construction

This appendix contains additional information on the construction of our panel dataset.

The incident data was aggregated to the district-month level, then balanced for months and all possible congressional districts, 1990-2024. Next, the data was merged with the aggregated PAC contribution data to create a primary dataset containing:

1. `congressional_district`: district identifier
2. `year`: year of observation
3. `month`: month of observation
4. `state`: state identifier
5. `congress`: congressional session
6. `incident_count`: number of shooting incidents
7. `total_killed`: number of fatalities
8. `total_wounded`: number of people wounded
9. `total_victims`: total number of victims
10. `treated`: binary indicator of whether a shooting took place in that district-month
11. `total_pac_donations`: total amount of PAC donations
12. `total_number_of_donations`: total count of PAC donations
13. `unique_candidates`: number of unique candidates in that district-month
14. `unique_pacs`: number of unique PACs that donated in that district-month

Territories and Washington DC, which do not elect voting members to the House Representatives are removed from the data, as well as "phantom" district-months, that cease to exist because of redistricting, or do not yet exist in earlier years but become districts in later years, for which there is no need to include. After this cleaning, there are 182,988 rows.

Then another two variables are generated to determine the months since the previous election and months to the next election. These variables will facilitate analyzing whether donations shift in response to incidents closer to elections.

1. `months_to_election`
2. `months_since_election`

The dataset is then appended with control variables at the congressional district and month level. Since monthly estimates are not available, yearly estimates are used as a proxy for the district-month level in each year. Our demographic and census controls include:

1. `pct_population_25_and_over_some_college_or_more`: percentage of population 25 and over with some college education or higher
2. `pct_population_25_and_over_bachelors_or_more`*: percentage of population 25 and over with bachelor's degree or higher
3. `pct_total_population_white_alone`: percentage of total population identifying as white alone
4. `pct_total_population_black_or_african_american_alone`*: percentage of total population identifying as Black or African American alone
5. `pct_total_population_asian_alone`: percentage of total population identifying as Asian alone
6. `pct_total_population_some_other_race_alone`: percentage of total population identifying as some other race alone
7. `median_household_income_inflation_adjusted`*: median household income, adjusted for inflation
8. `pct_civilian_population_labor_force_16_and_over_unemployed`*: percentage of civilian labor force 16 and over that is unemployed

**Denoting covariates which are included in our basic pattern DID analyses.*

Since ACS/US Census data is not available for 2001–2005, interpolation is used to impute the values. All the variables were imputed using linear interpolation except for unemployment, which we used natural spline interpolation, and for median household income in that year's dollars, for which we use an exponential growth model (log-linear). From 2006 onward, there are annual ACS estimates of the demographic variables we include.

We append data on congressional elections obtained from Leip (2025). Congressional elections take place every two years. The congressional election variables we append are the following:

1. `total_vote`: total number of votes cast
2. `marginpct`: margin of victory as a percentage
3. `pct_democratic`*: percentage of votes for Democratic candidates
4. `pct_republican`*: percentage of votes for Republican candidates
5. `pct_independent`: percentage of votes for Independent candidates
6. `pct_other`: percentage of votes for other candidates/parties

7. abs_democratic: absolute number of votes for Democratic candidates

8. abs_republican: absolute number of votes for Republican candidates

9. abs_independent: absolute number of votes for Independent candidates

For our purposes, we append the congressional election data to each district, repeating the values for every district-month in each year. Past House election results are appended from December following the November election through to November of the next election year. The next House election results are appended beginning in November of an election year until the October before the next election.

We append a final covariate regarding the mean household firearm rate (RAND 2024):

1. mean_HFR*: the average proportion of adults living in a household with a firearm (state level data but appended to each congressional district in a state for a given year). This data is available through 2018. For 2019-2024, the RAND researchers who produce this data informed us that it is methodologically acceptable to use the 2018 numbers since the rate changes so little over time.

Appendix E — List of Pro-Gun PACs

This appendix provides the names of pro-gun political action committees (PACs) that appear in our dataset. These groups were identified as unique, visible entries in the filtered data and are shown below in alphabetical order.

1. Arena PAC
2. Bull Moose Sportsmen's Alliance
3. Dallas Safari Club
4. Georgia Gun Owners PAC
5. GOA Victory Fund
6. Grass Roots NC/Forum for Firearms Educ
7. Great Lake Arms Collectors Assn
8. Gun Owners Action Fund
9. Gun Owners of America
10. Hunter Action Fund
11. Hunter Nation Action
12. Illinois State Rifle Assn
13. Montana Shooting Sports Assn
14. Myrna J Neeley-Friends of the 2nd Amend
15. National Rifle Assn
16. NRA Institute for Legislative Action
17. NRA Victory Fund
18. Ohio Gun Collectors Assn
19. Remington Arms
20. Secure Our Freedom Action Fund
21. Smith & Wesson
22. Sportsmen for Colorado
23. Texas Gun Owners for Constitutional Govt
24. Texas State Rifle Assn
25. US Concealed Carry Assn for Saving Lives
26. Vista Outdoor
27. Wallace & Wallace