Are Politicians Responsive to Mass Shootings? Evidence from U.S. State Legislatures

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Abstract

The United States leads the world in the number of mass shootings that occur each year even as policy making on firearms remains polarized along party lines. Are legislators responsive to mass shootings? We estimate the latent positions of 264 California state legislators on gun policy from their roll-call voting records on firearm-related bills from 2011 to 2022. Employing a stacked difference-in-differences design, we find that mass shootings within or near a state legislator's district do not alter their voting behavior on firearm policy, on average (with 95% confidence intervals spanning opposite directions). When we extend our analyses to an additional 5 states (approx. 1,600 more legislators) representing a range of gun policy environments, we find similarly null results, though we report evidence of heterogeneous effects from event-specific estimates. Our findings suggest that even the most heinous acts of violence generally fail to produce measurable effects on legislators' positions on firearm-related policy.

[Word Count: Approx. 8,650]

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1 Introduction

On May 24th, 2022, an 18-year-old gunman armed with a semi-automatic AR-15 style rifle entered Robb Elementary School in Uvalde, Texas, and fatally shot 2 teachers and 19 fourth-grade students. In the wake of the tragedy, state legislators called on Governor Greg Abbott (R) to convene a special legislative session to debate methods of reducing gun violence. Ultimately, the session was called, and legislation to raise the minimum age required to purchase semi-automatic rifles from 18 to 21 was introduced. In a surprise turn, two Republicans, Representatives Sam Harless (TX-126) and Justin Holland (TX-33), voted with Democrats to advance the legislation out of committee, though the bill died soon after. Both representatives defended their breaking with the Republican party, with Harless saying his vote "was the least [lawmakers] could do for the families" of the victims.[1]

Are legislators responsive to tragedies that occur in their districts? Scholarship on whether citizens hold governmental institutions and representatives accountable for negative shocks — including unanticipated ones — generally suggests that voters consider elected officials at least somewhat responsible when disasters occur under their watch (e.g., Fiorina, 1978; Healy and Malhotra, 2009; Gasper and Reeves, 2011; Heersink, Peterson and Jenkins, 2017). Under the theories presented in the extant scholarship on this subject, one might anticipate that legislators who represent districts experiencing increasing rates of crime adjust their voting behavior to be tougher on criminals (Arnold and Carnes, 2012), or that representatives of districts affected by economic shocks from international competition may vote in a more protectionist manner on trade-specific bills (Feigenbaum and Hall, 2015). One might also think that legislators representing areas devastated by wildfires (Hazlett and Mildenberger, 2020; Ramos and Sanz, 2020), floods (Arceneaux and Stein, 2006; Bechtel and Hainmueller, 2011; Stout, 2018), or other natural disasters (e.g., Malhotra and Kuo, 2008, 2009; Atkeson and Maestas, 2012; Carlin, Love and Zechmeister, 2014; Katz and Levin, 2016) vote more favorably for emergency aid spending measures, for instance, so that their respective electorates approve of their responses to crises when election day rolls around.

The current scholarship on behavioral responses to mass shootings — an example of a

[1] https://www.houstonpublicmedia.org/articles/news/politics/gun-control/2023/05/08/451146/texas-raise-age-assault-rifle-bill-passes-committee-house.

crisis event that legislators might respond to — has particularly focused on the effects of these events on public opinion (Gunn et al., 2018; Barney and Schaffner, 2019; Newman and Hartman, 2019; Rogowski and Tucker, 2019), the electoral behavior of affected constituencies (Hassell, Holbein and Baldwin, 2020), and the firearm-related policy environments of states (Luca, Malhotra and Poliquin, 2020), but there are mixed results regarding what effects these events may have on a variety of outcomes. In some instances, scholars find that mass shootings cause surges in engagement with gun policy (Reny et al., 2023) while others find that responses are limited by temporal, spatial, and political factors (Sharkey and Shen, 2021). In a similar vein, it appears that some mass shootings — particularly those with high levels of media coverage — coincide with increases in firearms purchasing while others do not (Iwama and McDevitt, 2021; Liu and Wiebe, 2019). Additional evidence suggests that legislators may lack the ability or incentive to adjust their behavior even when facing possible ideological, environmental, or electoral pressures (Butler and Hassell, 2018; Bromley-Trujillo and Poe, 2020). Even if we believe that mass shootings might lead to shifts in opinions on firearm-related policies — which is not obvious, as per Baxter-King (2024) — there may not be *enough* of a change in the electoral behavior of affected constituencies to cause a legislator to alter their voting behavior on relevant policies, even as some scholars find that members of Congress who are facing reelection pressures (and represent sizable "pro-gun constituencies") may be more likely to change how they vote on gun-related legislation (Bouton et al., 2014). Furthermore, it is not clear that changes in the voting behaviors or policy preferences of a legislator's constituency (should they occur) would prompt changes in a representative's actions (Rogers, 2017).

Our paper investigates the effects of exposure to mass shootings on the voting behavior of state legislators who represent the communities most directly impacted by these events. We focus our analysis on estimating the aggregate effect of mass shootings on the behavior of lawmakers rather than shooting-specific effects. Focusing on the average effect boosts our statistical power and precision by pooling rare and noisy events, while also yielding an informative estimate of how legislators typically respond. This approach avoids the pitfalls of multiple comparisons and overfitting to temporally idiosyncratic incidents, and relies on methods that account for heterogeneity across events to identify a single average treatment effect. We combine roll-call voting records of California state legislators with a novel dataset from the Giffords Law Center (GLC) that ranks every firearm-related bill introduced in the California State Assembly and Senate from 2011 to 2022 on a permissive to restrictive scale.[2] Using pairwise comparison and the Bradley-Terry model (Bradley and Terry, 1952), we construct a yearly score of each individual lawmaker's directionality of support for gun control legislation from the 2011-2012 to 2021-2022 sessions. Our scoring regime covers 264 individual California legislators over twelve years.

Using this measure of an individual legislator's latent position on firearms policy over time, we identify which legislators represented districts that experienced a mass shooting between 2011 and 2022. Through a difference-in-differences design accounting for heterogeneity in treatment timing, we estimate whether the occurrence of a mass shooting within a legislator's district causes a measurable change in their position on gun policy. We find that when a mass shooting occurs within a legislator's district, California lawmakers reduce their support for gun control policies by approximately 1.15% compared to all other legislators (ATT = -0.023, SE = 0.066, outcome measured on a -1 to 1 scale), though this estimate is a statistically insignificant null. Our results find no evidence of responsiveness in a legislators positionality on firearms-related policy and these statistically insignificant estimates persist across multiple estimation strategies and varied treatment assignments (such as estimations accounting for the distance of a legislator's district from a mass shooting). This finding conflicts with the self-reported assessments of responsiveness to mass shootings that these same legislators described in a series of qualitative interviews we conducted. In sum, our mixed-methods approach in California finds that legislators do not change their voting behavior following a mass shooting — though this finding only speaks to a single metric of legislator behavior in a single state.

As a robustness check on our California findings, we extend the analysis to a selected set of states that experienced mass shootings during the study period and for which standardized roll-call voting data (via LegiScan) were available. Our selection prioritizes both analytic feasibility and representativeness: we include states spanning the spectrum of firearm policy environments (from highly permissive to highly restrictive) based on Giffords Law Center

[2] We validated the GLC's bill rankings, which is discussed in greater detail in A.3.

(GLC) rankings. Specifically, we analyze Colorado (3 shootings; quintile 4), Florida (6; quintile 3), Georgia (1; quintile 2), Missouri (1; quintile 1), and Texas (6; quintile 2), with the latter included both for its high incidence of mass shootings and as a meaningful permissive-state contrast to California. This set captures substantial variation in policy context and accounts for approximately 42% of mass shootings in the United States during 2011–2022.

Ultimately, we find that across a twelve year period, 28 mass shootings — occurring in 6 states and requiring the scoring of nearly 1,900 individual state legislators over time — caused no discernible change in the firearm policy positions of state legislators on regulatory proposals on firearms, on average, when such an event occurs within their districts. Exceptions do occur — when we disaggregate our results and estimate shooting-specific effects, we find evidence that the aggregation method we utilize to account for varied treatment timing masks heterogeneity in the treatment effects of individual shootings. However, our null results hold when we also inspect other forms of behavior that legislators may engage in: we find that legislators do not cosponsor or author more or fewer firearm-related bills following a mass shooting, on average.

The following section discusses the theoretical framework situating these findings, and reports the results of a series of qualitative interviews with California lawmakers who represented affected districts at the time of mass shootings. These legislators largely reported positive self-assessments of their responsiveness to the crises brought about by mass shootings in their districts, claiming that they brought legislation to the floor or voted favorably on certain bills that they would not have in the absence of the shooting. Our assessments of the roll-call voting records of these same legislators (and others) tells a different story. Overall, our findings suggest that partisan polarization has led to a policy environment wherein even highly salient focusing events such as mass shootings, which may otherwise be reasonably thought to cause changes in a legislator's stance on firearms related policy, fail to produce any measurable effects on legislative voting behavior.

2 Theory and Qualitative Findings

To clarify why mass shootings may fail to shift legislators' voting behaviors on firearms policy, we draw on qualitative interviews with five current and former California state legislators who represented districts affected by mass shootings (2010–2022). These interviews, detailed in Appendix A.1, reveal how partisan polarization imposes structural and behavioral constraints that restrict legislator's ability to change their voting behavior. Below, we detail their accounts in the context of scholarly work on polarization, firearms politics, and legislative behavior to formalize our expectation of null effects.

2.1 The Limited Effects of Mass Shootings in an Era of Partisan Polarization

In an era of intense partisan polarization on firearms policy, do mass shootings affect the behaviors of legislators? We argue that the likely answer is no — we expect that partisan polarization creates powerful constraints that prevent legislators from changing their voting behaviors on firearm policy, even when these events occur in their own districts.

Our focus on state legislators is motivated by the contemporary landscape of firearms policymaking in the United States. While firearms policy is certainly also polarized at the federal level, Congress rarely votes on gun-related legislation, and state legislatures have increasingly become the primary venues for firearm policy debates (Luca, Malhotra and Poliquin, 2020). State lawmakers are responsible for considering and voting on the vast majority of firearms-related bills in the United States, making them the most relevant actors for understanding how mass shootings might influence legislative behavior on gun policy. If partisan polarization constrains responsiveness to mass shootings anywhere, it should be most observable at the state level where the bulk of firearms policymaking actually occurs.

Over the time that we study in this paper (2011-2022), public opinion on gun policy has become increasingly divided along partisan lines (Joslyn et al., 2017; Joslyn, 2020; Cook and Goss, 2020; Sides, Tausanovitch and Vavreck, 2022; Merry, 2023), and the partisanship of elected officials oftentimes serves as a heuristic to voters about where a candidate stands on firearm-related policy.[3] This polarized environment places immense pressure on state-level policymakers, who face distinct political costs for deviating from party orthodoxy. Gun rights advocates maintain exceptionally high levels of political engagement and organization (Lacombe, 2019), pay close attention to and hold strong opinions on firearms policy (Lacombe, Howat and Rothschild, 2019), and benefit from the outsize role of gun rights and industry-related interest groups in campaign contributions to elected officials (Baldwin, Iwasaki and Donohue, 2025; Richards, 2017). Legislators risk alienating these powerful constituencies — and jeopardizing their chances of reelection — if they break with their party's position (Bouton et al., 2014).

Our qualitative interviews reveal how these pressures operate in practice, with legislators we interviewed explaining how party leaders explicitly threaten sanctions against members who might consider deviating from the party line. Legislator C, a Republican, spoke about the role of partisan polarization bluntly, explaining what a party leader told them after a shooting in their district: "I was told directly, if I speak about [gun control], it's going to cost $1,000,000 per Tweet. For us to defend you in the next election, it's going to cost 1,000,000 bucks a Tweet." The direct threat of sanctions from party leadership that would affect a legislator's campaign warchest suggests that any sort of defection — even one as mundane as a Tweet — from policy positions preferred by the party would result in severe consequences.[4]

These partisan pressures help explain why scholarship on the politics of gun control characterizes debates in this policy domain as a reoccurring cycle of "outrage-action-reaction" that ultimately contributes to gridlock on firearm-related policy (Spitzer, 2023). Republicans face pressure to defend gun rights, while Democratic legislators face pressure to restrict firearm access — but crucially, both parties face pressure to maintain their existing positions rather than shift in response to events. Legislator A (D, Southern California) implied that support from party leadership for gun control bills came without typical political costs, saying, "Normally, getting a speaker[-prioritized] bill might have cost me political capital...

[3] `https://www.politico.com/news/2022/11/03/dems-finally-passed-gun-legislation-and-they-havent-paid-an-electoral-price-for-it-00064885`.

[4] We cannot confirm the veracity of Legislator C's claims about sanctions threatened by the California GOP. We can, however, confirm that Legislator C did not post a single Tweet related to a firearms policy proposal until 181 days after the shooting. This Tweet was expressing opposition to a permissive policy.

in this case it did not... Just not the way it normally works." This suggests that Democratic legislators are rewarded for advancing party-preferred policies after mass shootings, but not for moderating their positions or breaking with party orthodoxy. While other legislators may need to "walk [a] very fine line because of political differences in a community" (Legislator E; D, Southern California), mass shootings offer an opportunity to pass party-aligned bills, not shift individual positions. As Legislator B (D, Southern California) told us: "If you're a gun violence prevention advocate and you don't try to act in the aftermath of a horrific tragedy, you have, in many ways, lost an opportunity for public attention to be focused for long enough to get something to actually change."

This pressure to act, however, channels legislators toward advancing existing party positions rather than reconsidering their stances. Spitzer (2023) contends that events such as mass shootings may produce an initial demand for legislation which is then followed by the possibility or reality of policy being adopted, and, finally, a subsequent backlash against these gains from opposing groups.[5] This cyclicality all but ensures a stagnant policy environment, especially when one also considers the relative stability of public opinion on firearms policy in the aftermath of a shooting (Gunn et al., 2018; Barney and Schaffner, 2019; Newman and Hartman, 2019; Rogowski and Tucker, 2019; Hassell, Holbein and Baldwin, 2020; Baxter-King, 2024), and the polarized nature of state legislatures during this time period (Phillips et al., 2024).

We posit that as partisan polarization has increased and firearms policy has been delegated to state legislatures, Democratic and Republican legislators have become less willing to defect from their party when voting on firearm-related policy. This unwillingness to break ranks creates both structural constraints (legislators may already be voting at the ideological ceiling or floor on gun issues) and behavioral constraints (party discipline prevents defections even when legislators might be personally moved by a shooting). These twin constraints should render legislative voting patterns essentially immune to the influence of mass shootings, regardless of how legislators might be personally affected by these tragic events. Even with our qualitative interviews suggesting that California lawmakers perceive

[5] These groups need not be the traditional "gun rights" or "gun control" advocacy organizations, however. Legislator E described how retailers "vehemently opposed" mental health mandates in workplace violence bills that were introduced in the wake of one California shooting.

themselves as very responsive to mass shootings in their districts, we expect to find null results when assessing the effects of mass shootings on voting behavior. We predict that the partisan environment prevents defections in voting behavior, though we do not presume that other forms of legislative behavior would be similarly unaffected.

Thus, legislators may still respond symbolically (through appearances at public vigils, meetings with victims' families, or Tweeting about their thoughts and prayers), but we argue that voting on firearms policy is likely insulated from district-level shocks: partisan polarization rewards partisan fidelity, punishes deviation, and elevates national agendas to the point that mass shootings, on average, will have little effect on a legislator's voting on firearms-related policy.

3 Data and Methods

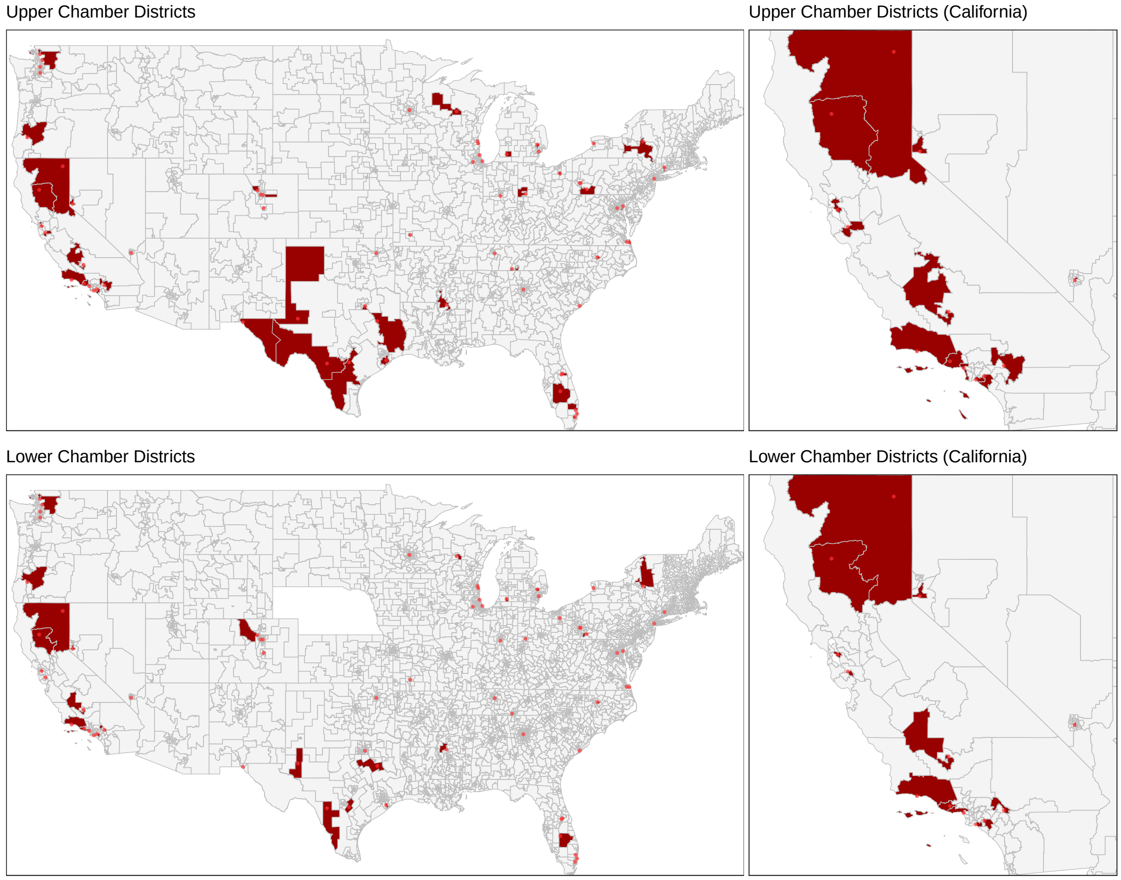
3.1 Mass Shootings in Scope of Study

To identify mass shootings for the purposes of this study, we rely upon a definition established by the Congressional Research Service, which is aligned with definitions used by researchers across a variety of disciplines (e.g., Luca, Malhotra and Poliquin, 2020; Peterson and Densley, 2022). Under this definition, a mass shooting is an incident in which four or more victims (not including the shooter) are murdered with firearms in a single, continuous incident in a public location that is unrelated to other types of criminal activity (such as gang violence).[6]

We consider all mass shootings that occur in the United States between 2011 and 2022 as within our scope of study. Figure 1 shows which districts in particular states were affected by mass shootings occurring during this time period. Additional details on these shootings and the selection decisions underlying the inclusion of these states in our analyses can be found in Table A.2.1. For California, we consider the effects of eleven mass shootings.

[6]We note that this definition certainly does not encompass all gun violence incidents that might reasonably act as focusing events or opportunities for agenda setting, but this definition covers a large number of shootings and is used widely in research on these incidents. See `https://sgp.fas.org/crs/misc/R44126.pdf`.

Figure 1: **U.S. State House and Senate Districts with Mass Shootings, 2011-2022**



Note: Red districts are those affected by a mass shooting, shown as the small, light red points at the location of the shooting. The map uses district boundaries adopted after the redistricting cycle following the 2010 Census, and only plots shootings that affected districts under these new maps (accounting for variation in timing of adoption of these maps across states). For a full list of affected districts and shootings (including those happening prior to redistricting), see A.2. Nebraska has a unicameral state legislature, so we do not report lower chamber districts and only show upper chamber districts on the map.

3.2 Measuring Legislators' Gun Policy Ideal Points

How polarized have legislators become on gun policy, and to what degree do mass shootings lead them to adjust their voting behavior on firearm-related bills? To answer this question, we first turn to a dataset maintained by the Giffords Law Center (GLC), a gun violence prevention advocacy organization.

3.2.1 GLC Firearm Bill Rankings and Bill Effects

From 2011 to 2023, the GLC hand-coded every bill introduced or enacted by every state legislature that was directly related to firearms policy — with the universe of legislation ranging from provisions allowing the permitless carry of concealed firearms, to large-scale budgetary items mentioning firearms in a single sub-item, to bans on assault-style weapons. This yields a total of 15,299 legislative attempts at the state level. For each of them, the GLC ranked it on a "weaken/neutral/strengthen" scale with regards to the bill's likely effect on a state's respective gun control policy environment (with "strengthen" bills making a state policy more restrictive and therefore more favorable to gun violence prevention advocates, for instance).[7]

Table 1: **Examples of Firearm-Related Legislation and Coding in California**

Session Year	Bill Number	Status	Description	GLC Ranking	Bill Effect
2013	A.B.1014	Enacted	Allows family members or law enforcement to petition for a Gun Violence Restraining Order (GVRO) if there is evidence that an individual poses a danger. The GVRO temporarily prohibits the individual from purchasing or possessing firearms or ammunition and permits law enforcement to remove existing firearms or ammunition.	Strengthen	+1
2013	S.B.916	Failed	This bill would allow a handgun manufacturer to return a model to the CA handgun roster without retesting if it was removed for reasons other than failing testing. It would expand exemptions for "new model" designation to include minor changes and permit dealers to sell off-roster handguns within 30 days.	Weaken	–1
2019	A.B.1009	Vetoed	This bill allows firearm transaction records to be submitted electronically, instead of by mail or in person, and authorizes the CA DOJ to charge reasonable processing fees for forms submitted by mail or in person.	Neutral	0

Note: Bill descriptions and coding provided conditionally as a part of a data sharing agreement with the Giffords Law Center (GLC) and cannot be shared as a part of the replication materials. Descriptions of bills have been edited from original bill descriptions provided by the GLC to remove any confidential information.

In section A.3 of the Appendix, we discuss our validation procedure used for the GLC Bill Rankings, and we ultimately accept that "weaken" laws (according to the GLC) can generally be considered permissive (-1), "neutral" as neutral (0), and "strengthen" laws as restrictive (+1). We refer to this -1/0/+1 score as the "Bill Effect" throughout this paper —

[7]Under the terms of a non-disclosure agreement dictating the amount and type of data that we are able to share in replication materials and publications, we arranged for conditional access to this as-of-yet untapped source of internal lobbying data.

a novel measure of the relative restrictiveness or permissiveness of a firearm-related legislative measure introduced in a state. Table 1 shows three examples of bills from California during the period that we study, with the original GLC ranking and the Bill Effect.

3.2.2 Measuring Legislators' Latent Position on Firearm-related Issues

Political Scientists have developed various methods of measuring a legislator's ideal points through their roll-call votes (e.g., Poole and Rosenthal, 1997; Ansolabehere, Snyder Jr and Stewart III, 2001; Shor and McCarty, 2011). To measure an individual legislator's position on firearm-related policies over time, we develop an issue-specific ideal point (λ), which we refer to as the "Gun Control Score", by combining the Bill Effects and the roll-call vote data for both chambers of the state legislatures:[8]

For state legislator i, let λ_i be their latent gun control ideal points in a given year. We first map each vote on firearm-related legislation in the direction of supporting restrictive policies (support for gun control) for direct comparison (including floor votes among the entire chamber and committee or procedural votes where available). We then employ the Bradley–Terry model (Bradley and Terry, 1952; Hopkins and Noel, 2022) so that in each of the pairwise comparisons between legislator i and legislator j, the probability of i being more supportive of restrictive policies than j is p_{ij}. Then, the log-odds corresponding to p_{ij} is:

$$\text{logit}(p_{ij}) = \lambda_i - \lambda_j \tag{1}$$

Assuming that each pairwise comparison is independent, the latent trait λ can be estimated by maximum likelihood. As we wish to compare legislators across time, we utilize at least two "bridge" legislators for every state and year to anchor the latent scores (λ_i) to establish intertemporal comparability. We assume that the underlying positionality of these legislators on gun control changes only modestly from one year to the next, and we utilize never-treated chamber leaders (i.e., Speaker, Speaker Pro-Tempore, Senate President Pro-Tempore, etc.) to identify these relatively stable bridging agents.[9] Finally, we rescale

[8]The California Assembly and Senate roll call vote data is accessed through California Legislative Information website (`https://leginfo.legislature.ca.gov`). Roll call voting data for other states is collected from LegiScan (`https://legiscan.com`).

[9]This assumption does not imply cross-state comparability. See Bailey (2007); Lewis and Tausanovitch

the scores to range from -1 to 1, from least to most supportive of restrictive firearms policies. For California legislators, the Gun Control Scores are calculated using 1,927 votes on firearm-related issues (on average, 160.6 votes each year).

Unlike the conventional methods of using a spatial choice model or Bayesian Item Response Theory (IRT) model on a subset of roll-call voting records, our scoring regime uses the direct Bill Effects to orient scores in the direction of supporting gun control policies. As robustness checks, we also estimate latent traits using the IRT model (Clinton, Jackman and Rivers, 2004) in A.6.1 and discuss alternative measures such as survey responses in A.4.1. Figure 2 shows the distribution of the Gun Control Scores over time for California state legislators. We pick up a small, upward trend for all California state legislators over the years, indicating that California's gun policy environment has become more restrictive just as the majority of legislators have become more favorable to restrictive policies, on average. However, there is still a clear separation between Democratic and Republican legislators, suggesting that partisan legislators are polarized on this policy domain.

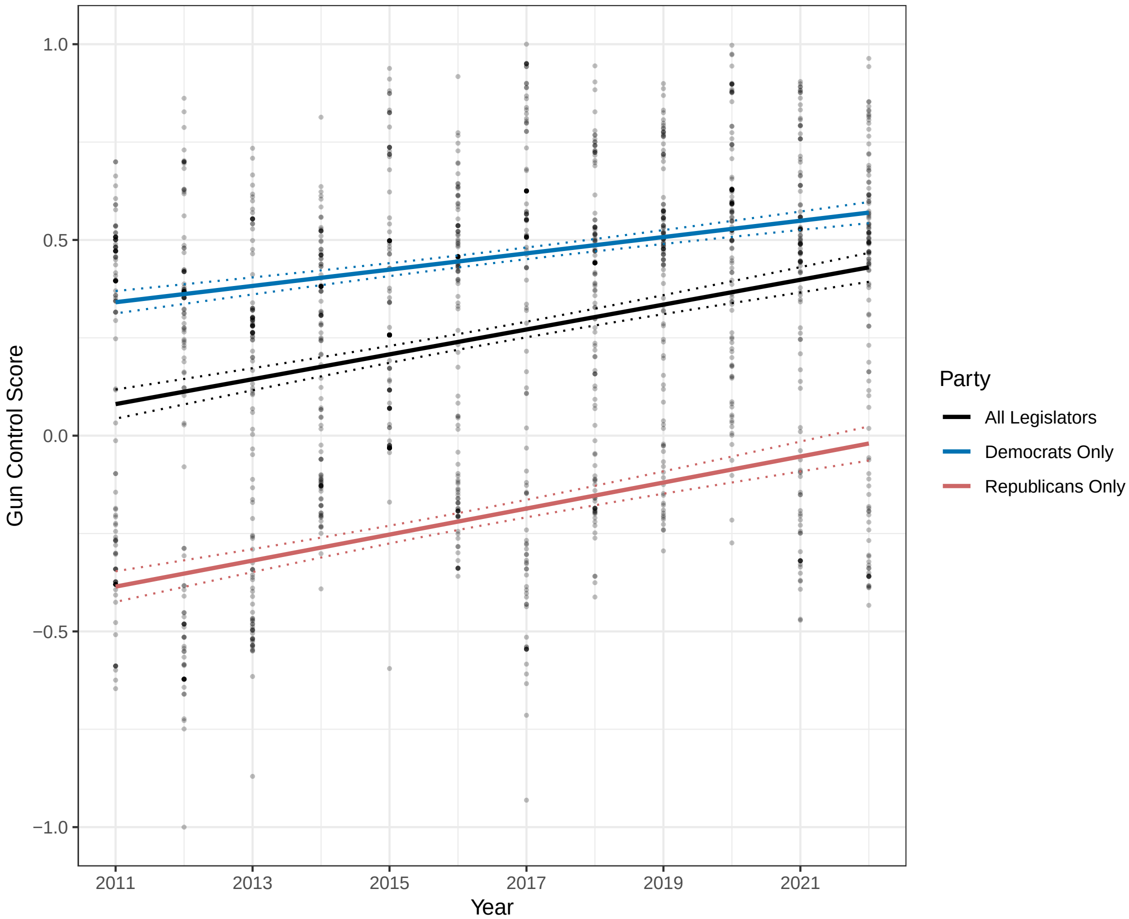
3.3 Treatment Assignment and Causal Identification

As seen in Figure 1 and Table A.2.1, the shootings we consider as "treatments" in this paper happened in different legislative districts across a wide time period, and in different states. Given this heterogeneity in treatment timing and location, traditional difference-in-differences (DiD) estimators (such as two-way fixed effects) are not appropriate for this setting as they fail to account for fluctuations in the effects of treatment due to adaptation, changing environmental conditions, or the immediacy of responses among treated units (Goodman-Bacon, 2021). For these reasons, we follow a stacked DiD estimation strategy similar to one used by Cengiz et al. (2019) to account for varied treatment timing and treatment effects. In Section A.5, we validate these results by employing the staggered DiD estimation strategy proposed by Callaway and Sant'Anna (2021).

The two central assumptions underlying these approaches are that of heterogeneous treatment adoption and parallel trends with never-treated units. For this first assumption, we

(2015); Lewis and Sonnet (2020) for discussions on the limitations on the comparability of ideal point estimates.

Figure 2: **Gun Control Score of California State Senate and Assembly Members by Party, 2011-2022**



Note: Each point is an individual legislator's score (N = 264, including legislators who switched party affiliation) for the corresponding year of the legislative session. Linear regression smoothers (with dotted standard errors) are fit to legislators in either party and across parties (colored by the party affiliation) to show general trends in partisan voting behaviors across session years.

posit that once units (legislators in state A) receive a treatment (a shooting in state A occurring within or near their district, depending on the specification), they remain treated throughout the entire period of observation and treatment is irreversible. For parallel trends with never-treated units, we assume that groups of units during periods where treatment is not yet applied serve as "clean" control units — in other words, legislators who have not had a mass shooting occur within or near their district (and never do within the time frame we study) serve as controls — and these units would have followed similar trends in voting behavior in all post-treatment periods. This assumption relies on two conditions that our setting satisfies: (1) there must be a sufficiently powered group of units that never received treatment (we have over 200 never-treated legislators in California, for example), and (2) these never treated units must be similar to those that receive treatment so that we can compare their outcomes. Additionally, we assume that there is limited anticipation of the treatment among all groups of units that are eventually treated. We report our validation and sensitivity analysis for these assumptions in Section A.5.3.

3.3.1 Formal Model

For our stacked DiD estimation strategy ala Cengiz et al. (2019), we create shooting-specific 2×2 datasets with clean controls for our California Analysis — in other words, datasets (g) including the outcome variable for the treated group and all other observations that are never treated within the observation window, removing units that were previously treated or will be treated in the future. These datasets are then stacked together and, with a shooting-specific identifying variable, a two-way fixed effects (TWFE) DiD regression is estimated on the stacked data, via the following regression

$$y_{it} = \alpha_{ig} + \omega_{ig} + \gamma_{tg} + \sum_{l=-K}^{-2} \mu_l D_{it}^l + \sum_{l=0}^{L} \mu_l D_{it}^l + \epsilon_{it} \tag{2}$$

where y_{it} denotes the Gun Control Score for legislator i in year t, ultimately yielding the average treatment effect on the treated units (ATT). Under this specification, we utilize relative-time indicators (i.e., "Years Since Shooting") for treatment (D_{it}^l) instead of a binary treatment indicator (i.e., D_{it}) from a canonical TWFE DiD regression. Here, D_{it}^l is an indicator for legislator i in year t and group E_i being k periods from the start of treatment (i.e., $D_{it}^l = \mathbb{I}[t - E_i = k]$), such that the first summation captures time periods leading to treatment, and the second summation captures those following treatment.

We include legislator- (α_{ig}), chamber- (ω_{ig}), and year-fixed effects (γ_{tg}), to account for individual- and time-specific heterogeneity and control for any unobserved factors that are constant within individuals in a chamber over time, or constant across all legislators within a time period. Robust standard errors are clustered at the legislator-, chamber-, and year levels. Further, to avoid issues with multicollinearity, we exclude the relative-time indicator for the period immediately prior to treatment. Hence, the μ_l's are our main parameters of interest and can be interpreted as the difference in the differences in Gun Control Scores across the treated and untreated legislators l years from a shooting, relative to the differences in Gun Control Scores between treated and untreated legislators in the excluded period ($k = -1$). One issue with the approach following Cengiz et al. (2019) is that the stacked regression estimator offers less flexibility for aggregation, and may introduce bias into the ATT (Baker, Larcker and Wang, 2022), and so to account for this, we turn to the formal specification from Callaway and Sant'Anna (2021) and report the results of a staggered DiD design in Section A.5.1.

3.3.2 Treatment Assignment

In assigning treatment, we consider the timing of a mass shooting during the course of a given year. If a shooting happens in the first 6 months of a year, the observations for that year are considered as post-treatment. If the shooting happens in the last 6 months, the observations for that year are considered pre-treatment, and the following year is considered post-treatment.[10] Additionally, our identification strategy requires both a pre- and post-shooting observation for legislators in order to assess the effects of the shooting on their respective voting behavior. To that end, we filter out observations for legislators who assume office in the same year that is considered as the first post-treatment period, and those whose last session in office is the final pre-treatment period.[11] See Section A.7 for additional details.

[10] The same strategy is ultimately used in Texas and Georgia, though most shootings occur between legislative sessions and therefore treatment timing is less attuned to what month the shooting occurred.

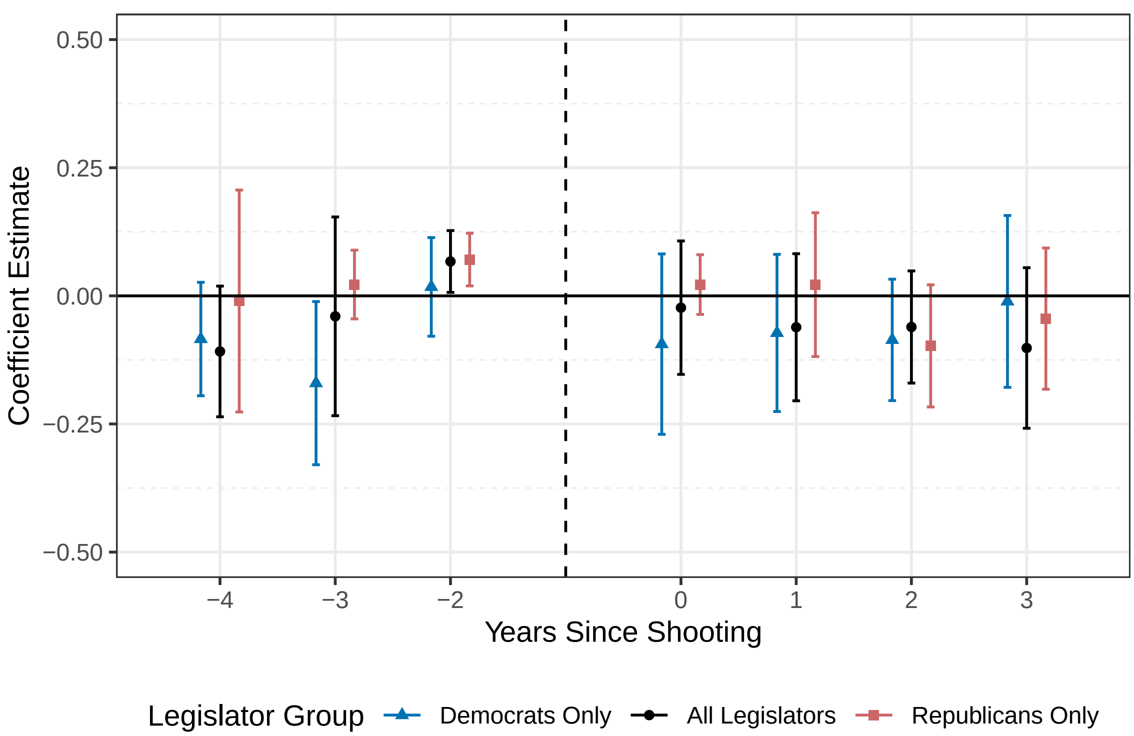
[11] From our original sample of 264 individual legislators in California with at least one year of a Gun Control Score, we are left with 255 total individual legislators for observation, and of the 24 initially treated legislators, we are left with 18 for our analyses, which can be seen in Figure A.7.1. We drop 3 additional control legislators in California who switched parties during the period of study. We similarly drop party-switchers in other states within our expanded analyses.

4 Results

4.1 Shootings Occurring Within a California Legislator's District Do Not Affect Their Position on Firearm-Related Policies

We report the results of our stacked DiD estimation for California legislators in Figure 3 and Table 2. For each time period relative to treatment,[12] we estimate the average treatment effect on the treated (ATT) legislator's Gun Control Score, and consider never treated legislators as our control group.

Figure 3: **ATT Estimates for Effect of Mass Shooting on Gun Control Score Demonstrate Null Results, Across Party, for California Legislators**



Note: This figure plots the ATT estimates for the periods prior to treatment ($k = -4$ to -2), the first post-treatment period ($k = 0$), and the periods 1 to 3 years following treatment ($k = 1$ to 3, respectively). The dashed vertical line represents the onset of treatment. 95% confidence intervals are based on robust standard errors clustered at the legislator-, chamber-, and year levels. We confine the periods for which we report our estimates to only those that are most proximate to treatment, as we expect that these periods would be the most responsive to the mass shooting.

Figure 3 suggests that the occurrence of a mass shooting within a legislator's district

[12] Under this design, the last post-treatment period of observation (effectively, t or k = -1) is dropped to account for multicollinearity.

does not cause a legislator to change their gun control positions, on average. This null finding persists across specifications comparing treated legislators to all other legislators (black circular points), and comparing treated legislators to untreated legislators who are members of the same party (red squares for Republican legislators and blue triangular points for Democrats, respectively). We also report the formal ATT estimates for the first post-treatment time period, $k = 0$, by each respective legislator group, in Table 2. In Table A.5.1, we report the full results of our staggered and stacked DiD designs across all three legislator groups.

Table 2: **No Immediate Effect of Mass Shootings on California Legislator's Support for Gun Control, 2011-2022**

	Gun Control Score		
	(1)	(2)	(3)
Shooting	-0.023	-0.094	0.022
	(0.066)	(0.090)	(0.030)
Legislator Group	All	Democrats	Republicans
# of Legislators	234	156	78
# of Mass Shootings	11	6	5
Legislator FE	✓	✓	✓
Chamber FE	✓	✓	✓
Year FE	✓	✓	✓
Pre-Treatment Mean	0.254	0.457	-0.241

Note: Robust standard errors are clustered at the legislator-, chamber-, and year levels in parentheses. Gun Control Score is measured from -1 to 1, from least to most supportive of restrictive firearms legislation. Table reports the estimated effects of mass shootings occurring within a legislator's district on a legislator's Gun Control Score for the first post-treatment period, $k = 0$. Pre-Treatment Mean is the average gun control score of control and treated legislators in the pre-treatment period.

Across all three specifications, our results are null, and considering that the Gun Control Score is measured on a -1 to 1 scale, the coefficient in Column 1 of Table 2 can be interpreted as a 1.15% decrease in a legislator's support for restrictive firearms policies. The ATT in column 2 represents a 4.7% reduction among California Democrats and the estimate in column 3 translates to a 1.1% increase among California Republicans, with 95% confidence intervals spanning opposite directions. In sum, we find no statistically significant change in a California legislator's annual Gun Control Score after a mass shooting occurs within the bounds of their district. We find similarly null results in Section A.5, in which we rely upon the staggered DiD method. In Section A.6.1, we report the results of these same model specifications but use a Bayesian IRT model to estimate positionality on gun policy as a robustness check (rather than using the Gun Control Score), and, again, find null results.

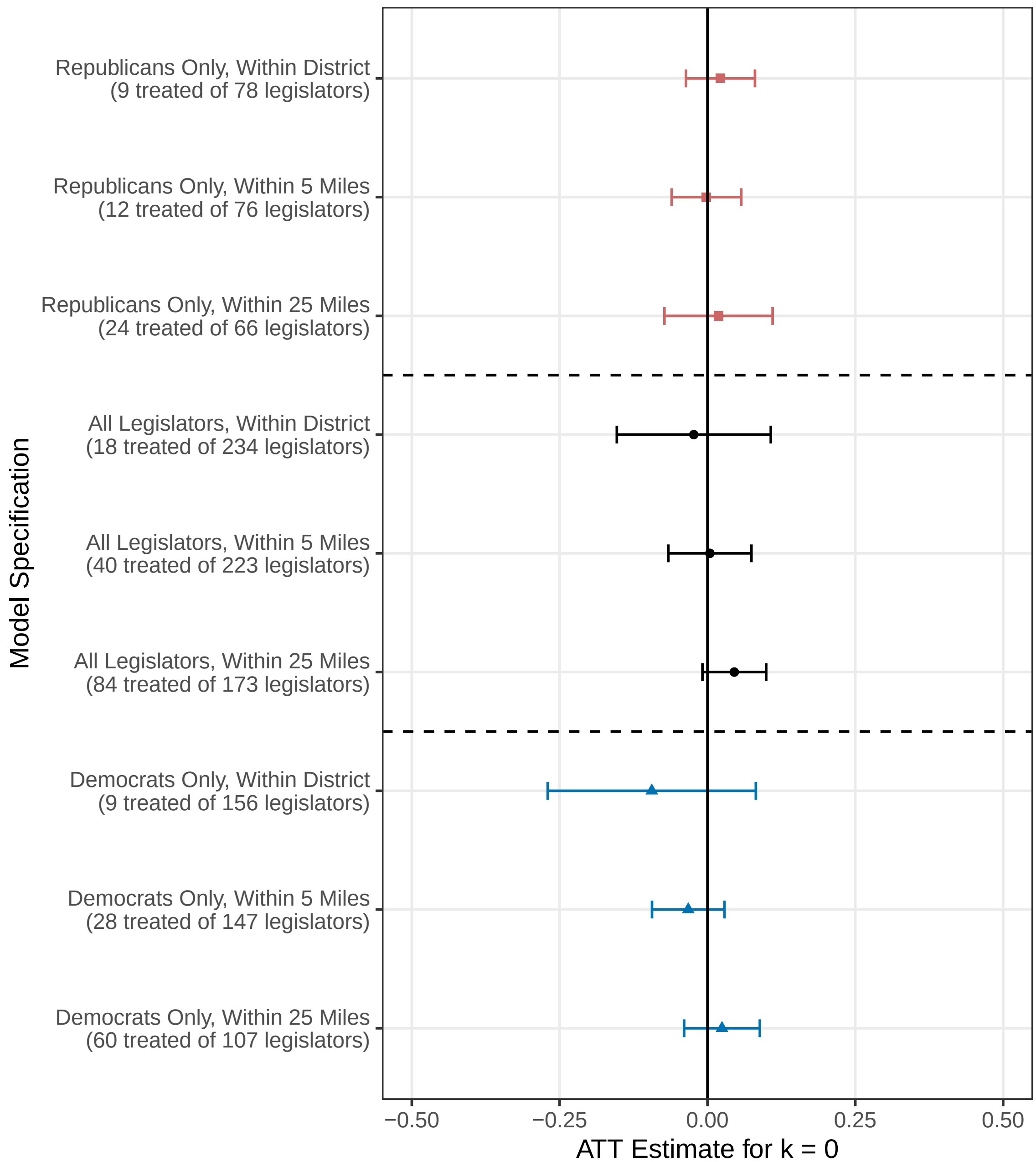
Our decision to limit the periods for which we report the ATT is motivated by a concern that periods far after treatment are subject to some source of unobserved confounding that our models are not accounting for. As only certain "types" of legislators (i.e., legislators who consistently get re-elected, or those who go on to serve in another chamber, for instance) persist as observations into the periods far after treatment (or, inversely, "survive" to eventually be treated from periods long before the shooting), the variation in the Gun Control Scores for these legislators is potentially biased by increased (or decreased) homogeneity in the observations, and a smaller number of observations in general. This can also be thought of as a form of selection bias: we are losing observations in the tails due to legislators not being re-elected or being term-limited from holding office again. With that in mind, our estimates closer to treatment are better powered and estimates far from treatment are less precise. This highlights some limitations that the stacked design features when compared to the staggered design that we use as a robustness check for our main results and report in Section A.5. For instance, our uncertainty estimates may be less realistic in the years far from treatment as a lack of observations of treated legislators may lead to narrower confidence intervals due to less variability in our observations.[13] In the following section, we widen our definition of treatment, and we ask whether legislators who represent a district *near* a mass shooting experience changes in behavior — not limiting treatment assignment to only those legislators who *represented* the districts in which the shootings occurred.

4.2 Null Effects of Mass Shootings Regardless of Distance from Shooting Location

One concern with our previous approach is that we may have been overly restrictive in our treatment assignment. Mass shootings are highly salient events that presumably affect

[13]We find that the confidence intervals in the periods far removed from treatment are generally larger under the design from Callaway and Sant'Anna (2021). Instances of particularly narrow confidence intervals in periods far from treatment appear to instead be driven by attrition among our treated units. Due to California's relatively strict term limit law (12 years in any legislative chamber), we are ultimately limited in assessing the long-running effects of a shooting as legislators tend to be term-limited from holding office before reaching, say, $k = 10$ and having sufficient pre-treatment observations to be included in our analysis.

Figure 4: **ATT Estimates for Effect of Mass Shooting on Gun Control Score Demonstrate Null Results, Across Parties, at Varying Distances from Shooting**



Note: We report the null effects of a mass shooting on the voting behavior of CA state legislators on firearm-related policy, with the stacked DiD models fit on all legislators, only Democratic legislators, and only Republican legislators — at varying distances of treatment assignment. For the models fit to legislators within 5 or 25 miles of a shooting, we consider legislators whose district boundaries intersect at any point within a 5 or 25 mile radii drawn from the location of a shooting as treated, respectively.

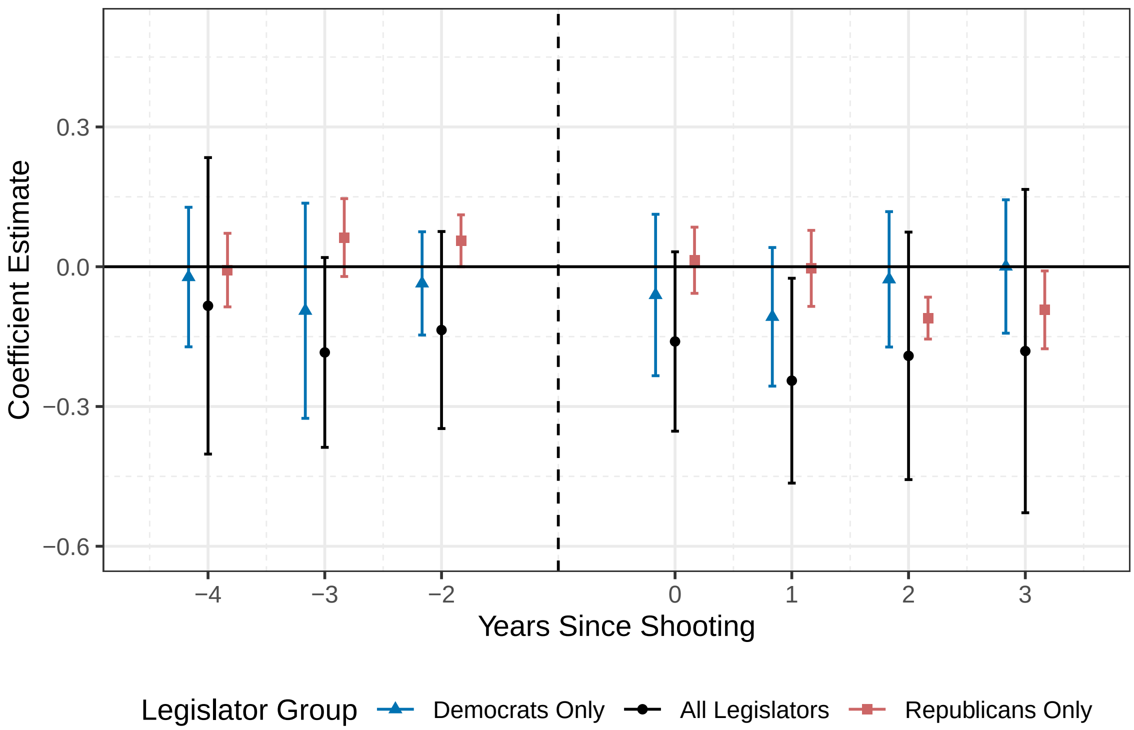
individuals in geographic areas that may be larger than the average legislative district in California (Hassell, Holbein and Baldwin, 2020; Reny et al., 2023), which suggests that we ought to account for possible spillover effects from mass shootings. To identify legislators who may reasonably be treated (beyond those representing the districts in which the shootings occur) we turn to neighboring districts. In Section A.8, we detail the strategy through which we identified legislators that would be affected if we expanded our definition of treatment beyond just shootings occurring within a legislator's district. In this section, we consider districts whose boundaries intersect with or are contained within 5 and 25 mile radii drawn from the location of the shooting, respectively.

As seen in Figure 4, as we expand the geographic range for treatment assignment, the number of total treated legislators within each model increases, and the total number of legislators decreases. As we expand the number of legislators that are treated by each shooting, we encounter the possibility of legislators being multiply-treated throughout our study period — in widening the boundaries, some districts and legislators receive treatment at multiple time periods. Following the framework of Cengiz et al. (2019), we retain multiply-treated units for their first treatment event, but remove them as treated units in subsequent shootings in order to avoid bias from additional exposures. This "first treatment only" approach yields a more robust estimation of the ATT in initial treatment periods without reducing our sample size. Even with this broadened definition of treatment, Figure 4 suggests that when the effects of a mass shooting are assumed to be felt by legislators who are geographically proximate to a shooting — and not just the representatives for the district in which the shooting occurred — mass shootings do not appear to cause changes in a legislator's positionality on gun control, regardless of party.

4.3 No Effect of Mass Shooting on Gun Control Score of Affected District's Representatives

Our prior results estimated the effect of a mass shooting on the Gun Control Score of the individual legislators representing the district at the time of the shooting. In this section, we estimate the effect of a mass shooting on the Gun Control Score of any of the legislators representing a district in which a shooting occurred. In this sense, “treated” legislators are those who held office in the lower or upper chamber of an affected district, though they need not have been in office at the time of a shooting. As we continue to rely on never-treated units as our control group, control units can only be legislators who never held office in a district that was previously or subsequently affected by a mass shooting.

Figure 5: **ATT Estimates for Effect of Mass Shooting on Gun Control Score Demonstrate Null Results When Treatment is Assigned to District, 2012-2022**

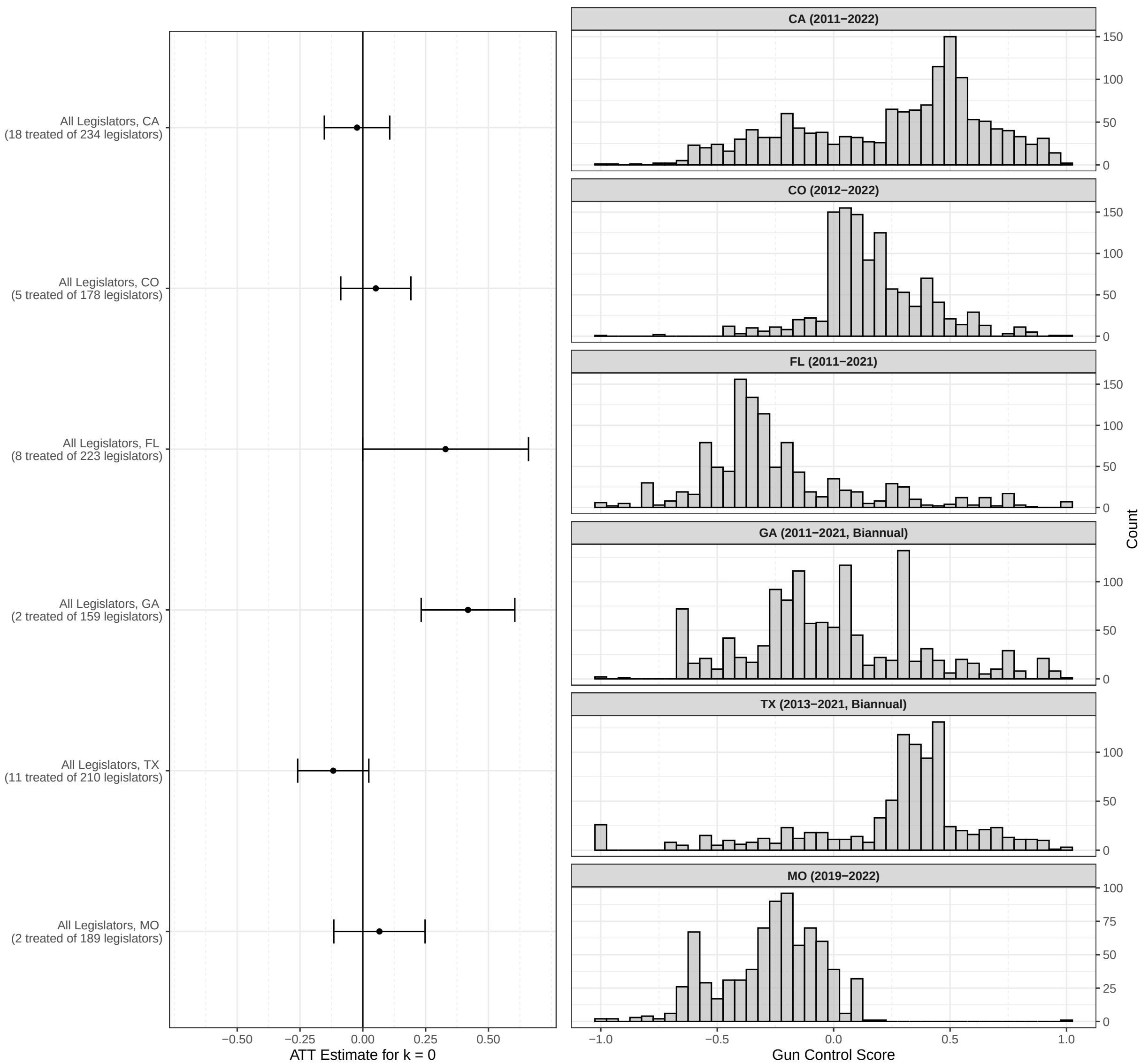


Note: We report the null effects of a mass shooting on the voting behavior of CA state legislators on firearm-related policy, with the stacked DiD models fit on all legislators, only Democratic legislators, and only Republican legislators — who, at any time, represented a district that was affected by a mass shooting. Full results and additional details can be found in Section A.9.

One might think that mass shootings cause shifts in opinions on firearms policy among the electorate that lead to changes in the firearms-related policy positions of elected officials representing said district. Scholarship demonstrating the relative stability of public opinion on firearms-related issues would suggest that this is not the case (Baxter-King, 2024; Hassell, Holbein and Baldwin, 2020), and our results are in line with these findings. Figure 5 demonstrates that mass shootings have no effect, on average, on the Gun Control Score of state legislators representing affected districts. See Table A.9.1 for the full results.

5 Null Result Holds Across Additional States

Figure 6: **Null Effects of Mass Shootings Across All States Generally Holds, 2011-2022**



Note: The left panel presents the coefficient estimates (ATT at $k = 0$) capturing the effect of mass shootings on the Gun Control Scores of legislators who experienced a shooting event in their district, relative to all other state legislators who did not (and never subsequently did). Study periods and the number of scored legislators vary by state due to data availability, and Gun Control Scores for Georgia and Texas are measured biannually. The right panel shows histograms of the distributions of Gun Control Scores for all legislator-year observations in each state for the entire study period. These scores should not be compared across states. States are arranged from most to least restrictive based on their GLC quintile ranking.

To validate our null result for California lawmakers and discern whether the finding is generalizable in other contexts, we utilized GLC bill rankings paired with roll-call voting records from LegiScan in five additional states — Colorado, Florida, Georgia, Missouri, and Texas — to estimate the Gun Control Scores for an additional 1,593 state legislators between 2011 and 2022.[14] The addition of these five states brings the total number of mass shootings that we can study to 28, as seen in Table A.2.1.

Our estimation strategy for each state follows the one outlined in Section 3.3.1. The results of this analysis can be seen in Figure 6, in which we plot the ATT estimates at $k = 0$ for each state and the underlying distribution of the Gun Control Score across all legislator-years.

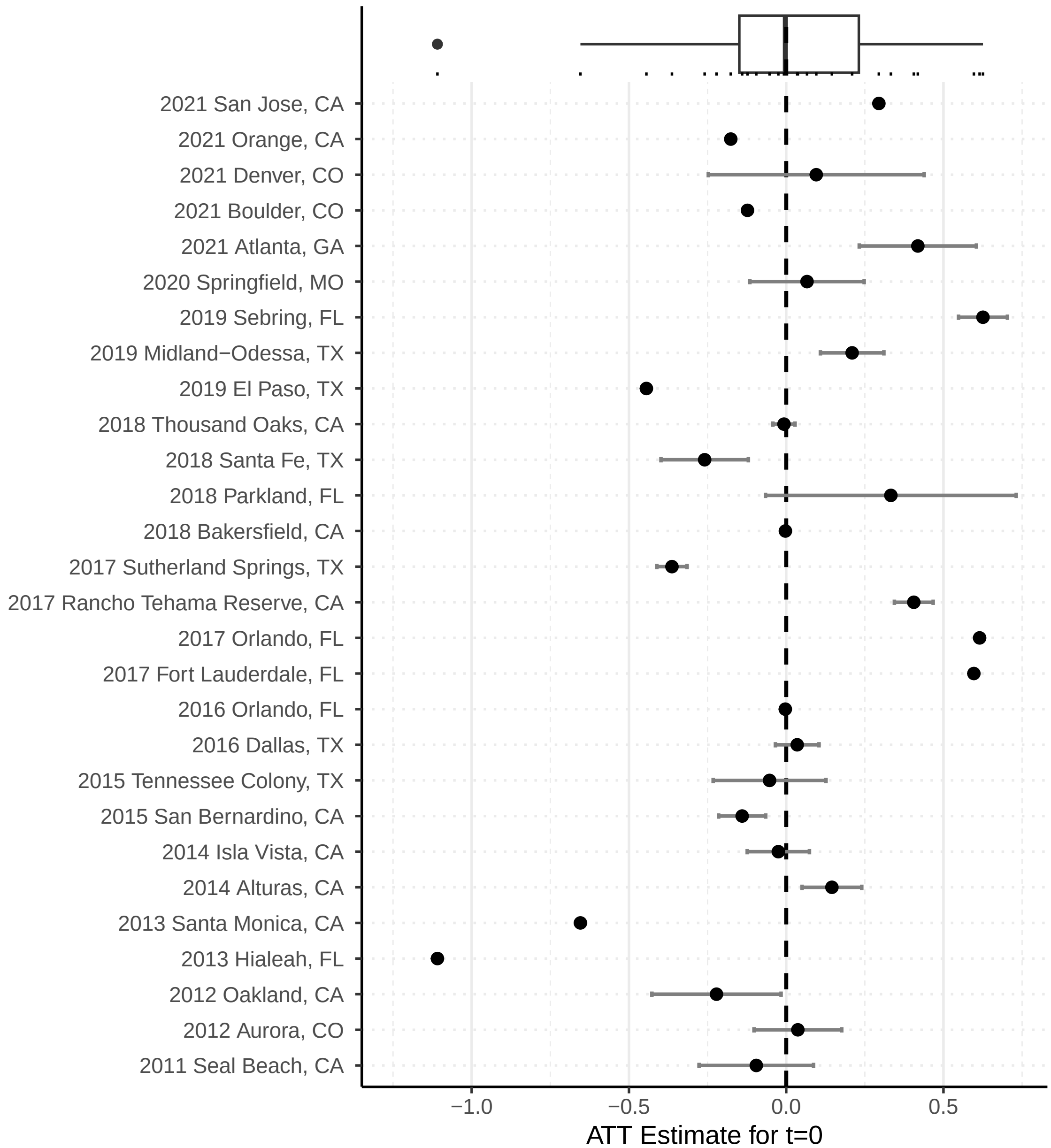
Figure 6 demonstrates that, as in California alone, mass shootings do not appear to have any statistically significant effect on the Gun Control Score of state legislatures representing the districts in which the shooting occurs in Colorado, Florida, Missouri, and Texas. One exception is Georgia, though due to the timing of the only shooting occurring in this state in this period, we are only able to estimate the effects of the shooting on one post-treatment period. However, this may suggest that individual shootings produce statistically significant results that are washed away through the aggregation strategy underlying our design. We investigate this possibility further in the following section. Taken together, our results in this section confirm the generalizability of the null effect that we estimate in California: mass shootings do not cause a change in a legislator's positions on firearm-related policy, on average.

6 Do Shootings Produce Differential Effects?

Although our primary analyses estimate the average effect of mass shootings on legislators' gun policy positions, disaggregated (event-specific) analyses provide important insights into heterogeneity (Figure 7). We re-estimate our stacked models using two-way fixed effects

[14]Unfortunately, merging the LegiScan data and the GLC Bill Rankings led to some roll call votes not being included. This is an issue inherent with the LegiScan data as not all roll call votes are collected in every state in every year. We only report results for states with the most complete roll call data, and we drop years for which multiple votes on known gun-related bills are missing.

Figure 7: **Event-Specific Heterogeneity in Shooting Effects, Aggregate Impacts Remain Near Zero**



Note: This figure presents the distribution and individual estimates of the average treatment effect on the treated legislators at $t = 0$ for mass shootings included in our overall analyses. The top panel displays the overall distribution of ATT estimates as a boxplot, with individual estimates indicated by rug marks and the dashed vertical line denoting the null (0) effect. The bottom panel shows the corresponding forest plot of ATT estimates for each shooting event, with 95% confidence intervals where sufficient treated legislator observations exist; estimates based on a single treated legislator are shown without intervals. Positive values indicate an increase in Gun Control Scores following a mass shooting, while negative values indicate a decrease.

difference-in-differences models run separately for each shooting in our dataset (without partisan restrictions) and find substantial variation across events. Several shootings are associated with significant increases in gun control support (e.g., 2014 Alturas; 2017 Rancho Tehama; 2021 San Jose; 2021 Atlanta), whereas others correspond to declines (e.g., 2012 Oakland; 2015 San Bernardino; 2017 Sutherland Springs; 2019 El Paso).

These heterogeneous effects appear to be shaped by contextual features, including the demographic and political composition of the district (Hassell, Holbein and Baldwin, 2020), the number and race of victims (Markarian, 2024; Walker, Collingwood and Bunyasi, 2020), and the intensity and framing of media coverage (Baxter-King, 2024). Highly salient shootings in progressive contexts (such as 2018 Parkland) tend to spur pro-restrictive shifts, while those in conservative contexts (2015 San Bernardino or 2017 Sutherland Springs, for example) often reinforce support for permissive policies. In the Supplemental Materials (Section A.10), we report disaggregated treatment effects across each shooting and by characteristics of interest and find that the "type" of shooting likely affects the directionality and scale of the affected legislator's response — though estimating an overall effect for, say, school shootings is complicated by pooling methodologies and constraints in comparing Gun Control Scores across states.

Importantly, while the aggregated average treatment effect across all shootings is close to zero, this masks substantial event-level heterogeneity. Focusing on the average effect minimizes spurious findings from multiple comparisons, but the disaggregated estimates highlight when and where legislative responses deviate meaningfully from the mean.

7 Do Legislators Respond to Shootings in Other Ways?

The preceding sections focus on only a subset of behaviors that legislators engage in: roll call voting on introduced bills. Would legislators respond to mass shootings in other ways that the Gun Control Score may fail to capture?

In the California State Legislature, legislators frequently sign bills as a cosponsor as a means of signaling support for a bill in a low-cost manner. We manually collected the sponsor and cosponsor lists of all firearm-related bills in both chambers of the California

State Legislature from 2011 to 2022. Following Fowler (2006), we construct a directed cosponsorship network, with each edge drawn from a cosponsor of a bill to its sponsor. Figure A.11.1 describes this network. This network is densely interconnected, particularly among Democrats, suggesting partisan homophily in the sponsorship network. Table A.11.1 records the most "important" legislators in each session and chamber, measured by highest Outdegree (the number of bills a given legislator cosponsors in a year) centrality, Betweenness centrality, and Eigenvector centrality.

How might mass shootings affect the sponsorship and cosponsorship behaviors of California lawmakers? Beyond Outdegree (which only captures the cosponsorship activities), we define "Activity" as the fraction of firearm-related bills each legislator sponsors and cosponsors out of all firearm-related bills in a year. Together, these measures serve as proxies for a legislator's level of activity in the gun policy domain, along distinct albeit complementary dimensions. In Table A.11.2, we report the results of our stacked DiD on these two outcomes of interest — Activity and Outdegree. We only find a statistically significant decrease in both the overall Activity measure and the Outdegree measure for treated Democrats, relative to other Democrats, following a shooting in the first post-treatment period. This suggests that treated Democratic legislators in California are less active in the gun policy domain, on average, in the year immediately following a mass shooting. This estimate, however, may be biased by possible pre-trending observed in the pre-treatment periods, suggesting a violation of the parallel trends assumption.

8 Conclusion

In this paper, we introduce a novel measure of an individual legislator's position on firearm-related issues, measured on a permissive (-1) to restrictive (+1) scale. In introducing this scale, the Gun Control Score, our study contributes a robust tool for understanding legislative behavior on policy-area-specific issues. This approach enables a more nuanced analysis of legislative responses to mass shootings, a topic that remains underexplored despite its policy relevance. We define treated legislators as those who represent the districts in which mass shootings occur and we assess the effects of a shooting on the behavior of an individual elected official. While our analysis does not identify statistically significant changes in voting behavior, the findings underscore the durability of partisan polarization in shaping legislative responses to mass shootings. This resilience is evident across diverse states, legislative contexts, and definitions of treatment.

In California, we find a statistically insignificant 1.15% reduction in treated legislator's support for restrictive firearm-related policies, compared to all other legislators, an insignificant 4.7% reduction among California Democrats, and an insignificant 1.1% increase among California Republicans. This null finding persists when we expand our definition of treatment to consider the role of geographic proximity and the effects of mass shootings on the voting behavior of legislators who represent districts near the shooting's location and not just the district in which the shooting occurred.

When we expand our analysis to include Colorado, Florida, Missouri, and Texas, we again estimate statistically insignificant effects of mass shootings on legislators compared to members of their own party. However, the aggregation method underlying the stacked DiD design masks the heterogeneity of treatment effects of individual shootings and our disaggregated results suggest that legislators' responses vary by incident. Our scoring regime ultimately covers a total of 1,864 unique, individual legislators across six states from 2011 to 2022. While our study does not include every U.S. state that experienced a mass shooting during this period, our selection — guided by data availability and designed to reflect a range of firearm policy environments — covers approximately 42% of all mass shootings nationwide between 2011 and 2022. Although drawn from a limited number of states, our included set captures meaningful variation in policy environments and provides a basis for estimating an aggregate effect — which, in our analysis, appears to be null, on average.

Our findings highlight the limits of mass shootings in altering entrenched partisan dynamics, suggesting that focusing events, broadly construed, may have differential impacts based on the issue domain and political context. Legislators we interviewed told us that mass shootings led them to sponsor additional gun safety legislation, or reconsider how they could best represent the victims of mass shootings, but also admitted that pressures from party leaders constrained their autonomy in responding to the tragedy at hand. Others suggested that political differences within their constituencies led them to walk a "fine line" as a representative — they could not push a policy agenda at odds with the wishes of the families of victims, for instance. This was in stark contrast to others who told us that the tragedy offered an opportunity to push through policy changes deemed "great" by some legislators and "unconstitutional Trojan horses" by others. Heterogeneity persisted across our quantitative and qualitative results. Future studies focusing on other policy domains could assess whether the limited effects estimated in this study are unique to firearm-related issues or generalizable to other policy areas marked by polarization. In this sense, policy-specific ideal points estimates need not be tailored to firearms-related policies alone.

This nuance helps contextualize our finding that individual shootings produce differential effects. Some scholarship suggests that particular mass shootings may lead to unique responses, with characteristics such as the race of victims leading to disparate legislative reactions or swings in public opinion (Markarian, 2024; Walker, Collingwood and Bunyasi, 2020).

Partisan polarization has contributed to a stagnant firearms policy environment at the federal level, and our findings suggest that this may persist at the sub-national level both within the California Legislature and across five other states with varying gun policy environments. Our results suggest that while state legislatures are active on gun policy issues, mass shootings do not cause any measurable changes in individual-level voting behaviors of elected officials, on average. Every legislator we spoke with said, at different points, that they felt a degree of responsibility to act in the aftermath of the shooting, and yet our results suggest that these events, on average, do not cause a legislator to change their positions on firearm-related policies. However, our disaggregated results could also be interpreted as evidence that individual legislators are responsive to mass shootings, even as aggregating across shootings and legislators washes this effect away. Further, our findings do not indicate that a state's firearm policy environment is necessarily stagnant — shootings may be changing the scope of legislation that lawmakers vote on (Luca, Malhotra and Poliquin, 2020), but that is beyond the scope of our current study.

This has mixed implications for the state of representative democracy, broadly construed. On the one hand, the stagnation we observe in the voting behavior of state legislators suggests that they are not accountable, on average, to focusing events that may otherwise be thought to incite changes in their behaviors. On the other hand, perhaps this is to be expected, given the relative stability of public opinion on firearm policies after mass shootings. Regardless, while we do not intend to claim that individual votes on niche policies by state legislators capture the attention of the mass public, the outcomes of these votes do have long-lasting and far-reaching implications for accountability and representation.

Ultimately, our study demonstrates that mass shootings, while profoundly tragic, fail to cause disruptions in the entrenched partisan polarization that dominates gun policy debates in the United States. This stagnation underscores the need for innovative approaches to policymaking that do not rely upon legislative inertia, particularly at the state level. Without addressing these barriers, the potential for meaningful policy responses to mass violence will remain constrained.

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Appendix

Intended for online publication only.

Contents

A.1 Qualitative Interview Procedures and Results

To inform our theoretical expectations, we conducted a series of qualitative interviews with California state legislators who represented districts affected by mass shootings that occurred within their district.[15] Our sample of 5 interview participants worked out to be a roughly 21% response rate of all legislators who represented districts affected by mass shootings during our period of study.[16] All interviews except for the one with Legislator E were conducted on Zoom, and the interview with Legislator E was conducted in their field office, in person, with their chief of staff also present.

The following questions were used to guide the interview:

- How did you first become aware of the mass shooting, and what were your initial reactions upon learning about it?
  - Follow-ups:
    - Did you feel personally affected by the shooting considering your role as an elected representative for the community?
    - How did you engage with the affected community members, survivors, and families of victims in the aftermath of the mass shooting?
- What were some of the primary concerns expressed by your community following the mass shooting, and how did community leaders address those concerns?
  - Follow-ups:
    - How did you collaborate with other legislators, government agencies, and community leaders in your response to the mass shooting?
    - Were there any specific challenges faced during this collaborative effort?

[15] As these interviews were conducted with public officials about their roles as public servants, our project is exempt from IRB regulations typically associated with human subject research. That said, we followed all relevant IRB guidelines to ensure the validity of our findings.

[16] All other affected legislators were contacted, though 5 directly refused to participate. All others either did not respond directly (6 legislators), or stopped responding after we made initial contact (7 legislators). We were unable to make contact with an additional legislator who did not have any contact information publicly available.

- In what ways did the mass shooting in your district influence your legislative priorities and agenda moving forward?
  - Follow-ups:
    - Can you discuss any legislative measures or policies you proposed or supported in response to the mass shooting?
    - What was the rationale behind these proposals?
- Were there any specific challenges or obstacles you encountered while attempting to implement legislative or policy changes in response to the mass shooting?
  - Follow-ups:
    - Did you face any obstacles or support from members of the opposing party?
    - Did you face any obstacles or support from interest groups?
- Looking back, what do you feel were the most significant lessons learned from your experience responding to the mass shooting, both personally and legislatively?
  - Follow-up:
    - What advice might you offer to other legislators or leaders whose communities are affected by mass shootings in the future?
- Is there anything else that you would like to share with us?

While all participants waived anonymity, we ultimately decided to keep all quotations anonymous due to the sensitive nature of the interviews. While all interviews were recorded and transcribed, we will not be publicly sharing the full transcripts of the interviews in order to protect the anonymity of the participants.

A.2 Locations and Details of Mass Shootings

In this paper, we consider the effect of mass shootings on legislative behavior in California from the 2011-2012 legislative session to the 2021-2022 session. To that end, we focus on shootings occurring between 2011 and 2022 in the state, though we also include the 2010 North Hollywood shooting that may have reasonably affected legislators serving in the 2011-2012 session. As these legislators would reasonably be considered treated in these early sessions, we cannot use them as control units in our later estimations. Table A.2.1 identifies the shootings that are included in our study — specifying the date, location, number of victims (not including the shooter), and the State Senate and Assembly districts in which the shooting occurred (accounting for redistricting following the 2010 Census).

Table A.2.1 highlights an important feature of the data worth considering: these mass shootings, like many others across the United States, are concentrated in urban areas with higher rates of population density (Peterson and Densley, 2022; Kwon and Cabrera, 2019). Of the California shootings studied in this paper, only two (the 2017 Rancho Tehama and 2018 Bakersfield shootings) occurred outside of the San Francisco Bay Area or Greater Los Angeles metropolitan areas. However, even as the California shootings studied in this paper tend to affect the Bay Area or Los Angeles more frequently, no individual district or legislator is repeatedly treated by having a mass shooting occur *within* their district on multiple occasions. Furthermore, of the California legislators representing these districts at the time of the shooting and included in our study, 9 were Democrats and 9 were Republicans, offering us even numbers of treated legislators from either party.

Table A.2.1: **Shootings Included in Study and Districts Immediately Affected**

State	Date	City	# Killed	Upper	Lower	Post-Treat
CA	10/12/2011	Seal Beach	8	35	67	2012
CA	4/2/2012	Oakland	7	9	16	2012
CO	7/20/2012	Aurora	12	29	29	2013
CA	6/7/2013	Santa Monica	5	26	50	2013
FL	7/26/2013	Hialeah	6	36	103	2014
CA	2/20/2014	Alturas	4	1	1	2014
CA	5/23/2014	Isla Vista	6	19	35	2014
TX	11/14/2015	Tennessee Colony	6	3	8	2016
CA	12/2/2015	San Bernardino	14	23	63	2016
FL	6/12/2016	Orlando	49	9	47	2016
TX	7/7/2016	Dallas	5	23	108	2017
FL	1/6/2017	Fort Lauderdale	5	34	100	2017
FL	6/5/2017	Orlando	5	13	47	2017
TX	11/5/2017	Sutherland Springs	25	21	44	2018
CA	11/14/2017	Rancho Tehama	5	4	3	2018
FL	2/14/2018	Parkland	17	29	81	2018
TX	5/18/2018	Santa Fe	10	11	24	2018
CA	9/12/2018	Bakersfield	5	14	32	2019
CA	11/7/2018	Thousand Oaks	12	27	44	2019
FL	1/23/2019	Sebring	5	26	66	2019
TX	8/3/2019	El Paso	23	29	76	2020
TX	8/31/2019	Midland-Odessa	7	31	82	2020
MO	3/15/2020	Springfield	4	30	135	2020
GA	3/16/2021	Atlanta	8	36	40	2021
CO	3/22/2021	Boulder	10	18	13	2021
CA	3/31/2021	Orange	4	37	68	2021
CA	5/26/2021	San Jose	9	15	27	2021
CO	12/27/2021	Denver	5	32	2	2022

Note: Data on shooting location and number of victims (not including the shooter) comes from the Violence Project. Upper refers to the upper chamber district in which the shooting occurred and Lower refers to the respective lower chamber district. Both of these districts are identified through mapping the latitude/longitude location of a shooting to the appropriate state legislative district shapefiles from the United States Census Bureau. These locations are cross-referenced with records from the Gun Violence Archive, when available. The 2010 North Hollywood, CA shooting is effectively dropped from our analyses – as we do not have pre-shooting observations, we cannot consider the effects of the shooting on the voting behavior of affected legislators, so the legislators representing this district that appear in our data from 2011-2022 are considered as previously treated, and removed from subsequent models. Post-Treat reports the year that is considered as $k = 0$ in our formal models, and is assigned as the year of the shooting date if the date of the shooting occurs within the first six months of the year, or the subsequent year following the year of the shooting date if the shooting occurs within the last six months of the year. For shootings occurring before the redistricting done following the 2010 Census, we report the district that was affected under the earlier maps, and cross-reference district boundaries after the Census to ensure that legislators who were redistricted to represent a new area are still considered treated if they previously represented an affected district. District shapefiles from this earlier period are sourced primarily from the Redistricting Data Hub.

A.3 Validating the GLC Legislative Scoring Regime

As the GLC claims on their website, "Giffords is an organization dedicated to saving lives from gun violence." In other official communications, the GLC states that it "is fighting to end the gun lobby's stranglehold on our political system," which may reasonably lead one to believe that the GLC scoring system could suffer from pro-gun control bias in their scoring of state firearm-related legislation. To validate their scoring regime, we hired a team of research assistants (RAs) to hand-code a sample of the bills also coded by the GLC to assess the degree to which the GLC data may have been biased due to the original coder's association with a gun control advocacy organization. We provided the RAs with instruction on the types of firearms policy that exist within the United States and instructed them to code bills on a -1 to +1 scale, with -1 being considered a "permissive" bill that makes it easier for individuals to access/purchase/own/possess firearms, 0 being a neutral policy, and +1 being a "restrictive" policy that makes it harder for someone to access/purchase/own/possess firearms.

After coding samples of bills from all states and from California in particular, we found that our RAs had an average of a 73.5% match with the GLC coding scheme, and 80% consistency with each other. After digging into this further, we determined that the majority of mismatches between the RAs and the GLC were from the RAs considering many of the procedural budgetary bills as neutral, and the GLC coding some bills our RAs coded as neutral as "strengthen" or "weaken" based on very specific clauses or items in the bills that our RAs had not considered to be as relevant to the overall bill. Overall, we are confident that our RAs' coding is consistent with the GLC coding scheme, and given the small number of uncoded bills in California (25), we used the bill ranking provided by the GLC when available, and filled in all missing bill rankings with assessments from our RAs (and confirmed by the authors). We are therefore left with 366 bills that were voted on between 2011 and 2022 in the California Legislature.

A.4 Measuring Firearm-Related Policy Preference

Leading scholars in political methodology have developed various methods to estimate legislators' ideal points using data from roll-call votes, survey responses, campaign contributions, political speeches, and social media. But how do these ideological measures translate to a context in which we wish to specifically capture a legislator's positionality on a policy domain as particular as gun control? Considering that other scholars argue that gun control preferences are not effectively computed through ideology estimates derived from a legislator's *overall* roll call voting behavior (Ansolabehere, Snyder Jr and Stewart III, 2001), we are constrained in our ability to rely on such canonical measures.

We choose to employ the Bradley–Terry model to pairwise compare the legislators in terms of supporting restrictive gun policies (Bradley and Terry, 1952). Similar to the DW-NOMINATE scores, our scores reveal the *relative* positions of legislators on gun control in each legislative session. In terms of measuring state legislators' stances on specific issue areas, two other common practices are to 1) use survey responses from the individual legislators, and 2) use the spatial choice model or Bayesian Item Response Theory (IRT) model on a subset of roll-call voting records (Jeong, 2018). We discuss these alternative measures of gun-policy-specific ideal points in Sections A.4.1 and A.4.2, and estimate the effects of shooting using these alternative measures in A.6.

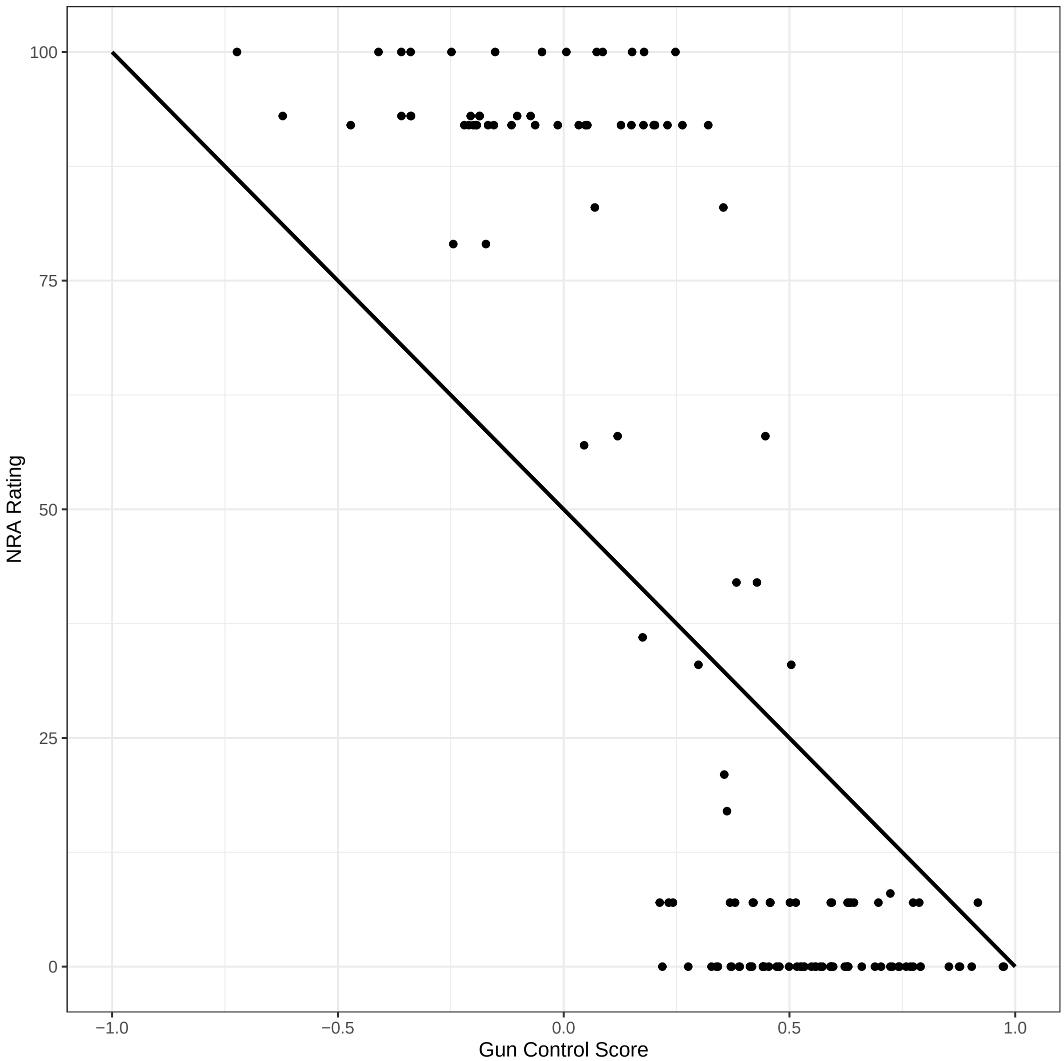
A.4.1 NRA Ratings

As for survey responses from state legislators on firearms policy, the most commonly used source is the National Rifle Association (NRA) scorecards, which rate state legislators' favorability towards firearms policies from self-reported surveys.[17] Upon close examination of the NRA scores, they suffer from severe missingness and inconsistencies in measurement that pose issues that ultimately prevent us from analyzing them.[18] However, as Figure A.4.1 shows, the Gun Control Score tracks more variation in legislators' scoring, as most legislators' NRA ratings stayed unchanged from past elections. At the same time, when a legislator's NRA score changes, we find that the Gun Control Score also moves in the same direction.

[17] The NRA sent surveys to candidates to assess their alignment with NRA core values, and grade them from a F to A+ letter grade scale.

[18] See, for example, https://www.thetrace.org/2016/11/nra-gun-record-rating-system-straight-a-students.

Figure A.4.1: **NRA Scoring Compared to Gun Control Score**

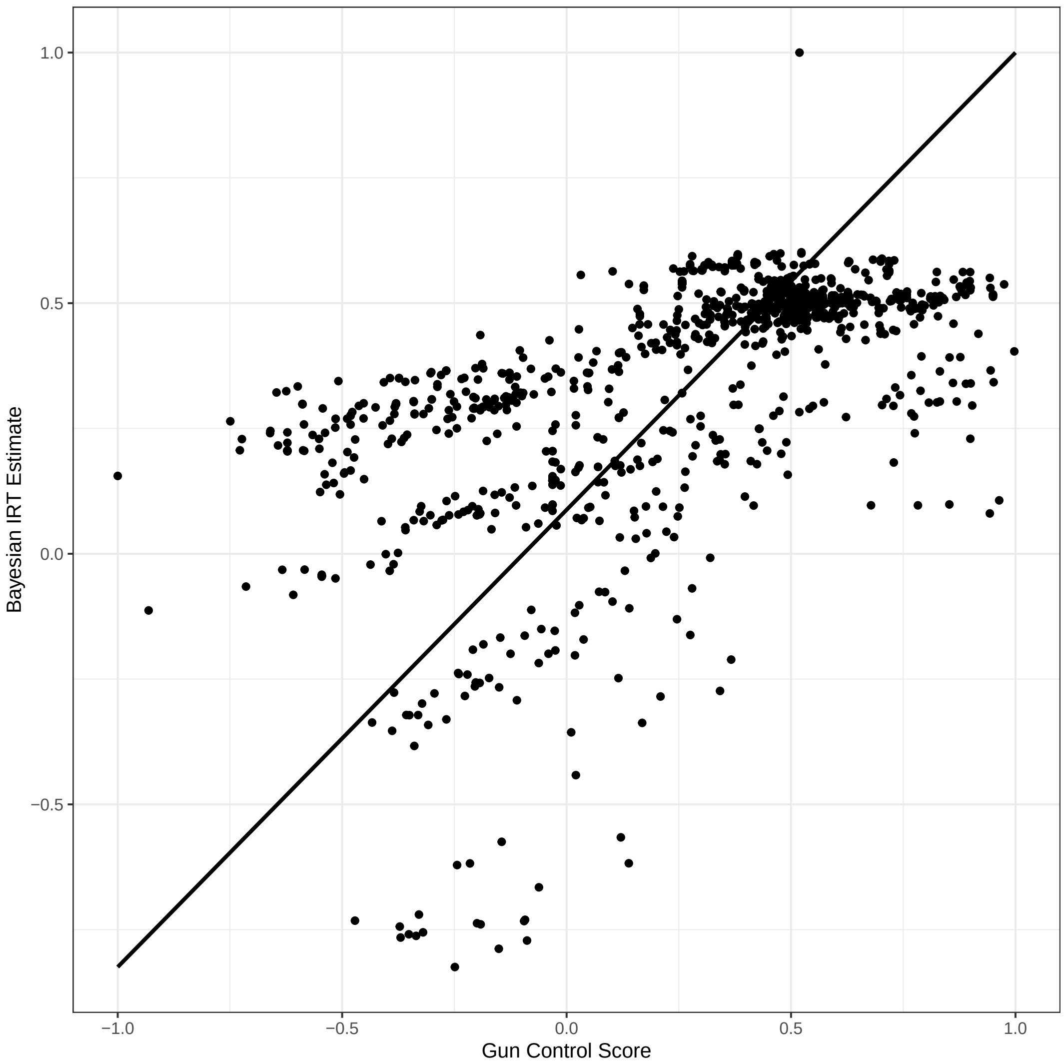


Note: This figure plots the correlation between the NRA scoring for legislators in California (where available) and the Gun Control Score. We convert an A+ rating to a score of 100, an F rating to 0, and scale the intermediate scores proportionally. The correlation is -0.84.

A.4.2 Bayesian IRT Scores

We also estimate the latent positions of California legislators on firearm-related issues using the Bayesian IRT model (implemented through the `brms` package in R) and the same subset of roll-call votes as described in Section 3.2.2 (Clinton, Jackman and Rivers, 2004; Bafumi et al., 2005). Figure A.4.2 compares the Gun Control Score and the estimated gun-related ideal points (rescaled from -1 to 1).

Figure A.4.2: **Bayesian IRT Estimates Compared to Gun Control Score**



Note: This figure plots the correlation between the Bayesian IRT estimates for legislators in California and the Gun Control Score.

A.5 Results Using Staggered Difference-in-Differences

A.5.1 Staggered Difference-in-Differences

Our staggered DiD design follows the formal specification from Callaway and Sant'Anna (2021) and estimates the aggregated average treatment effect on treated units (ATT) across treatments as

$$ATT(g,t) = \mathbb{E}[Y_t(g) - Y_t(0)|G_g = 1] \tag{3}$$

where g denotes a particular group of treated units at time period t. Hence, $ATT(g,t)$ is the expected difference between the observed Gun Control Score for treated legislators and untreated legislators at time t. In other words, this yields the group-time average treatment effect (Callaway and Sant'Anna, 2021; Baker, Larcker and Wang, 2022).

In practice, we need to estimate group-time-specific treatment effects to estimate $ATT(g,t)$, and we do so via the following regression

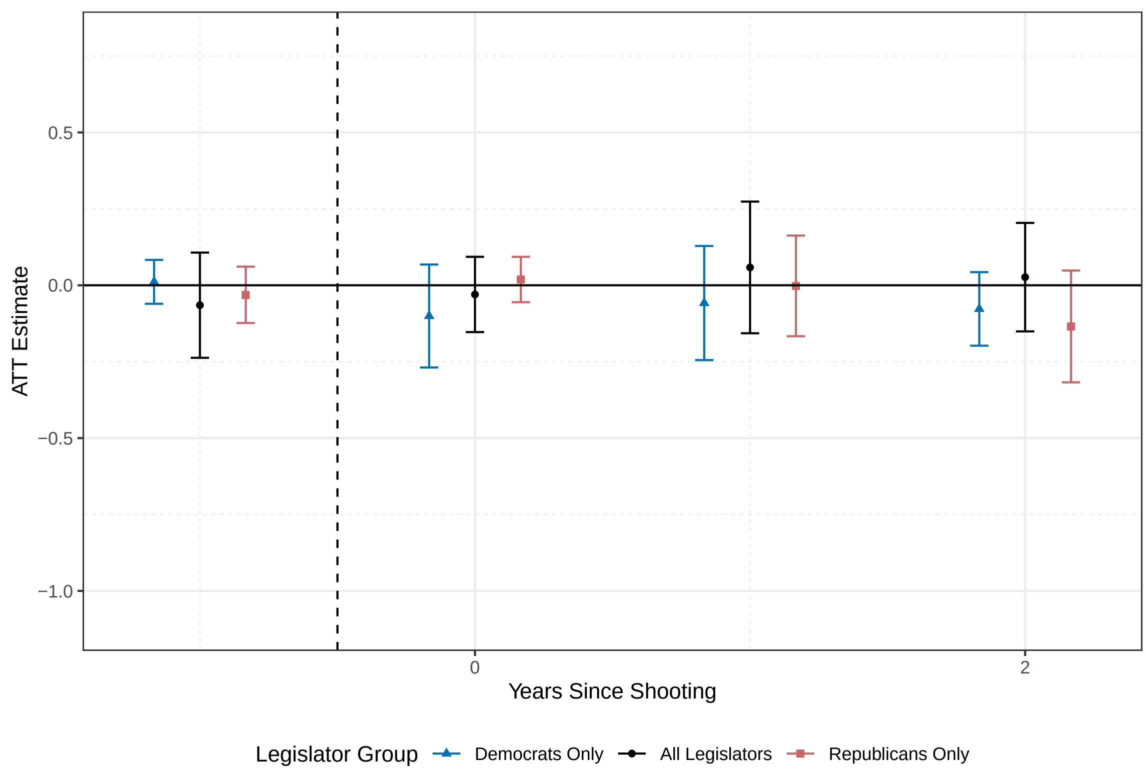
$$y_{it} = \alpha_1^{g,t} + \alpha_2^{g,t} \times \mathbb{I}[E_i = g] + \alpha_3^{g,t} \times \mathbb{I}[T = t] \; + \beta^{g,t} \times (\mathbb{I}[E_i = g] \times \mathbb{I}[T = t]) + \epsilon^{i,t} \tag{4}$$

where y_{it} denotes the Gun Control Score for legislator i in time t and group g. $\alpha_2^{g,t}$ represents the effect associated with group g at time t, with $\mathbb{I}[E_i = g]$ indicating if legislator i belongs to group g. We also include a time indicator effect, $\alpha_3^{g,t} \times \mathbb{I}[T = t]$, where $\alpha_3^{g,t}$ represents the effect associated with time t for group g. Using never-treated legislators as our control units, $\beta^{g,t}$ serves as an estimator for $ATT(g,t)$, as this term represents the interaction effect for group g at time t. Ultimately, the unbalanced nature of our panel poses some issues for estimation insofar as we are limited in our ability to explicitly model the outcome evolution for individual legislators who drop out of (and, sometimes, back into) the data. For this reason, we rely on the doubly-robust approach, which improves the robustness of our model against possible misspecification and has no impact on our identification (Callaway and Sant'Anna, 2021; Sant'Anna and Zhao, 2020).

A.5.2 Shootings Occurring Within a Legislator's District Do Not Affect Their Voting Behavior

Figure A.5.1 presents the results of our staggered DiD estimation following (Callaway and Sant'Anna, 2021), for California legislators. We report the results of three models following the specification outlined in Equation 3: one comparing all treated legislators to all others regardless of party, and two that compare treated and untreated legislators within party. Across the specifications for all legislators, Democrats, and Republicans, we find null results — this suggests that the occurrence of a mass shooting within a legislator's district does not cause a legislator to change their voting behavior on firearm-related policies. Relative to all other legislators, as well as legislators within one's party, legislators who experience a shooting in their district do not change their voting behavior.

Figure A.5.1: **ATT Estimates for Staggered DiD Demonstrate Null Results Across Party**



Note: We report the null effects of a mass shooting on the voting behavior of CA state legislators on firearm-related policy, with models fit on all legislators, only Democratic legislators, and only Republican legislators.

We limit the time periods relative to treatment that we report to those that contain at least one treated legislator in each model specification, and report the formal estimates in Table A.5.1.

As discussed in the main text, the stacked DiD approach suffers from some issues that the staggered design resolves. One such benefit with the staggered design is that our uncertainty estimates are more realistic in the years far from treatment, in comparison to the stacked DiD results. Instead of a lack of observations leading to narrower confidence intervals due to less variability in our observations (as we observe in Figure A.5.1), we find that the confidence intervals in the periods far removed from treatment are generally larger under the design from (Callaway and Sant'Anna, 2021). Instances of particularly narrow confidence intervals in periods far from treatment appear to instead be driven by attrition among our treated units. This trade-off can be seen in the calculation of the 95% confidence intervals in the latter time periods in Fig. A.5.1, as our data are essentially affected by attrition or survivorship bias.

We report our estimates across our two DiD designs in Table A.5.1 and find very similar estimates at $t = 0$.

In sum, this finding can be interpreted as evidence that (1) mass shootings do not affect the Gun Control Scores of legislators who represent districts in which shootings occur compared to all other legislators; (2) mass shootings do not affect the Gun Control Scores of Democrats representing affected districts relative to other Democrats; and (3) mass shootings do not affect the Gun Control Scores of Republicans representing affected districts relative to other Republicans.

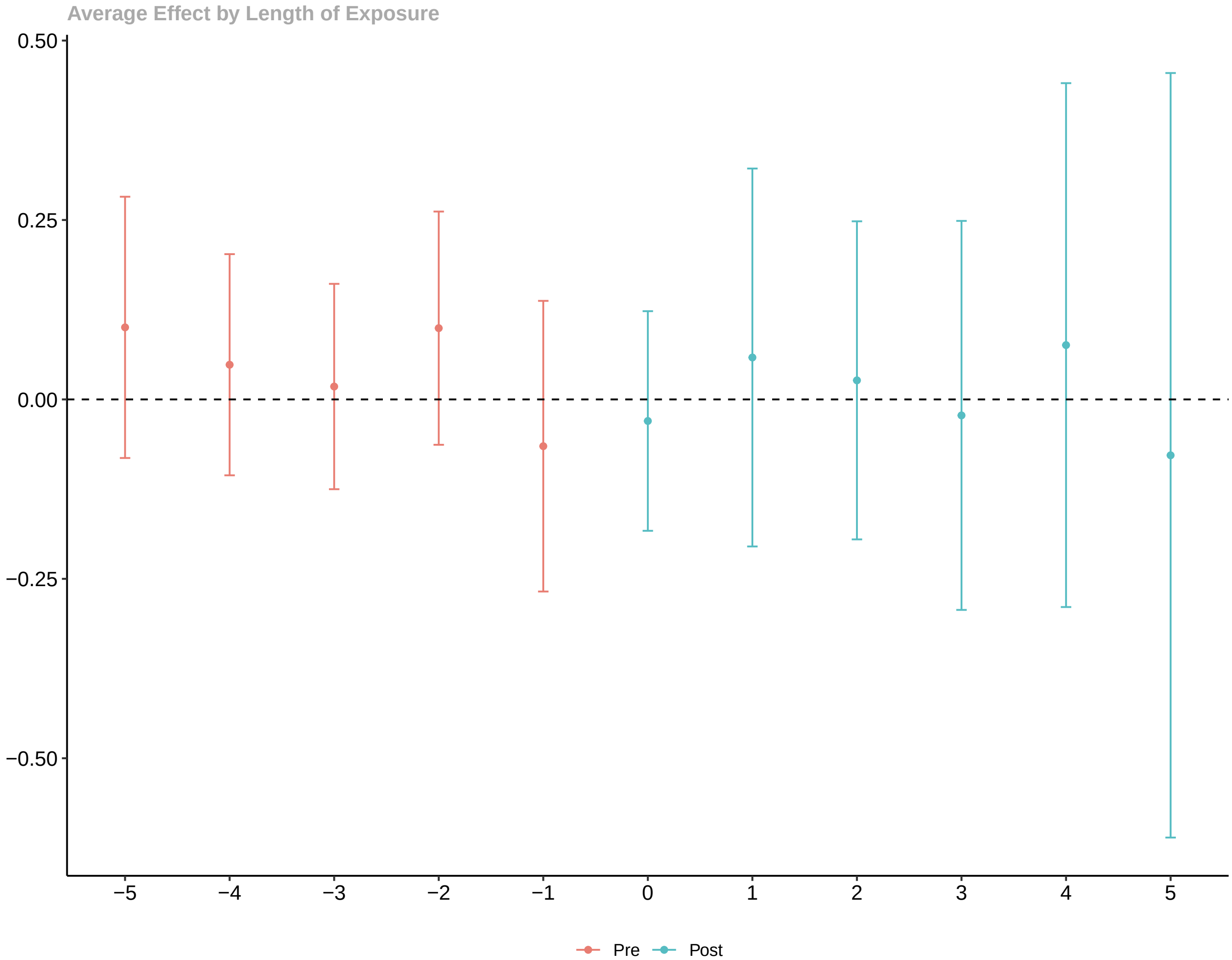
Table A.5.1: **ATT Estimates for Stacked and Staggered DiD in California**

	Overall		Democrats		Republicans	
Time	(1)	(2)	(3)	(4)	(5)	(6)
-4	-0.108	0.048	-0.084	0.177	-0.01	-0.081
	(0.065)	(0.069)	(0.056)	(0.062)	(0.11)	(0.021)
-3	-0.04	0.018	-0.17	-0.139	0.022	0.019
	(0.099)	(0.063)	(0.081)	(0.074)	(0.034)	(0.093)
-2	0.067	0.099	0.017	0.136	0.071	0.05
	(0.031)	(0.071)	(0.049)	(0.11)	(0.026)	(0.041)
-1		-0.065		0.011		-0.031
		(0.088)		(0.037)		(0.047)
0	-0.023	-0.03	-0.094	-0.1	0.022	0.019
	(0.066)	(0.063)	(0.09)	(0.086)	(0.03)	(0.038)
1	-0.061	0.058	-0.072	-0.058	0.022	-0.002
	(0.073)	(0.11)	(0.078)	(0.095)	(0.072)	(0.084)
2	-0.061	0.027	-0.086	-0.077	-0.098	-0.135
	(0.056)	(0.091)	(0.06)	(0.061)	(0.061)	(0.093)
3	-0.102	-0.022	-0.011	0.022	-0.044	-0.127
	(0.08)	(0.116)	(0.086)	(0.063)	(0.07)	(0.076)
4	-0.045	0.076	0.13	0.153	-0.129	-0.216
	(0.092)	(0.15)	(0.054)	(0.083)	(0.096)	(0.099)
# of Legislators	234	255	156	172	78	83
Legislator FE	✓		✓		✓	
Chamber FE	✓		✓		✓	
Year FE	✓		✓		✓	
Est. Strategy	CDLZ	CS	CDLZ	CS	CDLZ	CS

Note: Table records the estimated effects of mass shootings occurring within a legislator's district on a legislator's Gun Control Score for varying time units (with standard errors reported in parentheses). Estimation strategy refers to the difference-in-differences design utilized in each estimation, with CDLZ referring to the stacked DiD following Cengiz et al. (2019), and CS referring to the staggered DiD following Callaway and Sant'Anna (2021). CDLZ leaves out time period -1 to account for multicollinearity. Due to the aggregation by group and time of treatment in CS, year-fixed effects are superfluous, though we include these fixed effects in the CDLZ models. We cluster standard errors at the legislator level for CS models.

A.5.3 Parallel Trends Assumption Holds Overall

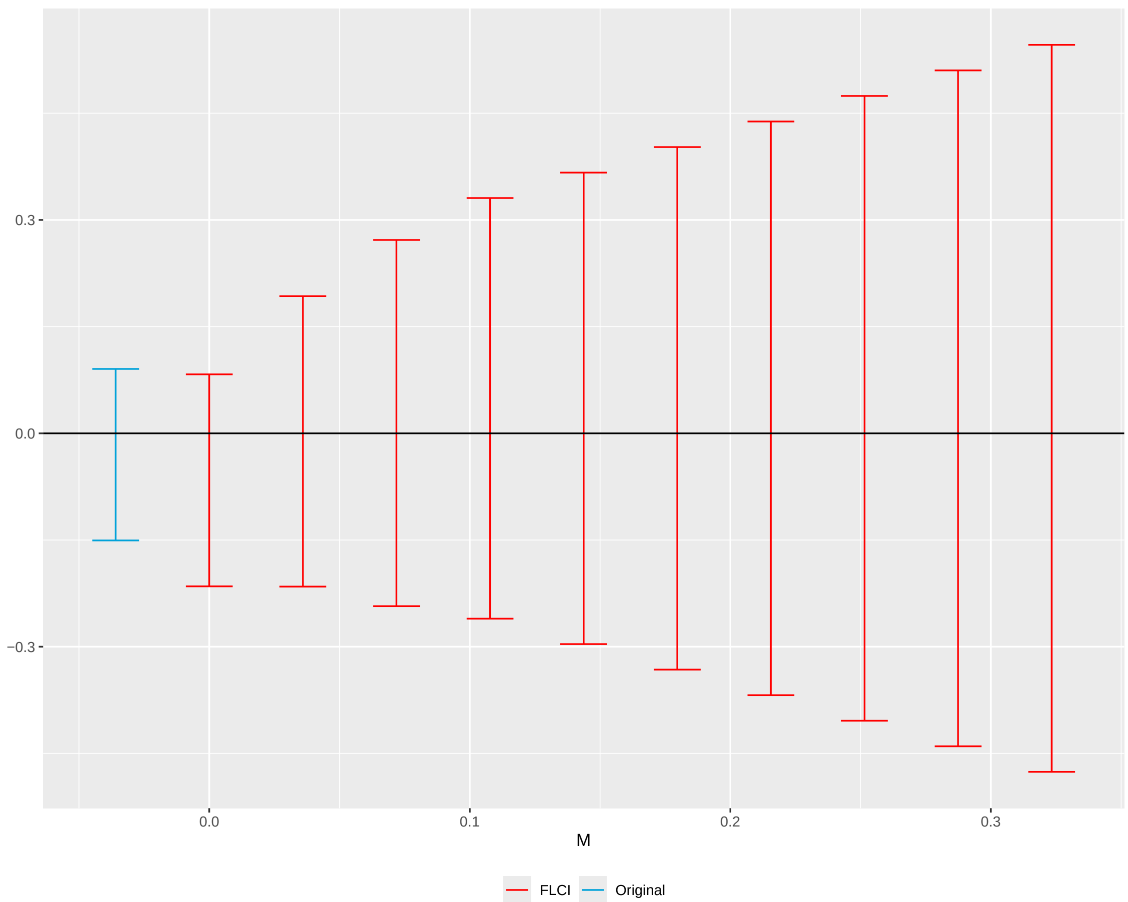
Figure A.5.2: **The Average Effect by Length Of Exposure for California**



Note: This figure plots the aggregated ATT at given time periods using the "dynamic" specification of the `did` package for a model fit to all legislators in California (Callaway and Sant'Anna, 2021).

We also combine the staggered DiD and the sensitivity analysis for potential violations of the parallel trends assumption (Callaway and Sant'Anna, 2021; Rambachan and Roth, 2023) in Figure A.5.3 and Figure A.5.4.

Figure A.5.3: **Sensitivity Analysis of the Parallel Trends Assumption in California Using Smoothness Restrictions**

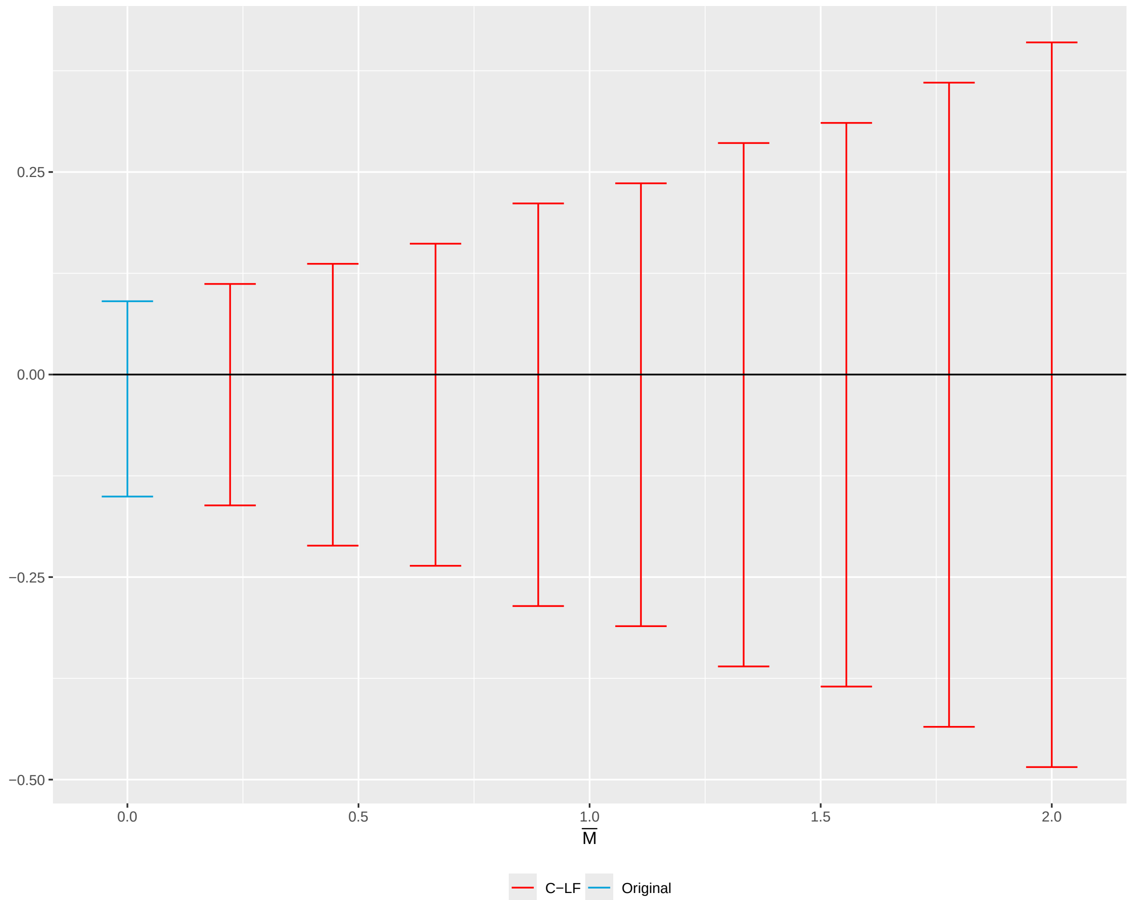


Note: We implemented the sensitivity analysis using the `HonestDiD` package under smoothness restrictions (Rambachan and Roth, 2023). We plot the 95% confidence interval of the in-district treatment effect in blue, and the robust confidence intervals of the treatment effect allowing for non-linearity of the pre-trend up to M in red.

Figure A.5.3 plots the original staggered DiD estimate for the effect of a mass shooting on the Gun Control Score of a California lawmaker in blue. The red fixed length confidence intervals (FLCIs) report different effect estimates along values of M, where $M = 0$ allows only for linear violations of the parallel trends and larger deviations from linearity are allowed under larger values of M (Rambachan and Roth, 2023). The breakdown value is 0: we cannot rule out null effects for the effect of a mass shooting on the Gun Control Score of

California legislators.

Figure A.5.4: **Sensitivity Analysis of the Parallel Trends Assumption in California Using Relative Magnitudes Restrictions**



Note: We implemented the sensitivity analysis using the `HonestDiD` package under relative magnitudes restrictions (Rambachan and Roth, 2023). We plot the 95% confidence interval of the in-district treatment effect in blue, and the robust confidence intervals that allows for parallel trends violations up to $\bar{M}$ times the worst pre-treat violation of the parallel trends in red.

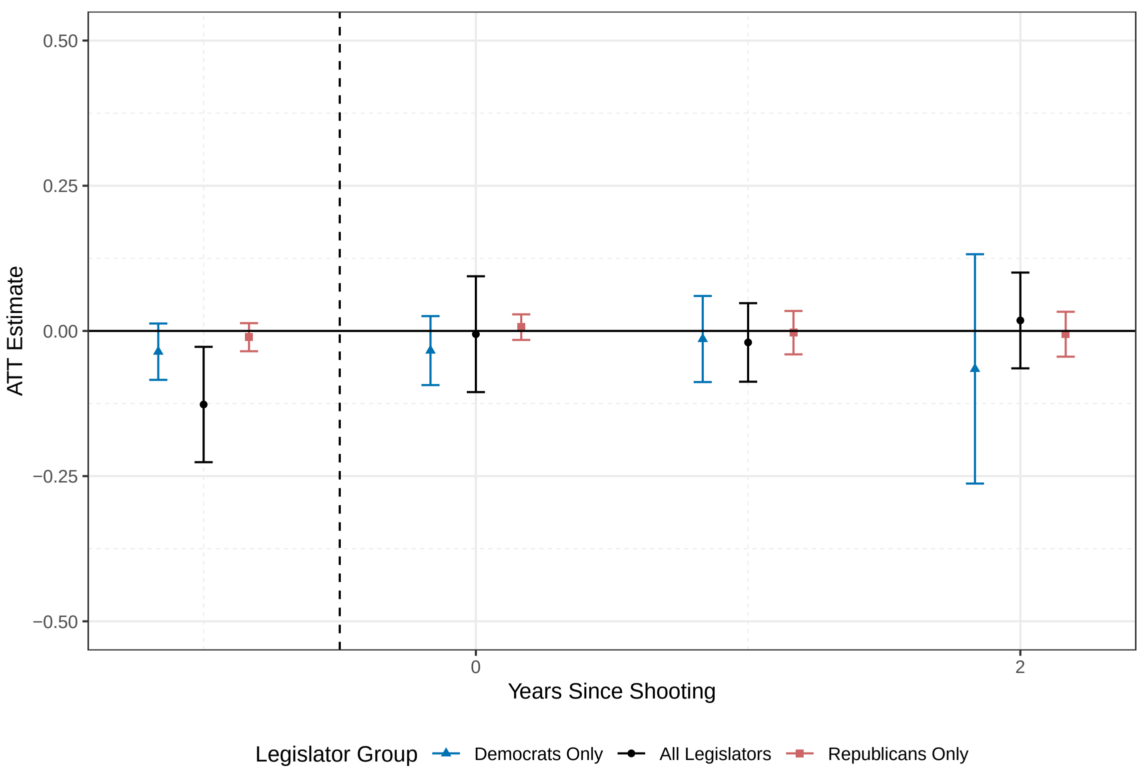
In Figure A.5.4, we see that there is no "breakdown value" (Rambachan and Roth, 2023) for a significant effect. Were we to see a significant effect at, say, $\bar{M} = 1$, this would suggest that a significant result is robust to allowing for violations of parallel trends no larger than the maximum pre-treatment violation. Given the null finding across all values, this suggests that we cannot rule out null results even when allowing for large or small violations of parallel trends.

A.6 Results Using Alternative Measurement Strategies

A.6.1 Results Using IRT Estimates

Following the same staggered DiD design, we report the ATT estimates using the IRT estimates instead in Figure A.6.1. Similarly, the null effect after a shooting ($t = 0$ and beyond) holds for all sets of comparisons.

Figure A.6.1: **ATT Estimates for Staggered DiD Demonstrate Null Results Across Party When Using Bayesian IRT Model**

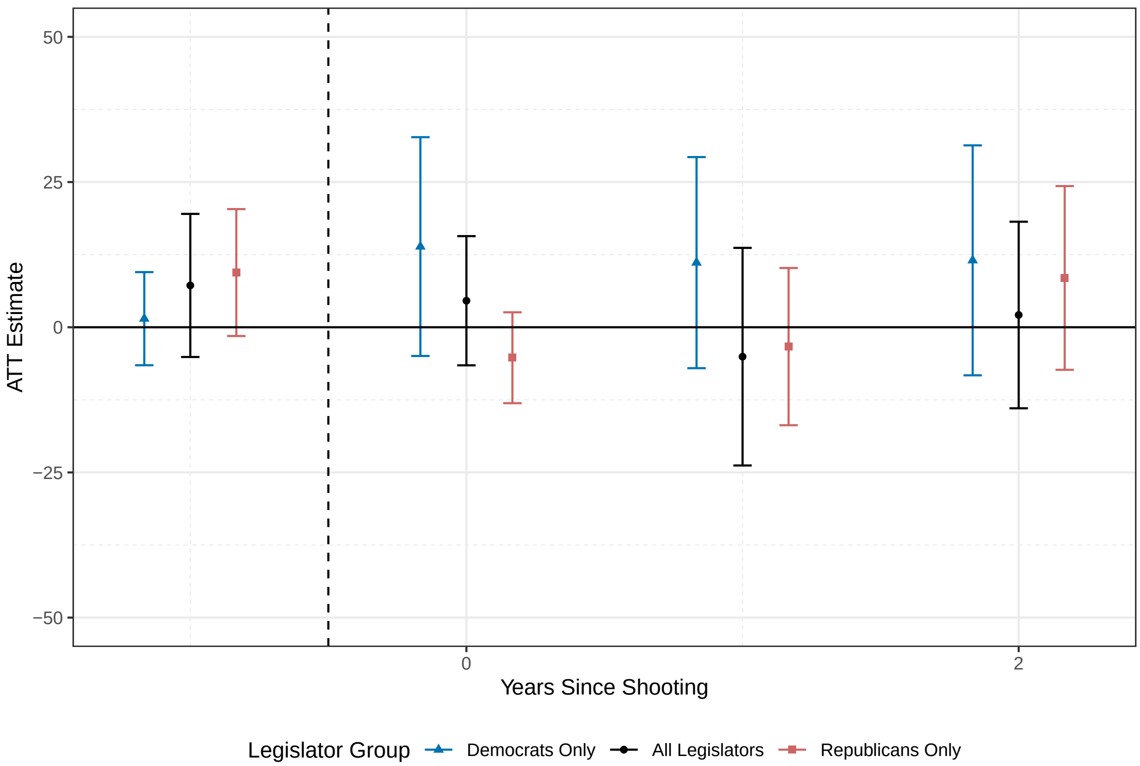


Note: We report the null effects of a mass shooting on the voting behavior of CA state legislators on firearm-related policy, with models fit on all legislators, only Democratic legislators, and only Republican legislators.

A.6.2 Results Using Rankings

Following the same staggered DiD design, we report the ATT estimates using the relative rankings of legislators in California — grouping legislators by year and ranking their Gun Control Scores relative to one another within each year — in Figure A.6.2. Similarly, the null effect after a shooting ($t = 0$ and beyond) holds for all sets of comparisons.

Figure A.6.2: **ATT Estimates for Staggered DiD Demonstrate Null Results Across Party When Using Bayesian IRT Model**

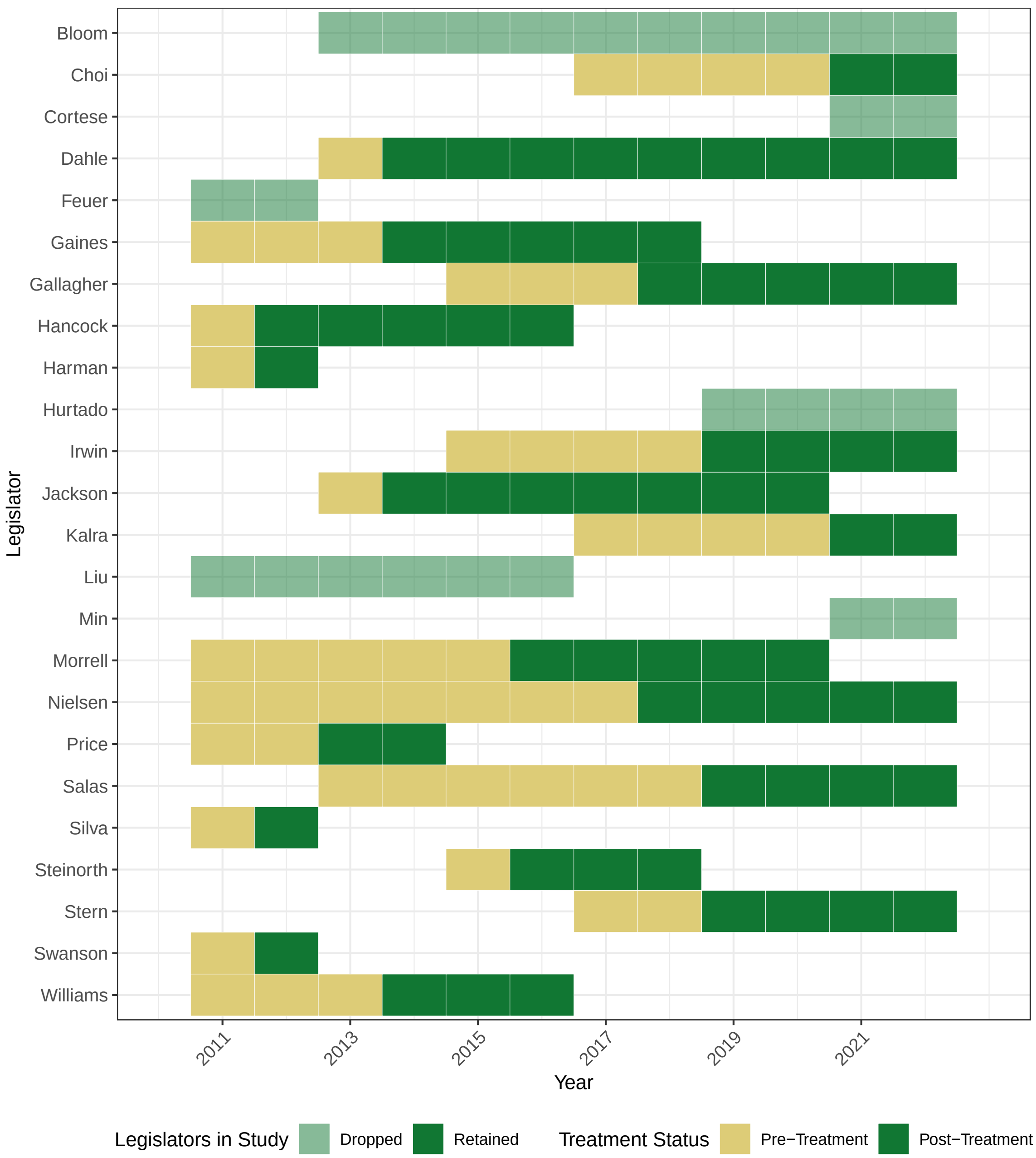


Note: We report the null effects of a mass shooting on the voting behavior of CA state legislators on firearm-related policy, with models fit on all legislators, only Democratic legislators, and only Republican legislators.

A.7 Treatment Timing in California

In California, Legislators Bloom, Cortese, Feuer, Hurtado, Liu, and Min (the semi-transparent green observations) are dropped due to the fact that we do not have at least one pre-treatment and one post-treatment observation. Two other treated legislators, Assemblyman Mike Feuer and State Senator Carol Liu, are dropped because they were previously treated by the North Hollywood shooting (so we do not have a pre-treatment observation for them, only post-treatment). This can be seen in Figure A.7.1.

Figure A.7.1: **Treatment Timing for Legislators Representing Districts in Which a Mass Shooting Occurred**



Note: Dropped observations appear semi-transparent in the figure. Under our identification strategy, any legislators missing at least one untreated and one treated observation must be dropped, leaving us with 18 treated legislators. A shooting in months 1-6 of a year leads to that year being considered post-treatment, while a shooting occurring in months 7-12 of a given year leads to that year being considered pre-treatment.

A.8 Treatment at Varying Distances

We rely on state legislative district shapefiles from the United States Census Bureau[19] to identify district boundaries following the 2012 redistricting cycle. These boundaries are used for shootings occurring after the adoption of the new district boundaries, which varies across states in our study. For shootings occurring before the redistricting done following the 2010 Census, we report the district that was affected under the earlier maps, and cross-reference district boundaries after the Census to ensure that legislators who were redistricted to represent a new area are still considered treated if they previously represented an affected district. District shapefiles from this earlier period are sourced primarily from the Redistricting Data Hub.[20] Shooting locations are from the Violence Project,[21] and are cross-referenced with records from the Gun Violence Archive,[22] when available.

To determine shootings that are affected under the varying distance, we calculate a 5 and 25 mile radii, in euclidean distance, around each shooting's location. We use the North American Datum of 1983 (NAD83) geodetic datum as our Coordinate Reference System (CRS). Figure A.8.1 shows these buffers, in blue and green, respectively. Figure A.8.1 zooms in on Southern California and shows shootings occurring between 2012 and 2022 only.

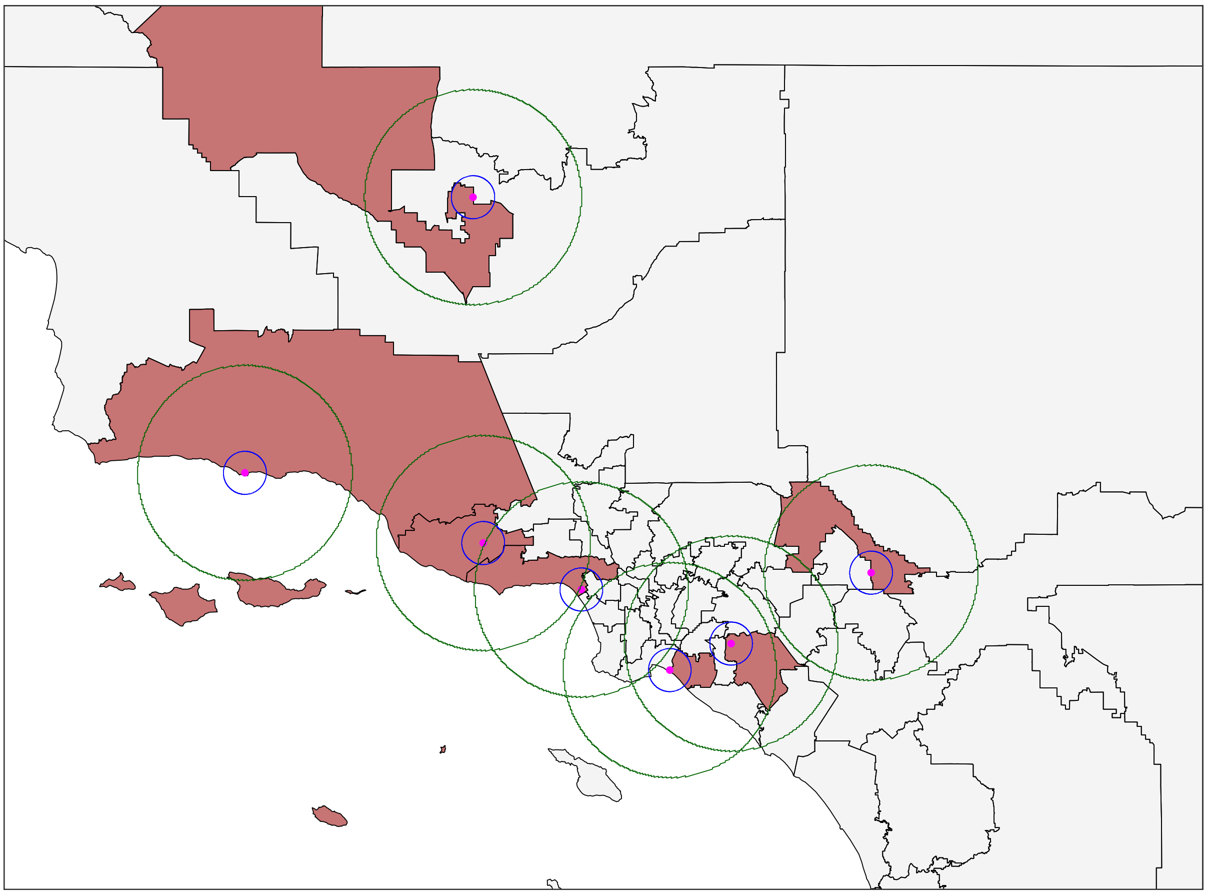
[19] `https://www.census.gov/geographies/mapping-files.html`.

[20] `https://redistrictingdatahub.org`.

[21] `https://www.theviolenceproject.org`.

[22] `https://www.gunviolencearchive.org`.

Figure A.8.1: **Treatment Assignment Strategy for Varying Distances**



Note: Legislative district shapefiles used to determine shooting location and affected districts vary across states and redistricting cycles. In this figure, we use boundaries defined after the 2012 redistricting in California, and zoom in on shootings occurring in Southern California between 2012 and 2022. We only map lower chamber districts in this figure as an example. Districts shaded red as those in which the shooting (red points) occurred. The blue buffer represents a 5 mile radius (euclidean distance) from the shooting's location, while green buffers are 25 mile radii.

A.9 Assigning Treatment to a District

To estimate the effects of a mass shooting on the Gun Control Score of a district, we first confine our period of study to 2012-2022 to remove any concerns that we are not appropriately accounting for redistricting. Our estimation strategy is similar to those underlying our analyses of individual legislators, though we include chamber- and year-fixed effects and cluster standard errors by chamber and year. The full results can be seen in Table A.9.1.

Table A.9.1: **ATT Estimates for Effect of Mass Shooting on District**

	Gun Control Score		
	(1)	(2)	(3)
-4	-0.084 (0.162)	-0.022 (0.076)	-0.007 (0.04)
-3	-0.184 (0.104)	-0.095 (0.118)	0.063 (0.043)
-2	-0.136 (0.108)	-0.036 (0.057)	0.056 (0.028)
0	-0.161 (0.098)	-0.061 (0.088)	0.014 (0.036)
1	-0.245 (0.112)	-0.108 (0.076)	-0.004 (0.042)
2	-0.191 (0.136)	-0.027 (0.074)	-0.11 (0.023)
3	-0.181 (0.177)	0 (0.073)	-0.093 (0.043)
4	-0.26 (0.178)	0.021 (0.075)	-0.16 (0.076)
Legislator Group	All	Democrats	Republicans
N	13,638	9,952	3,686
# of Treated Legislators	42	20	22
Chamber FE	✓	✓	✓
Year FE	✓	✓	✓
Pre-Treatment Mean	0.239	0.448	-0.233

Note: Robust standard errors are clustered at the chamber and year level. Gun Control Score is measured from -1 to 1, from least to most supportive of restrictive firearms legislation. Table reports the estimated effects of mass shootings occurring within a district on the Gun Control Score of the district. Individual legislators who switch parties throughout the duration of study are removed. Pre-Treatment Mean is the average gun control score of control and treated legislators in the pre-treatment period.

A.10 Disaggregating ATEs for Event-Specific Estimates

In principle, one could summarize the event-specific estimates of the effect of mass shootings on legislators' gun control scores by pooling across incidents, for example using inverse-variance weighting or other meta-analytic techniques (Goodman-Bacon, 2021). Our ability to do so in this paper is constrained by several features of our data and overall design.

First, some shootings involve only a single treated legislator due to vacancies in office and other discrepancies. In these cases, the standard error for the estimate is undefined or uninformative, and including such events in a weighted average would artificially down-weight events with meaningful but precisely estimated effects or, conversely, inflate the influence of single-unit estimates.

Second, the Gun Control Score outcome is constructed separately for each state and is not directly comparable across states. Differences in scoring methodology, legislative composition, and available roll-call votes mean that a unit change in one state does not have the same substantive meaning as a unit change in another. Consequently, averaging estimates across states could misrepresent the magnitude and direction of effects, producing misleading "pooled" coefficients that do not reflect the underlying heterogeneity.

For these reasons, we report event-specific estimates without pooling across states, and we caution against interpreting any cross-state averages as reflecting a generalizable or standardized effect. This approach, seen in Table A.10.1 preserves the interpretability of individual-event effects while acknowledging meaningful variation across contexts. We include relevant shooting-specific characteristics that are sourced from Peterson and Densley (2022).

Table A.10.1: **Disaggregated ATT Estimates of Individual Shooting Incidents**

Shooting	ATT ($t = 0$)	State	N Legislators	N Treated	Characteristics
2011 Seal Beach	-0.095 (0.093)	CA	110	2	8 killed; Custody dispute
2012 Oakland	-0.222 (0.105)	CA	110	2	7 killed; University shooting
2013 Santa Monica	-0.654	CA	74	1	5 killed; Spree near University
2014 Alturas	0.145 (0.048)	CA	115	2	4 killed; Tribal/eviction dispute
2014 Isla Vista	-0.025 (0.05)	CA	115	2	6 killed; University shooting
2015 San Bernardino	-0.14 (0.038)	CA	116	2	14 killed; Terror related
2017 Rancho Tehama Reserve	0.406 (0.031)	CA	112	2	5 killed; Spree near school
2018 Bakersfield	-0.003	CA	95	1	5 killed; Domestic dispute
2018 Thousand Oaks	-0.007 (0.018)	CA	96	2	12 killed; Nightclub shooting
2021 Orange	-0.176	CA	93	1	4 killed; Workplace shooting
2021 San Jose	0.295	CA	93	1	9 killed; Workplace shooting
2012 Aurora	0.037 (0.071)	CO	87	2	12 killed; Theater shooting
2021 Boulder	-0.123	CO	80	1	10 killed; Supermarket shooting
2021 Denver	0.096 (0.175)	CO	97	2	5 killed; Spree shooting
2021 Atlanta	0.419 (0.095)	GA	159	2	8 killed; Hate-inspired
2013 Hialeah	-1.109	FL	110	1	6 killed; Apartment complex shooting
2016 Orlando	-0.003	FL	135	1	49 killed; Hate-inspired; Nightclub shooting
2017 Fort Lauderdale	0.597	FL	107	1	5 killed; Airport shooting
2017 Orlando	0.615	FL	107	1	5 killed; Workplace shooting
2018 Parkland	0.333 (0.203)	FL	147	2	17 killed; School shooting
2019 Sebring	0.626 (0.04)	FL	109	2	5 killed; Workplace shooting
2020 Springfield	0.066 (0.093)	MO	189	2	4 killed; Supermarket shooting
2015 Tennessee Colony	-0.053 (0.092)	TX	150	2	6 killed; Campsite shooting
2016 Dallas	0.035 (0.035)	TX	150	2	5 killed; Hate-inspired
2017 Sutherland Springs	-0.363 (0.025)	TX	140	2	25 killed; Church shooting
2018 Santa Fe	-0.259 (0.071)	TX	140	2	10 killed; School shooting
2019 El Paso	-0.445	TX	152	1	23 killed; Hate-inspired; Supermarket shooting
2019 Midland-Odessa	0.21 (0.051)	TX	153	2	7 killed; Spree shooting

Note: Table reports the disaggregated ATT estimates for each shooting in the study at $k = 0$. Each estimate is obtained from an individual TWFE DiD regression for each individual shooting. Models incorporate legislator, chamber, and year fixed effects. Shooting year refers to post-treatment period. The number killed in each shooting does not include the shooter, even in cases where the shooter also died

A.11 Cosponsorship Network Analysis and Gun Policy Activity

A.11.1 Cosponsorship Network

Figure A.11.1: **Cosponsorship Network Among California Legislators on Firearm-related Legislation**

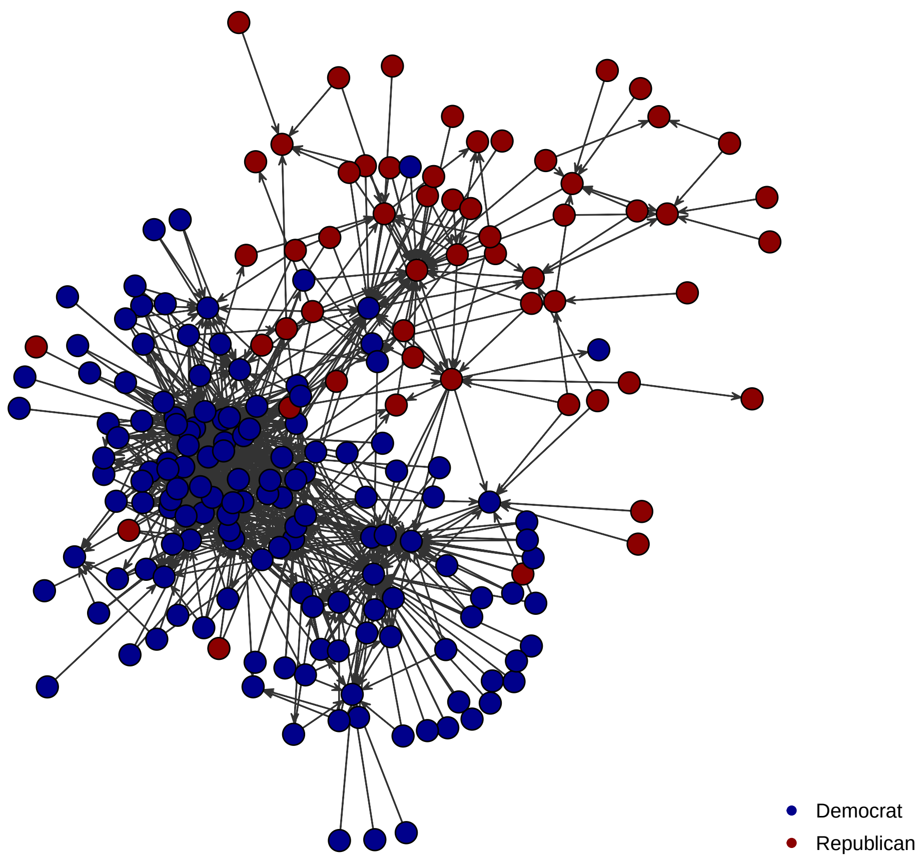


Table A.11.1: **Legislators with Highest Centrality in Each Chamber and Session**

Session	Most Bills Cosponsored	Highest Betweenness Centrality	Highest Eigenvector Centrality
State Assembly			
2011	Huffman	Ammiano	Huffman
2013	Ting	Achadjian	Ting
2015	Gipson	Chiu	Gipson
2017	Chiu	Gloria	Chiu
2019	Chiu	McCarty	Chiu
2021	Cristina Garcia	Ting	Cristina Garcia
Senate			
2011	Hancock	De Leon	Hancock
2013	Steinberg	Steinberg	Steinberg
2015	Leno	Hall	Leno
2017	Portantino	Wiener	Wiener
2019	Wiener	Wiener	Wiener
2021	Wiener	Portantino	Wiener

A.11.2 Alternative Behavior

Table A.11.2: **ATT Estimates for Effect of Mass Shooting on Legislators' Activity in Gun Policy Domain**

	Activity			Outdegree		
	(1)	(2)	(3)	(4)	(5)	(6)
-2	-0.019	-0.031	-0.004	-1.026	-1.491	0.001
	(0.008)	(0.008)	(0.004)	(0.452)	(0.322)	(0.32)
0	-0.01	-0.027	0.001	-0.771	-1.957	0.158
	(0.01)	(0.013)	(0.006)	(0.496)	(0.639)	(0.32)
1	-0.01	-0.006	-0.01	-0.833	-0.626	-0.443
	(0.008)	(0.008)	(0.006)	(0.538)	(0.505)	(0.365)
2	-0.014	-0.034	0	-0.516	-1.475	0.185
	(0.009)	(0.011)	(0.006)	(0.548)	(0.821)	(0.38)
Legislator Group	All	Democrats	Republicans	All	Democrats	Republicans
# of Legislators	234	156	78	234	156	78
# of Shootings	11	6	5	11	6	5
Legislator FE	✓	✓	✓	✓	✓	✓
Chamber FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Pre-Treatment Mean	0.016	0.018	0.010	0.799	0.964	0.397
N	7,924	5,898	2,026	7,924	5,898	2,026

Note: Standard errors are clustered at the legislator-, chamber-, and year level in parentheses. Table reports the estimated effects of mass shootings occurring within a legislator's district on a legislator's activity in the gun policy domain for periods $k = -2$ to 2. Activity is a measure of the number of bills sponsored or cosponsored by a legislator related to gun policy out of all bills related to gun policy in a given year. Outdegree is a measure of legislator's level of activity in cosponsoring firearms-related legislation. Pre-Treatment Mean is the average gun control score of control and treated legislators in the pre-treatment period.

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