The Effect of Real-World Events on Political Attitudes and Preference Intensity: Evidence from Mass Shootings

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Abstract

Since the earliest studies of public opinion, scholars have highlighted the difficulty in persuading people to change their minds. Nevertheless, decades of research suggest that real-world events may play a unique role in shaping public opinion. Studies of terrorist attacks, war casualties, wildfires, and other policy-relevant events underscore the impact of these events on people in close geographic and temporal proximity. In contemporary U.S. politics, mass shootings constitute a unique opportunity to study the effects of recurring real-world events on preferences. Previous studies of mass shootings leverage increasing availability of survey data to explore how this type of event shapes preferences for gun control, but arrive at divergent conclusions over whether or how these events affect support for restrictive gun policies. In this paper, I combine nearly 500,000 interviews conducted between July 2019 and January 2021 with the locations of 10 mass shootings to estimate whether mass shootings affect political attitudes and the intensity of those attitudes. By examining subsets of these events through two causally credible research designs, I am able to estimate very small effects on public opinion. My findings suggest that mass shootings have no appreciable effects on the gun policy attitudes of people living in close geographic proximity; nor do mass shootings affect the policy preferences of people around the country, on average. Similarly shootings do not usually change the intensity of peoples' preferences on gun policies among those in proximity to the incidents or elsewhere. The most egregious mass shootings, however, appear to result in nationwide increases in the perceived importance of gun control policies in the days after the incidents. Unlike terrorist attacks, war deaths, and wildfires, gun violence does not seem to change Americans' policy attitudes, no matter how close or far away they are from the incidents.

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1 Introduction

In 1970, William R. Tobler wrote what is now known as the First Law of Geography: "everything is related to everything else, but near things are more related than distant things" (Tobler 1970, 236; 2004, 2). Evidence from across the social sciences seems to confirm that geographically-concentrated events can affect the attitudes and behavior of geographically proximate people. With respect to public opinion and political behavior, researchers have uncovered evidence that living close to a terrorist attack affects how people perceive risk (Fischhoff et al. 2003; Hopkins 2018), increasing local war deaths affect the outcomes of national elections (Kriner and Shen 2007), worsening local economic conditions decrease support for incumbents (Benedictis-Kessner and Warshaw 2019), and that natural disasters shape support for ballot initiatives (Hazlett and Mildenberger 2020).

Despite the demonstrated effects of these policy-relevant events, work on the effects of gun violence has shown mixed results – even when using the same data. Moreover, mass shootings are increasingly a ubiquitous part of contemporary American life: between 2014 and 2023, 8.4 mass shootings occurred annually, up nearly 5 shootings per year compared to the previous decade.[1] Still, mass shootings have not systematically led to more restrictive gun control policy (Luca, Malhotra, and Poliquin 2020) nor have scholars found consistent effects on public opinion, turnout, or election outcomes (Newman and Hartman 2019; Barney and Schaffner 2019; Hartman and Newman 2019; Yamauchi 2020; Rogowski and Tucker 2019; Zhang and Liu 2024; Hassell, Holbein, and Baldwin 2020; García-Montoya, Arjona, and Lacombe 2022; Hassell and Holbein 2023).

In this paper, I leverage over 500,000 interviews (from weekly surveys across nearly two years and 10 mass shootings) to investigate whether mass shootings affect two types of political attitudes: which policies people prefer as well as how intensely they desire these policies. The high-frequency and large-scale nature of the survey data allows me to execute research designs that isolate the effects of shootings on public opinion among similar people, in ways not possible with previous data. Specifically, the occurrence of mass shootings during this data collection provides an unusual opportunity to evaluate whether policy-relevant events influence political attitudes.

I rely on two research designs to estimate the effects of mass shootings: a difference-in-differences design that reweights respondents in areas not recently exposed to shootings to resemble people living close to mass shootings, and an interrupted time series design that explores national and regional effects in the days after the shootings. I find no evidence that mass shootings influence the gun policy attitudes of spatially and/or temporally proximate individuals. Using a conjoint analysis with nearly 5 million individual experiments, I similarly show that mass shootings do not generally affect the intensity with which people hold existing gun-policy attitudes, though two high-casualty incidents on adjacent days led peo-

[1] https://www.motherjones.com/politics/2012/12/mass-shootings-mother-jones-full-data/

ple nationwide to rank gun policy as more important, on average, than they did before the shootings.

My findings provide some context for the lack of policy response to mass shootings. Mass shootings fail to increase support for gun control among nearby individuals or people across the country, and usually do not cause people to care more intensely about gun control policy. Moreover, only highly unusual mass shootings cause changes in public opinion at the national level, and these effects are constrained to smalls shifts in preference intensity, not support for restrictive gun policies. From the perspective of models of retrospective voting, policy feedback, and electoral accountability, these findings constitute limits on the general public's ability to learn about and demand changes to public policy (Fiorina 1981; Mettler and Soss 2004; A. L. Campbell 2012; Ashworth 2012).

2 Theory and Previous Research

Despite Tobler's First Law of Geography and the ample evidence amassed in support of the idea that what is close matters more, a growing body of literature argues that in America, politics has nationalized, with the public becoming increasingly interested in national political news and less engaged in state and local politics (Jacobson 2015; Abramowitz and Webster 2016; Hopkins 2018). At the same time, the two major political parties have polarized, making people more likely to perceive major differences between the two major political parties (Sides, Tausanovitch, and Vavreck 2022), vote for candidates from one party at all levels of office (Sides, Tausanovitch, and Vavreck 2022; Hopkins 2018), hold similar policy preferences to co-partisans but different positions than out-partisans (Hill and Tausanovitch 2015), and feel animosity towards ordinary people from the other party (Orr and Huber 2020; Iyengar et al. 2019; Sides, Tausanovitch, and Vavreck 2022) Both the nationalization and polarization of American politics may reduce the effect of local context on public opinion and political behavior: citizens may not learn about changes in their local environment or may hold political attitudes shaped by national political forces that are resistant to local considerations.

In this environment, it is worth considering if, when, and how changes policy-relevant events affect individuals' political behavior. Recent evidence suggests that persuasive information moves most peoples' attitudes in the same direction and by about the same amount, regardless of previous attitudes or party affiliation (Coppock 2023). Perhaps, changes in local context expose individuals to strong doses of persuasive information, causing attitudes to update similarly across groups of people. Alternatively, attitudes might diverge as people reason about the implications of public policy (Popkin 1991; Zaller 1992) or receive messages clarifying the positions of the two major political parties (Zaller 1992; Coppock 2023; though see Fowler and Howell 2023).

2.1 Why Do We Expect Shootings to Affect Attitudes?

Why do we expect attitudes to change based on geographic and temporal proximity to a policy-relevant events like mass shootings? A number of literatures provide useful clues, depending on whether we conceptualize mass shootings as a potential "focusing event" (Kingdon and Thurber 2011), an event that affects perceptions of the country's performance (Fiorina 1981), information for inattentive people (Popkin 1991), or a policy outcome that affects either beliefs about public policy (Mettler and Soss 2004; A. L. Campbell 2012) or perceptions of the competence of incumbent officials (Ashworth 2012). In general, in order for a policy-relevant event to affect political attitudes, citizens must first learn about the event and connect it to political discussions (Hopkins 2018; Zaller 1992). The literatures on real-world events and changes in local context identify four primary ways in which people might receive information about an event. Each information channel varies over the geographies it will reach, the duration of time it will send message about the event, and the types of information and arguments those messages will contain. These messages then interact with peoples' existing beliefs and partisan attachments to potentially shape public opinion.

First, people might be directly exposed to the event, such as knowing someone killed in a war (Gartner, Segura, and Wilkening 1997, 675; Karol and Miguel 2007, 635) or being personally affected by a wildfire (Hazlett and Mildenberger 2020) or an oil spill (Bishop 2014). Alternatively, citizens in a somewhat wider geographic area might learn about an event through other people in their social network. The role of social networks in shaping political attitudes has a long history in the study of political behavior, with seminal works acknowledging the influence of conversing with family and friends on political attitudes (Berelson, Lazarsfeld, and McPhee 1986; A. Campbell et al. 1960, 67; Downs 1957, 229). As summarized by Johnston and Pattie (2011, 17) "Where you are influences who you interact with; who you interact with influences what you learn and how you interpret the information and knowledge gained; and such local sources of 'valued' information influence how you behave."

The news media provides another, and possibly more important, channel through which people may learn about policy-relevant events. While some scholars view citizens as capable of connecting information about the real-world, for example changes in their local environments to politics (Gartner, Segura, and Wilkening 1997; Popkin 1991), other scholars argue for a central role for the news media. Hopkins (2018, 5, 96) argues that people rarely identify the implications of changes in their environment for politics and require the news media to frame this information in a way that allows individuals to update political attitudes and evaluations.[2] Importantly, national and local media represent two distinct mechanisms for learning about an event: in the case of mass shootings, local and national coverage of mass shootings cover the event with different frames (Holody, Park, and Zhang 2013; Holody and Daniel 2017; Holody and Shaughnessy 2022) and for different durations (Holody, Park, and Zhang 2013).

While the type of information that people receive may depend on where they live, their attitudes may also depend on the amount of time that has passed since the event happened. This

[2] See Karol and Miguel (2007) and Gartner, Segura, and Wilkening (1997) for similar arguments.

prediction is partially motivated by a common empirical phenomenon. Even when citizens' attitudes shift, people tend to return to their previously held relatively quickly. Evidence for this rapid decay includes studies of political advertising (Hill et al. 2013), political canvassing (Kalla and Broockman 2018), and survey experiments (Coppock 2023). More specifically, individuals' attitudes may return to their baseline opinions as peoples' attention and news coverage move to other important topics related to politics and public policy (Downs 1972; Peters and Hogwood 1985). Altogether, these factors suggest that people living close to the mass shooting and paying attention to the news shortly after the incident will hear more, and potentially qualitatively different, coverage.

This information that citizens receive from direct exposure, social networks, or local or national media can affect attitudes in two main ways. First, the information could move the political attitudes of most people in the same direction (Coppock 2023; Zaller 1992). Alternatively, the information could cause public opinion to diverge, either in the direction of individuals' existing beliefs or along party lines (Zaller 1992; Coppock 2023; Lenz 2013; though see Fowler and Howell 2023). Which scenario occurs depends on two factors: peoples' beliefs and partisan attachments as well as the types of information circulates in the four information channels.

Additionally, politically-relevant events might not affect citizens' positions on policy issues and instead change how intensely they care about the policies in question.[3] The literature on agenda setting and public opinion suggests that when people hear more about a policy area, in particular from the news media, people are primed to think about that policy issue (Iyengar and Kinder 1987). This explanation for why real-world events might affect individuals' preference intensities is almost mechanical: people simply cannot hold all relevant political considerations in their mind at once and having heard about about a policy area recently makes them prefer those policies more intensely (Zaller 1992; Peterson and Simonovits 2018). If citizens are primed to focus on a particular policy issue, perhaps by the news media after a real-world event, their downstream evaluations of political candidates for office may also shift in response (Iyengar and Kinder 1987; Stevens et al. 2011; Hart and Middleton 2014; Thesen, Green-Pedersen, and Mortensen 2017; Peterson and Simonovits 2018; Matthews 2019).

However, as mentioned earlier, recent changes in U.S. politics may weaken the ability of policy-relevant events to affect citizens' political attitudes. First, polarization may have crystallized individuals' policy preferences, reducing the opportunity for outside information to shape their political attitudes. The influence of party cues, in particular on citizens' policy preferences may be driven by strong partisan identities and individuals' need to maintain a positive social identity (A. Campbell et al. 1960; Green, Palmquist, and Schickler 2004; Mason 2018). Alternatively, party cues may serve as a powerful heuristic, allowing individuals to infer which

[3] It is worth clarifying the meaning of "preference intensity" with respect to policy issues. In this study, I specifically refer to how much weight citizens place on a policy issue in deciding which political candidates to support in elections. This idea originally stems from Downs (1957) but has been considered and tested by many other scholars (e.g. Stokes 1963; Aldrich and McKelvey 1977; Krosnick 1988; Niemi and Bartels 1985; Fournier et al. 2003; Visser, Krosnick, and Simmons 2003; Dennison 2019). This definition contrasts with other meanings of "issue salience", in particular ones focused on the subjective importance of an issue to an individual citizen (Dennison 2019).

policies they would support upon gathering additional information (Downs 1957; Fowler 2020). In either case, the increasing power of partisan cues may have led people to hold increasingly crystallized opinions on policy, reducing the influence of local events in shaping public opinion (Tesler 2015). Second, nationalization has led people to focus increasingly on national politics, possibly exposing them to a large number of repeated policy-relevant events. Repeated treatment from similar events may have led people to develop highly informed political attitudes on the associated policy issues or, in some cases, caused a sense of fatigue and disconnection from those news stories.

Moreover, where people place their attention, when they do attend to politics, has also changed in recent years. While nationalization affects politics in many ways, of particular relevance to this study is the shift in citizens' attention towards national politics: people are increasingly less interested in and knowledgeable about subnational politics (Hopkins 2018, 3, 83). Moreover, the decline of local news outlets in the United States might specifically reduce the amount of information available at the local level (Hayes and Lawless 2018). While these trends are likely related to the polarization, for the purposes of this study they constitute a separate way in which local context might have reduced influence on the attitudes of nearby residents: not only might people hold stronger attitudes but they might receive less information about local events. This decrease in attention to subnational politics could increase the ability of policy-relevant events to draw attention back to the local level and fill in information about local conditions. However, this decrease in interest in local politics may have the reverse effect, weakening the ability of events to serve as local focusing events. Finally, even when a local event occurs, people may have already been exposed to similar events across the country.

These broad trends in public opinion and political behavior raise the possibility that mass shootings may still affect attitudes, but that Tobler's First Law of Geography may be waning in influence. To understand whether this is the case, the ideal research design would measure the effect of local events immediately after the incident. This means collecting data across a wide geographic range (since we don't know where events will happen) and with a high tempo (since we don't know when events will occur). Previous studies of the effects of gun violence have amassed data with some of these properties, but have returned mixed results, perhaps due to the inability to measure attitudes in the days and weeks leading up to or immediately after the shooting for people closest to the incidents

2.2 Prior Evidence from Studies of Mass Shootings

Evidence from previous studies of mass shootings and policy preferences provides contradictory evidence about whether mass shootings affect attitudes. In two papers, Hartman and Newman analyze more than 55,000 survey respondents from the Cooperative Election Studies (CES) collected between 2010-2012, including a panel dataset between the years (Newman and Hartman 2019; Hartman and Newman 2019). The authors combine these data with the locations of mass shootings that occurred in the United States between 1966 and 2010. The analysis calculates how far each survey respondent lives from mass shootings that occurred over the last 50 years using zip codes as the geographic identifier and finds a modest difference in support for gun control for a person moving from a place with no mass shootings in the last 50 years to a person living closest to a mass shooting. They corroborate these differences using panel data on 9,500 people, several hundred of whom lived within 100 miles of a mass shooting between 2010 and 2012. In the second paper, which updates and corrects decisions related to the analysis, these effects obtain even when controlling for multiple exposures to shootings. Notably, Hartman and Newman report that the average respondent in the full 50-year analysis lived 84 miles away from an incident. In the panel analysis, all respondents within 100 miles of at least one shooting as are "treated." In this paper, the conclusion that people who live "close" to a mass shooting exhibit more restrictive gun policy attitudes is essentially defining "close" as 84 miles on average and defining temporal proximity as shootings that occurred up to 44 years prior to the date of interview.

Using the same data, Barney and Schaffner (2019) suggest corrections to the panel analysis (that Hartman and Newman apply in their second paper), modify the research design by extending it to three panel waves, and change the measure of "close" to illustrate whether the effects persist at closer distances. The authors also push toward causal identification by looking for effects within person and within time periods. With these methods, their results suggest no effects, on average, of mass shootings on gun policy preferences, but some evidence of polarizing effects by political party. They find that some Democrats living within 25 miles become more supportive of restrictions and Republicans living within 25 miles become less supportive of restrictions.

To make sense of these diverging results, Yamauchi (2020) takes up the inherent challenge in these papers: attempting to execute a difference-in-difference (DID) design with data and measures that are not-well suited to the exercise. Specifically, the conclusions drawn in prior work rely on differences between people living close to shootings and those living farther away, but it is hard to identify the shootings as the cause of any attitude change within person because the measure of policy preferences only starts in 2010. While comparing people in different places could account for unobserved differences across space, the research design assumes that these places were were not on different trajectories before treatment occurred. To investigate this assumption, Yamauchi focuses on treatments occurring between the last two years of the panel (2012 to 2014) so that he can compare pre-treatment trends between 2010 and 2012. Yamauchi proposes a method for estimating difference-in-differences designs with ordinal outcome measures, which allows him to estimate the effect of mass shootings on people responding "don't know" to the gun control question, rather than answer that gun control should be more or less strict. He concludes that mass shootings do not increase support for gun control or polarize public opinion, but instead cause people to shift from having no opinion on whether gun control should be more or less strict to having an opinion on the issue. As in Hartman and Newman (2019), he concludes that this effect is concentrated in zip codes that have not experienced a shooting within 100 miles in the previous decade.

While each analysis approaches the research question with a well-motivated research design, one common challenge is that these designs do not account for the ways in which the attitudes of people living more than 100 miles away from a mass shooting may change differently over time than the attitudes of people who live close to the incident. The designs therefore implicitly rely on a stronger identification assumption that the attitudes of both the treated and control groups move in parallel over time. To consider the impact of this assumption, consider how education might lead peoples' attitudes to trend differently over time. If individuals with more education pay more attention to national news, and the media extensively discusses gun control around the same time as a mass shooting, then the attitudes of more educated individuals might shift during this period. In contrast, the attitudes of less educated people across the country might remain the same, or experience smaller changes. If a mass shooting happens in a place with higher levels of education, and we compare these "treated" individuals to people elsewhere in the country with, on average, lower levels of education, we might conclude that mass shootings affect support for gun control, even if there is no effect. In this example, comparing the (more) educated individuals near the incident to individuals with similar levels of education across the country would lead us to the correct inference that the mass shooting had no effect on the political attitudes of nearby people.

Other research provides similarly mixed results. Rogowski and Tucker (2019) analyzes a smaller panel from The American Panel Survey (TAPS) with respondents from shortly before and after the mass shooting at Sandy Hook Elementary School in December 2012. The authors conclude that this incident did not support for gun control at the national level or in the Northeast more specifically. Somewhat in contrast, Zhang and Liu (2024) analyzes data from immediately before and after the mass shooting at an Orlando nightclub in 2016. The study concludes that the incident increased attention to policy issues related to terrorism as well as support for spending on counterterrorism. However, the authors argue that the event did not affect attention or policy preferences related to gun control. Finally, Sharkey and Shen (2021) finds that people in places with mass shootings are negatively emotionally impacted in the days after the incident, but that this effect quickly decays and is concentrated among Democrats rather than Republicans.

Other recent studies of gun violence and geographic proximity use administrative data sources to study the people who live closest to the incident. This literature is more conclusive and finds that school shootings have minimal effects on new voter registrations and Democratic vote share at the county level (Hassell, Holbein, and Baldwin 2020; García-Montoya, Arjona, and Lacombe 2022; Hassell and Holbein 2023). While this set of studies investigate different outcomes (i.e. political participation and election outcomes rather than political attitudes), the robust research designs and the consistent null effects challenge the mixed results from the literature relying on survey data.[4]

[4] In related work, Morris and Shoub (2023) does find that police killings may affect political participation and policy attitudes among the individuals who live closest to incident.

3 Data, Measurement, and Research Designs

3.1 Challenges in Drawing Causal Estimates from Survey Data

Identifying the effect of mass shootings on voters' policy preferences is complicated by stringent data requirements. In particular, studies applying causal designs to observational survey data face four primary challenges. First, studies would ideally measure citizens' policy preferences immediately before and after a mass shooting. Second, in order to investigate the parallel trends assumption core to these designs, public opinion needs to be measured in multiple periods before the event. Third, studies require large survey samples in order to precisely estimate the effects of mass shootings, especially given the modest effect sizes we might expect in contemporary U.S. politics. Finally, studies need to sample survey respondents from the communities closest to the mass shooting rather than those further away.

To illustrate these challenges, consider the 2010-2012-2014 panel survey from the Cooperative Election Study (CES), commonly used in previous studies of how living closer to mass shootings shapes political attitudes (Newman and Hartman 2019; Barney and Schaffner 2019; Hartman and Newman 2019; Yamauchi 2020).[5] While this panel represents the best data available to study the effect of geographic proximity to a shooting during this period, the structure of this survey introduces four primary challenges in estimating the effects of mass shootings on public opinion. First, panelists' support for gun control are measured up to two years after a mass shooting occurs, allowing the analyst to estimate only the most durable effects of geographic proximity and precluding analysis of the decay of the effect or estimates of the effect of temporal proximity. (Previous studies of the role of temporal proximity are also limited by smaller sample sizes, e.g. Rogowski and Tucker (2019) and Zhang and Liu (2024)). Second, the small number of pre-treatment measurements hampers assessments of the parallel trends assumption of difference-in-differences model, as looking for evidence that this assumption is violated requires at least two pre-treatment measurements.[6] Third, the CES sample comprises a relatively small set of individuals living particularly near to the mass shootings.[7] Finally, the

[5] The Cooperative Election Study asked respondents, "In general, do you feel that the laws covering the sale of firearms should be made more strict, less strict, or kept as they are?" Each study varies slightly in research design, but all use difference-in-differences designs to compare support for gun control among people living closer to mass shootings to individuals living further away.

[6] In particular, investigating this assumption is impossible in two-period difference-in-differences designs, including analyses of the mass shootings that occurred between 2010 and 2012, as noted by Hartman and Newman (2019). While an additional period is available when studying events taking place between 2012 and 2014, attrition means that only about 50% of respondents are available for analysis in 2014, as discussed in Yamauchi (2020).

[7] For example, Hartman and Newman (2019) estimates the effect of mass shootings on panelists living within 100 miles of the event. Barney and Schaffner (2019) focus on a smaller geography, with the primary analysis of polarizing results analyzing panelists living within 25 miles of an event. In supplementary materials, the study also presents results for different levels of geography, including 10 miles, 50 miles, 75 miles, and 100 miles as well as models defining treatment by media market.

sample size of the CES panel further complicates estimating effects of mass shootings when focusing on subgroups of individuals.[8]

3.2 Survey Data from the Democracy Fund + UCLA Nationscape Project

In this study, I address these challenges using nearly 500,000 interviews from the Democracy Fund + UCLA Nationscape project. This project conducted weekly surveys of 6,300 respondents collected between July 2019 and January 2021 from the survey firm Cint, previously known as Lucid. Each survey wave is weighted to be representative at the national and state levels. Benchmarking analyses find that this data collection and weighting process replicates as well as online samples collected by the Pew Research Center (Holliday et al. 2021). For this study, I geocode respondents' ZIP Codes in order to calculate respondents' distance from mass shootings as well as identify the media market in which the respondent resides. The large number of interviews collected each week means I can analyze the people in closest geographic proximity to 6 mass shootings during this period, as well as individuals' attitudes in the two weeks before and after an additional two mass shootings. This provides a clearer picture of the role of geographic and temporal proximity on political attitudes.

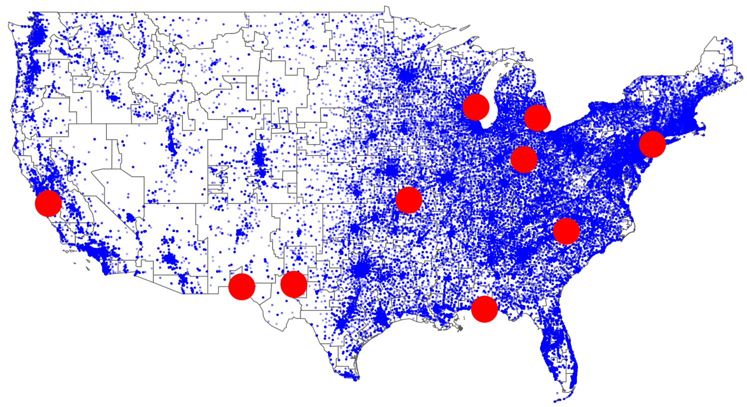
Figure 1 visualizes the locations of respondents from the UCLA Nationscape Project in blue. I also present the locations of the 10 incidents during the study period in red; definition and measurement of "mass shootings" are discussed later in this section. While some mass shootings occurred in places with a high density of individuals, for example incidents in New Jersey, Wisconsin, and Ohio, other incidents occurred in places with fewer survey respondents, for example El Paso, Texas.

3.3 Measurement: Changes in Public Opinion

Interviews from the UCLA Nationscape project collected several outcomes of particular relevance to this study. The first primary outcome is a scale that measures support for restrictive gun policies. The scale is constructed from four specific policies; respondents were asked if they supported a large number of public policies, including "require background checks for all gun purchases," "ban all guns," "ban assault rifles," and "limit gun magazines to 10 bullets." Respondents could answer each question with one of three responses: "Agree" (recoded to 1) and "Disagree" or "Not Sure" (both recoded to 0). The scale was calculated by summing four binary outcomes and dividing by four. The unweighted Kuder-Richardson 20 score was 0.64 for this scale. Table A1 presents the distribution of responses for each policy question. The weighted mean of the final scale is 0.53 and includes values at 0 (10%), 0.25 (27%), 0.5 (18%), 0.75 (31%), and 1 (14%). See Table A1 for additional information about the distribution of this scale and its components.

[8]While unusually large for a panel survey, the CES panel includes 19,533 respondents in 2012 and 9,500 respondents in 2014.

Figure 1: Distribution of Mass Shootings and Survey Respondents Between July 2019 - Jan 2021



Blue dots indicate respondents from the Democracy Fund + UCLA Nationscape Project, red dots indicate mass shootings, and geography boundaries indicate media markets

The other primary outcome is the intensity of individuals' preferences for restrictive gun policies. This measure is calculated from a unique feature of the UCLA Nationscape surveys: each of nearly 500,000 respondents participated in a conjoint experiment (Sides, Tausanovitch, and Vavreck 2022; Tausanovitch 2024). In this study, I calculate an approximate measure of the intensity of individual citizens' preferences, rather than the more common estimator of the Average Marginal Component Effect (AMCE). Specifically, for each person I calculate the number of times they picked a conjoint set that had one of their preferred gun policies divided by the number of conjoint sets they saw that had at least one gun policy. Conceptually, this constitutes a within-person marginal mean for a small number of experiments per person, and for people who saw at least one conjoint set.

A secondary outcome is citizens' vote intention. Respondents were asked, "If the general election for president of the United States was a contest between Joe Biden and Donald Trump, who would you support?"[9] Respondents could answer "Joe Biden" (coded as 1), "Donald Trump" (coded as 0), or "Don't Know" (coded as 0.5). I also include a supplementary outcome: whether people think the country is on the right track. Respondents were asked, "Would you say things in this country today are…" and could answer "Off on the wrong track" (coded as 1) "Generally headed in the right direction" (coded as 0) or "Not sure" (coded as 0.5).

[9] After the Democratic primary, the question changed to "If the election for president were going to be held now and the Democratic nominee was Joe Biden and the Republican nominee was Donald Trump, would you vote for…" Respondents could answer Donald Trump Joe Biden, Someone else, I am not sure/don't know, or I would not vote.

3.4 Measurement: Public Mass Shootings

To identify public mass shootings, I use databases of mass shootings and incidents of gun violence maintained by Mother Jones and a combined project between Northeastern University, the Associated Press, and USA Today.[10] These projects use conventional understanding of a 'mass shooting'. Specifically, these projects use thresholds of four individuals killed by gunfire, excluding cases involving gang activity, drug-related activity, or other cases involving criminal activity.[11]

3.5 Research Design

In order to investigate whether mass shootings affect the attitudes of geographically and temporally proximate citizens, I rely on two research designs. First, I estimate whether people living closer to a mass shooting experienced larger shifts in attitudes than people living further away, using a weighted difference-in-differences design. Second, I compare the attitudes of people who were interviewed in the two weeks immediately before and after a mass shooting occurred.

3.5.1 Estimating the Effect of Geographic Proximity (Distance)

The first research design investigates whether mass shootings affect political attitudes in the areas closest to the incident. For each mass shooting, I estimate a difference-in-differences model that compares changes in attitudes between respondents who live close to the mass shooting to people who live at least 100 miles from a mass shooting during this period. To assess whether the largest changes in attitudes occur for people very close to the shootings, I estimate separate models for ten distances: 0-10 miles from a shooting, 10-20 miles away, and so on up to 90-100 miles away. Within each group, I compare the attitudes of respondents from up to 20 weeks after a mass shooting to people from the same places up to 20 weeks before the mass shooting.[12] Crucially, I reweight each period of treatment data (e.g. 0-10 miles during the 10 weeks before the shooting) and control data (e.g. 100+ miles during the 10 weeks after

[10] Mother Jones (Link) and AP/Northeastern/USA Today (Link)

[11] Previous studies about the effects of mass shootings on public opinion (Newman and Hartman 2019; Barney and Schaffner 2019; Hartman and Newman 2019; Yamauchi 2020) rely primarily on the data provided by the Stanford Mass Shootings in America (MSA) database. This project defined mass shootings as "3 or more shooting victims (not necessarily fatalities), not including the shooter. The shooting must not be identifiably gang, drug, or organized crime related." Unfortunately, this database was discontinued in 2016. https://swap.stanford.edu/was/20161202200317/http:/library.stanford.edu/projects/mass-shootings-america

[12] I selected this time threshold to maximize the number of mass shootings included in the analysis. While some mass shootings occur to close to the beginning of the survey project to include in this research design, the next earliest incident (Pensacola, Florida on 2019-12-06) occurred almost exactly 20 weeks after the first interview (2019-07-18).

the shooting) to resemble the demographics of the treated sample from immediately after the incident (e.g. 0-10 weeks after).[13]

Motivating the Design

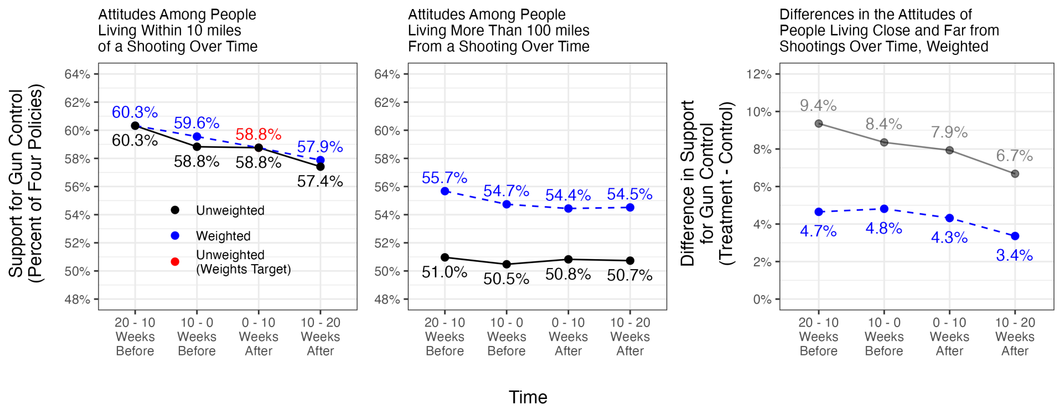
Importantly, I reweight each comparison group (i.e. people who live far away or people from the treated area but not from immediately after the shooting) to match the demographics of the treated group (i.e. people close geographic proximity who took the survey immediately after the shooting). This weighting process allows respondents who are more similar to the treated group to exert greater influence over the analysis, and achieves two goals. First, we obtain a better measurement of the change in public opinion in the treated area, for example the change in public opinion among people living within 0-10 miles from a shooting between the 10 weeks before the incident to the 10 weeks after. Second, we compare this change in attitudes to a better counterfactual: the change in attitudes among similar people that live elsewhere in the county.

To illustrate this weighted difference-in-differences method that I will use in the following tests, examine Figure 2. Here, I plot time on the horizontal access and support for restrictive gun policy on the vertical access. Panel 1 visualizes support for gun control among the roughly 6,500 respondents living within 10 miles of any of the 6 mass shootings for which I have at least 20 weeks of data before and after the incident. The black line presents unweighted average opinion for these individuals while the blue line presents the weighted averages (see below). Similarly, in Panel 2, I plot support for restrictive gun policies among the "control group:" respondents living more than 100 miles from any of these mass shootings. The black line indicates the unweighted averages while the dashed blue line indicates the weighted averages.

Figure 2 suggests a few immediate findings. First, people who live within 10 miles of a mass shootings already hold more liberal gun control attitudes than people living more than 100 miles away from any shooting. Second, support for gun control is declining on average in treated areas, even before the shooting occcurs. Some of this decline is due to sample composition: in some places I have smaller samples in each period (e.g. Pensacola, Florida or Springfield, Missouri) that are more susceptible to sampling variability than larger places (e.g. Jersey City, New Jersey). I therefore reweight the sample from each treatment period to resemble the demographics of the people living within 10 miles of the shooting in the 10 weeks after the incident (i.e. the red dot in Panel 1). The blue dots indicate that the reweighted estimates experience somewhat less decline than the unweighted estimates.

[13] I use entropy balancing weights to achieve mean balance on several demographic variables for each distance bin and mass shooting: race/ethnicity (white vs. non-white), gender (male vs. female), age (continuous), education (college and above vs. some college and below), household income (less than \$55,000 or more than \$55,000), political party (7-point party identification, with "leaners" counted as partisans), and gun ownership (whether anyone in the household owns a gun). In certain analyses (e.g. by political party), some demographics are dropped to achieve weights. Weights are generated using the `ebal` package in R.

Figure 2: Calculating Changes in Support for Gun Control Before and After Mass Shootings



This figure includes data from the 6 mass shootings for which 20 weeks of data is available before and after the incident. Panel 2 includes 6 groups of control units: one for each mass shooting. Respondents from more than 100 miles away may therefore be included multiple times, if the individual took the survey within 20 weeks of multiple mass shootings. The formal effect estimate from the stacked difference-in-differences model accounts for this repeated inclusion by including geographic and temporal fixed effects for each shooting.

Third, Panel 2 demonstrates that support for gun control is lower in places farther away from mass shootings. However, after reweighting these data to match the demographics of the treated units sampled immediately after the incident (i.e. the red dot in Panel 1), average support for gun control is higher in our comparison group. Moreover, we observe the same decline in support between 20 - 10 weeks before and 10 - 10 weeks before as we do in the treated data in Panel 1.

Finally, in Panel 3 of Figure 2, I plot the differences in support for gun control between the treated and control groups. Looking at the trend in the weighted averages, we observe essentially no change in support for gun control between 20 - 10 weeks before the shooting and 10 - 0 weeks before the shooting. Then, in the 10 weeks after the shooting, we observe about a -0.5 percentage point (i.e. -0.4929 percentage point) difference in attitudes, with a larger decline in gun policy support in the 10 - 20 weeks after the incident. In Section 4.1, Table 1 presents a formal version of this estimate using a stacked difference-in-differences model; this design is described in more detail in the next subsection. Table 1 also contrasts this naive estimate to a more rigorous estimate that compares the attitudes of people from the same ZIP Codes before and after a mass shooting, rather than simply comparing people from the same general areas (e.g. people 0-10 miles away to people 100+ miles away).

Statistical Model

To formally estimate the effect of geographic proximity to mass shootings on political attitudes, I rely on a stacked difference-in-differences design that uses the weights described in the previous section. Specifically, I combine respondents into four periods of 10 weeks: 20-11 weeks before, 10-0 weeks before, 0-10 weeks after, and 11-20 weeks after. I then estimate how the difference in gun control attitudes between treatment and control areas changes from before the event (i.e. 10-0 weeks before) to after the event (i.e. 0-10 weeks after and 11-20 weeks after). I estimate this model for each distance bin (e.g. 0-10 models), with weights for each shooting and distance bin (e.g. Jersey City, New Jersey and 0-10 miles). Distance to a mass shooting is measured at the ZIP Code level. I also conduct one test of the parallel trends assumption, estimating how attitudes changed from 20-10 weeks before mass shootings to 10-0 weeks after the incident as a placebo test. In some analyses, I attempt to recover the most precise estimate possible, and collapse the data into just two time periods: 20-0 weeks before and 0-20 weeks after.

Equation 1 presents the statistical model used to estimate the difference-in-differences model. I include fixed effects for both time (i.e. 10-week period) and space (i.e. ZIP Code), respectively represented by δ_t and α_z. By controlling for ZIP Code, I aim to compare the attitudes of respondents from after the mass shooting to the attitudes of respondents from the same ZIP Code from before the mass shooting. Because distance to a mass shooting is calculated at the ZIP Code level, the association between distance and gun policy preferences from the reference period is captured by the ZIP Code fixed effects and cannot be estimated separately. In Table 1, I also present one model with fixed effects for the treatment and control group for each mass shooting.

$$Y_{itz} = \beta_1 * Treatment_{itz} + \alpha_z + \delta_t + \epsilon_{itz} \tag{1}$$

By including fixed effects for respondents' ZIP Codes and the 10-week period in which respondents took the survey, I have an opportunity to make a causal claim about the effects of mass shootings on local communities. If we assume that the association between distance and gun control attitudes would not have changed absent the mass shooting and that no other confounding events took place in these communities, then any change in this association represents the causal effect of the mass shooting on peoples' attitudes, as summarized by a linear model. Models predicting placebo outcomes (i.e. support for policies related to abortion, healthcare, and the environment) are presented in Section S5 and Section S6.

Four mass shootings that occurred during the period of the UCLA Nationscape data collection are excluded from this analysis of geographic proximity. Two incidents are excluded due to the exceptionally small samples recruited from the surrounding communities: Gilroy, California (7/28/19) and Odessa, Texas (8/31/19). In addition, two more cases are excluded as they occurred under three weeks after the beginning of data collection: El Paso, Texas (8/3/19) and Dayton, Ohio (8/4/19).

3.5.2 Estimating the Effect of Temporal Proximity (Time)

To estimate the immediate effect of public mass shootings on public opinion, I use an Unexpected Event during Surveys Design (Muñoz, Falcó-Gimeno, and Hernández 2020). This design compares the gun policy attitudes of survey respondents from a particular geography (e.g. national or media market) immediately after the event to the attitudes of survey respondents from the same geography immediately prior to the event. I follow Muñoz, Falcó-Gimeno, and Hernández (2020) in defining a bandwidth around the incident and using a weighted regression model with time as a running variable to estimate the effect of the incident.

The UCLA Nationscape project is unusual with the length of data collection, which lasted over a year and a half. Rather than splitting the survey data into two parts, as discussed in Muñoz, Falcó-Gimeno, and Hernández (2020), I default to a bandwidth of 16 days, the longest bandwidth available for all incidents; the first mass shooting during the study period occurred 16 days after the start of the data collection in in El Paso, Texas.

The statistical model presented in Equation 2 summarizes the Unexpected Event During Survey Design for a single mass shooting. Y_{itg} represents the outcome, for example the gun policy scale, which is measured for individual i at a number of days before or after the mass shooting t. The geography of the respondent g (e.g. media market) is constant within a given model datasets. The variable $post_{itg}$ is an indicator for $Days_{itg} \geq 0$, or whether the respondent was interviewed after the event. Each respondent is also assumed to have an unobserved residual u_{itg}

$$Y_{itg} = \beta_0 + \beta_1 * Post_{itg} + \beta_2 * Days_{itg} + \beta_3 * Days_{itg} * Post_{itg} + u_{itg} \qquad (2)$$

The estimator of interest is β_1, which captures the change in public opinion from the day before the event to the day after. Following the guidance of Muñoz, Falcó-Gimeno, and Hernández (2020), the day of the mass shooting is excluded, given respondents from that day represent a mix of treatment and control cases.

4 Findings

4.1 Local Context: Geography Proximity to Mass Shootings

Does geographic proximity to a mass shooting affect citizens' support for restrictive gun policies? In Table 1, I present results from an analysis of people who live within 10 miles of a mass shooting and that took the survey within 20 weeks of the incident. This design compares support for four restrictive gun control policies among individuals from the 10 weeks after the mass shooting to people from the places from the 10 weeks before the mass shooting.

Column 1 compares changes in attitudes between people from the same general areas: 0-10 miles away (i.e. treatment) and 100+ miles away (i.e. control). Here, we uncover the same -0.5 percentage point from Panel 3 of Figure 2. This estimate is from a stacked difference-in-differences model with geographic fixed effects for living in the general treatment or control areas, interacted with incident. This model also uses the weights constructed in Figure 2.

While the weights help account for possible differential trending among different types of people, it is impossible to observe all covariates needed to construct a perfect counterfactual sample. I therefore reestimate this effect with the stronger research design that conducts a within-ZIP Code comparison.

Table 1: The Effect of Living within 10 Miles of a Mass Shooting on Support for Gun Control (0 - 1 Scale)

	(1) Simple Treatment vs. Control Comparison	(2 - 4) Within ZIP Code Comparison			
	0-10 Weeks After	0-10 Weeks After	10-20 Weeks After	10 - 0 Weeks Before	0 - 20 Weeks After
Estimate	-0.00493	0.00068	-0.01420	-0.00014	-0.00415
	(0.00721)	(0.01108)	(0.01125)	(0.01137)	(0.00683)
# of Observations	518,744	518,744	511,104	517,220	1,050,544
# Treated Respondents	3,085	3,085	3,380	3,191	6,694
# of Mass Shootings	6	6	6	6	6
Distance (Miles)	0 - 10	0 - 10	0 - 10	0 - 10	0 - 10
Geographic FE	Treat/Control	ZIP	ZIP	ZIP	ZIP
Time FE (Weeks)	10	10	10	10	20
Weights	Yes	Yes	Yes	Yes	Yes

Note: *** 0.001, ** 0.01, * 0.05, + 0.10. Each column presents an estimate from a stacked difference-in-differences model. Each model compares changes in support for gun control in the either the 10 or 20 weeks after the shooting to the 20 weeks before the incident. The model includes fixed effects at the shooting-ZIP Code and shooting-period level. Each period is reweighted to achieve mean balance with the first X weeks after the shooting in the treated area. Interviews from the day of the incident are dropped. Survey data are from the UCLA Nationscape Project

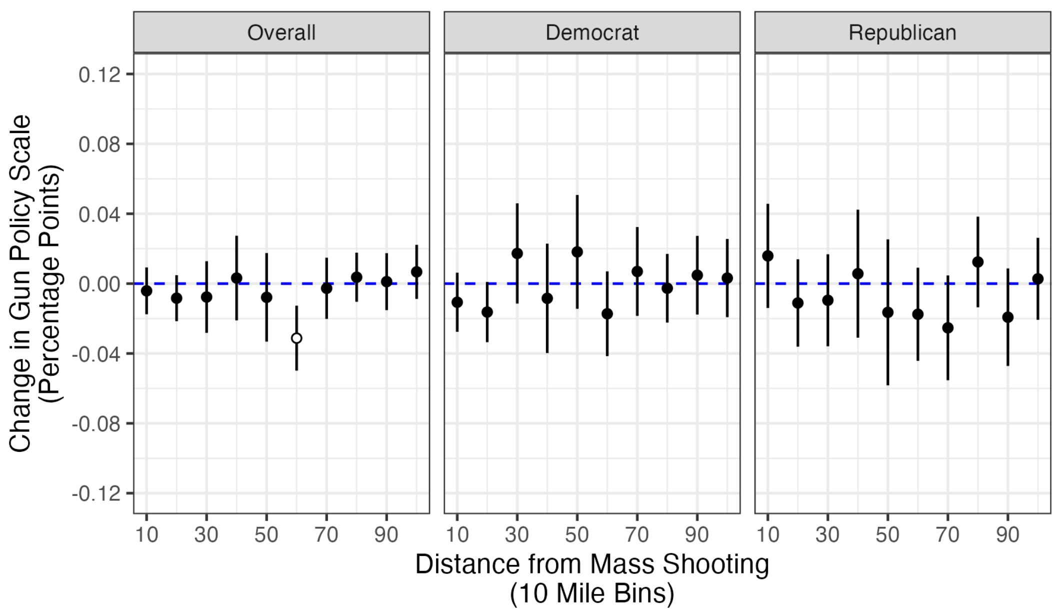
In Column 2, I switch from examining changes in attitudes within the treatment and control groups to comparing people from within the same ZIP Codes. By including fixed effects for every ZIP Code, interacted with mass shooting, I am also able to account for unobserved differences across small geographic areas that do not vary over time, in addition to comparing similar types of people through the reweighting procedure. Table 1 demonstrates that the estimated effect of living within 10 miles of a mass shooting changes only slightly, from about -0.5 percentage points to about 0.07 percentage points. While the standard error of the estimate from this stronger researcher design is somewhat larger, we are still able to rule out effects larger than 0.022 or smaller than -0.021 with 95% confidence.

In Column 3, I estimate the effect of living within 10 miles of a mass shootings on attitudes in the following 10 weeks (i.e. 10 - 20 weeks after a mass shooting). While the magnitude of the coefficient is somewhat larger, the effect is not statistically significant. In Column 4, I conduct a placebo test by comparing support in the 10 - 0 weeks before mass shootings to the 20 - 10 weeks prior to the incident. I find no evidence of challenges to the parallel trends assumption.

Column 5 presents the highest-powered estimate of the effect of mass shootings on public opinions in the weeks after the incident. Comparing changes in attitudes between the 20 weeks before a shooting to the 20 weeks after the incident is the closest comparison to research designs of previous papers, albeit still focused on a much shorter window of time. I estimate a small effect of -0.4 percentage points, which is not statistically significant, and am able to rule out effects larger than 0.0092 and smaller than -0.0175.

In Figure 3, I turn to estimating the effect of living different distances from a mass shooting, evaluating the effect of living 0-10 miles away, 10-20 miles away, and so on up to a distance of 90-100 miles. Each dot represents an estimate from a single difference-in-differences model, estimated for the distance bin indicated on the x-axis. These estimates pool data over 20 week periods and thus correspond to Column 5 from Table 1. In fact, the first dot in Panel 1 is the same estimate from Column 5 of Table 1.

Figure 3: The Effect of Geographic Proximity to Mass Shootings on Support for Gun Control



*** 0.001, ** 0.01, * 0.05, + 0.10. Each estimate presents a stacked difference-in-differences model that compares changes in support for gun control in the 20 weeks after the shooting to the 20 weeks before the incident. The model includes fixed effects at the shooting-ZIP Code and shooting-period level. Each 20 week period is reweighted to achieve mean balance with the first 20 weeks after the shooting in the treated area. Independents who "Lean" towards a party are counted as partisans. Interviews from the day of the incident are dropped. Survey data are from the UCLA Nationscape Project

Overall, Figure 3 strongly suggests that geographic proximity to mass shootings does not affect support for gun control. In Panel 1, which presents the models estimated for all individuals, only one of the 10 estimates is statistically significant, suggesting that this estimate is driven by sampling variation. Looking at Panels 2 and 3, none of the estimates for Democrats or Republicans is statistically significant. The by-party estimates thus suggest that the overall null effect of mass shootings is not driven by a polarizing effect, in which Democrats become more supportive of gun control while Republicans grow to oppose restrictive gun policies.

4.2 National Effects: Temporal Proximity to Mass Shootings

4.2.1 Trends in News Coverage and the Public's Attention

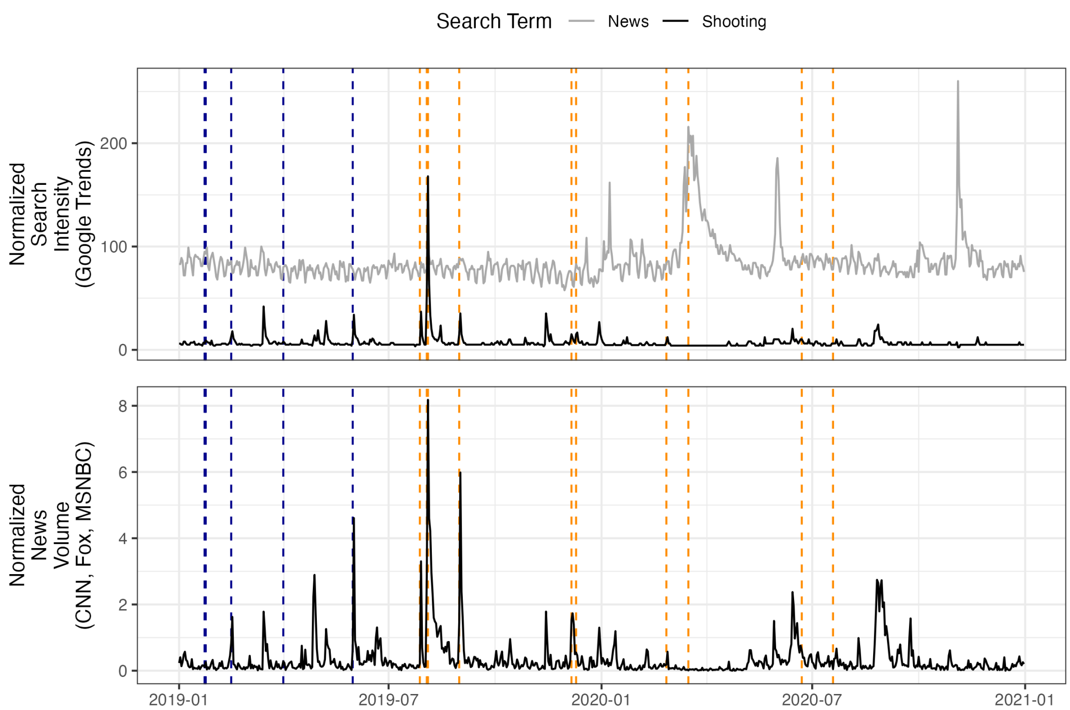
Do mass shootings affect the political attitudes of temporally proximate citizens? Before examining changes in political attitudes at the national level, I test whether the media covered these incidents andwhether the public paid attention to this coverage. Panel 2 of Figure 4 investigates trends in TV news coverage during this period; this graphical analysis suggests that news coverage of shootings spiked after most mass shootings during this period, in particular after the two back-to-back shootings on August 3 and 4, 2019. These increases in coverage appears to coincide with increased attention to mass shootings on the part of citizens. Panel 1 presents the normalized volume of Google Searches for the word 'shooting' during this period and confirms that engagement spikes after these incidents.

Altogether, Figure 4 suggests four primary conclusions. First, the national news media appears to have extensively covered mass shootings during this period. Second, the volume of coverage varies by event, with a large volume of coverage of two high-casualty back-to-back shootings in El Paso, TX and Dayton, OH in early August 2019 and, to a lesser extent, of another high casualty shooting in Odessa, TX on August 31, 2019. The figure also reveals substantially less national coverage of the other incidents in 2019 and 2020. Third, people appear to seek out information about the mass shootings as they occur. Finally, the other spikes in both panels suggest that other events not currently included in the study appear to have generated news coverage and engagement with gun violence during this period. These conclusions concord with those of (Reny et al. 2023) which studies the effects of mass shootings on national-level engagement and media coverage. Additionally, this figure highlights that even "lower salience" events generate interest and engagement with the occurrence of a mass shooting, if not necessarily searches for "gun control" or "gun rights."

4.2.2 Estimating Changes in Policy Preferences

In Figure 5 I estimate the effect of mass shootings on support for gun control at the national level. For this analysis of national public opinion, I include all mass shootings not immediately preceded by another event. For example, I exclude the mass shooting in Jersey City, New Jersey in December 2019 which occurred four days after a mass shooting in Pensacola, Florida. In each panel, I present an unweighted estimate and an estimate using nationally representative

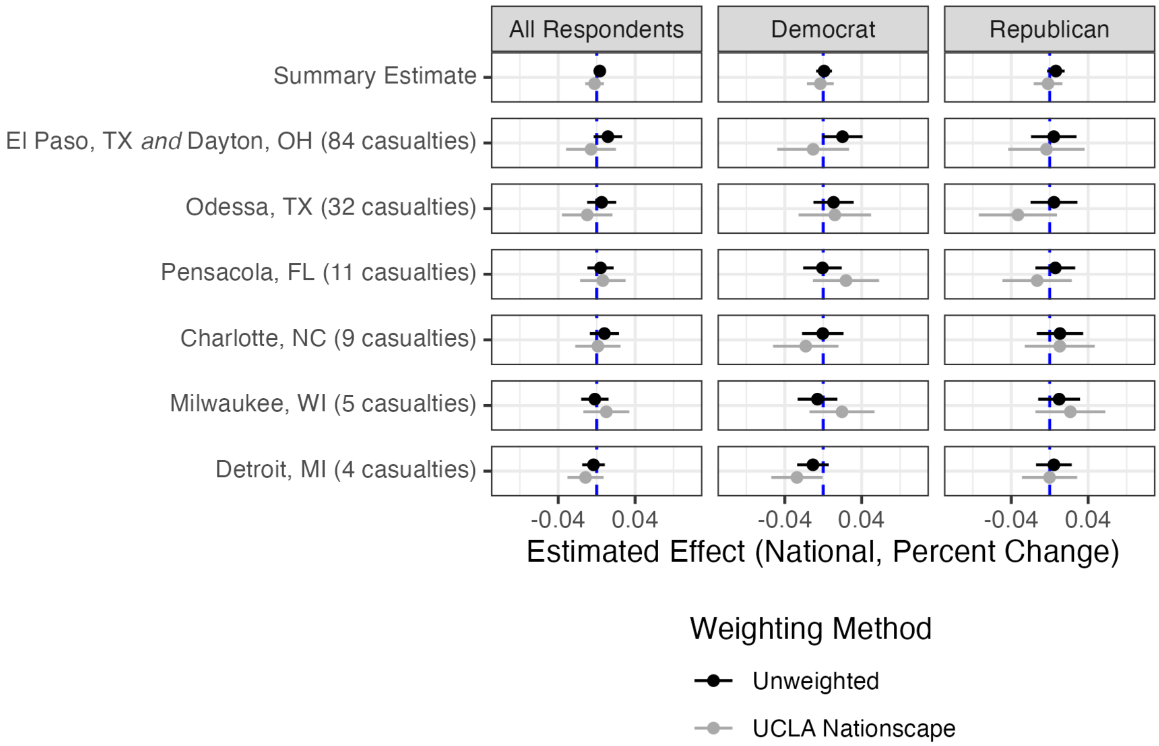
Figure 4: National Trends in News Coverage of and Attention to Mass Shootings.



Panel 1 presents a daily time series of the volume of Google Searches for the term ‘shooting’ as a percentage of the maximum number searches for the term ‘news’ between 2019-01-01 and 2020-12-31. Panel 2 presents the volume of daily news coverage related to shootings from three national networks (CNN, Fox, and MSNBC) during the same period. Data on television news coverage are from the GDELT project. Vertical lines indicate mass shootings during 2019 and 2020. Orange dashed lines indicate mass shootings during the period that the UCLA Nationscape Project fielded surveys.

weights. Note that these models do not reweight the data, as in the geographic proximity analysis, given the larger sample sizes of treated units and the lack of a pure control group.

Figure 5: The Effect of Temporal Proximity on National Support for Gun Control



Each dot indicates an estimate for the effect of a mass shooting on public opinion on political attitudes the day after the incident. Error bars indicate 95% confidence intervals. Each estimate is from an Unexpected Event During Survey design with a bandwidth of 16 days and controlling for several individual-level covariates. Mass shootings were included in this analysis if no other incidents occurred in the immediately preceeding days. For each mass shooting, two estimates are presented: an unweighted estimate an estimate using the original weights from the UCLA Nationscape project. One day is subtracted from the running variable, except for the El Paso/Dayton case in which two days are subtracted. In the label for each mass shooting, the number indicates the number of casualties (i.e. deaths and injuries) from the incident.

The first row presents a summary estimate of the effect of mass shootings on support for gun control the day after the incident for all respondents, as well as separate estimates for Democrats and Republicans. Below this first row, each subsequent row presents estimates for an individual mass shooting, ordered by the number of casualties (i.e. deaths and injuries) involved in the incident. The first incident combines two different mass shootings that occurred on subsequent days: El Paso, TX on August 3, 2019 and Dayton, OH on August 4, 2019. No individual incident affected support for gun control, either overall or for a particular political party.

Of particular interest is a uniquely tragic incident. In August 2019, two mass shootings occurred on adjacent days: El Paso, Texas on August 3, 2019 and Dayton Ohio on August 4, 2019. I consider this a combined treatment. Not only did these incidents together involve over 80 casualties (i.e. deaths and injuries), more than double the casualties of the next most devastating incident, but Figure 4 demonstrates that these back-to-back shootings caused an unusually large increase in news coverage and public attention. Perhaps surprisingly, Figure 5 provides minimal evidence that the El Paso and Dayton shootings affected support for gun control. Ultimately, Figure 5 provides minimal evidence that temporal proximity to a mass shooting affects support for gun control nationally.

4.3 The Interaction of Geographic and Temporal Proximity

In Figure 3 I find that geographic proximity to a mass shooting produces no change in policy preferences in the weeks after the shooting. Similarly, in Figure 5 I find that temporal proximity to a mass shooting produces no change in policy attitudes at the national level. Do geographic and temporal proximity interact, leading to short-term changes in public opinion in a wider – but still geographically proximate – area?

Table 2 presents estimates of how mass shootings affect support for gun control in the media market in which the mass shootings occurred. Each column presents a summary estimate that averages the effect across all mass shootings in the period. Specifically, I estimate a separate model for each event and combine the noisy individual estimates into a summary estimate using inverse-variance weighting.[14]

Starting with Column 1, the estimated effect across all respondents regardless of political party is near zero and precisely estimated. This result suggests that mass shootings did not affect aggregate public opinion in the surrounding media markets in the days after the incidents. Columns 2 and 3 present separate models estimated for Democrats, Republicans. While splitting the sample by political party increases the uncertainty of each pooled estimates these estimates provide minimal evidence that public opinion polarizes immediately after the incident. Altogether, these findings suggest that even the people who may encounter the most information and messages related to gun control after a mass shooting – outside of those in the very closest geographic proximity – are not persuaded to update their policy positions.

[14] See Section S3.

Table 2: The Effect of Mass Shootings on Support for Gun Control in the Surrounding Media Markets

PANEL A: UNWEIGHTED ESTIMATES			
	Overall	Democrat	Republican
Pooled Estimate	0.01325	0.03300	-0.04968
	(0.03212)	(0.04974)	(0.04820)
Mass Shootings	8	8	8
Bandwidth (Days)	16	16	16
Weights	No	No	No
Geography	DMA	DMA	DMA

PANEL B: WEIGHTED ESTIMATES			
	Overall	Democrat	Republican
Pooled Estimate	0.01832	0.03741	-0.04657
	(0.03180)	(0.05122)	(0.04817)
Mass Shootings	8	8	8
Bandwidth (Days)	16	16	16
Weights	Yes	Yes	Yes
Geography	DMA	DMA	DMA

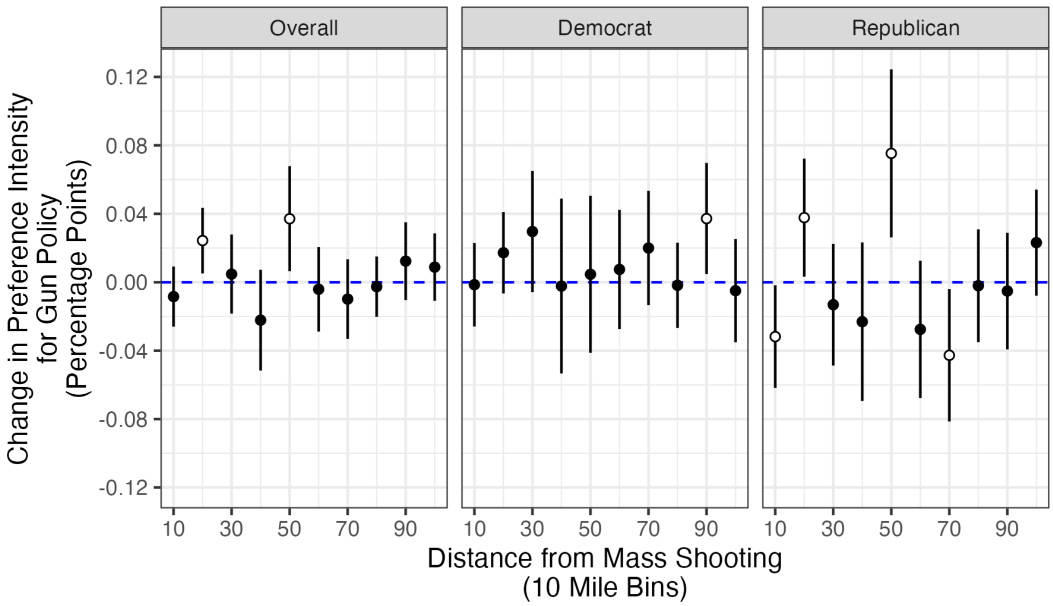
Note: The regression models predict the gun policy scale from days before or after the shooting, interacted with an indicator for after the event. When weights are used, they are calculated using entropy balancing to reweight each 16-day period to have mean balance with the demographics of the week after the mass shooting. Weights are constructed using binned versions of party identification, race/ethnicity, age, gender, education, and income. (Party and race/ethnicity are dropped when estimating models by party.) These models use a bandwidth of 16 days and exclude respondents from the day of the incident. The summary estimate presented in this table is calculated using inverse-variance weighting and the standard error is presented in parentheses below the estimate. The number of media markets equals the number of mass shootings.

4.4 Effects on Preference Intensity

Does geographic proximity mass to mass shootings affect the intensity of individuals' preferences? In Figure 6, I estimate the effect of proximity to a mass shooting on preference intensity, using the same weighted difference-in-differences research design as in Figure 3. While sporadic estimates are positive and statistically significant, most estimates are small and not significant. In particular, mass shootings do not increase the intensity of preferences for people who live closest to the incident (i.e. 0-10 miles), where we would most expect to see changes in intensity. Overall, this figure suggests that mass shootings do not cause people to care more intensely about gun control policy in the months after the incident.

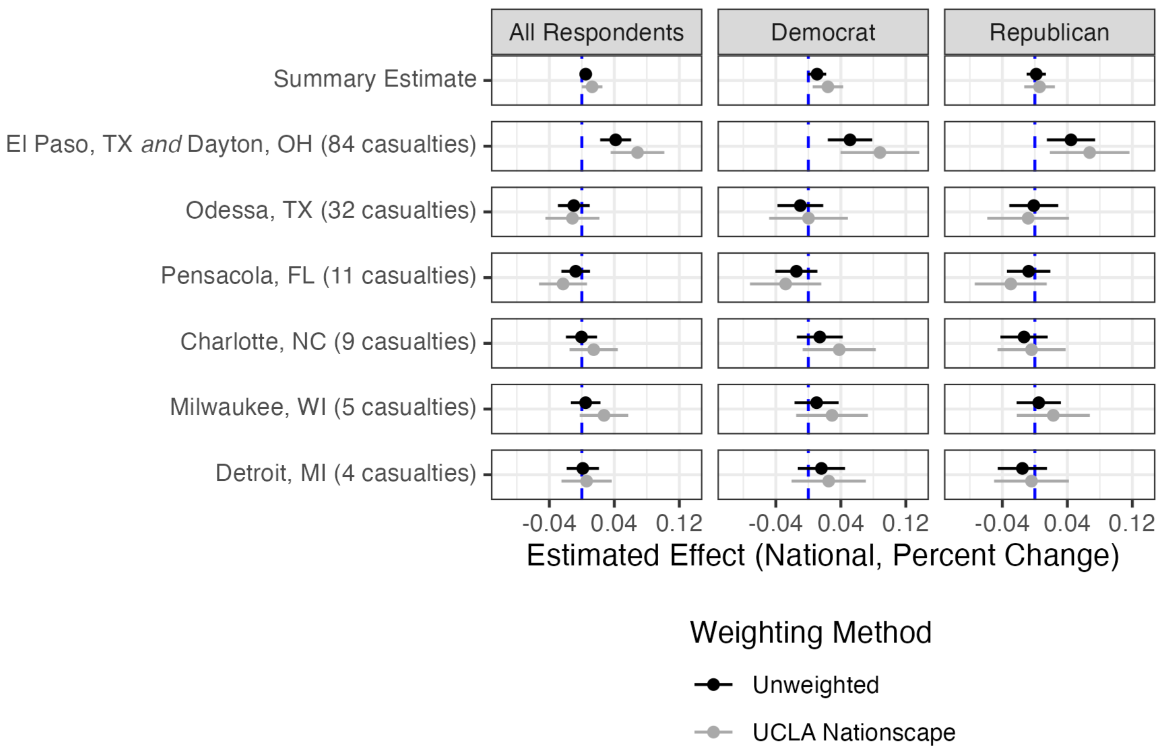
In Figure 7, I estimate the effect of temporal proximity to mass shootings on the intensity of peoples' gun policy preferences. As with the national analysis of policy preferences, the

Figure 6: The Effect of Geographic Proximity to Mass Shootings on Preference Intensity



*** 0.001, ** 0.01, * 0.05, + 0.10. Each estimate presents a stacked difference-in-differences model that compares changes in preference intensity for gun control policies in the 20 weeks after the shooting to the 20 weeks before the incident. The model includes fixed effects at the shooting-ZIP Code and shooting-period level. Each 20 week period is reweighted to achieve mean balance with the first 20 weeks after the shooting in the treated area. Independents who “Lean” towards a party are counted as partisans. Interviews from the day of the incident are dropped. Survey data are from the UCLA Nationscape Project first row presents a summary estimate of the effect of mass shootings on preference intensity the day after the incident. On average, mass shootings during this period did not affect the intensity of respondents' preferences for gun policy. Each additional row visualizes estimates for one mass shooting, ordered by the number of casualties. For five of the six incidents, the estimates are near zero and not significant, confirming that mass shootings generally do not affect the how intensely people care about gun control policies.

Figure 7: The Effect of Temporal Proximity on Preference Intensity



Each dot indicates an estimate for the effect of a mass shooting on public opinion on political attitudes the day after the incident. Error bars indicate 95% confidence intervals. Each estimate is from an Unexpected Event During Survey design with a bandwidth of 16 days and controlling for several individual-level covariates. Mass shootings were included in this analysis if no other incidents occurred in the immediately preceeding days. For each mass shooting, two estimates are presented: an unweighted estimate an estimate using the original weights from the UCLA Nationscape project. One day is subtracted from the running variable, except for the El Paso/Dayton case in which two days are subtracted. In the label for each mass shooting, the number indicates the number of casualties (i.e. deaths and injuries) from the incident.

However, while the back-to-back incidents in August 2019 did not affect which policies people preferred, Figure 7 suggests this combined treatment did affect how intensely people cared about gun-related policies. Across all three groups, the unweighted estimate suggests that the back-to-back incidents increased the chance of people selecting conjoint sets with their preferred gun policy items by about four percentage points. In particular, the mass shootings in El Paso, TX and Dayton, OH in early August led to a particularly high level of news coverage and public attention during the period of study. (See trends in news coverage and attention in Figure 4). This combined treatment – two high-casualty mass shootings occurring on adjacent days – represents a highly unusual event, even in a news environment that regularly covers mass shootings.

To consider whether the exceptional nature of the shootings in El Paso, TX and Dayton, OH led to this increase in preference intensity, consider row 3 of Figure 7. This row presents estimates for another high-casualty shooting: Odessa, TX on August 31, 2019, which involved nearly 40% of the casualties of the back-to-back incidents. While Figure 4 confirms that this incident led to a spike in news coverage, and to a lesser degree an increase in attention, Figure 7 demonstrates that this other incident did not increase the intensity of citizens' preferences. related to gun control policy. Altogether, these findings suggest that mass shootings do not generally affect the intensity of peoples' policy preferences. However, the mass shootings in El Paso, TX and Dayton, OH provide suggestive evidence that the most unexpected and unusual incidents may move preference intensity by a small amount.

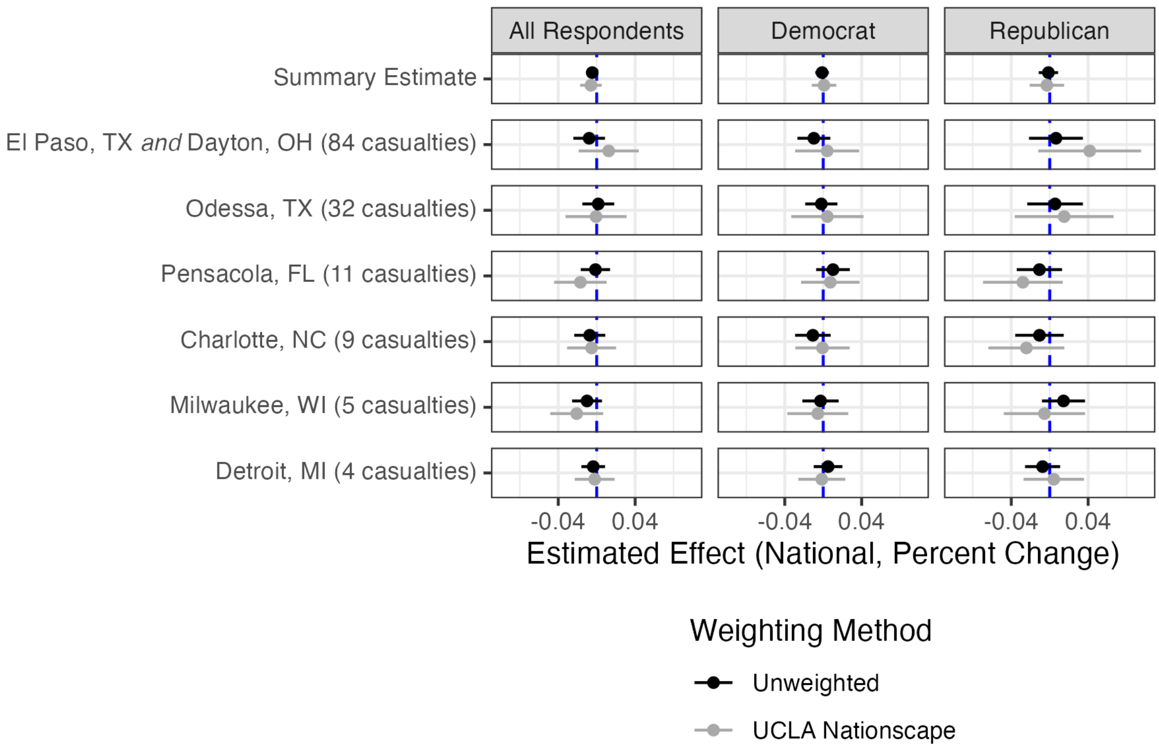
Finally, in Table A3 I estimate whether living in the media market surrounding a given mass shooting affects how strongly people support restrictive gun policies and similarly concludes that most mass shootings during this period did not affect the strength of regional individuals' attitudes, even in the days after the incident. Instead, changes in preference intensity seem to occur similarly across the country and to be driven by the highest profile mass shootings.

4.5 Evaluations of Elected Officials

Are evaluations of elected officials affected by the national increase in preference intensity after the most salient mass shootings, or the change in policy preferences among geographically proximate people? In Figure 8, I estimate whether the back-to-back August 2019 shootings on affected individuals' political evaluations, specifically whether people said they would vote for Biden if the election were held that day. The figure provides consistent evidence that even the most salient mass shootings during this period did not affect who people supported in the presidential race.

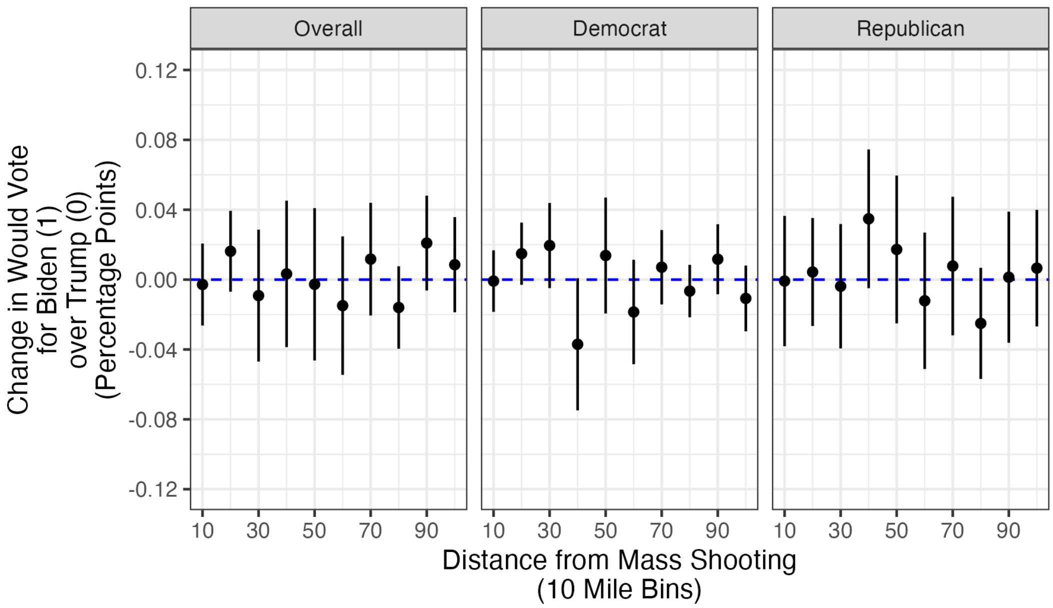
While mass shootings do not affect policy preferences or preference intensity at the local level, mass shootings might plausibly affect candidate evaluations through a mechanism like retrospective voting. In Figure 9 I also evaluate whether changes in support for gun control affected downstream evaluations. I find no evidence that mass shootings affected vote intention in the 2020 presidential election. Similarly, mass shootings do not affect vote intention in the media market where the incident occurs (see Table A4). Altogether, these findings strongly suggest that even when mass shootings affect support for gun control or the intensity of peoples' preferences over restrictive gun policies, these changes do not affect evaluations of elected officials.

Figure 8: The Effect of Temporal Proximity on Candidate Evaluations



Each dot indicates an estimate for the effect of a mass shooting on public opinion on political attitudes the day after the incident. Errorbars indicate 95% confidence intervals. Each estimate is from an Unexpected Event During Survey design with a bandwidth of 16 days and controlling for several individual-level covariates. Mass shootings were included in this analysis if no other incidents occurred in the immediately preceeding days. For each mass shooting, three estimates are presented with each using a different approach to weighting: no weights, the original weights from the UCLA Nationscape project, and reweighted data using entropy balancing weights and 8 day periods. One day is subtracted from the running variable, except for the El Paso/Dayton case in which two days are subtracted. In the label for each mass shooting, a number indicates the number of casualties (i.e. deaths and injuries) from the incident.

Figure 9: The Effect of Geographic Proximity to Mass Shootings on Candidate Evaluations



*** 0.001, ** 0.01, * 0.05, + 0.10. Each estimate presents a stacked difference-in-differences model that compares changes in vote intention in the 20 weeks after the shooting to the 20 weeks before the incident. The model includes fixed effects at the shooting-ZIP Code and shooting-period level. Each 20 week period is reweighted to achieve mean balance with the first 20 weeks after the shooting in the treated area. Independents who "Lean" towards a party are counted as partisans. Interviews from the day of the incident are dropped. Survey data are from the UCLA Nationscape Project. A mass shooting in Detroit, Michigan is excluded, as the survey question measuring vote intention changed during the post-treatment period.

5 Discussion

Mass shootings draw massive amounts of attention from the media and citizens, potentially creating the conditions for shifts in political attitudes. However, despite the ubiquitous nature of mass shootings – or perhaps because they occur regularly – I provide evidence that these high-profile and high-casualty incidents exert no detectable influence over peoples' policy preferences Though these findings are null, they are derived from hundreds of thousands of cases, suggesting that these findings are not driven by a lack of statistical power. Specifically, in analyzing these nearly 500,000 interviews between July 2019 and January 2021, I find no effect of shootings on support for restrictive gun control policies, either among the people living in closest geographic proximity in the weeks after the incident or among people at the national or media market levels in the days after the shooting. This null effect is not driven by polarization of public opinion, in which growing Republican opposition to gun control cancels out increased support from Democrats.

These findings contrast with conclusions from previous research that mass shootings increase support for gun control (Newman and Hartman 2019; Hartman and Newman 2019) or polarize public opinion (Barney and Schaffner 2019; Yamauchi 2020). Moreover, mass shootings do not lead to shifts in support for gun control at the national level, even shortly after the shootings, a finding that resonates with Rogowski and Tucker (2019) and Zhang and Liu (2024). These findings suggest that when it comes to policy preferences it no longer seems that Tobler's First Law of Geography holds: in a polarized America, attitudes appear resistent even to tragic events that happen extremely close to home.

In terms of the intensity of peoples' preferences, I find that individuals' political priorities are also difficult to move. I provide evidence that most mass shootings do not affect the intensity of peoples' policy preferences, at the local, media market, or national levels. I do find, however, that the most egregious incidents may lead to increased news coverage, public attention, and ultimately more intense preferences for gun-related policies. While more evidence is needed, this may suggest that the most salient of real-world events may affect the intensity of preferences for people across the country. This finding, that events can affect the perceived importance of policy issues, might plausibly have had important implications for the 2020 presidential race. While the contemporary U.S. electorate is evenly divided, many people are cross-positioned on policy issues, including gun control (Sides, Tausanovitch, and Vavreck 2022, 73). Changes in the intensity of peoples' preferences might then theoretically cause voters to become truly cross-pressured, leading them to switch their vote to the out-party candidate. However, in this study the increase in preference intensity for gun-related policies did not affect how people intended to vote in the 2020 presidential election, even in the days immediately after the incident.

This study represents just one test of how policy-relevant events affect the political behavior of people. I focus on a particularly salient type of event that occurs in a political moment when the two political parties have clearly differentiated positions on the relevant policy issues. These factors help us to place bounds on the effects of certain types of policy-relevant events on political attitudes. Other types of events may have greater effects on public opinion, including events that bear on less polarized policy issues and that happen with less frequency. However, recent evidence from the COVID-19 pandemic suggests that even the emergence of an alarming new public challenge has minimal effects on attitudes towards existing policies (Sides, Tausanovitch, and Vavreck 2020) and modest effects on support for political candidates (Warshaw, Vavreck, and Baxter-King 2020). Ultimately, I conclude that real-world events are highly constrained in their ability to influence public opinion.

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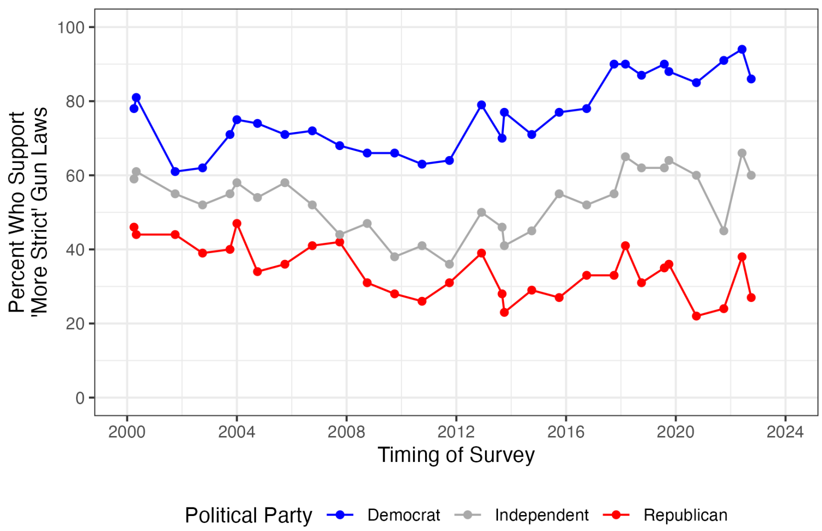
S1 Trends in Support for Gun Control: Gallup

Figure A1: Trends in Support for Gun Control: Each point presents the proportion of respondents in a Gallup survey who identified as a Democrat, Independent, or Republican who supported stricter gun laws. Respondents were asked, 'In general, do you feel that the laws covering the sale of firearms should be made more strict, less strict or kept as they are now?'



S2 Supplementary Gun Policy Scale Information

Table A1: Distribution of Support for Restrictive Gun Policies

Panel A: Distribution of Individual Scale Items						
VARIABLE	ALL RESPONDENTS			PROPORTION AGREE BY PARTY		
	Agree	Disagree	Not Sure	Democrat	Independent	Republican
Background Checks	0.86	0.08	0.07	0.91	0.80	0.84
Ban Assault Rifles	0.56	0.29	0.15	0.74	0.47	0.42
Limit Magazine Sizes	0.49	0.29	0.22	0.65	0.39	0.38
Ban All Guns	0.20	0.67	0.13	0.29	0.16	0.11

Panel B: Distribution of Additive Scale				
Scale Value	Overall	Democrat	Independent	Republican
0	0.10	0.05	0.16	0.12
0.25	0.27	0.15	0.30	0.39
0.5	0.18	0.18	0.18	0.19
0.75	0.31	0.41	0.25	0.24
1	0.14	0.21	0.10	0.07

Table A2: Proportions are calculated using nationally representative weights. Percent agreement by political party is calculated as the percent of each political party that responded ”Agree” to the survey question. Independents who “lean” towards a political party are included with members of that party.

S3 Aggregating Estimates: Inverse-Variance Weighting

When calculating a summary estimate for the national-level and media market-level interrupted time series analyses, I aggregate individual estimates using an inverse-variance weighted average. This pooling procedure is similar to the “stacked” difference-in-differences designs sometimes used in cases of staggered treatments (Baker, Larcker, and Wang 2022; Cengiz et al. 2019). Essentially, estimates with less uncertainty (i.e. more respondents) are allowed to affect the pooled estimate more than estimates with a higher variance (i.e. fewer respondents). The formula for this aggregation is:

$$\bar{\mu} = \frac{\sum_{j=1}^{n} \hat{\mu}_j * \hat{\sigma}_j^{-2}}{\sum_{j=1}^{n} \hat{\sigma}_j^{-2}}$$

where $\hat{\mu}$ is the estimate and $\hat{\sigma}_i$ is the standard error of the estimate. To calculate the 95% confidence interval, I estimate the variance for

$$Var(\bar{\mu}) = \sum_{j=1}^{n} (\sigma_j^2)^{-1}$$

S4 Supplementary Findings

Table A3: The Effect of Mass Shootings on Preference Intensity for Gun Policies in the Surrounding Media Markets

PANEL A: UNWEIGHTED ESTIMATES			
	Overall	Democrat	Republican
Pooled Estimate	-0.01113	0.02715	-0.07690
	(0.04344)	(0.07101)	(0.07178)
Mass Shootings	8	8	8
Bandwidth (Days)	16	16	16
Weights	No	No	No
Geography	DMA	DMA	DMA

PANEL B: WEIGHTED ESTIMATES			
	Overall	Democrat	Republican
Pooled Estimate	-0.01362	0.01368	-0.08216
	(0.04579)	(0.07283)	(0.07246)
Mass Shootings	8	8	8
Bandwidth (Days)	16	16	16
Weights	Yes	Yes	Yes
Geography	DMA	DMA	DMA

Note: The regression models predicting the gun policy scale from days before or after the shooting, interacted with an indicator for after the event, use entropy balancing weights that reweight each 16-day period to have mean balance with the demographics of the week after the mass shooting. Weights are constructed using binned versions of party identification, race/ethnicity, age, gender, education, and income. (Party and race/ethnicity are dropped when estimating models by party.) These models use a bandwidth of 16 days and exclude respondents from the day of the incident. The summary estimate presented in this table is calculated using inverse-variance weighting and the standard error is presented in parentheses below the estimate. The number of media markets equals the number of mass shootings.

Table A4: The Effect of Mass Shootings on Vote Intention in the Surrounding Media Markets

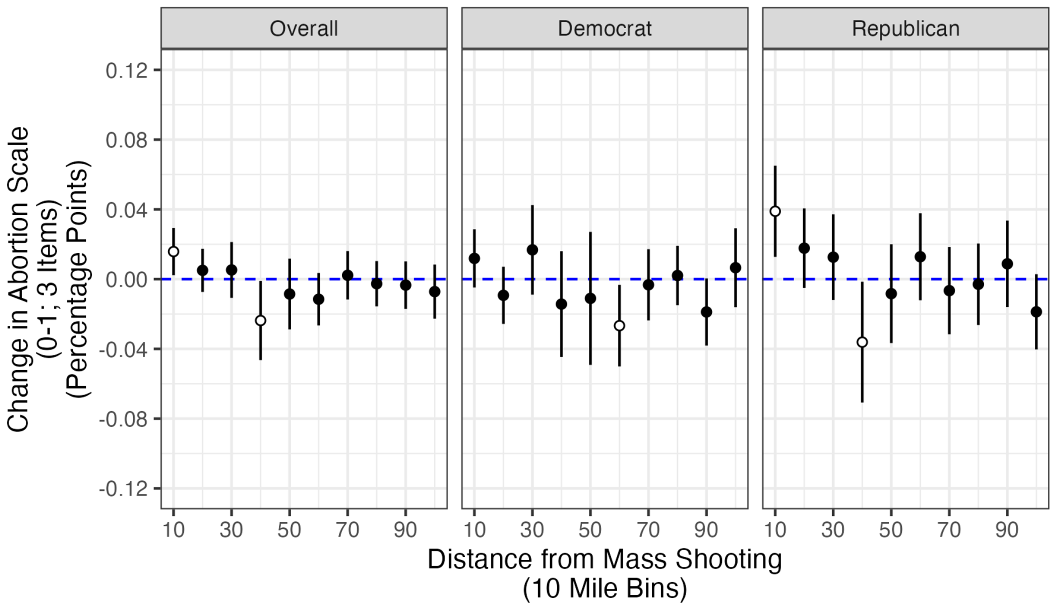
PANEL A: UNWEIGHTED ESTIMATES			
	Overall	Democrat	Republican
Pooled Estimate	-0.00468	-0.00616	0.00935
	(0.03896)	(0.09937)	(0.06112)
Mass Shootings	8	8	8
Bandwidth (Days)	16	16	16
Weights	No	No	No
Geography	DMA	DMA	DMA

PANEL B: WEIGHTED ESTIMATES			
	Overall	Democrat	Republican
Pooled Estimate	-0.01427	-0.01326	-0.00033
	(0.03771)	(0.09715)	(0.06001)
Mass Shootings	8	8	8
Bandwidth (Days)	16	16	16
Weights	Yes	Yes	Yes
Geography	DMA	DMA	DMA

Note: The regression models predicting the gun policy scale from days before or after the shooting, interacted with an indicator for after the event, use entropy balancing weights that reweight each 16-day period to have mean balance with the demographics of the week after the mass shooting. Weights are constructed using binned versions of party identification, race/ethnicity, age, gender, education, and income. (Party and race/ethnicity are dropped when estimating models by party.) These models use a bandwidth of 16 days and exclude respondents from the day of the incident. The summary estimate presented in this table is calculated using inverse-variance weighting and the standard error is presented in parentheses below the estimate. The number of media markets equals the number of mass shootings.

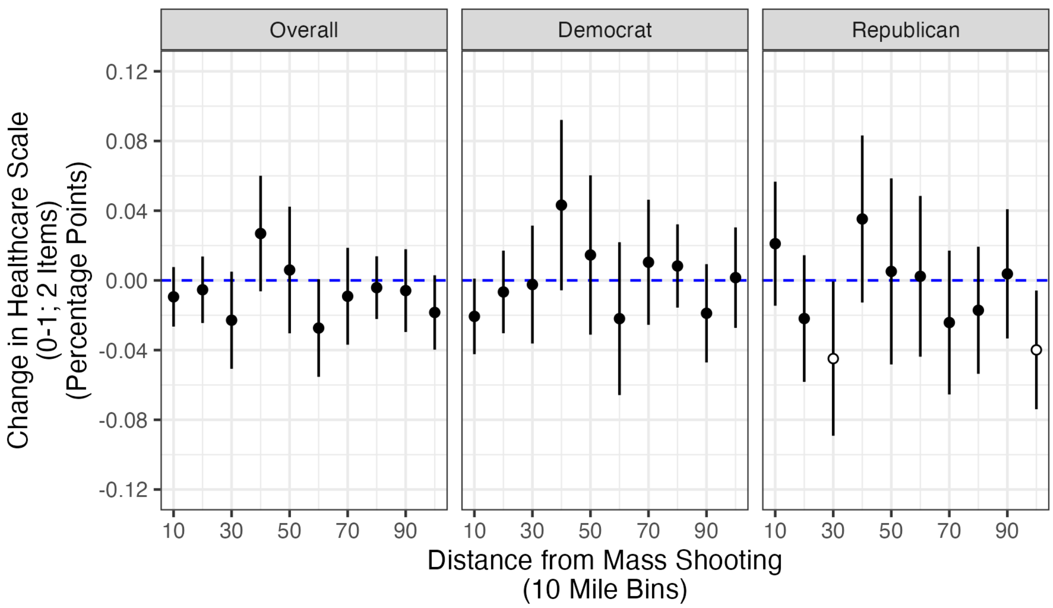
S5 Placebo Outcomes: Policies

Figure A2: The Effect of Geographic Proximity to Mass Shootings on Support for Abortion Policy



*** 0.001, ** 0.01, * 0.05, + 0.10. Each estimate presents a stacked difference-in-differences model that compares changes in the outcome in the 20 weeks after the shooting to the 20 weeks before the incident. The model includes fixed effects at the shooting-ZIP Code and shooting-period level. Each 20 week period is reweighted to achieve mean balance with the first 20 weeks after the shooting in the treated area. Independents who “Lean” towards a party are counted as partisans. Interviews from the day of the incident are dropped. Survey data are from the UCLA Nationscape Project

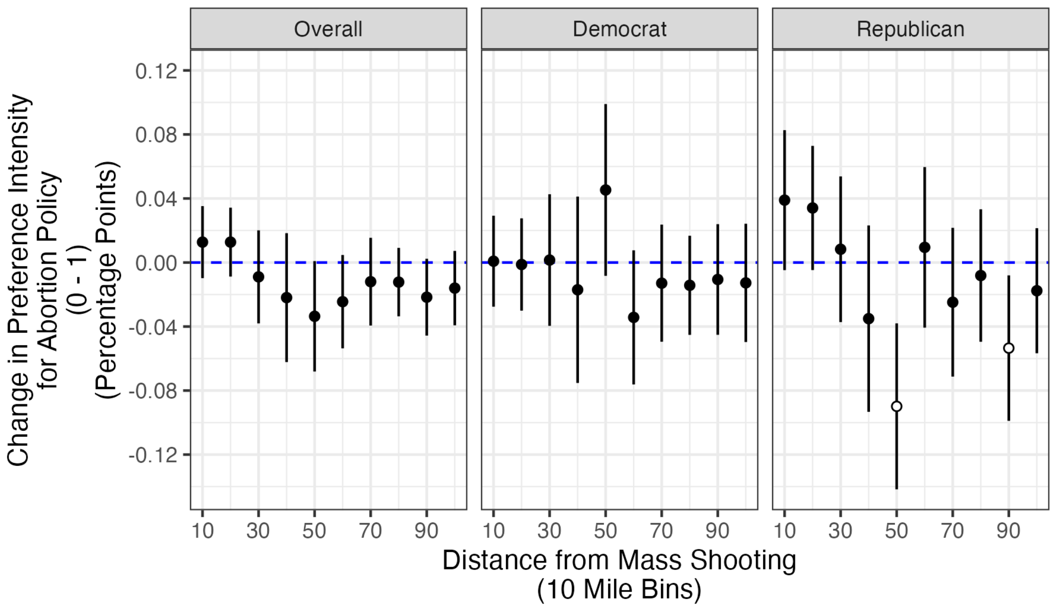
Figure A3: The Effect of Geographic Proximity to Mass Shootings on Support for Healthcare Policy



*** 0.001, ** 0.01, * 0.05, + 0.10. Each estimate presents a stacked difference-in-differences model that compares changes in the outcome in the 20 weeks after the shooting to the 20 weeks before the incident. The model includes fixed effects at the shooting-ZIP Code and shooting-period level. Each 20 week period is reweighted to achieve mean balance with the first 20 weeks after the shooting in the treated area. Independents who "Lean" towards a party are counted as partisans. Interviews from the day of the incident are dropped. Survey data are from the UCLA Nationscape Project

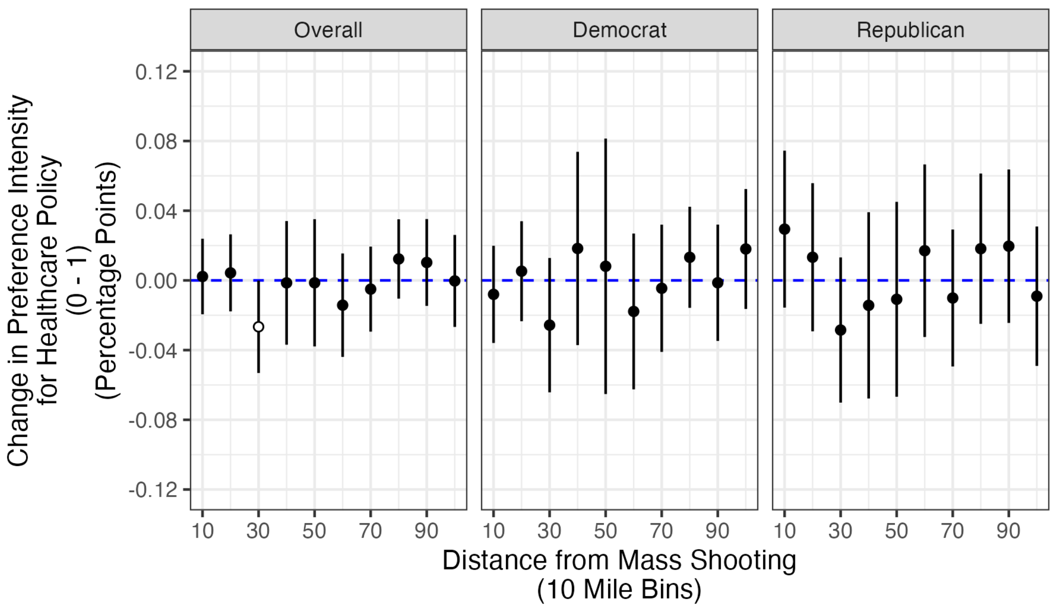
S6 Placebo Outcomes: Preference Intensity

Figure A4: The Effect of Geographic Proximity to Mass Shootings on Preference Intensity for Abortion Policy



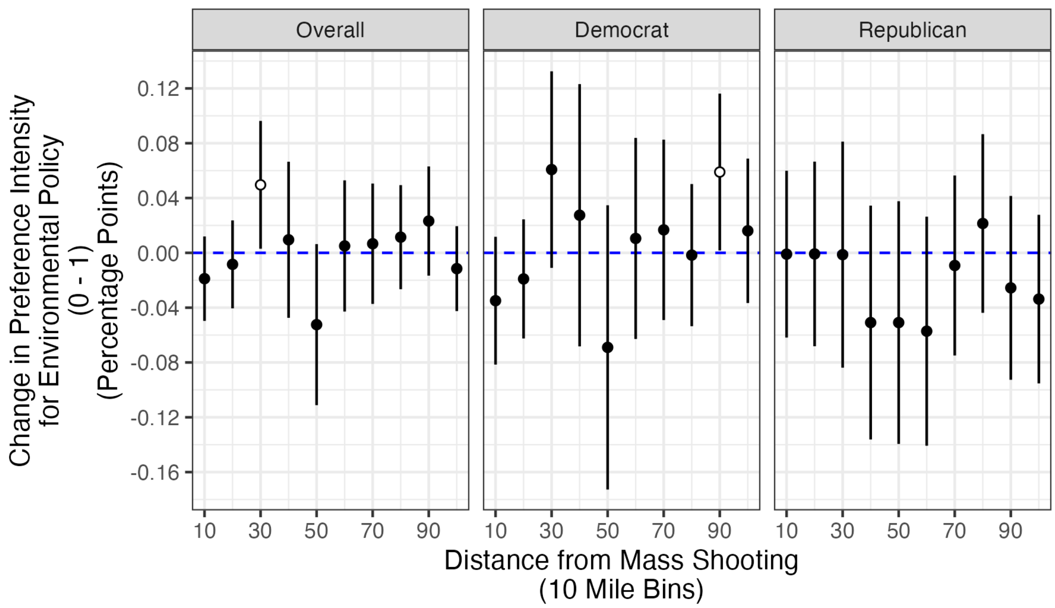
*** 0.001, ** 0.01, * 0.05, + 0.10. Each estimate presents a stacked difference-in-differences model that compares changes in the outcome in the 20 weeks after the shooting to the 20 weeks before the incident. The model includes fixed effects at the shooting-ZIP Code and shooting-period level. Each 20 week period is reweighted to achieve mean balance with the first 20 weeks after the shooting in the treated area. Independents who "Lean" towards a party are counted as partisans. Interviews from the day of the incident are dropped. Survey data are from the UCLA Nationscape Project

Figure A5: The Effect of Geographic Proximity to Mass Shootings on Preference Intensity for Healthcare Policy



*** 0.001, ** 0.01, * 0.05, + 0.10. Each estimate presents a stacked difference-in-differences model that compares changes in the outcome in the 20 weeks after the shooting to the 20 weeks before the incident. The model includes fixed effects at the shooting-ZIP Code and shooting-period level. Each 20 week period is reweighted to achieve mean balance with the first 20 weeks after the shooting in the treated area. Independents who "Lean" towards a party are counted as partisans. Interviews from the day of the incident are dropped. Survey data are from the UCLA Nationscape Project

Figure A6: The Effect of Geographic Proximity to Mass Shootings on Support for Environmental Policy



*** 0.001, ** 0.01, * 0.05, + 0.10. Each estimate presents a stacked difference-in-differences model that compares changes in the outcome in the 20 weeks after the shooting to the 20 weeks before the incident. The model includes fixed effects at the shooting-ZIP Code and shooting-period level. Each 20 week period is reweighted to achieve mean balance with the first 20 weeks after the shooting in the treated area. Independents who "Lean" towards a party are counted as partisans. Interviews from the day of the incident are dropped. Survey data are from the UCLA Nationscape Project

S7 Locations of Previous Mass Shootings

Figure A7: Map of Mass Shootings Between 2018 - 2020: Geography boundaries indicate media markets.