Online Appendix For:

Mobilize for Our Lives? School Shootings and Retrospective Voting in U.S. Elections

[Not to be included in printed versions]

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1. Supplemental Background and Conceptual Framework

1a. Gun Violence and Public Opinion

Many have argued that the potential for gun violence to change attitudes and spur citizen action around gun control comes largely from its ability to shape the news agenda and bring attention to the topic (Iyengar and Kinder 1987; King, Schneer, and White 2017; McCombs and Shaw 1972). When the frame of the issue is consistent it can influence both the public opinion and the actions of citizens who consume that news coverage (Abrajano, Hajnal, and Hassell 2017; DellaVigna and Kaplan 2007; Gerber, Karlan, and Bergan 2009; Iyengar and Kinder 1987; Martin and Yurukoglu 2017). Moreover, many mass shootings generate significant news coverage (Duwe 2000; Nass 2018).

Coverage alone, however, may not be enough for an attitudinal or behavioral shift. For news coverage to be effective in changing attitudes and behaviors it may need to be consistent in its framing (Abrajano et al. 2017), as competing frames tend to cancel each other out (Chong and Druckman 2007a, 2007c; Sniderman and Theriault 2004). With gun violence in the United States, public discussion often does not swing wildly in favor of gun control, as both sides of the issue emphasizing distinct policy solutions (Bump 2018; Singman 2018). There is some evidence that many mass shootings spur discussions both on the right and on the left about guns and gun control (Gunn et al. 2018; Hughes 2018).

Perhaps because of these competing forces, studies of the effects of mass shootings on public opinion have been somewhat mixed in their findings. While some analyses have shown a link between mass shootings in a local community and public support for gun control (e.g. Gunn et al. 2018; Newman and Hartman 2017, 2019, but see Rogowski and Tucker 2019), there is some disagreement about whether this effect is unifying or polarizing (Newman and Hartman 2017, 2019; Barney and Schaffner 2019).[1,2]

What is untested, however, is whether gun violence impacts behavior in the electoral domain. We might expect traumatic events like mass shootings to shape engagement rates and how citizens vote (especially the latter if attitudinal changes occur). Individuals who are on the margins of being actively engaged in politics might be spurred to do so in response to a salient tragedy like a shooting. It's important to note that this effect *does not* require long-term exposure or a long issue attention cycle—it may occur even if any attitudinal shifts or increases in citizen interests are transient. To elicit an effect on voter registration, and subsequent voter turnout and voter choice downstream, individuals need only to be mobilized in the short term by a senseless shooting and then fill out the requisite registration forms. This effect may help individuals overcome a key barrier to voting and, as a result, may help set individuals on a path towards becoming voters in the future. Depending on who is mobilized or changes their opinions on gun control in response to a shooting, we may also see shifts in election outcomes in the aftermath of shootings.

[1] This disagreement comes down to how to code school shootings. Those interested in delving into this debate should thoroughly read Newman and Hartman (2017, 2019) and Barney and Schaffner (2019). A further citation in this realm is Rogowski and Tucker (2019), who explore the effect of the Sandy Hook shooting on public opinion towards gun control. They find "no evidence that Americans granted greater support for gun control after the Sandy Hook shooting" (903) and "no evidence of attitude polarization as a result of Sandy Hook" (904). The articles that have shown an effect have tended to include multiple shootings in their sample. Our null electoral results are more consistent with a polarizing attitudinal shift or a null attitudinal shift than a uniform shift in one political direction. (Though note that any attitudinal shifts may be independent from engagement levels overall.)

[2] While we do not have any direct evidence of this fact, there are theoretical reasons to suspect that shootings' effect on public opinion—regardless of whether they are polarizing or indicative of increased support overall—may be fleeting. We know, of course, that the focus of the news media, and the subsequent focus of Americans quickly moves on to other topics (Downs 1996; Nass 2018). We also know that the effects of framing tend to be short lived. Subjects involved in framing experiments often show large effects after exposure to a particular frame, but those effects tend to disappear quickly and may not show any effects even as little as a day later (Chong and Druckman 2007b). Because of the relationship between public opinion and the news agenda (McCombs and Shaw 1972; Zaller 1992; Zaller and Feldman 1992), and the rapidly-declining media coverage of most major incidents, the ability of significant events, such as mass shootings, to have a lasting impact on public opinion may be somewhat constrained.

On the other hand, however, there are also good reasons to believe that even if we observe shifts in public opinion as the results of mass shootings, these opinions may not translate into action that have any impact on voter participation or election outcomes. There is widespread recognition that political behavior is different from political opinions—individuals can (and do) often express their interest in politics, while failing to engage in politics (Holbein and Hillygus 2020). Individuals are often willing to lie or decline to respond when they know their views are not perceived as socially acceptable (Berinsky 1999, 2004; Schuman and Presser 1980; Vogel and Ardoin 2008).[3] Opinions are often influenced by social norms and social expectations (Berinsky 2004; Hopkins 2009; LaPiere 1934; Schuman and Presser 1980; Vogel and Ardoin 2008), and many times do not align with actual actions which may be subject to other ideological and situational pressures (Butler and Hassell 2018; Han 2008; Levine 2015).

Given the focus of the previous literature on attitudes, our paper investigates the direct impact of exposure to mass shootings on citizen electoral behaviors. While much a growing body of research has explored the attitudinal effects (or non-effects) of mass shootings, to our knowledge there are no systematic studies that explore the potential effects on actual validated political behaviors.

[3] Tragic events may create situations where social norms activate social desirability; indeed, individuals are often willing to lie or decline to respond when they know their views are not perceived as socially acceptable and this may be especially the case in the wake of highly salient tragedies, like school shootings.

1b. Why Voter Registration is One of the Outcomes We Explore

There are theoretical, practical, and policy reasons to look at voter registration as one of our behavioral outcomes. We examine voter registration for four reasons. First, because it is the behavioral hurdle that citizens seeking to make their voices heard to policymakers (at various levels of government) need to clear before they can vote to change the status-quo. Second, registration is also an important outcome in the realm of gun violence given that activism in this area has focused intensely on registering individuals to vote (Daugherty 2018). Third, some rudimentary analyses of voter registration patterns conducted by journalists after the Parkland shooting suggest that these efforts may have borne fruit—resulting in large gains in voter registration (Daugherty 2018). However, these analyses are relatively simplistic in their design—leaving open, for example, that any differences in rates of voter registration are driven by other factors (e.g. seasonality in registration patterns) — and not illuminating whether changes in registration are a general phenomenon or localized to select shootings. Fourth, examining patterns of voter registration allow us to leverage the (exogenous) precise timing of school shootings to estimate strong regression discontinuity (in time) models. All of these reasons suggest that using registration as an, but not the only, outcome is vitally important to understanding the (non)effects of school shootings on American elections. To be clear, however, we are not arguing there will definitely be effects. It's possible that the effects on registration are null. But there is enough theoretical reason to look at registration.

To be clear, in analyzing the effects of school shootings on voter registration first in the manuscript, we are *not* assuming that this would be the first psychological response by voters. We are testing whether voters have any response in the electoral realm (i.e. registration, turnout, vote share, incumbent reelection rates, party composition of future governments). But it need not be the case that a (potential) voter "shocked" by a school shooting would first go to voter registration. If a school shooting were to mobilize a new voter (i.e. one who is not registered), however, they would have to register at some point. Or, if it were to mobilize a registered voter who hadn't voted in a while, they would have to vote (and we would see this in an uptick in turnout). More generally, we are not assuming that other non-electoral behaviors don’t happen after a shooting. We are simply testing whether there is an effect through elections as some literature might suggest there should be.

1c. Policy Responses to Gun Violence

Although Congress has not taken significant action in response to mass shootings, there is some evidence that mass shootings act as a focusing event to raise gun policy on the political agenda as the amount of gun legislation considered and passed at the state level has increased in recent years (Luca et al. 2020). However, that action is not always consistently in the direction of more gun control. For example, legislation passed in the Florida legislature following Parkland implemented a number of gun restrictions, but also expanded gun rights on school property by passing a program whereby school employees could be trained and carry guns on school campuses (Lord 2018). The lack of action at the federal level in the United States is (perhaps) surprising given the substantive changes in public opinion towards favoring more gun control that have occurred in recent years (Gallup 2019).

This is even more surprising given the array of potential solutions at the disposal of policymakers (at various levels) that could help to mitigate school shootings. Gun control measures, background checks, weapons bans, weapons buyback programs, mental health services/screening, additional security, and active shooter drills are just some of the few potential policies that policymakers could implement to address gun violence. This is not to say that all of these approaches are likely to be equally effective; indeed, research on this topic has shown that some policies show more promise than others (e.g. Cook and Ludwig 2000). It is to say, however, that policymakers can be reasonably connected to the outcomes related to mass violence.

1d. Natural Disasters and Retrospective Voting

Table A1 summarizes the literature on 16 different natural disaster types across 59 studies on the effects of natural disasters on elections.[4] Table A1 shows that a dominant majority of studies on natural disasters focus on instances where these events "successfully" sparked some form of retrospective voting.[5] Though there are some studies that question findings such as shark attacks and sporting team losses (e.g. Fowler and Montagnes 2015; Fowler and Hall 2018), these appear to be outliers rather than reflective of a general pattern in this literature of focusing on both successful and unsuccessful instances of retrospective voting and drawing lessons from what conditions lead to either. The overwhelming majority of disaster studies find some kind of retrospective voting effect. In fact, it is *very* rare to find natural disaster studies that find null effects.

As we argue in the paper, this is unfortunate as it restricts our understanding of the underlying conditions that promote retrospective responses. We argue that in order to push this literature forward—perhaps in meta-analyses or other literature aggregation techniques—it would be helpful to have both successful and unsuccessful cases when trying to determine what inputs best predict retrospective responses. If the only cases we explore are those where retrospective voting is "successful" we constrain the amount of variation we can explain. After all, predicting non-existent variation is not possible. Our paper adds to this literature in important ways by showing that some of the most common characteristics thought to promote active retrospective voting (i.e. voter attention, issue salience, media coverage, governmental control in the issue space, etc.) are not always sufficient to do so. This suggests a rethinking of the factors necessary for retrospective responses.

[4] As Ang et al. (2020) note, disasters or crises are usually defined as "shocks to the normal state of affairs, sharing three elements: threat, urgency, and uncertainty."

[5] We use the term "success" here to simply mean that the disaster affected citizens' attitudes and/or behaviors in a way that had downstream consequences for elections.

One should also consider the political climate (e.g. interest groups) and lessons from opinion change (e.g. counter-frames) in deciding whether enough retrospective voting is likely to occur.

Table A1: Previous Natural Disaster Studies Focus on Successful Instances of Retrospective Voting

[1.] Disaster Type	**[2.] Studies that examine the relationship between disasters and retrospective responses**	**[3.] Did studies in column 2 find that disaster encouraged retrospective voting?**	**[4.] Studies that argued there was no retrospective response to the disaster**	**[5.] Reply from authors who argued there was a retrospective response**
Tornadoes	Healy and Malhotra 2010	Yes		
Floods	Arceneaux and Stein 2006; Bechtel and Hainmueller 2011; Cole et al. 2012; Fair et al. 2017; Gasper and Reeves 2011; Healy and Malhotra 2009; Heersink et al. 2017; Stout 2018; Mazepus and van Leeuwen 2019	Yes		
Hurricanes/wind storms	Atkeson and Maestas 2012; Malhotra and Kuo 2008; Eriksson 2016; Malhotra 2008; Velez and Martin 2013; Malhotra and Kuo 2009; Gomez and Wilson 2008; Pahontu 2020; White et al. 2007; Chen 2013; Sinclair et al. 2011; Hopkins 2012	Yes		
Terrorist Attacks	Getmansky and Zeitzoff 2014; Gould and Klor 2010; Hersh 2013; Montalvo 2011; Kibris 2011	Yes		
Climate Change	Obradovich 2017	Yes		
Famines/ Droughts	Achen and Bartels 2004, 2017; Barnhart 1925; Cavalcanti 2018; Chen and Yang 2015	Yes		
Wildfires	Gradin 2020; Lazarev et al. 2014; Ramos and Sanz 2020; Hazlett and Mildenberger 2019	Yes		
Earthquakes	Arase 2012; Carlin et al. 2014; Eckstein 2014; Katz and Levin 2016; Uslaner 2016	Yes		
Shark Attacks	Achen and Bartels 2004, 2017	Yes	Fowler and Hall 2018	Achen and Bartels 2018
Poor Sporting Outcomes	Healy et al. 2010; Busby et al. 2017	Yes	Fowler and Montagnes 2015	Healy et al. 2015
Spikes in Crime	Arnold and Carnes 2012; Bateson 2012	Yes		

Failing Schools	Berry and Howell 2006; Holbein 2016; Holbein and Hassell 2019; Kogan et al. 2016; Chingos et al. 2012;	Yes		
War Casualties	Mueller 1973; Grose and Oppenheimer 2007; Karol and Miguel 2007; Kriner and Shen 2007	Yes		
Oil Price Shocks	Ramsay 2011; Wolfers 2002	Yes		
Economic Crises	Fiorina 1978, 1981; Healy and Lenz 2014, 2017	Yes		
Pandemics	Achen and Bartels 2004, 2017; Leininger and Schaub 2020	Yes		

2. Measuring School Shootings

Measuring gun violence in the United States is no easy task. This is, in part, because of its prevalence. That being said, the Federal Government—and other non-government actors—does much to track mass shootings. As a recent Rand Corporation report[6] noted,

> In the 1980s, the Federal Bureau of Investigation (FBI) defined mass murderer as someone who "kills four or more people in a single incident (not including himself), typically in a single location" (Krouse and Richardson, 2015). However, the government has never defined mass shooting as a separate category, and there is not yet a universally accepted definition of the term. Thus, media outlets, academic researchers, and law enforcement agencies frequently use different definitions when discussing mass shootings, which can complicate our understanding of mass shooting trends and their relationship to gun policy.

One of the advantages of focusing on school shootings is that, given that these events are widely covered by the media and (in recent years) online (e.g. Burns and Crawford 1999; Chyi and McCombs 2004; Hawdon et al. 2012; Maguire et al. 2002; Muschert and Carr 2006; Park et al. 2012), the chances of these tragedies being documented is much higher than day-to-day gun violence (e.g. domestic disputes, single-victim injuries/murders, etc.).

Two of the most commonly-used sources for gun violence within schools in the United States are the Stanford Mass Shootings of America (MSA) data project and Wikipedia, which (as in all its pages) has crowd-sourced (while quality-controlling) a list of these tragedies. These data sources are widely used in studies of the (non-electoral) effects of shootings (e.g. Barney and Schaffner 2019; Kwon and Cabrera 2019a, 2019b; Luca et al. 2020; Newman and Hartman 2017, 2019; Zhang et al. 2017). Importantly, the MSA only tracks mass school shootings, which it defines as "3 or more shooting victims (not necessarily fatalities), not including the shooter." Wikipedia's "List of school shootings in the United States" page has these mass school shootings and others that have fewer than 3 victims and, as such, is much more thorough than the MSA dataset. Among Wikipedia's list, the two have a high degree of overlap. Indeed, from 2000 through (mid) 2016—when the MSA dataset ends—the Wikipedia page contains all but one of

[6] See "Mass Shootings: Definitions and Trends" *Rand Corporation Report*, Gun Policy in America, March 2, 2018

the school shootings (i.e. 26/27 or 93%) listed in the MSA's archive of mass school shootings.[7] Of those observations that make it into our end sample, 100% of the shootings listed in the MSA are in the Wikipedia files.[8] Conversely, the Wikipedia list also includes an additional 30 shootings that fit the MSA's definition, but are, in fact, not included in the MSA list. These additional shootings are widely sourced and (where sourcing is sometimes lacking) a simple Google search reveals that Wikipedia is not simply picking up on non-existent school shootings. These patterns should lend some pause to scholars using the MSA alone to study the effect of school shootings. It further suggests that the Wikipedia list is much more thorough than the MSA when it comes to school shootings. While it's hard to know whether both lists are missing school shootings, we can be reasonably confident (given the Federal Government's efforts) that we are not systematically missing mass shootings. While it is possible that no dataset on its own (or combined across different sources) contains all school shootings, our intent is to be as comprehensive as possible.

In the paper, we do not automatically throw away shootings that do not qualify as a mass shooting. It is true that in some specifications, we restrict our analyses to mass shootings to test for robustness. As this subset of school shootings is even better documented that the (already highly covered) subset of mass shootings that occur in schools, the fact that our results do not change should provide assurance to those who are concerned that we are missing school shootings. However, it is also our goal to be thorough in not focusing only on mass shootings alone. We follow the approach taken by Wikipedia

[7] The exception is a shooting that occurred on April 10, 2017 at Springwater Trail High School in which 10 individuals were injured and 0 died. In this comparison, we exclude shootings that were nominally connected to schools (e.g. off-campus fraternity parties) or that were clearly domestic disputes involving students at a given school.

[8] Recall that our main specifications treatment status is coded based on having a shooting in the year of an election. Odd-year shootings (like that which occurred at the Springwater Trail High School in 2007) are excluded from the end sample so they do not end up influencing our results. We code treatment this way because including non-election year shootings means that we are including shootings that occurred between 11 (i.e. in December of the previous year) and ≈ 24 months (i.e. in November of the previous election cycle, after Election Day) prior to the Election. To us, this seems like an unreasonably long amount of time to expect retrospective effects to occur. However, even if we include these shootings in our treatment group (paired with the next election) and included the missing shooting at Springwater Trail High School, our conclusions are unchanged (see Figures A15 and A16 below).

in documenting not just an arbitrarily-defined threshold for a mass event. Including non-mass shootings allows us to *test*, rather than simply *assume*, whether these tragedies' impacts vary by the number of injuries/fatalities.

Table A1 provides the list of school shootings as well as some additional information about where and when the shooting occurred and how many deaths and injuries there were. See Figure 1 in the paper for a map of school shootings over the past two decades.

Table A2: School Shootings, 2006-2015

Date	Location	Deaths	Injuries	Shooting Number
13-Jan-06	Longwood, Florida	0	1	1
23-Feb-06	Roseburg, Oregon	0	1	2
14-Mar-06	Reno, Nevada	0	2	3
24-Aug-06	Essex Junction, Vermont	2	3	4
30-Aug-06	Hillsborough, North Carolina	0	2	5
2-Sep-06	Shepherdstown, West Virginia	3	0	6
17-Sep-06	Pittsburgh, Pennsylvania	0	5	7
27-Sep-06	Bailey, Colorado	2	0	8
29-Sep-06	Cazenovia, Wisconsin	1	0	9
2-Oct-06	Nickel Mines, Pennsylvania	6	3	10
9-Oct-06	Joplin, Missouri	0	0	11
3-Jan-07	Tacoma, Washington	1	0	12
7-Mar-07	Compton, California	0	1	13
16-Apr-07	Blacksburg, Virginia	33	23	14
10-Oct-07	Cleveland, Ohio	1	4	15
6-Nov-07	Miami Gardens, Florida	0	1	16
4-Feb-08	Memphis, Tennessee	0	1	17
8-Feb-08	Baton Rouge, Louisiana	3	0	18
11-Feb-08	Memphis, Tennessee	0	1	19
12-Feb-08	Oxnard, California	1	0	20
14-Feb-08	DeKalb, Illinois	6	21	21
14-Aug-08	Federal Way, Washington	1	0	22
21-Aug-08	Knoxville, Tennessee	1	0	23
2-Sep-08	Willoughby, Ohio	0	0	24
16-Oct-08	Detroit, Michigan	1	3	25
26-Oct-08	Conway, Arkansas	2	1	26
12-Nov-08	Fort Lauderdale, Florida	1	0	27
9-Jan-09	Chicago, Illinois	0	5	28
26-Apr-09	Hampton, Virginia	0	3	29
18-May-09	Cambridge, Massachusetts	1	0	30
18-May-09	Larose, Louisiana	1	0	31
16-Jun-09	San Francisco, California	0	3	32
3-Sep-09	San Bruno, California	0	1	33
16-Oct-09	Conway, South Carolina	1	0	34
5-Feb-10	Madison, Alabama	1	0	35
12-Feb-10	Huntsville, Alabama	3	3	36
19-Feb-10	DeKalb, Illinois	0	1	37
23-Feb-10	Littleton, Colorado	0	2	38
9-Mar-10	Columbus, Ohio	1	1	39
8-Sep-10	Detroit, Michigan	0	2	40
28-Sep-10	Austin, Texas	1	1	41

1-Oct-10	Salinas, California	1	0	42
8-Oct-10	Carlsbad, California	0	2	43
29-Nov-10	Marinette, Wisconsin	1	0	44
6-Dec-10	Aurora, Colorado	0	1	45
5-Jan-11	Omaha, Nebraska	2	2	46
25-Mar-11	Martinsville, Indiana	0	1	47
31-Mar-11	Houston, Texas	1	5	48
23-May-11	Pearl City, Hawaii	0	1	49
24-Oct-11	Fayetteville, North Carolina	0	1	50
8-Dec-11	Blacksburg, Virginia	2	0	51
9-Dec-11	Edinburg, Texas	0	2	52
10-Jan-12	Houston, Texas	0	1	53
27-Feb-12	Chardon, Ohio	3	3	54
6-Mar-12	Jacksonville, Florida	2	0	55
2-Apr-12	Oakland, California	7	3	56
16-Aug-12	Memphis, Tennessee	0	2	57
27-Aug-12	Perry Hall, Maryland	0	1	58
7-Sep-12	Normal, Illinois	0	0	59
26-Sep-12	Stillwater, Oklahoma	1	0	60
6-Oct-12	Mobile, Alabama	1	0	61
19-Oct-12	Chicago, Illinois	1	0	62
31-Oct-12	Los Angeles, California	0	4	63
14-Dec-12	Newtown, Connecticut	28	2	64
7-Jan-13	Fort Myers, Florida	1	0	65
10-Jan-13	Taft, California	0	2	66
12-Jan-13	Detroit, Michigan	0	1	67
15-Jan-13	St. Louis, Missouri	0	2	68
15-Jan-13	Hazard, Kentucky	3	0	69
16-Jan-13	Chicago, Illinois	1	0	70
22-Jan-13	Houston, Texas	0	3	71
31-Jan-13	Phoenix, Arizona	0	0	72
31-Jan-13	Atlanta, Georgia	0	2	73
18-Mar-13	Orlando, Florida	1	0	74
12-Apr-13	Christiansburg, Virginia	0	2	75
16-Apr-13	Grambling, Louisiana	0	3	76
18-Apr-13	Cambridge, Massachusetts	1	0	77
14-May-13	Birmingham, Alabama	0	0	78
7-Jun-13	Santa Monica, California	6	4	79
20-Aug-13	Decatur, Georgia	0	0	80
23-Aug-13	Sardis, Mississippi	1	2	81
30-Aug-13	Winston-Salem, North Carolina	0	1	82
4-Oct-13	Pine Hills, Florida	0	2	83
21-Oct-13	Sparks, Nevada	2	2	84

2-Nov-13	Greensboro, North Carolina	0	1	85
3-Nov-13	Lithonia, Georgia	0	2	86
13-Nov-13	Pittsburgh, Pennsylvania	0	3	87
4-Dec-13	Winter Garden, Florida	0	1	88
13-Dec-13	Centennial, Colorado	2	0	89
19-Dec-13	Fresno, California	0	1	90
9-Jan-14	Jackson, Tennessee	0	1	91
13-Jan-14	New Haven, Connecticut	0	1	92
14-Jan-14	Roswell, New Mexico	0	3	93
17-Jan-14	Philadelphia, Pennsylvania	0	2	94
20-Jan-14	Chester, Pennsylvania	0	1	95
21-Jan-14	West Lafayette, Indiana	1	0	96
24-Jan-14	Orangeburg, South Carolina	1	0	97
25-Jan-14	Los Angeles, California	1	0	98
27-Jan-14	Carbondale, Illinois	0	1	99
28-Jan-14	Nashville, Tennessee	0	1	100
30-Jan-14	Palm Bay, Florida	0	1	101
31-Jan-14	Des Moines, Iowa	0	1	102
10-Feb-14	Salisbury, North Carolina	0	1	103
10-Feb-14	Lyndhurst, Ohio	0	0	104
12-Feb-14	Los Angeles, California	0	1	105
22-Feb-14	Augusta, Georgia	0	1	106
12-Mar-14	Miami, Florida	1	0	107
25-Mar-14	College Park, Georgia	0	0	108
9-Apr-14	Greenville, North Carolina	0	0	109
11-Apr-14	Detroit, Michigan	1	0	110
4-May-14	Augusta, Georgia	0	1	111
5-May-14	Augusta, Georgia	0	1	112
8-May-14	Lawrenceville, Georgia	0	1	113
14-May-14	Richmond, California	0	1	114
5-Jun-14	Seattle, Washington	1	3	115
10-Jun-14	Troutdale, Oregon	2	1	116
9-Sep-14	Miami, Florida	0	1	117
11-Sep-14	Taylorsville, Utah	0	1	118
27-Sep-14	Terre Haute, Indiana	0	1	119
30-Sep-14	Albemarle, North Carolina	0	1	120
30-Sep-14	Louisville, Kentucky	0	1	121
3-Oct-14	Fairburn, Georgia	1	0	122
24-Oct-14	Marysville, Washington	5	1	123
20-Nov-14	Tallahassee, Florida	1	3	124
20-Nov-14	Miami, Florida	1	1	125
5-Dec-14	Claremore, Oklahoma	1	0	126
12-Dec-14	Portland, Oregon	0	4	127

15-Jan-15	Milwaukee, Wisconsin	0	3	128
16-Jan-15	Ocala, Florida	0	2	129
4-Feb-15	Frederick, Maryland	0	2	130
14-Feb-15	Merced, California	1	0	131
23-Feb-15	Daytona Beach, Florida	0	3	132
30-Mar-15	University City, Missouri	0	1	133
13-Apr-15	Goldsboro, North Carolina	1	0	134
16-Apr-15	Paradis, Louisiana	0	1	135
27-Apr-15	Lacey, Washington	0	0	136
24-May-15	Flint, Michigan	0	7	137
27-Aug-15	Savannah, Georgia	1	0	138
3-Sep-15	Sacramento, California	1	2	139
14-Sep-15	Cleveland, Mississippi	2	0	140
30-Sep-15	Harrisburg, South Dakota	0	1	141
1-Oct-15	Roseburg, Oregon	10	9	142
9-Oct-15	Flagstaff, Arizona	1	3	143
9-Oct-15	Houston, Texas	1	1	144
22-Oct-15	Nashville, Tennessee	1	3	145
1-Nov-15	Winston-Salem, North Carolina	1	1	146
20-Nov-15	North Las Vegas, Nevada	1	0	147

3. Supplementary Results: Google Search Data

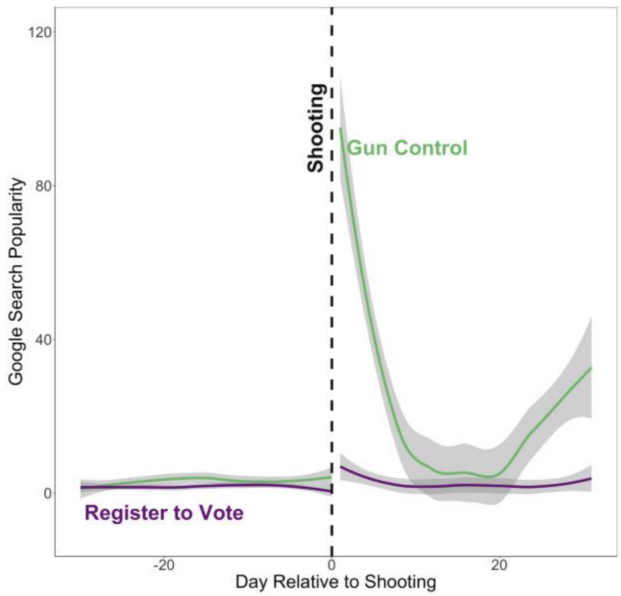
To measure whether and how citizens' interest in voter registration change in response to a school shooting, we use data from Google searches (e.g., Bail 2012; Choi and Varian 2012; Stephens-Davidowitz 2014; Street et al. 2015).[9] This allows us to get a measure of whether shootings spark changes in citizen interests in the immediate aftermath of a shooting and to explore how long any shifts in these interests lasts, thus getting a sense as to whether school shootings act as a focusing event (Kingdon 1984). These patterns have been shown to benchmark well with data from administrative data sources (e.g. in the health domain; see Pelat et al. 2009). As such, this information provides us with daily behavioral measures of citizens' interest in getting information about registering to vote.[10] Figure A1 shows the regression discontinuity in time results of school shootings on searches for gun control. It isolates the results down to the states where the shootings occurred; however, the results presented below also hold nationally. As can be seen, there is a spike in interest in gun control, but not corresponding interest in registering to vote (a fact that is consistent with what we find in the paper).

[9] According to Google, their Trends data are normalized to the time and location of a query. Simultaneously, Google only provides daily data on search patterns for narrow windows (less than a year). As a result, we adjust Trends data relative to the first day in our time series.

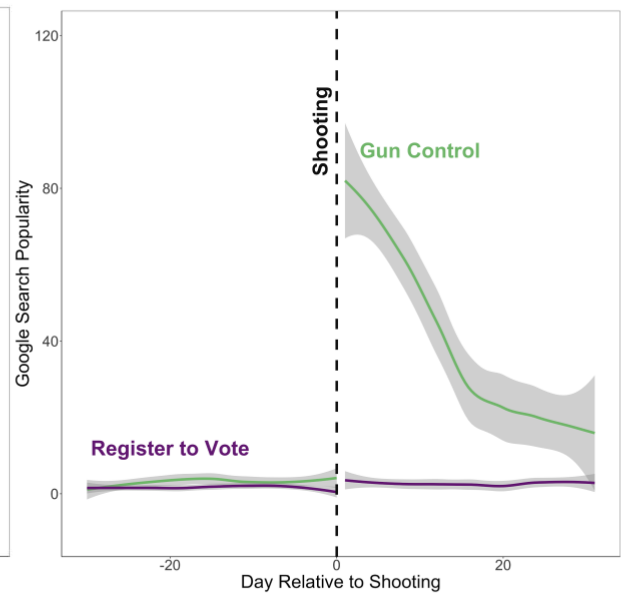
[10] It's hard to know exactly the most common ways that citizens find information about registering to vote. Outside of the internet, opportunities to register to vote sometimes arise in school or work settings or when one interacts with government agencies. Still, given the dominance of the internet as a means of acquiring information in contemporary society—and Google's stranglehold on more than 90% of the market share for online searches—we think our approach is justified as a means of exploring how citizens gather information about registering to vote.

Figure A1: Effect of School Shootings on Google Search Patterns

A.) Effect of Sandy Hook Shooting (2012) on Search Patterns in Connecticut



B.) Effect of Parkland Shooting (2018) on Search Patterns in Florida

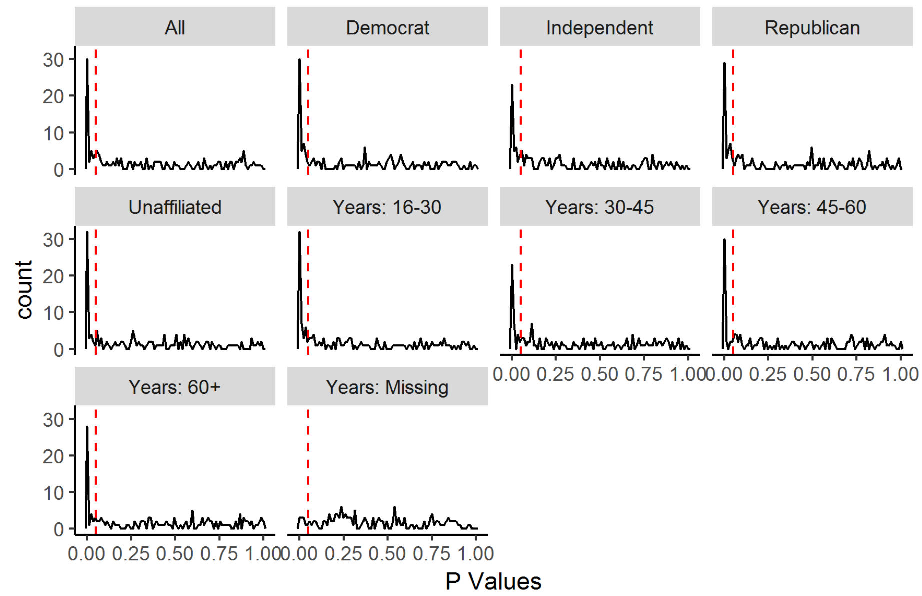


Effect of school shootings on Google Searches for “gun control” and “register to vote” in the same state as the shooting. Lines are from a lowess model, with corresponding 95% confidence intervals shown. In panels C and D, day 0 is the day of the school shooting. Here are the corresponding effect sizes from a regression discontinuity model. Sandy Hook’s effect on searches for “gun control”: 1.24σ (p=0.000). Sandy Hook’s effect on searches for “register to vote”: 0.59σ (p=0.256). Parkland’s effect on searches for “gun control”: 2.89σ (p=0.000). Parkland’s effect on searches for “register to vote”: 0.67σ (p=0.173).

4. Additional Analyses: Registration Data

Figure A2 displays the distribution of p-values from our 147 regression discontinuity models. In Figure A2, each facet focuses on a different subgroup, with the "All" panel showing the distribution of coefficients among the pooled sample. If school shootings were to have a significant effect on patterns of voter registration (but not saying anything about the substantive effects or the direction of effects), we would expect to see a cluster of p-values below the 0.05 threshold. Indeed, across all subgroups (and the pooled sample) there is a spike in the distribution of p-values close to zero. Among the pooled sample, we find that 44 out of our 147 coefficients (29.9%) are significant at the 5% level. This is more than expected just by chance. However, given that we are dealing with multiple inferences with 147 regression models, there are reasons to correct for this. If we use the Bonferroni correction (1936), only 22 out of our 147 coefficients (15%) are significant. This is still more than expected by chance, but not as striking. Across various subgroups, we see a spike of significant estimates. These range from 18% (Democrats and young people) to 8% (Independents) of the adjusted p-values being significant. Indeed, if we just look at the p-values from the single cutoff estimates, we might conclude that school shootings have a significant effect on patterns of voter registration. However, as we saw in the paper the effects are all small.

Figure A2: RDD Estimates of the Effect of School Shootings on Voter Registration (p-values)



Distribution of p-values from our 147 single-cutoff regression discontinuity models estimating the effect of school shootings on patterns of voter registration. Figures are kernel densities. Figures are faceted by the different subgroups that we run our models ("All"=the pooled sample). Years reference the ages of the individuals considered. Models employ the Calonico, Cattaneo, and Titiunik (2014) optimal bandwidth.

4a. Multi-Treatment Estimates: Registration Data

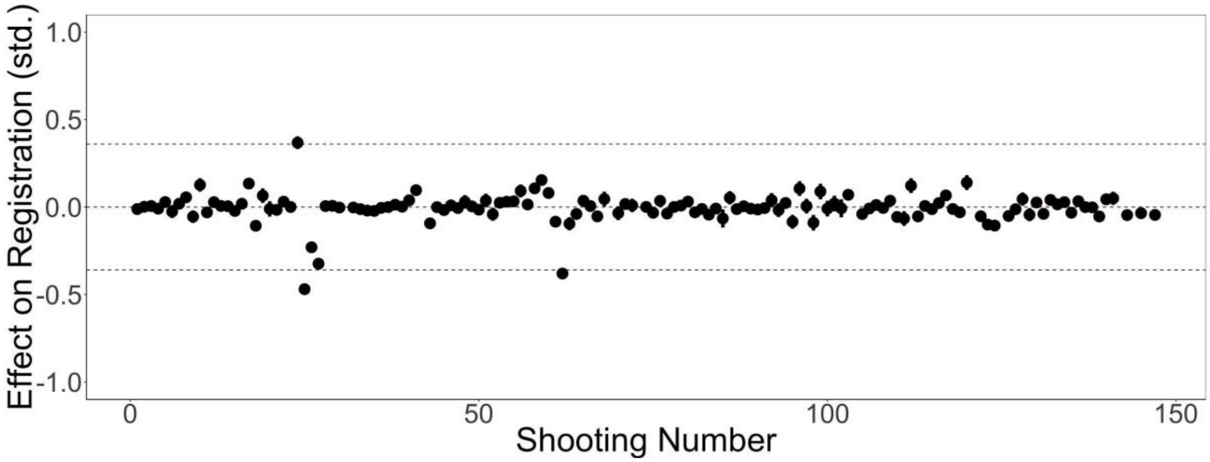
In the regression discontinuity models discussed in the text, we treat school shootings as independent of one another (we call these our "single-cutoff RDDs"). This allows us to get some traction on whether shootings individually have differential effects. And this assumption is reasonable given the spread of school shootings over time. Though there are many school shootings, only very rarely do shootings overlap in the immediate days around the cutoff that we are leveraging for causal identification. However, given that shootings might spread (via social contagion) or shooting treatments might be contaminated by secondary shooting treatments or both, we also run a specification (provided here in the Online Appendix) where we attempt to model all school shootings in the same model simultaneously. This allows us to see whether any effects for an individual shooting are actually attributable to another tragic occurrence. Despite our dataset being large (N ≈ 15 million), given that we have 147 shootings in our dataset, this model specification places a bit of dimensional strain on our dataset. For this reason, we are unable to run a full-on multi-cutoff regression discontinuity design. Doing so would include (at least) 294 variables (the running variables and treatment variables for all 147 shootings). Given that methods papers outlining the multi-cutoff regression discontinuity design often recommend the full set of interactions between treatments and running variables (Papa, Willett, and Murnane 2011), this approach is not feasible in our application. So, instead in this specification we include all the school shooting variables along with county and month-year fixed effects.[11] This approach leverages the panel nature of the data and runs something more similar to a multi-treatment difference-in-differences. It controls for all observable and unobservable factors that remain constant within counties (e.g. local voting culture or electoral administration) and within months (seasonal patterns in voter registration). It leverages variation within counties and within months of a given year in our multi-year sample.

[11] Even with this approach, we have some collinearity issues in estimation. As a result, 10 out of the 147 shootings will not estimate.

Our single-cutoff estimates might overestimate the effect of school shootings on voter registration, as they do not take into account for potentially overlapping shootings. Figure A3 shows our multi-treatment effect estimates from a model where we include all shooting treatment variables together along with county and month-of-year fixed effects. We display our estimates with a coefficient plot of all the estimates. These estimates are even more precise than the RDD given that we are using a much longer time series here and not restricting the bandwidth.

In the pooled sample, about one third of the coefficients (57%) are significant at the unadjusted 5% level. However, some of this may be because we are running multiple statistical tests; indeed, only 31% of coefficients clear the Bonferroni multiple inferences correction (Bonferroni 1936). Some of this might also be because of our large sample size. Turning to the substantive effects, even when the effects are statistically distinct from zero, they are negligible—allowing us to rule out substantively meaningful effects. In 97.8% of the coefficients we can rule out Hartman and Hidalgo's (2018) default meaningful effect size using equivalence testing. The median effect size is a paltry -0.03% of a standard deviation (mean= -0.5%) and the median confidence intervals illustrate our ability to rule out large effects [lower: -2.2%, upper: 2.2%]. These conclusions do not change in our party or age subgroups. Across all slices of the electorate that we observe, the results are consistently small to non-existent. Among Republicans, for example, the median effect size is 0.1% of a standard deviation; among Democrats the median effect size is -0.1% of a standard deviation. Again, this suggests that school shootings have no systematic effects on voter registration.

Figure A3: Multi-Treatment Estimates of Shootings' Effect on Overall Registration Rates



Coefficient estimates for our multi-treatment models that include all shootings together in the same model. The right figure is a coefficient plot, with standardized effect sizes on the y-axis and the shooting number (ordered by when the shootings occurred) on the x-axis. Reference lines show a 0 effect (middle) and 36% of a standard deviation: Hartman and Hidalgo's (2018) default value for equivalence testing.

4b. Incorporating Geographic Proximity: Registration Data

Thus far we have shown that school shootings have little to no effect on patterns of voter registration. However, this may be because we are looking nationwide. What if we look at the areas surrounding where the school shootings happen? When we restrict our models to within 100 miles of the shooting, we find similar results.[12] Though doing so cuts down on our statistical power, we still find a similar pattern to what we've just outlined. The median effect size is 0.5% of a standard deviation with coefficients roughly equally balanced on the positive (53.7%) and negative (46.3%) side, 95.2% of the shooting coefficients are small (85%) or medium (10.2%) by Cohen's standard, 91.2% are small by Hartman and Hidalgo's standard, only 6.8% (10//147) are significant at unadjusted levels, and only 0.7% (1/147) are significant at the Bonferroni levels. This suggests that school shootings do not spark a wave of new people registering to vote *even when* those shootings occur close by.

[12] We have also run models that use counties only within 50 miles. While producing qualitatively similar results, these are too underpowered given that we are severely cutting down on the number of counties within that small of an area.

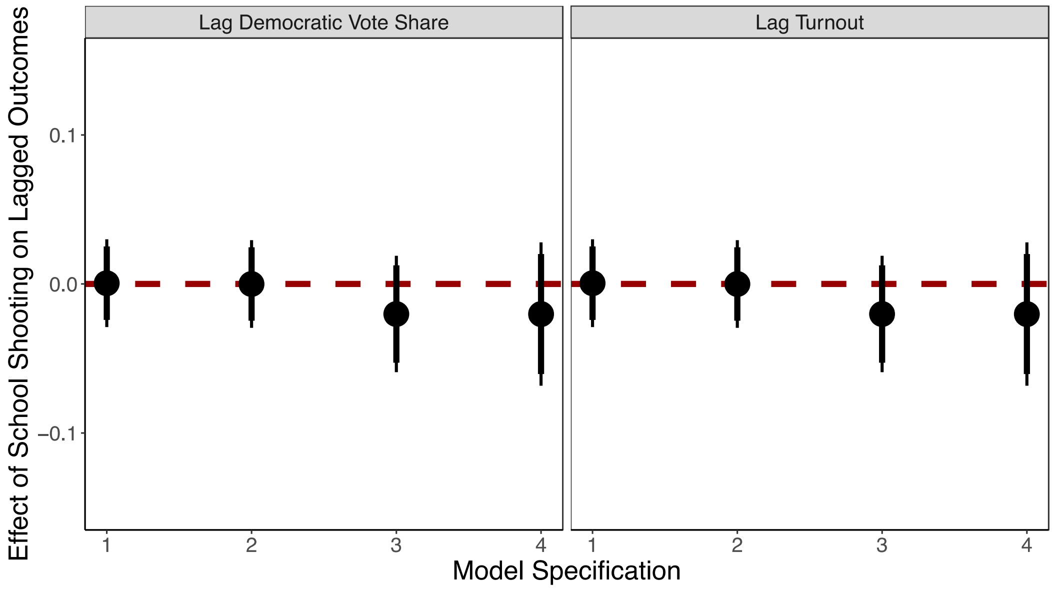
4c. Controlling for Lagged Registration: Registration Data

Though our multi-treatment models take into account seasonality in patterns of voter registration (by including month fixed effects), some may desire us to go one step further. To do so, we include lagged registration counts for the previous year. This is conceptually equivalent to looking for discontinuous treatment effects after the shooting and comparing it to (any) discontinuous treatment effects prior to the shooting—i.e. a difference in discontinuities design. In addition to helping to address seasonality, including lagged registration should also improve our (already high) levels of statistical power given the strength of the relationship between lagged and contemporary measures.

When we take this approach, we find similar results. The median effect size is 0.6% of a standard deviation. Just under two thirds (64%) of the coefficients are positive; however, all of the shooting coefficients are small (91.2%) or medium (8.8%) by Cohen's standard, 98.6% are small by Hartman and Hidalgo's standard. Among these, only 1/147 (0.7%) are positive, significant at adjusted levels, and substantively meaningful. While 9.5% (14/147) are significant at the Bonferroni levels, we appear, again, to be having mostly small effects. Though appear to spark some form of opinion change and mass discussion online, school shootings do not mobilize action in a way that would place pressure on elected officials.

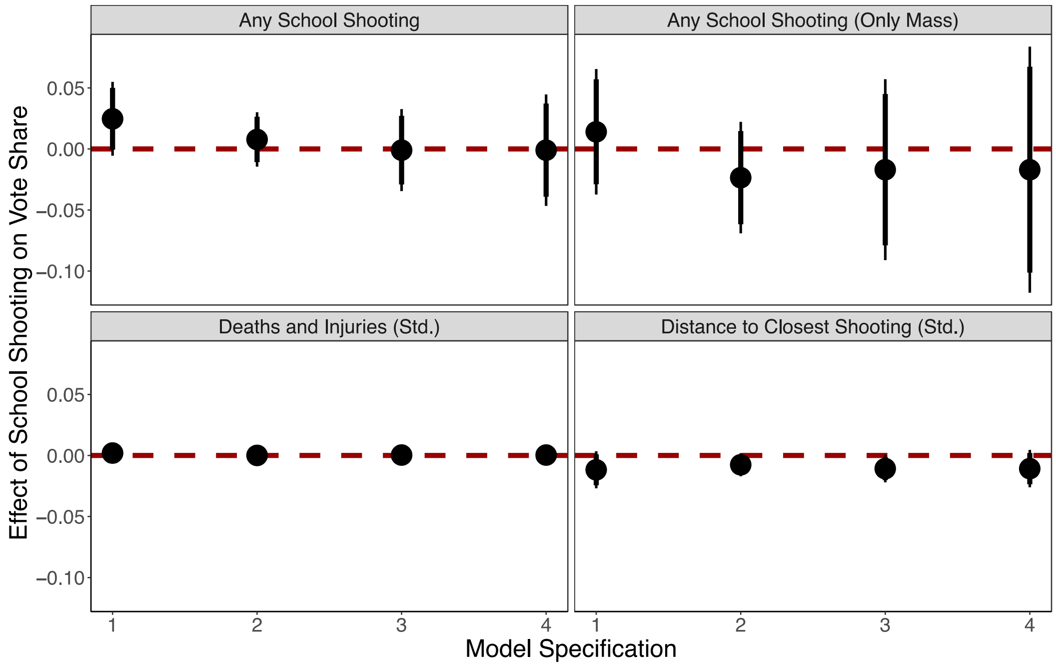
5. Additional Analyses and Robustness Checks: Elections Data

Figure A4: Placebo Tests—Effect of School Shootings on Lagged Voter Turnout and Vote Share



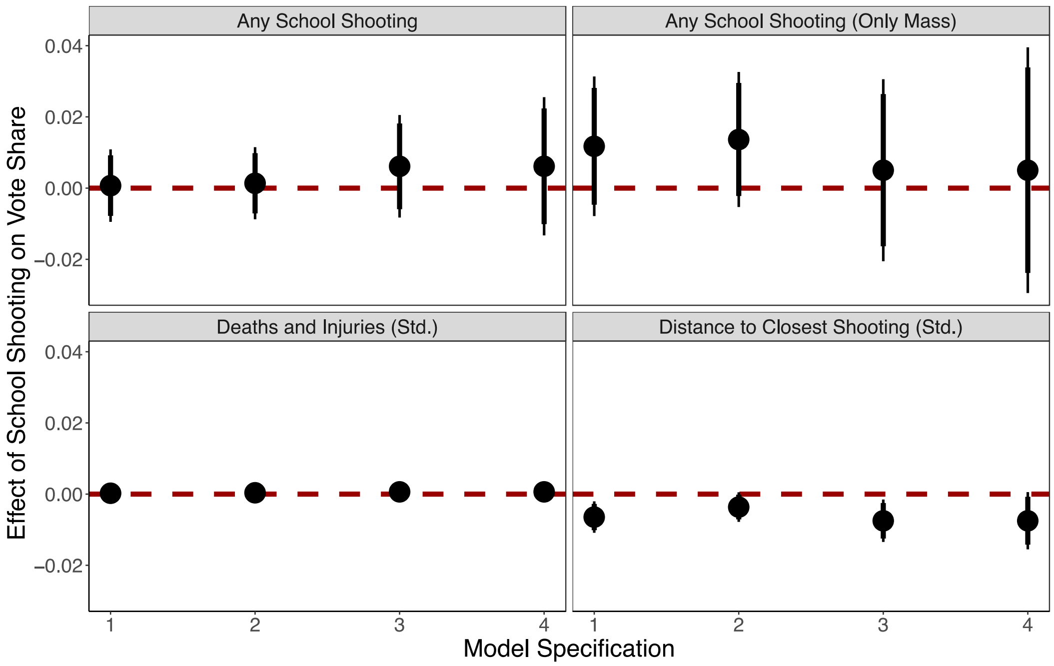
Coefficient plots for our difference-in-differences models looking at the effect of school shootings on lagged Democratic vote share (first panel) and voter turnout (second panel). Elections data come from Dave Leip's Atlas of U.S. Presidential Elections. Treatment here is having any school shooting in a county. Model 1 includes county and year fixed effects, Model 2 adds controls for unopposed Republican/Democrat candidates running, Model 3 adds a linear time trend for each county, and Model 4 adds a quadratic time trend for each county. Model N's range from 12,170 to 12,175. Standard errors are clustered at the county level.

Figure A5: Effect of School Shootings on Vote Share (Close by Control Counties)



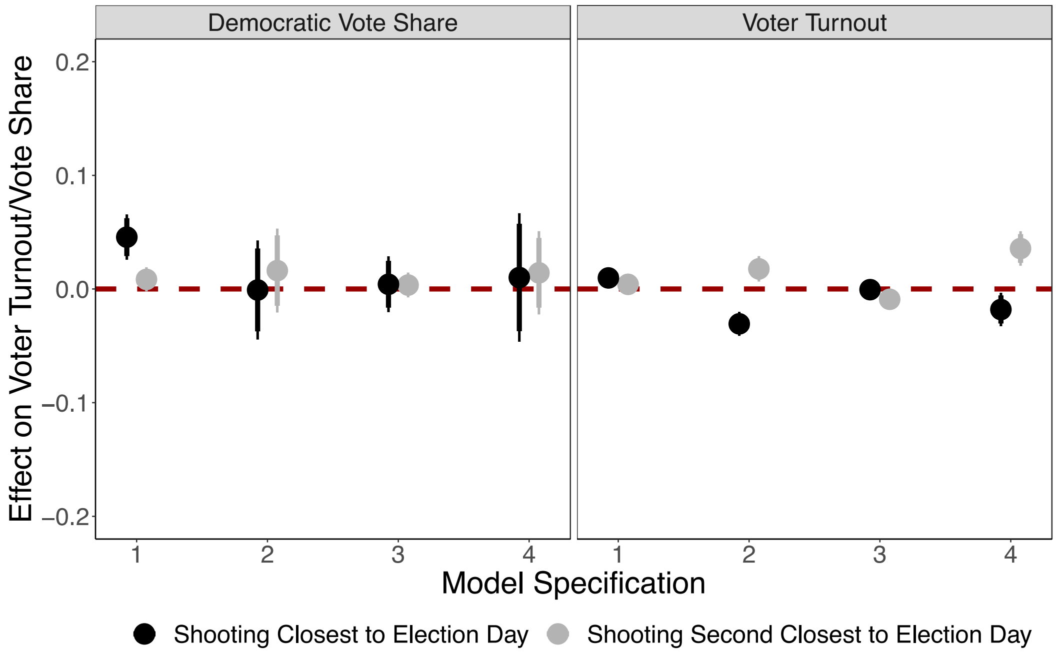
Coefficient plots for our difference-in-differences models looking at the effect of school shootings on Democratic vote share. Elections data come from Dave Leip's Atlas of U.S. Presidential Elections. These specifications focus on counties within 235 miles of the closest school shooting for treated counties—i.e. they include only control counties that are close to treated counties. Each of the facets shows a different way of measuring exposure to a school shooting. The first is an indicator for having any school shooting in a county-year, the second is the same as the first but the treatment group includes only mass shootings (i.e. those that have 4+ injuries/deaths), the third is the number of deaths/injuries from school shootings in a county-year(standardized), the last is the distance to the closest school shooting in the county-year (standardized). Model 1 includes county and year fixed effects, Model 2 adds controls for unopposed Republican/Democrat candidates running, Model 3 adds a linear time trend for each county, and Model 4 adds a quadratic time trend for each county. Model N's range from 7,250 to 7,309. Standard errors are clustered at the county level.

Figure A6: Effect of School Shootings on Voter Turnout (Close by Control Counties)



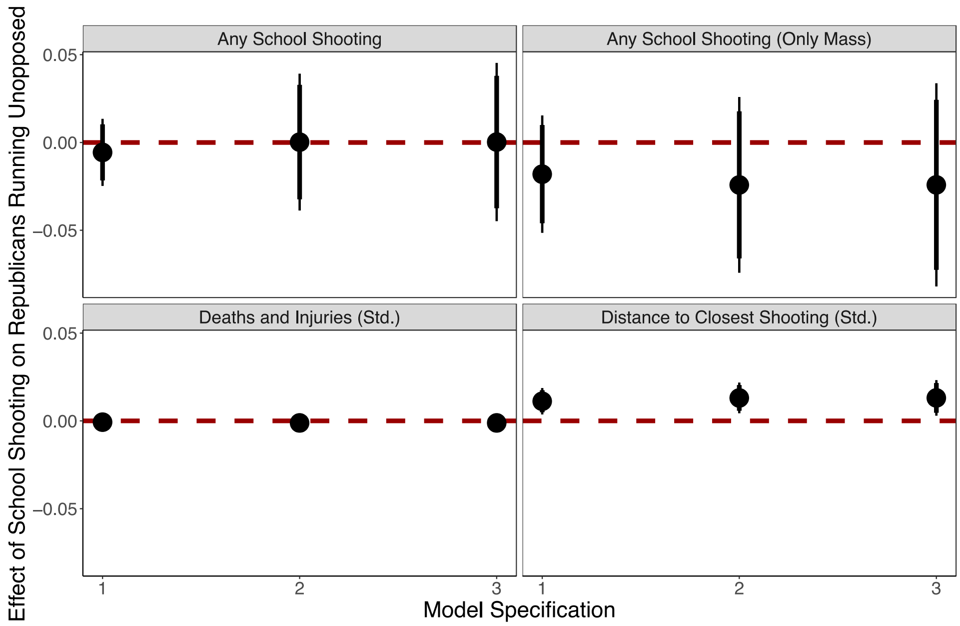
Coefficient plots for our difference-in-differences models looking at the effect of school shootings on voter turnout. Elections data come from Dave Leip's Atlas of U.S. Presidential Elections. These specifications focus on counties within 235 miles of the closest school shooting for treated counties—i.e. they include only control counties that are close to treated counties. Each of the facets shows a different way of measuring exposure to a school shooting. The first is an indicator for having any school shooting in a county-year, the second is the same as the first but the treatment group includes only mass shootings (i.e. those that have 4+ injuries/deaths), the third is the number of deaths/injuries from school shootings in a county-year(standardized), the last is the distance to the closest school shooting in the county-year (standardized). Model 1 includes county and year fixed effects, Model 2 adds controls for unopposed Republican/Democrat candidates running, Model 3 adds a linear time trend for each county, and Model 4 adds a quadratic time trend for each county. Model N's range from 7,037 to 7,184. Standard errors are clustered at the county level.

Figure A7: Effect of School Shootings on Voter Turnout and Vote Share, Taking into Account Timing of the Shooting Relative to Election Day



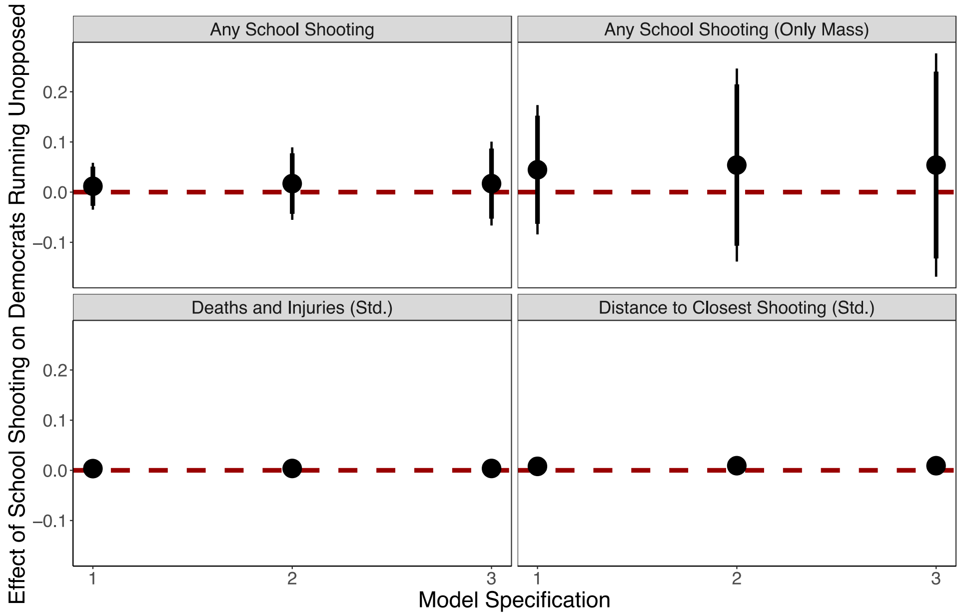
Notes: Effect of school shootings on Democratic vote share (left panel) and voter turnout (right panel) when taking into account the timing of the shooting relative to Election Day. Elections data come from Dave Leip's Atlas of U.S. Presidential Elections. Coefficient plots for our difference-in-differences models looking at the effect of the most recent (black dots/bars) and the second most recent (grey dots/bars) school shootings relative to Election Day. Treated counties are those that are within 100 miles of a shooting. Model 1 does not control for the interaction between treatment and shooting timing nor a linear interaction between year and the county fixed effects; Model 2 has the former, but not the latter; Model 3 has the latter, but not the former; and Model 4 has them both. All models control for whether the election was uncontested for Republicans and Democrats, county fixed effects, and year fixed effects. Model N's range from 14,784 to 15,358. Standard errors are clustered at the county level.

Figure A8: Effect of School Shootings on Republicans Running Unchallenged



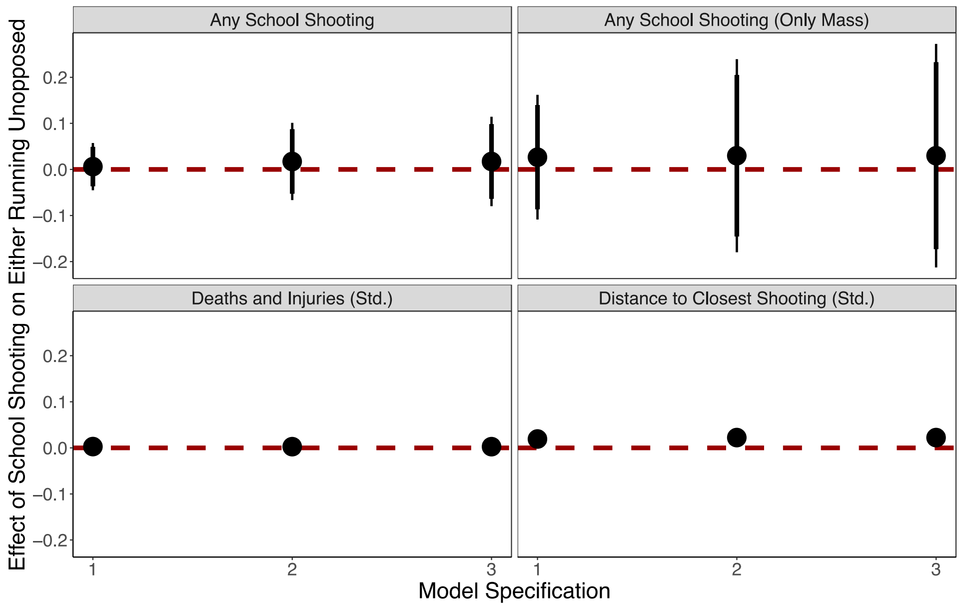
Notes: Effect of school shootings on the chances that a Republican runs unchallenged. Unchallenged races are those in which Democrats receive less than 5% of the vote. Elections data come from Dave Leip's Atlas of U.S. Presidential Elections. Each of the facets shows a different way of measuring exposure to a school shooting. The first is an indicator for having any school shooting in a county-year, the second is the same as the first but the treatment group includes only mass shootings (i.e. those that have 4+ injuries/deaths), the third is the number of deaths/injuries from school shootings in a county-year(standardized), the last is the distance to the closest school shooting in the county-year (standardized). Model 1 includes county and year fixed effects, Model 2 adds a linear time trend for each county, and Model 3 adds a quadratic time trend for each county. Model N's range from 15,308 to 15,358. Standard errors are clustered at the county level.

Figure A9: Effect of School Shootings on Democrats Running Unchallenged



Notes: Effect of school shootings on the chances that a Democrat runs unchallenged. Unchallenged races are those in which Republicans receive less than 5% of the vote. Elections data come from Dave Leip's Atlas of U.S. Presidential Elections. Each of the facets shows a different way of measuring exposure to a school shooting. The first is an indicator for having any school shooting in a county-year, the second is the same as the first but the treatment group includes only mass shootings (i.e. those that have 4+ injuries/deaths), the third is the number of deaths/injuries from school shootings in a county-year(standardized), the last is the distance to the closest school shooting in the county-year (standardized). Model 1 includes county and year fixed effects, Model 2 adds a linear time trend for each county, and Model 3 adds a quadratic time trend for each county. Model N's range from 15,308 to 15,358. Standard errors are clustered at the county level.

Figure A10: Effect of School Shootings on Either Party Running Unchallenged

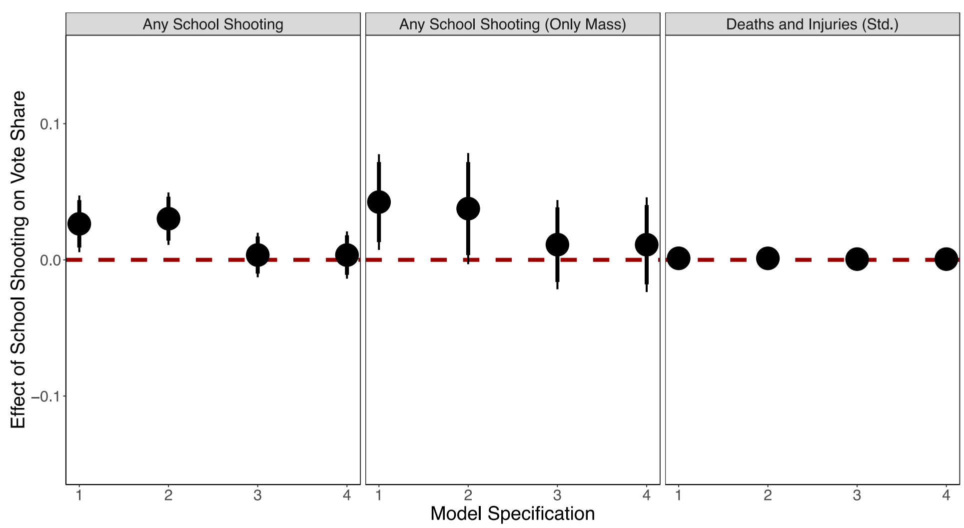


Notes: Effect of school shootings on the chances that either Democrat or a Republican runs unchallenged. Elections data come from Dave Leip's Atlas of U.S. Presidential Elections. Each of the facets shows a different way of measuring exposure to a school shooting. The first is an indicator for having any school shooting in a county-year, the second is the same as the first but the treatment group includes only mass shootings (i.e. those that have 4+ injuries/deaths), the third is the number of deaths/injuries from school shootings in a county-year(standardized), the last is the distance to the closest school shooting in the county-year (standardized). Model 1 includes county and year fixed effects, Model 2 adds a linear time trend for each county, and Model 3 adds a quadratic time trend for each county. Model N's range from 15,308 to 15,358. Standard errors are clustered at the county level.

In the state-level results that we presented in the paper, we provided evidence from a longer time series (2000-2018). Here we replicate that longer time series for federal level elections. As can be seen, there is some evidence at first blush that school shootings push elections towards Democrats. The coefficients on the far left—those with two-way fixed effects—indicate a 3-4 percentage point gain in the areas surrounding a shooting following that event. This result is very similar to what Yousaf (2018) finds in a working paper that examines the effect of mass shootings (not restricted to those that happen in schools). Given that Yousaf (2018) focuses primarily on mass shootings, the closest analog is actually in the second panel. This too indicates an effect that pushes elections towards Democrats.

However, as the figure shows this result is highly sensitive to model specification and coding of the treatment. Once we include controls for unchallenged races or add linear/quadratic time trends for counties or both, the effect disappears. Moreover, this effect does not hold when we use deaths and injuries (standardized) as our treatment.

Figure A11: Effects of School Shootings, Extended Time Series (2000-2018)

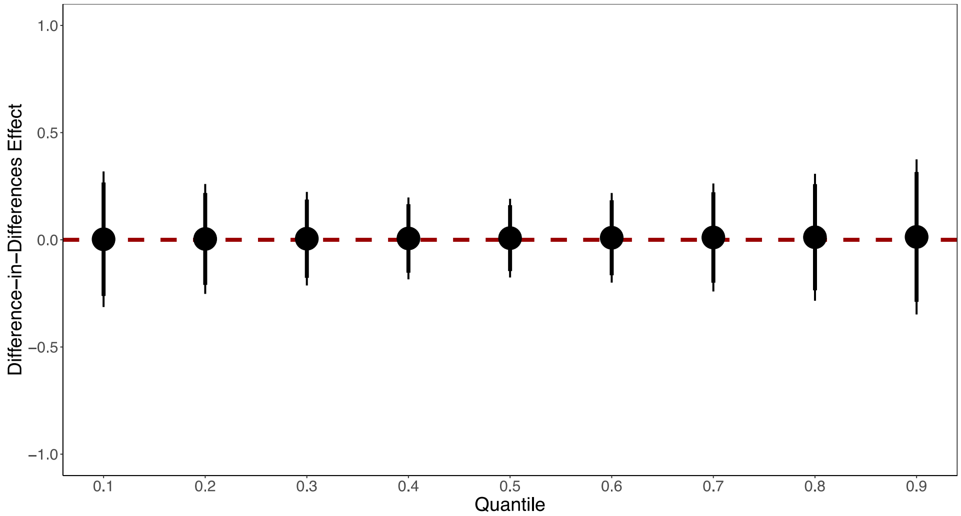


Notes: Effect of school shootings on Democratic vote share from 2000-2018 (229 school shootings over this time period). Elections data come from Dave Leip's Atlas of U.S. Presidential Elections. Each of the facets shows a different way of measuring exposure to a school shooting. The first is an indicator for having any school shooting in a county-year, the second is the same as the first but the treatment group includes only mass shootings (i.e. those that have 4+ injuries/deaths), and the third is the number of deaths/injuries from school shootings in a county-year(standardized). Model 1 includes county and year fixed effects, Model 2 adds whether Democrats/Republicans are running unopposed, Model 3 adds a linear time trend for each county, and Model 4 adds a quadratic time trend for each county. Model N's range from 30,542 to 30,625. Standard errors are clustered at the county level.

Some readers may wonder whether there are any polarizing effects on election outcomes. While answering this question is difficult at the county-level, we can get some traction of whether there is variation in our treatment estimates by the party vote share of the area. To do so, we use two approaches. The first simply stratifies the models by party vote share in the previous federal election—running the models again separating areas that tend to vote for Democrats from those that vote for Republicans (we split at the median level here). The second uses quantile regression. This approach is more data-driven than the first estimating the effect of school shootings at different points along the vote share distribution. If shootings were to polarize the public in elections, we would expect to see a negative effect at lower quantiles (i.e. areas that don't typically vote for a Democrat become more Republican-supportive in the election after a shooting) and positive effects at higher quantiles (i.e. areas that typically vote for a Democrat become more Democrat-supportive in the election after a shooting).

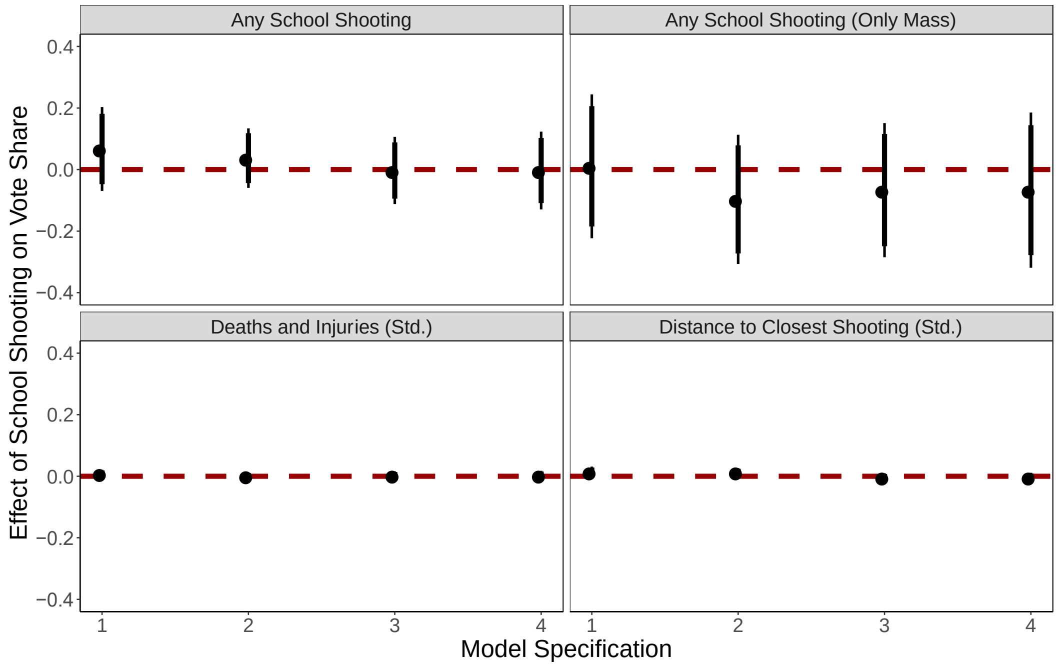
In practice, this is not what we observe. The effects of a shooting are remarkably consistent across high and low democrat areas. We see no differences in effect sizes in high or low Democrat areas and, as Figure A12 shows, little to no variation in effects across quantiles. School shootings do not polarize the electorate in terms of their voting patterns.

Figure A12: Effects of School Shootings on Polarization of Election Outcomes, Quantile Regression



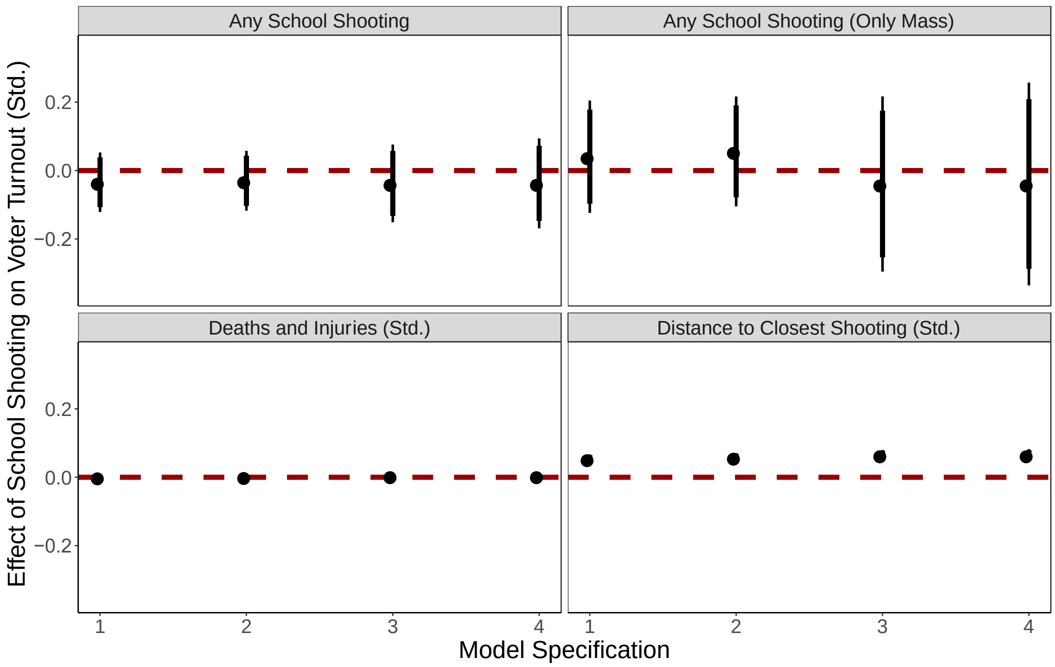
Notes: Effect of school shootings on Democratic vote share across the distribution of this outcome (i.e. quantile regression). Elections data come from Dave Leip's Atlas of U.S. Presidential Elections. Treatment is an indicator for having any school shooting in a county. Models contain county and year fixed effects. Model N= 15,358.

Figure A13: Effect of School Shootings on Vote Share (Standardized Outcomes)



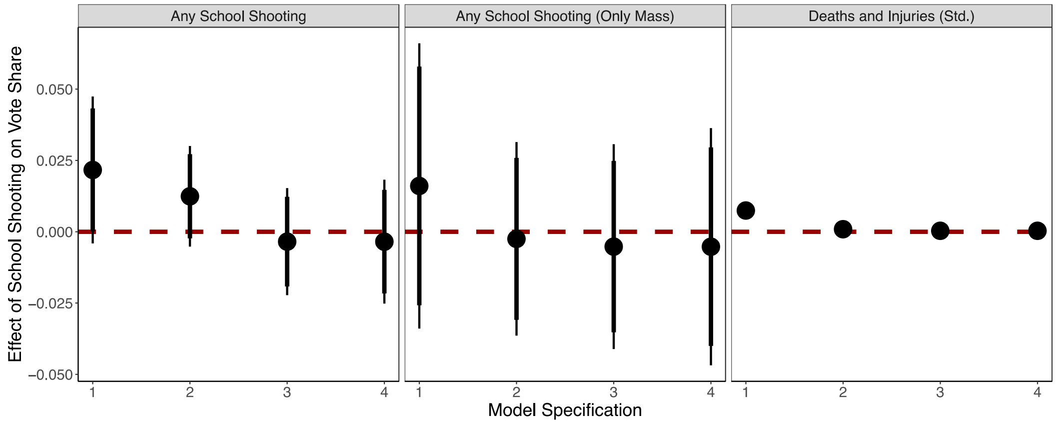
Notes: Coefficient plots for our difference-in-differences models looking at the effect of school shootings on Democratic vote share. Elections data come from Dave Leip's Atlas of U.S. Presidential Elections (2006-2014). Each of the facets shows a different way of measuring exposure to a school shooting. The first is an indicator for having any school shooting in a county-year, the second is the same as the first but the treatment group includes only mass shootings (i.e. those that have 4+ injuries/deaths), the third is the number of deaths/injuries from school shootings in a county-year(standardized), the last is the distance to the closest school shooting in the county-year (standardized). Model 1 includes county and year fixed effects, Model 2 adds controls for unopposed Republican/Democrat candidates running, Model 3 adds a linear time trend for each county, and Model 4 adds a quadratic time trend for each county. Standard errors are clustered at the county level.

Figure A14: Effect of School Shootings on Voter Turnout (Standardized Outcomes)



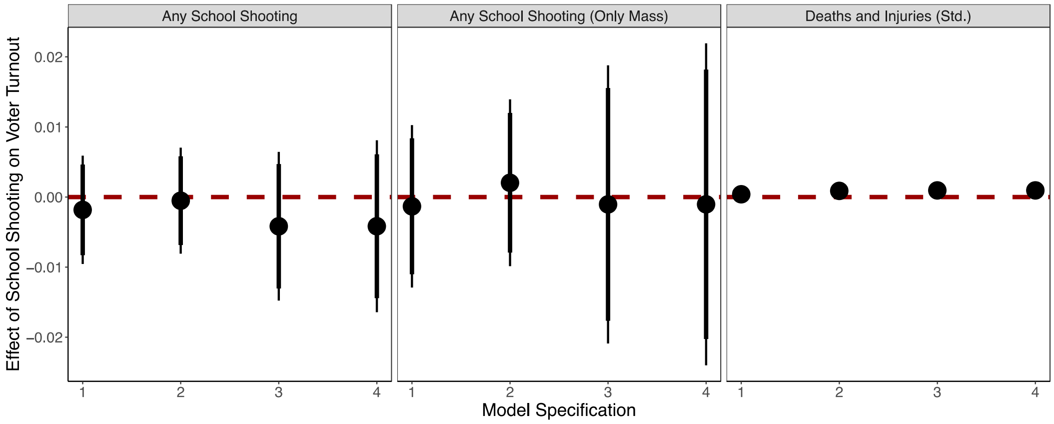
Notes: Coefficient plots for our difference-in-differences models looking at the effect of school shootings on voter turnout. Elections data come from Dave Leip's Atlas of U.S. Presidential Elections (2006-2014). Each of the facets shows a different way of measuring exposure to a school shooting. The first is an indicator for having any school shooting in a county-year, the second is the same as the first but the treatment group includes only mass shootings (i.e. those that have 4+ injuries/deaths), the third is the number of deaths/injuries from school shootings in a county-year(standardized), the last is the distance to the closest school shooting in the county-year (standardized). Model 1 includes county and year fixed effects, Model 2 adds controls for unopposed Republican/Democrat candidates running, Model 3 adds a linear time trend for each county, and Model 4 adds a quadratic time trend for each county. Standard errors are clustered at the county level.

Figure A15: Effect of School Shootings on Vote Share (Odd Year Shootings Included)



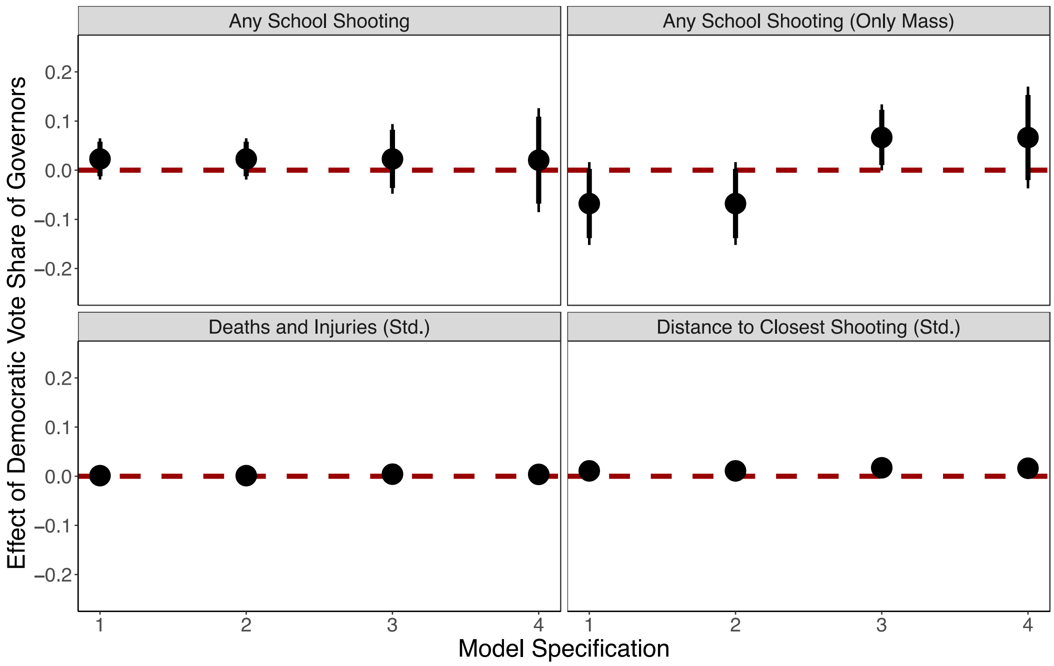
Notes: Coefficient plots for our difference-in-differences models looking at the effect of school shootings on Democratic vote share when we include odd-year shootings in the treatment. Elections data come from Dave Leip's Atlas of U.S. Presidential Elections (2006-2014). Each of the facets shows a different way of measuring exposure to a school shooting. The first is an indicator for having any school shooting in a county-year, the second is the same as the first but the treatment group includes only mass shootings (i.e. those that have 4+ injuries/deaths), and the third is the number of deaths/injuries from school shootings in a county-year(standardized). Model 1 includes county and year fixed effects, Model 2 adds controls for unopposed Republican/Democrat candidates running, Model 3 adds a linear time trend for each county, and Model 4 adds a quadratic time trend for each county. Standard errors are clustered at the county level.

Figure A16: Effect of School Shootings on Voter Turnout (Odd Year Shootings Included)



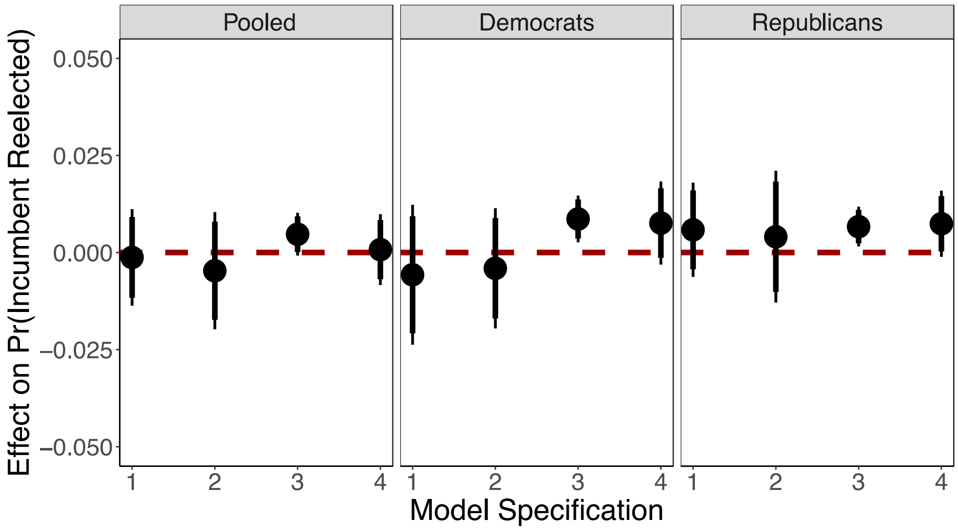
Notes: Coefficient plots for our difference-in-differences models looking at the effect of school shootings on voter turnout when we include odd-year shootings in the treatment. Elections data come from Dave Leip's Atlas of U.S. Presidential Elections (2006-2014). Each of the facets shows a different way of measuring exposure to a school shooting. The first is an indicator for having any school shooting in a county-year, the second is the same as the first but the treatment group includes only mass shootings (i.e. those that have 4+ injuries/deaths), and the third is the number of deaths/injuries from school shootings in a county-year(standardized). Model 1 includes county and year fixed effects, Model 2 adds controls for unopposed Republican/Democrat candidates running, Model 3 adds a linear time trend for each county, and Model 4 adds a quadratic time trend for each county. Standard errors are clustered at the county level.

Figure A17: Effect of School Shootings on State Elections (Different Treatments)



Notes: Coefficient plots for our difference-in-differences models looking at the effect of school shootings on gubernatorial vote. Elections data come from panel of gubernatorial elections from 2006-2014. Each of the facets shows a different way of measuring exposure to a school shooting. The first is an indicator for having any school shooting in a county-year, the second is the same as the first but the treatment group includes only mass shootings (i.e. those that have 4+ injuries/deaths), the third is the number of deaths/injuries from school shootings in a county-year(standardized), the last is the distance to the closest school shooting in the county-year (standardized). Model 1 includes county and year fixed effects, Model 2 adds controls for unopposed Republican/Democrat candidates running, Model 3 adds a linear time trend for each county, and Model 4 adds a quadratic time trend for each county. Standard errors are clustered at the county level.

Figure A18: Effect of School Shootings on Incumbent Reelection Rates



Notes: Coefficient plots for our difference-in-differences models looking at the effect of school shootings on incumbency reelection rates. Data contains incumbent reelection in House districts from 2006-2018. Models 1 and 2 use the number of school shootings (standardized) as the treatment. Models 3 and 4 use the number of deaths and injuries from shootings (standardized) as the treatment. Models 1 and 3 contain district and year fixed effects; Models 2 and 4 add a linear time trend for districts. Model N's by panel: 3,014; 1,318; 1,368.

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