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HITS CLOSE TO HOME:

Shootings and support for the Sweden Democrats

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WORKING PAPER SERIES 2020:15

QOG THE QUALITY OF GOVERNMENT INSTITUTE
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Box 711, SE 405 30 GÖTEBORG
December 2020
ISSN 1653-8919
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Hits close to home: Shootings and support for the Sweden Democrats

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December 23, 2020

Abstract

Do instances of public gun violence increase support for Radical Right Parties? Using a pseudo-experimental design, this study investigates the impact of shootings in Sweden 2011-2018 on support for the Sweden Democrats. Using GIS techniques to combine both election results and survey data with police data on shootings, shootings show no consistent political effects. While there are some methodological caveats, the results might reflect that perceptions and fear of crime are more important determinants of political attitudes than actual crime.

1 Introduction

An integral message of radical right and populist parties is that violent crime is getting out of control, and that immigration is the cause of it. Studies of electorates in a wide range of countries have consequently shown that feelings of insecurity and threat are consistent predictors of votes for populist or anti-immigration parties.

However, the connection between actual crime and political outcomes is less clear. For instance, while a central part of Donald Trump's message is that crime is on the rise, violent crime in the United States has decreased over the last decades. Nevertheless, surveys show that voters do believe that crime is increasing (Pew Research 2019).

This begs the question of to what extent voters respond to actual levels of violent crime. Are episodes of violent crime met with increased support for a family of parties that have made opposition to such crime a centerpiece of their platform, or is demand for such parties more driven by a general feeling of threat?

In this paper, I employ a pseudo-experimental design to estimate the causal impact of gun violence related to organized crime on support for the Sweden Democrats in Swedish parliamentary elections. Exploiting the fact that shootings are likely to be randomly distributed in relation to the election, areas where a shooting took place just after an election can be thought of as a control group to areas where shootings happened just before an election.

Sweden is an interesting test case, given that the last decade has seen both the entry of the Sweden Democrats into parliament, as well as an uptick in killings in the world of organized crime. Many of them has happened in public, with military-grade weapons, several times injuring bystanders. As a result, these episodes have been highly discussed in national media, and as many of the perpetrators have been immigrants or descendants

of immigrants, the shootings have also been used in the rhetoric of the Sweden Democrats.

However, while the pseudo-experimental design is validated by balance tests, the results of the analyses show that there is no consistent impact of shootings on electoral behavior. Voters exposed to shootings in their immediate vicinity just before the election are not more likely to support the Sweden Democrats than those not exposed to shootings. That fears of crime is a consistent predictor of support for the Sweden Democrats does thus not mean that actual crime serves the same function.

The rest of this paper proceeds as follows. In the theoretical section, I review the literature on crime and support for the populist and radical right, as well as the case. The methods and data section describes pseudo-experimental design and data used. The third section presents the result of the main analysis, as well as auxiliary analyses that corroborate the main findings. The final section concludes.

2 Theory and hypothesis

One of the most important trends in European politics the last decades is the emergence and spectacular success of the "radical right," a loosely defined group of parties that can be said to contain parties such as the French *Front National*, the Italian *Lega* and the Austrian Freedom Party (Rydgren 2008). While there has been much discussion about the similarities and differences between these parties and if they really constitute a family, the key tenet in the message of virtually all these parties is opposition to immigration.

In spite of the disproportionate attention given to the study of radical right parties, most of the research point to the fact that their voters behave as voters in general do: they support the party whose policy positions are closest to themselves, on issues they find important (Van der Brug, Fennema, and Tillie 2000). And the most important

issue that determines support for these parties is, naturally, resistance to immigration (Rydgren 2008; Van der Brug, Fennema, and Tillie 2000; Ivarsflaten 2008; Allen 2017).

This in turn leads into the question of what causes resistance and openness to immigration. Much research has been devoted to investigating whether economic hardship and competition causes anti-immigration sentiments, and in turn, votes for the radical right. While there is some evidence that this might be the case (Arzheimer 2009; Dal Bó et al. 2018), in general research suggests that the perception of immigration's impact of the economy in general matter more than changing personal circumstances (Hainmueller and Hopkins 2014).

The main message of the radical right is that the nation is under threat, from corrupt and globalist elites within, and immigrants outside. Violent, criminal immigrants are the embodiment of this message, threatening the security of citizens. Psychological research has also shown that feelings of threat increase ethnocentrism and xenophobia (Huddy et al. 2005). A political message against immigration, that plays up threats, is thus reinforced by itself. Anti-immigrant sentiments leads to feelings of greater threat, which in turn increases anti-immigrant sentiments.

Issues of law and order as well as immigration are generally "owned" by the right (Seeberg 2017). Increasing salience of these issues are therefore likely to lead to a corresponding increase in support for right and radical right parties. High-profile crimes that garner attention in national media, or crimes that are highly visible to the individual, could elevate the issue on voters' agenda.

Examples of how radical right politicians connect crime to immigration abound. When Donald Trump announced he was for running for president of the United States, he proclaimed that "When Mexico sends its people, they're not sending their best... They're

sending people who have lots of problems, and they're bringing those problems with us. They're bringing drugs. They're bringing crime. They're rapists. And some, I assume, are good people." Similar sentiments are common in Europe. After a brutal murder early 2018, the leader of Italy's *Lega*, Matteo Salvini stated that "The moral responsibility of every incident of violence that happens in Italy is that of those who have filled it with illegal immigrants" (Local 2018). And in a recent State of the Union speech, Hungary's Viktor Orban claimed that "Migration boosts crime - in particular, crimes against women - and spreads the disease of terrorism among us" (Post 2019).

2.1 Previous research on crime and radical right support

Messages that connect immigration to crime are thus common among radical right parties. This does however not necessarily imply that actual crime are related to support for the radical right. Perceptions about the prevalence of crime, and worries over it, might be driven by other factors, such as media coverage. Nor does exposure to crime for the individual need be predictive of perceptions of crime more generally. For instance, the link between objective immigration rates and support for anti-immigration parties is weaker than one might expect, finding support in some studies and not in others (Dinas and Spanje 2011).

Several intertwined strands of literature have bearing on the matter at hand. A large body of research deals with attitudes: the link between crime rates on attitudes towards immigration; the link between immigration rates and worries for crime, the link between worries for crime and attitudes towards immigration and vice versa; the effects of news reporting on attitudes towards immigration; the effect of crime levels on the saliency of the immigration issue, and so on.

In general, worries about crime is related to worries about, and opposition to, immigration (Fitzgerald, Curtis, and Corliss 2012). Influx of immigrants can also lead to stronger fears for crime (even if crime levels are not affected) (Nunziata 2015). But there is also experimental evidence that safety concerns are less important than a sense of cultural threat in determining attitudes towards immigrants (Sniderman, Hagendoorn, and Prior 2004), and observational data from the United States that fail to find a link between local crime rates and anti-immigrant sentiments (Hopkins 2010).

Another set of studies instead focus on voting and voting intentions as outcomes to be explained. A review of parliamentary elections in Europe (Smith 2010) did find that radical right parties fared better when crime levels were higher, especially if immigration also was high. A study from the Netherlands found that higher local crime rates were associated with an increased probability of voting for the *List Pim Fortuyn* among voters that had anti-immigrant attitudes, and conversely that immigration rates were associated with more voting for the same party among voters who were “tough on crime” (Dinas and Spanje 2011). Similarly, voters who believed that immigration increased crime were more likely to vote for radical right parties in a study of six European countries (Rydgren 2008). Media exposure to news about crime and immigration were associated with higher likelihoods of voting for anti-immigration parties, although issues of causality is hard to sort out (Burscher, Spanje, and Vreese 2015).

On the other hand, there are also studies that do not find connections between radical right support and the saliency of the crime issue (Dennison 2019), or between media reporting that links immigration to crime (in contrast to immigration news in general) (Damstra et al. 2019). A sceptical perspective is offered by Cas Mudde, who notes that while crime is a high priority for supporters of the radical right, it “seems to play only

a marginal role in motivating people to actually vote for a populist radical right party,” taking a backseat to “xenophobia and political resentment” (Mudde 2007:224).

2.2 Previous research on effects of shootings

Existing research on the effects of shootings is naturally focused on the United States, where mass shootings with many fatalities are comparatively common. Benjamin Newman and Todd Hartman are the authors of a study similar to the present one, where respondents that live in proximity to a mass shooting are compared to respondents that do not. In the original study (Newman and Hartman 2017), they find that such exposure increases support for gun control. However, David Barney and Brian Schaffner (2018) have put forth valid methodological criticism that question the findings, instead finding that if there is any effect, it is conditional on party identification, with Democrats becoming more supportive of gun control, and Republicans less.

The polarizing effect of shootings is found also by Hasin Yousaf (2018), in an ambitious study. Using difference-in-difference estimation strategies, Yousaf also find that on average, Republicans fare worse in subsequent elections in districts that experience mass shootings.

In contrast, a recent study found no effect of school shootings in the US, either on voter registration, turnout or electoral outcomes (Hassell, Holbein, and Baldwin 2020). Extreme events leading to the death of innocents, even children, does apparently not always have political consequences.

It is important to keep in mind the political context when interpreting the findings. While theory suggests that exposure to crime should increase saliency of the law and order issue and benefit parties that are perceived to be strong in that area, mass shootings in the

United States are often framed differently. In public debate after shootings, the primary issue is often perceived to be access to guns, rather than crime in general, since many of the high-profile perpetrators have no prior convictions. Importantly, Yousaf exclude familial shootings and “felony mass shootings,” meaning shootings that are connected to crime in some way, such as criminal competition or armed robbery. Such shootings that are perceived to be connected to organized crime and gangs could most likely have different effects than the more “random” public mass shootings common to the United States.

2.3 Shootings in Sweden

The case in this study is Sweden, a country in the intersection of three different trends. First, while levels of violence are low in a global perspective, deadly violence has been on the rise in the last few years, especially gun violence, which is now high in a European perspective (NCP 2019; Sturup, Gerell, and Rostami 2019). Second, Sweden has had one of the highest levels of asylum immigration in Europe, especially in relation to the 2015 refugee crisis. Today, 19.1 percent of the Swedish population was born abroad, compared to 13.7 percent in the United States. The change from what was often seen as “small, homogenous country” is stark; the proportion foreign-born was 11.3 percent as recently as the year 2000.

Large immigration has provided a basis for the third important trend: the meteoric rise of the Sweden Democrats, who entered the *Riksdag* in 2010, and currently hold the third most seats. Sweden was for long an exception to the European rule, where radical right parties were represented in most national parliaments, but not in Sweden. The reasons, scholars have argued, was that the immigration issue was long kept off the

political agenda (Dahlström and Esaiasson 2013). In contrast, it has now been one of the most salient issues for years.

The ground is thus ripe for the Sweden Democrats to politicize shootings. On their website state they state that “Our country is ill. Women, children and elders are afraid to go out at night. Rival gangs from the suburbs fight in the streets... As a result of the uncontrolled immigration, terrorists with combat experience walk the streets and exploit our welfare- and asylum system” (Sweden Democrats 2019). While the Sweden Democrats more than any other party connect the issue to immigration, high-profile shootings have also been used by the Conservative party to attack the Social Democratic government. The prime minister, in turn, recently tried to tie shootings to the use of narcotics by rich youth in wealthy suburbs.

Shootings mainly happen in the three most populous cities, and are usually related to criminal competition by rival gangs (Sturup, Gerell, and Rostami 2019). The targets of shootings are often criminals themselves: bystanders are seldom killed in the shootings, although there are exceptions. In one of the most violent episodes, gunmen in 2015 opened fire with automatic weapons on a restaurant in Gothenburg, killing a leader of a rival gang, but also a local boy, and injuring many others.

Information about the ethnic background of criminals is in itself highly politicized in Sweden, and statistics are not readily available. In a debate article in 2018, the leader of the Sweden Democrats wrote “the connection between immigration and crime has been so sensitive that it has been the subject of thought policing. They have covered up and kept statistics secret.” (Åkesson 2018). Surveys further reveal that the public’s confidence in news reporting is lowest specifically in the areas of immigration and crime, with a majority agreeing the media “doesn’t tell the whole truth about immigration”

(Andersson 2017).

Many of the criminal gangs that perpetrate shootings have a disproportionate number of members that are immigrants themselves, or are children of immigrants. In an investigation of 192 gang members in Stockholm, the newspaper Expressen found that 82 percent were immigrants or had two immigrant parents (Petersson 2017).

In terms of issue ownership, the Sweden Democrats are generally seen to have the best policy on immigration. On law and order, the Conservative party are strongest. Sweden Democrats here fare about as well as the Social Democrats (Johan Martinsson 2019).

2.4 Hypothesis

Several political actors have tried to politicize shootings in Sweden. But given the strong perceived connection between this type of violence and immigration, the issue ownership of the Sweden Democrats on the immigration issue and their strong stance on law and order, the hypothesis is that voters who live in close proximity to shootings will increase their support of the Sweden Democrats.

3 Empirical strategy and data

The main challenge to identifying the political effects of shootings is that they are not randomly distributed. Far from it - shootings related to crime to a higher degree happen in cities, and in disadvantaged neighborhoods (Sturup, Rostami, et al. 2018). Simply comparing areas where shootings took place to areas where they did not will undoubtedly result in biased estimates.

To solve this problem Hasin Yousaf (2018) use a difference-in-difference design, where counties that experience mass shootings are compared to other (similar) counties before

and after the shooting. Included in the analysis is also county and time fixed effects, meaning that all time-invariant county-specific factors are accounted for, as well as time-specific shocks or trend that are common to all counties. In order for the estimation to show an effect of shootings, counties that experience shootings should thus after the shooting deviate from their own pre-shooting average, in a way that is different from how the other counties developed. Affected counties would, in the absence of a shooting, have followed the same path. Moreover, Yousaf only includes counties that either have experienced mass shootings at some point, or are neighbours of them.

However, as noted by Yousaf, if the unobserved process that causes mass shootings is time-varying, the identifying assumption might not hold. Counties that experience shootings today may be different from counties that did so in the past. This might be very relevant in Sweden, given that shootings related to organized crime has increased in prevalence only the last decade (Sturup, Rostami, et al. 2018). Yousaf’s solution is to compare “failed” and “successful” mass shootings, and finds similar effects.

Several factors make the strategy employed by Yousaf inapplicable to this study. First, it requires data over a longer period of time: Yousaf has data for over a decade, with congressional elections every other years. The Swedish shootings data was only systematically collected from 2011, and general elections only happen every four years. The Sweden Democrats have also only been a political force to be reckoned with since 2006 or 2002.

Second, American mass shootings are decidedly more severe and violent than the Swedish ones investigated here; fortunately almost none live up to the threshold of four killed in one event. It is therefore likely that any effects of them will be more transient, and not certain to persist years or several years after the event. In contrast, in Yousaf’s

study, counties are treated as affected at all times after a shooting has taken place.

And finally, given the smaller intensity of the Swedish shootings, it is likely that effects will be more limited geographically. The lowest level at which election data is available is the precinct level, but the number of precinct and their boundaries are adjusted each election, making tracking them over time difficult.

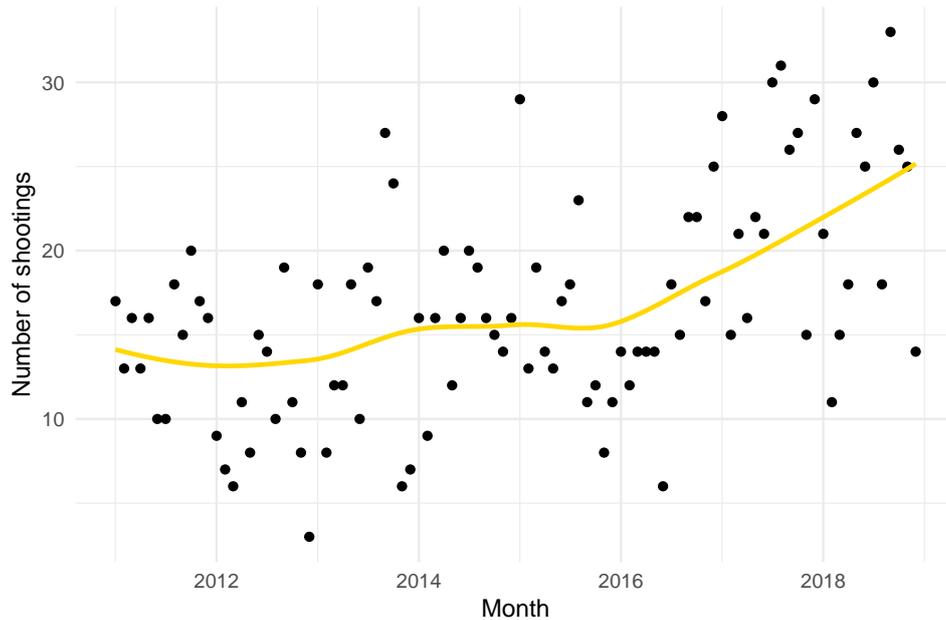
3.1 Identifying assumption

In this paper, I instead employ a pseudo-experimental design, using the timing of the shooting in relation to the election as leverage. The identifying assumption is that in a sufficiently small window of time, shootings are randomly distributed in time in relation to the election. Therefore, election precincts where shootings took place just before an election should be similar in all relevant aspects to precincts where shootings happened just after an election. But only in the former precincts can the violence be taken into account in the poll booth. Therefore, the latter precinct can be used as a control group. Below, I substantiate this assumption.

3.2 Data

The main challenge is to acquire data on shootings. No national databases of gun violence incidents exist, and crime statistics are not sufficiently granular either in time or in location. Therefore, I have requested lists of “confirmed shootings” from regional police departments in Sweden, which for the three major city regions are compiled since 2011, and have received the data for all the police districts in the southern part of Sweden, covering 74% of the population, including the three major cities. These record the date and location of each shooting, with location given in either coordinates (long/lat or RT90

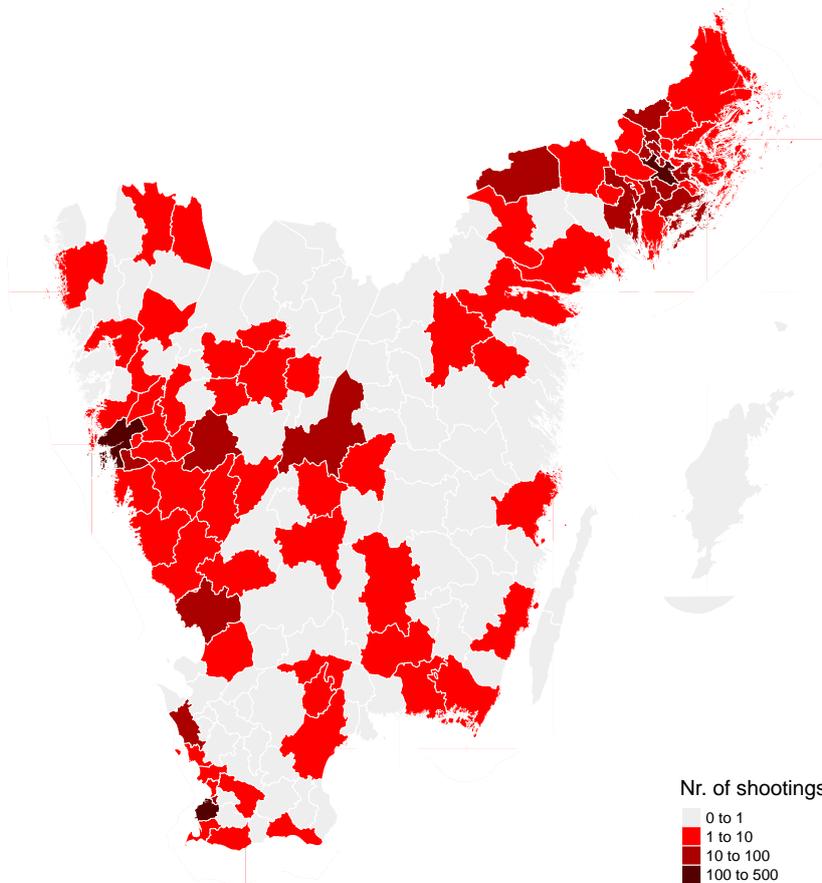
Figure 1: Number of shootings per month



or SWEREF coordinate systems) or adress. A problem for the sake of this study is that the locations are "washed" - the last digits of coordinates have been rounded off, and only the names of streets are given, not specific numbers. The reason is that the police wants to protect the identities of victims or bystanders affected by the shooting; precise locations could otherwise lead to specific addresses. Furthermore, sometimes it is not possible to ascertain the exact location of a shooting (Gerell 2018). As a consequence, it is not meaningful to look at effects within small radii around shootings, since the exact location is not known. In some cases, numbers of killed or wounded are given, but coverage is far from complete, and the data is therefore not useful. In total, there are 1591 recorded shootings up until January 1st, 2019. Figure 1, which shows the number of shootings per month, reveals the rising trend the last years.

Geographically, we can see that the shootings primarily are located to the regions surrounding the cities: Combined, the three most populous municipalities of Stockholm, Gothenburg and Malmö has had 995, 62.5% of the total. The shootings data is via GIS

Figure 2: Number of shootings per municipality



techniques assigned to the electoral precinct or zip codes in which they occurred.

3.3 Do voters notice shootings?

An important question is whether shootings make a mark in the public consciousness and the media. This study includes shootings where no one was killed or hurt, whereas much of the American research has focused on much more violent episodes, with multiple dead. It is thus far from certain that the Swedish electorate and media, while less desensitized, will take notice of the shootings in the dataset. If not, there is still a chance that shootings in the immediate vicinity of voters affects their attitudes. But if shootings generally “make the news,” the expectation that they will have political effects is stronger.

I have combined monthly data on the number of articles in Swedish print media that mention the words “skjutning” (shooting) or “skottlossning” (gunfire) with Google trends data on the intensity of searches on the same terms (calculated as the average of the two search terms). The Google data is measured as a scale from 0 to 100, where 100 represents peak interest in the term during the period January 2011 to December 2018.

Table 1: Analysis of the correlation between number of shootings, media reporting and Google searches in Sweden 2011-2018.

	Dependent variable:		
	Google	Media	Google
	(1)	(2)	(3)
Shootings	0.381** (0.162)	9.116*** (2.938)	0.156 (0.154)
Media			0.025*** (0.005)
2012	3.781 (3.404)	50.416 (61.587)	2.534 (3.073)
2013	4.095 (3.306)	-14.471 (59.814)	4.453 (2.975)
2014	3.871 (3.308)	-53.578 (59.842)	5.197* (2.989)
2015	9.069*** (3.307)	45.349 (59.834)	7.947*** (2.984)
2016	12.327*** (3.310)	-20.700 (59.882)	12.839*** (2.979)
2017	18.573*** (3.572)	59.863 (64.626)	17.092*** (3.229)
2018	24.228*** (3.487)	-0.213 (63.088)	24.233*** (3.136)
Constant	1.333 (3.386)	421.910*** (61.253)	-9.107** (3.785)
Observations	96	96	96
Adjusted R ²	0.557	0.135	0.641

Note: *p<0.1; **p<0.05; ***p<0.01

Table 1 shows the results of three regressions. In the first model, I regress the search

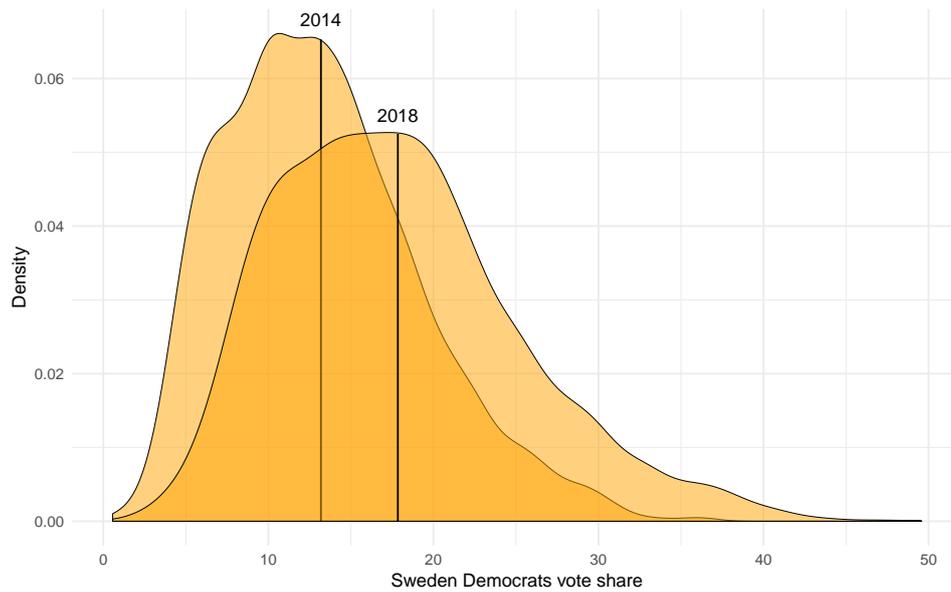
data on the number of shootings (as well as year dummies). We can see that there is an independent effect of the number of shootings each month on the intensity of google searches on the subject, even when controlling for the general time trend. Shootings are also related to the intensity of media reporting - each additional shooting is expected to result in an additional 9 articles on the subject. Finally, when analyzing Google searches, and including both media and actual shootings in the same model, the coefficient for number of shootings is halved, and only media reporting retains its statistical significance. Naturally, a large part of the effect of shootings on public conciousness is channeled through media's reporting. On the other hand, it is certainly so that media reports more on violent and gruesome shootings. So the coefficient for media could also primarily be picking up the severity of the shootings. Regardless, the analysis shows that the public do take notice of shootings, at least on the national level.

3.4 Election data

Election data is accessed from the Swedish election authority. They provide shapefiles over electoral districts, as well as vote tallies in each district, together with information about the number of eligible voters. From this data I calculate the vote share for the different parties, especially the Sweden democrats. Figure 3 shows the distribution of vote shares in the districts in the parliamentary elections of 2014 (leftmost distribution) and 2018 (rightmost). Reflecting the increase in national vote share from 5.6% to 10.2%, the rightmost distribution is located higher on the scale, and even reaches upward towards 50% in some districts.

I also calculate the proportion of women in the electorate, and a proxy for the proportion of immigrants. This last number is not directly available, but there is data on

Figure 3: Distributions and means of Sweden Democrat vote share in districts, elections 2014 and 2018



the number of foreign nationals that are eligible to vote in the local elections, for which the requirement only is permanent residence of three years, not citizenship. While far from perfect, it is likely that this variable is highly correlated both with the proportion of immigrants and the proportion with immigrant background.

A problem for identification when comparing different districts is that the level of violence may be an important factor in choices of where to live. Regardless of levels of demographic covariates, people with a preference for more security might actively choose to move out of the city. Otherwise similar areas might thus differ in the psychological makeup of the citizenry.

For instance, in a survey of Gothenburg in 2016, the respondents least likely to agree with the statement "Gothenburg is a safe city" lived in the wealthy suburb of Torslanda, at the coast. Respondents in Majorna-Linné, an area where crime rates are much higher, were 20 percentage points more likely to agree with the statement. However, with the design proposed here, all areas in the pseudo experiment are expected to have similar

levels of violence, on average.

The news consumption of citizens is also naturally influenced by their preexisting worries and priorities. Citizens worried about the consequences of immigration could very well select into more news about crime that can plausibly be linked to immigration, such as shootings. It is thus hard in cross-sectional designs to disentangle the effect of actual violent crime on support for the Sweden Democrats. Information about shootings in the immediate neighborhood that is the electoral precinct could in contrast travel by word of mouth.

3.5 Balance

The assumption that precincts affected by shootings before and after the election are similar becomes more credible the smaller the time window used, but the number of affected precincts also diminishes. In Table 2, I categorize the precincts into four categories, depending on whether they saw a shooting before or after the election, as well as neither or both. The final category is important to exclude from the treatment group: respondents there were affected by a shooting in advance of the election, but the precinct is also of a different type than those only affected by a single shooting.

Table 2: Number of observations depending on time window

	Shooting in relation to election:			
	Neither	Before	After	Both
14 days	8490	17	26	1
30 days	8451	35	44	4
60 days	8382	59	82	11
90 days	8308	102	113	11
120 days	8249	135	134	16

With the smallest time windows, the number of observations in each of the two relevant groups is too small for any credible analysis. A reasonable tradeoff here is the 90 days

window, in which there are more than a hundred precincts in each group. Still, the effect of a shooting on SD vote share will have to be substantial in order for the effect to be detectable at conventional levels of statistical significance, about 2.5 to 3 percentage points.

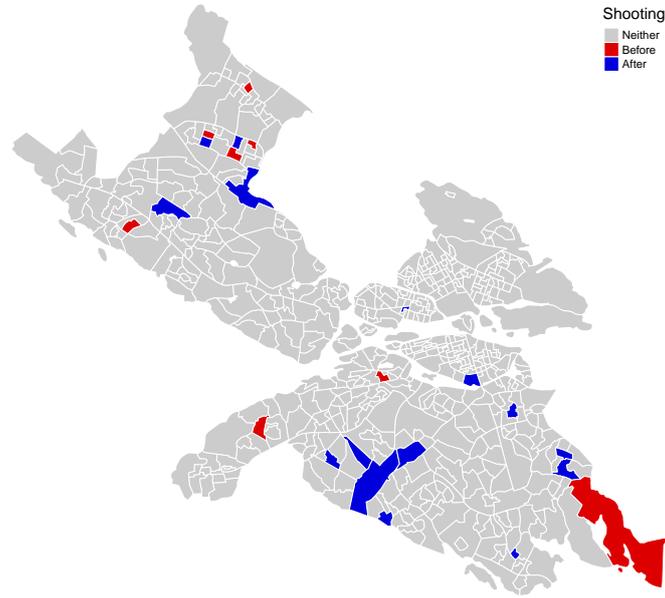
To test whether the precincts have similar characteristics, Table 3 presents mean values of five covariates: Population, area, percentage men, percentage foreign citizens and a dummy variable for whether there was at least one shooting in the district, in the 1000 days leading up to the start of the time window.

Table 3: Summary statistics

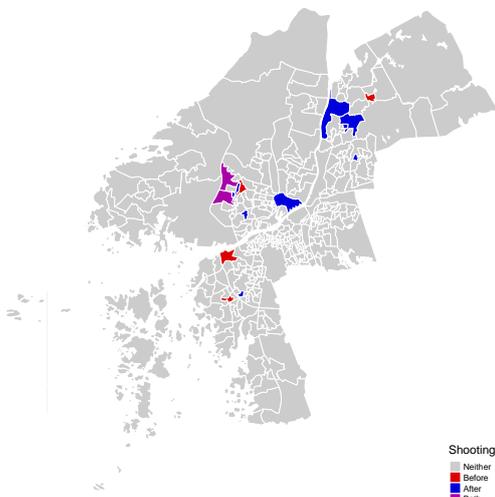
	Neither	Before	After	Both
n	7,488	102	113	11
Population	1,260.1	1,316.4	1,267.2	1,293.3
Km2	26.7	6.2	5.5	1.9
% men	49.3	48.6	48.7	49.6
% foreign	7	12.8	12.2	21.3
Previous shootings	0.1	0.4	0.5	0.5

In general, the “before” and “after” precincts are more similar to each other than to the “neither” or “both” precincts. For instance, the percentage of foreign citizens is lower in the “neither” precincts, which are more often located outside of the big cities, and higher in the “both” precincts, located in low-income areas of the big cities. None of the differences between the “before” or “after” districts is statistically significant. The maps of Figure 4 also show the seemingly random distribution of the before and after precincts, often being located very close to each other.

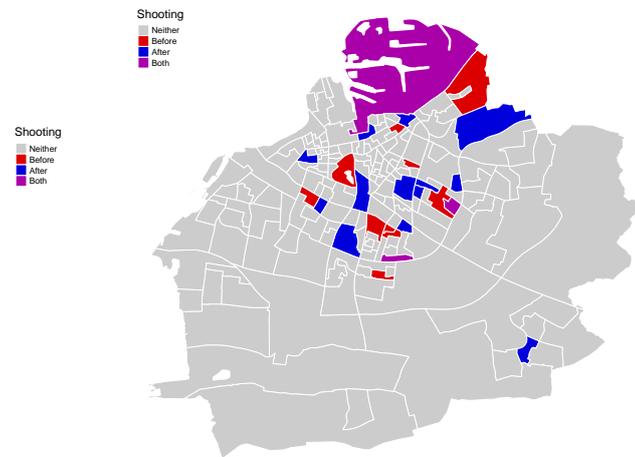
Figure 4: Maps of affected districts in big cities 2018



(a) Stockholm



(b) Göteborg



(c) Malmö

3.6 Estimation

The main analysis is a comparison, with OLS regression, with SD vote share as the outcome and exposure to shootings as the independent variable. More specifically, it takes the form of the following equation:

$$VoteSD_i = \beta_0 + \beta_1 Beforeorafter_{i,w} + \beta_2 Before_{i,w} + \beta_3 \mathbf{X} + \beta_4 \mathbf{T} + \epsilon_i \quad (1)$$

Where $Beforeorafter_{i,w}$ is an indicator that signifies that the precinct was exposed to a shooting in in time window w before *or* after the time of the election, and where $Before_{i,w}$ is an indicator that the shooting happened before the election. The first term thus signifies the precinct “being in the experiment” and the other that the precinct was “treated.” X is a vector of dummy variables for the municipality of the precinct, to account for local unobserved factors. T is a vector of dummy variables, one for each election. The analysis is estimated with OLS regression.

3.7 The Swedish Citizen Panel

In order to further understand how shootings affect voters’ considerations, I also analyze survey data from the Swedish Citizen Panel (Martinsson et al. 2019), a panel of respondents, mainly recruited through non-probability methods, surveyed regularly since 2010. New respondents are recruited continuously, and answers are given over the internet. Some general questions are asked regularly, including preferred party if there was an election today, worry about organized crime, and opinion on refugee reception. Respondents are also asked to describe the three most important current issues, in free text. I will analyze this text data for occurrence of certain words related to immigration and crime.

In order to determine whether respondents have been affected by a shooting in their vicinity, I combine the geographical shooting data with data on the postal code of the respondent. There are about 10,000 postal codes in Sweden, which means that they are more fine-grained than the roughly 5,000 electoral precincts. Unfortunately, however, respondents are generally only asked about their postal code of residence at the time of recruitment into the Citizen Panel, and the data will thus be inaccurate for respondents that move. Data exists on municipality of residence at each survey, but the municipal division is too coarse in order to be relevant for testing the hypothesis, at least in the larger cities where most shootings take place. To alleviate some of the problem, I restrict the analysis to respondents whose stated postal code is located in the municipality of residence at the time of the survey, even though this solution does not take care of the problem of respondents moving within municipalities.

This data will be analyzed using a similar design as the election data, but based on shootings' relation to the times at which respondents answered the survey. Respondents who answered before a shooting in their vicinity are the control group, and respondents who answered after are the treated group. The idea is, again, that the timing of the shooting is random in relation to the survey.

However, there are caveats when analyzing survey data with responses spread out in time. Importantly, survey answers generally flow in with a diminishing pace, with most surveys being answered immediately after it is sent out, or after reminders. This is the case also with the Citizen Panel. Therefore, dividing respondents temporally will on average result in more respondents answering a survey before a shooting, compared to after. Moreover, they are likely to be different in other respects; as noted by Munoz, Falco-Gimeno and Hernandez (2019), respondents that are harder to reach have a higher

probability of being interviewed after any event. I will therefore in the models control for the respondent’s age, gender, country of birth (Sweden or abroad), and whether there have been at least one shooting in the area before the treatment period. Table 3 shows the descriptive statistics for covariates over four categories of respondents, depending on the timing of the shooting in relation to the survey response.

Table 4: Summary statistics - citizen panel

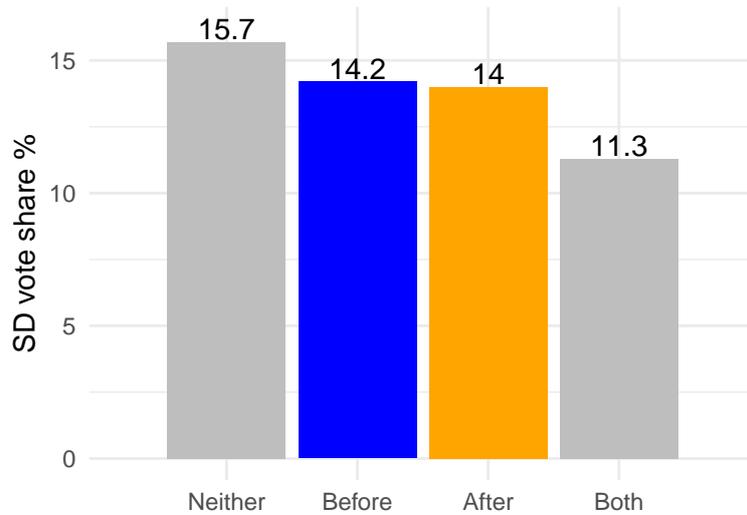
	Neither	Before	After	Both
n	46,531	286	364	29
Age	53.5	49.7	50.5	45.8
Woman	0.36	0.40	0.34	0.48
Educated	0.52	0.53	0.56	0.59
Foreign born	0.06	0.06	0.09	0.10
Previous shooting	0.05	0.75	0.22	0.62

A mitigating factor that might otherwise be present in studies of “unexpected events” (Muñoz, Falcó-Gimeno, and Hernández 2019) is that there are many shootings in the dataset, which reduces the risk that differences between the control and treatment group are driven by other contemporaneous events.

4 Results

As a first test, I in Figure 5 plot the average vote share of the Sweden Democrats in the four types of precincts, without any controls, averaged over both elections. The similarity between the before and after precincts is striking: in precincts that saw a shooting no more than 90 days before the election, SD received 14.2 percent, and in precincts with a shooting no more than 90 days after, SD received 14 percent. There is thus no substantial effect of shootings here, looking at the raw numbers. Nonetheless, a true effect might be hidden behind differences between municipalities or election years. I therefore proceed

Figure 5: Average SD vote share in four types of precincts



with regression analysis.

For the first analysis, I run five models. In the first model, the only independent variable apart from the year and municipality fixed effects is a dummy variable indicating that a shooting happened in the 90 windows preceding or following the election. The coefficient for the variable is negative, which shows that support for the Sweden Democrats is lower in areas that are usually hit by shootings. In Model 2 the “treatment” variable is added, the dummy for whether the shooting happened before the election. The coefficient for the variable is positive, as expected in the hypothesis, indicating that a shooting that happens before the election is associated with a 0.5 percentage points increase in support for the Sweden Democrats, but it is not statistically significant. The coefficient also changes very little in models 3 and 4, where additional covariates are added. The main consequence of introducing additional variables is that the coefficient for the “any shooting” variable becomes weaker, as we to a higher degree can control for the characteristics of the precinct. Notably, precincts with a higher share of immigrants both have more shootings and weaker support for SD.

Confidence intervals are quite wide - in the most basic model 2, it is for the Shooting before election variable -0.543 to 1.583. We can thus not conclusively determine that there is an effect, but it cannot be ruled out either.

Table 5: Regression results. Dependent variable: Sweden Democrats election result. 90 day window. District boundaries.

	(1)	(2)	(3)	(4)
Shooting happened	-0.474* (0.283)	-0.703* (0.378)	-0.605 (0.379)	-0.279 (0.360)
Shooting before election		0.505 (0.552)	0.487 (0.552)	0.582 (0.522)
Previous shootings			-0.451*** (0.175)	-0.107 (0.170)
Population				-0.001*** (0.0002)
Km2				0.022*** (0.001)
Perc. men				0.152*** (0.021)
Perc. foreign				-0.065*** (0.010)
Year	4.894*** (0.099)	4.896*** (0.099)	4.905*** (0.099)	4.911*** (0.094)
Observations	7,714	7,714	7,714	7,714
Adjusted R ²	0.686	0.686	0.687	0.720

Note:

*p<0.1; **p<0.05; ***p<0.01

Varying the time window in Table 6 does not substantially alter the conclusions. The estimated coefficient for the treatment variable however around 0.5 regardless of whether the time window is 60, 90 or 120 days. The shortest window, 30 days, yields a negative coefficient, but there are then fewer than 50 observations in each category (see Table 2) and the confidence interval ranges from -3.038 to 2.343.

Table 6: Main analysis with different time windows, District boundaries

	Window:			
	30 days	60 days	90 days	120 days
	(1)	(2)	(3)	(4)
Shooting	-0.013 (0.591)	-0.806* (0.428)	-0.636* (0.373)	-0.544 (0.341)
Before election	-0.535 (0.902)	0.400 (0.675)	0.503 (0.542)	0.443 (0.481)
Previous shootings	-0.417** (0.165)	-0.380** (0.167)	-0.427** (0.171)	-0.469*** (0.174)
Observations	8,534	8,534	8,534	8,534
Adjusted R ²	0.675	0.675	0.675	0.675

Note:

*p<0.1; **p<0.05; ***p<0.01

4.1 Interaction analysis

Two possible interactions are worth exploring. First, whether shootings have similar effects in districts that have different number of immigrants. If the number of immigrants is high, there is possibly little demand for an anti-immigration party to begin with. Second, inhabitants of districts that see many shootings are perhaps jaded, and affected less by an additional shooting than residents in areas which are unaccustomed to displays of violence. I therefore in Table 7 interact both the control and treatment variables with first the proportion of immigrants, and then the dummy variable for whether there was a shooting in the precinct in the preceding years. Neither of the relevant interaction terms for “Shooting before election” are statistically significant.

4.2 Other parties

To further explore the possible political consequences of the shootings, I set the election results of all the other parties represented in parliament as the dependent variable and apply the same model. All models include year and municipality fixed effects. Results

Table 7: Regression results. Dependent variable: Sweden Democrats election result. 90 day window. District boundaries.

	(1)	(2)	(3)
Shooting happened	-0.703* (0.378)	1.112 (0.688)	-0.186 (0.467)
Shooting before election	0.505 (0.552)	1.084 (1.049)	-0.110 (0.675)
Perc. foreign		-0.050*** (0.010)	
Previous shootings			-0.411** (0.183)
2018	4.896*** (0.099)	4.934*** (0.099)	4.904*** (0.099)
Shooting happened * Perc. foreign		-0.123*** (0.045)	
Shooting before election * Perc. foreign		-0.049 (0.069)	
Shooting happened * Previous shootings			-1.206 (0.794)
Shooting before election * Previous shootings			1.746 (1.169)
Constant	12.481*** (0.589)	13.016*** (0.596)	12.527*** (0.589)
Observations	7,714	7,714	7,714
Adjusted R ²	0.686	0.689	0.687

Note:

*p<0.1; **p<0.05; ***p<0.01

are presented in Table 6. While the dummy variables for any shooting, and shooting in the previous period show expected patterns - parties on the left do better in more violent areas - there are no significant effects of the treatment variable, shooting before the election. The strongest coefficients are for the Social Democrats (0.625) and the Sweden Democrats (0.503). Notably, the coefficient for the result on the conservative party is negative, in contrast to what one could expect if shootings raise the saliency of the law and order issue.

Table 8: Main analysis on all parties. 90 day window

	Party:							
	Left (1)	Soc. dem (2)	Green (3)	Centre (4)	Lib (5)	Chr. dem (6)	Cons (7)	Swe. dem (8)
Shooting	1.117*** (0.366)	6.970*** (0.875)	-0.362* (0.207)	-1.239*** (0.297)	-1.227*** (0.214)	-0.794*** (0.207)	-3.758*** (0.714)	-0.605 (0.379)
Before election	-0.165 (0.532)	0.632 (1.272)	-0.121 (0.301)	-0.131 (0.431)	-0.154 (0.311)	-0.050 (0.301)	-0.627 (1.039)	0.487 (0.552)
Previous shootings	2.073*** (0.168)	8.813*** (0.402)	-0.727*** (0.095)	-1.607*** (0.136)	-1.771*** (0.098)	-0.746*** (0.095)	-5.595*** (0.329)	-0.451*** (0.175)
Observations	7,714	7,714	7,714	7,714	7,713	7,714	7,714	7,714
Adjusted R ²	0.391	0.273	0.562	0.410	0.416	0.467	0.346	0.687

Note:

*p<0.1; **p<0.05; ***p<0.01

4.3 Citizen panel

A possible interpretation of the null findings is that these events might affect perceptions of crime or the consequences of immigration, without translating into different party choices. Election results can not tell us anything about whether that is the case. Therefore, I now turn to analysis of the Citizen Panel data. First, I run an analysis on the relationship between worry about organized crime (1 = “Not at all worried” to 5 = “Very worried”) and support for the different parties.

Table 9: Effect of crime attitude on party support. Municipality and survey wave fixed effects.

	Party:							
	Left (1)	Soc. dem (2)	Green (3)	Centre (4)	Lib (5)	Chr. dem (6)	Cons (7)	Swe. dem (8)
Worry crime	-0.056*** (0.003)	-0.010*** (0.003)	-0.008*** (0.002)	-0.004* (0.003)	0.0005 (0.003)	0.004** (0.002)	0.009*** (0.003)	0.082*** (0.003)
Age	-0.003*** (0.0004)	0.002*** (0.0004)	-0.001*** (0.0003)	0.0001 (0.0003)	0.001*** (0.0003)	-0.00003 (0.0002)	0.002*** (0.0004)	0.0002 (0.0003)
Woman	0.083*** (0.008)	0.042*** (0.008)	0.028*** (0.006)	0.028*** (0.006)	-0.021*** (0.006)	-0.021*** (0.004)	-0.049*** (0.008)	-0.115*** (0.008)
Educated	0.017** (0.009)	-0.023*** (0.008)	0.034*** (0.006)	0.026*** (0.006)	0.044*** (0.006)	0.008** (0.004)	-0.005 (0.008)	-0.096*** (0.008)
Foreign born	0.003 (0.017)	-0.006 (0.016)	-0.019 (0.012)	-0.033*** (0.013)	-0.011 (0.012)	-0.001 (0.008)	-0.003 (0.016)	0.033** (0.016)
Observations	8,900	8,900	8,900	8,900	8,900	8,900	8,900	8,900
Adjusted R ²	0.075	0.021	0.024	0.026	0.012	0.018	0.020	0.144

Note:

*p<0.1; **p<0.05; ***p<0.01

The results show consistent and strong effects of attitudes towards crime on support for the different parties, with the strongest effect being obtained for the Sweden Democrats: For each step up on the worry scale, support for SD is expected to increase with about eight percentage points. The question then is whether shootings increase the

worry about crime.

In Table 10 I replicate the main analysis on survey data, where stated support for the Sweden Democrats are regressed on the two variables indicating whether there was a shooting 90 days before or after the survey, and whether it happened before the survey. Thereafter, control variables are introduced in models 3 and 4.

Table 10: Main analysis with controls. 90 day window.

	Dependent variable: SD support.			
	(1)	(2)	(3)	(4)
Shooting	-0.006 (0.015)	0.008 (0.020)	0.007 (0.020)	0.003 (0.020)
Before survey		-0.033 (0.030)	-0.037 (0.030)	-0.034 (0.030)
Previous shooting			0.008 (0.008)	0.004 (0.008)
Age				0.001*** (0.0001)
Woman				-0.106*** (0.004)
Educated				-0.107*** (0.004)
Foreign born				0.020*** (0.007)
Observations	47,210	47,210	47,210	46,919
Adjusted R ²	0.018	0.018	0.018	0.060

Note: *p<0.1; **p<0.05; ***p<0.01

As with the analysis of the election results, we can see no significant effect of being exposed to a shooting in the postal code area on support for the Sweden Democrats; in fact, the coefficient is negative. Even though there were issues of balance related to the nature of data collection in surveys, the coefficient for the main variable is not affected by the addition of respondent characteristics as control variables in model 4: older people

and foreign-born support the Sweden democrats more, while women and the highly educated do so to a lesser extent.

Repeating the analysis on all the parties in Table 11, we can see that the traditional centre-right parties do worse in postal codes where there have been shootings within the 1000 days leading up to the survey, and the left party does better, reflecting the different demographical make-up of the different postal code areas. However, there are no reliable estimated effects of the “Shooting before survey” variable, with the possible exception of the Green Party. But the coefficient is only significant at the 90 percent level, and given the many tests in the table, should not be assigned too much importance.

Table 11: Main analysis on all parties. Citizen panel. 90 day window.

	Party:							
	Left (1)	Soc. dem (2)	Green (3)	Centre (4)	Lib (5)	Chr. dem (6)	Cons (7)	Swe. dem (8)
Shooting	0.038** (0.019)	-0.030 (0.021)	0.012 (0.012)	-0.017 (0.014)	-0.0004 (0.014)	-0.0001 (0.009)	-0.030 (0.020)	0.007 (0.020)
Before survey	0.019 (0.028)	0.050 (0.031)	-0.032* (0.018)	0.030 (0.021)	-0.028 (0.022)	0.009 (0.014)	0.026 (0.030)	-0.037 (0.030)
Previous shooting	0.055*** (0.007)	0.007 (0.008)	-0.004 (0.005)	-0.011** (0.006)	-0.024*** (0.006)	-0.012*** (0.004)	-0.031*** (0.008)	0.008 (0.008)
Observations	47,210	47,210	47,210	47,210	47,210	47,210	47,210	47,210
Adjusted R ²	0.019	0.018	0.013	0.010	0.011	0.008	0.013	0.018

Note:

*p<0.1; **p<0.05; ***p<0.01

4.4 Issues and salience

But the main advantage of the Citizen Panel data was that it allows us to dig deeper in the underlying attitudes. Given that the analysis in Table 9 showed that worry about crime increased support for the Sweden democrats, the results suggest that shootings have little effect on attitudes. But to be sure, I first analyze how shootings affect responses to

two survey questions: First, to what extent the respondent is worried about organized crime and second, what the respondent thinks of a proposal to accept fewer refugees in Sweden (1 = “Very bad” to 5 = “Very good”). Models are estimated with the same control variables as in the previous analysis.

Table 12: Issue analysis. 90 day window.

	Issue:	
	Worry crime	Fewer refugees
	(1)	(2)
Shooting	-0.032 (0.037)	-0.110*** (0.031)
Before survey	0.017*** (0.001)	0.012*** (0.001)
Previous shooting	0.076*** (0.018)	-0.482*** (0.015)
Age	-0.202*** (0.019)	-0.506*** (0.015)
Woman	-0.038 (0.036)	-0.085*** (0.029)
Observations	16,412	37,107
Adjusted R ²	0.075	0.135
<i>Note:</i>	*p<0.1; **p<0.05; ***p<0.01	

No discernible effects can be found of the treatment variable, and the coefficients are negative, in contrast to the hypothesis. The control variables in contrast yield expected effects: Older respondents are more worried about organized crime and want to accept fewer refugees, while the opposite is true for respondents with a college education. Women are more supportive of refugee reception, but also worry more about organized crime than men.

Finally, to see if shootings raise the saliency of issues of crime or immigration, I analyze the free text responses to the question of what the most important problems facing the

Swedish society are. To this end, I have created six dummy variables that check whether the respondent mentioned a specific text string in their response. The five words are “invandr” (immigr), “krimi” (crimi), “skjut” (shoot), “flykt” (refug), “migra” (migra) and, as a comparison, “rasism” (racism). Again, there are no statistically significant effects of the “shooting before survey” variable, lending support to the conclusion that an additional shooting catalogued in this dataset do not make much of an impact on voters and survey respondents.

Table 13: Word analysis. 90 day window.

	Word:					
	'invandr'	'krimi'	'skjut'	'flykt'	'migra'	'rasism'
	(1)	(2)	(3)	(4)	(5)	(6)
Shooting	0.007 (0.006)	-0.005 (0.003)	-0.0001 (0.0004)	-0.004 (0.004)	-0.001 (0.001)	0.003 (0.003)
Before survey	-0.016* (0.009)	-0.004 (0.005)	-0.0001 (0.001)	-0.004 (0.006)	0.002 (0.002)	0.003 (0.004)
Previous shooting	0.0004 (0.002)	0.001 (0.001)	0.00004 (0.0002)	0.003** (0.001)	-0.0004 (0.0004)	0.004*** (0.001)
Age	0.0001*** (0.00004)	-0.00004** (0.00002)	0.00000 (0.00000)	0.00002 (0.00002)	0.00002** (0.00001)	-0.0003*** (0.00002)
Woman	-0.012*** (0.001)	-0.004*** (0.001)	-0.0001 (0.0001)	0.005*** (0.001)	-0.0001 (0.0002)	0.005*** (0.001)
Educated	-0.018*** (0.001)	-0.003*** (0.001)	-0.0001* (0.0001)	-0.005*** (0.001)	-0.0005** (0.0002)	0.001 (0.001)
Foreign born	0.002 (0.002)	0.002 (0.001)	0.0002 (0.0002)	0.0005 (0.001)	0.0002 (0.0004)	0.001 (0.001)
Observations	95,393	95,393	95,393	95,393	95,393	95,393
Adjusted R ²	0.045	0.011	0.001	0.018	0.0001	0.013

Note:

*p<0.1; **p<0.05; ***p<0.01

5 Conclusion

When a young mother, holding her baby, was shot point blank in the head in the middle of the street of Malmö in August 2019, there was a public outcry of anger and frustration. Many politicians expressed their dismay, but one of the more noteworthy quotes came from the party leader of the Sweden Democrats, Jimmie Åkesson. Accused by an opponent of trying to politicize the event, Åkesson stated that "This is something my party has warned of for a long time. I have the right to point fingers, and I intend to do so."

Given the strong emphasis of the Sweden Democrats and other "radical right" parties on crime and issues of law and order, it is reasonable to expect that incidents of public violence will increase the saliency of the issue and support for these parties.

In this paper, I have leveraged a pseudo-experimental design, where election precincts and survey respondents exposed to shootings in the vicinity in advance of an election or survey are contrasted to counterparts that were exposed to similar incidents, but after the election or survey. Somewhat surprisingly, shootings does not appear to impact neither election results, nor do they impact expressed support for parties, nor attitudes towards crime or immigration, and does not increase the saliency of the same issues.

Methodological explanations for the null finding might be low power in the analyses: about 200 precincts and about 500 survey respondents constitute the identifying observations. There might be effects, but too weak to be detectable. Another possibility is that not all shootings have an effect, only the ones where someone is killed. Data on fatalities is however incomplete, and fatalities are likely too few to work in the design. Alternatively, it might be the case that none of the shootings fail to make an impact on a desensitized public: several studies have found only weak effects of horrific mass murders in the United States.

A more substantial explanation is instead that voters are affected by shootings, but not specifically those that happen in their vicinity, but rather the high-profile events such as the murder of the mother described at the beginning of this section. Some incidents become highly publicized and fuel political debate, others pass under the radar. For instance, the analysis of Google search data and media coverage indicated that media reporting on the subject was a more important determinant of search interest than the actual number of shootings. Similarly, even though previous research has demonstrated that victimization is a strong determinant of fear of future crime, fear of crime in groups is inversely related to victimization rates of the group as a whole (Rueda and Stegmüller 2016).

All in all, the results of this paper demonstrate that while crime is an important part of the rhetoric of radical right parties, it is far from certain that actual crime is an important determinant of support for said parties.

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